Social Networks and the Dynamics of Labor Market Outcomes: Evidence from Refugees Resettled in the U.S. *

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Abstract

This paper examines the dynamic implications of social networks for the labor market outcomes of political refugees resettled in the U.S. Using a theoretical model of job information transmission within social networks, the paper shows that the relationship between the size of a social network, the vintage of network members and labor market outcomes is non-monotonic. To test this prediction, I use an empirical strategy which exploits the fact that resettlement agencies distribute refugees across cities, precluding individuals from sorting into locations. The results indicate that an increase in the number of social network members resettled in the same year or one year prior leads to a deterioration of labor market outcomes, while a greater number of long-tenured network members improves the probability of employment and raises the hourly wage for newly arrived refugees.

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1 Introduction

Social networks are generally viewed by economists as a partial solution to information problems or other market frictions. However, their role in the labor market is more difficult to assess. This paper examines one mechanism through which social networks affect labor market outcomes, job information transmission, and shows that the dynamics between changes in network size and labor market outcomes lead to heterogeneous effects across network members. Using new data on refugees recently resettled in the U.S., the paper provides empirical evidence on the relationship between changes in the size of a network, the tenure of its members, and labor market outcomes.

The empirical literature has shown that informal search methods play a major role in the labor market: between 30 to 60% of jobs in the U.S. are reported to be found through informal social network contacts (Ioannides and Loury, 2004). The existing evidence suggests that information transmission and job referrals within networks improve outcomes. For example, Munshi (2003) shows that among Mexican migrants to the U.S., an exogenous increase in the size of a social network significantly increases its members' probability of employment and the probability of employment in a higher-wage industry.

Relatively little attention has, however, been paid to within-network competition for job information. Calvo-Armengol and Jackson (2004) develop a model of the labor market which suggests that the structure of a social network affects persistence in unemployment levels within the network, and that initial differences in employment levels across networks can lead to long-run inequality between groups. They also show that in the short-run, there is a negative correlation in employment outcomes between some individuals within a network since they compete for a finite number of known jobs. In this paper, I extend their approach to analyze the short-run labor market implications of this competition effect when social network size changes over time. As in Boorman (1975) and Calvo-Armengol and Jackson (2004), I assume that individuals have a random probability of receiving job information, and this information is either used to obtain a job or passed on to an unemployed member in the individual's social network. This basic structure is embedded into an overlapping generations framework.

The model predicts that, depending on the vintage of other network members, having access to a larger network may lead to a deterioration of individuals' labor market outcomes due to competition among unemployed members for job information. The competition effect arises not because of an increase in labor supply in the face of fixed demand but occurs even when the probability of receiving job information is constant irrespective of network size. This implies a non-monotonic relationship between the size of a social network and labor market outcomes, as a function of network members' tenure in the market. Changes in social network size will differentially influence labor market outcomes over time: an increase in the size of a given cohort will first decrease the employment rate and average hourly wage of cohorts who arrive close in time to the large cohort, but will improve outcomes for those cohorts that arrive sufficiently later. Competition over job information within the network, which negatively impacts network members' outcomes, can, in the short run, mitigate the network's ability to overcome labor market imperfections.

The job information transmission model presented in this paper predicts that wage offers follow the same pattern as the probability of employment. However, the analogous prediction for wages of those who are employed only holds for certain parameter values. The network has an effect on average wages through two mechanisms: first, the network influences the number of job offers an individual receives. This affects the wage since an individual's wage is increasing in the number of offers he receives. However, wages of jobs obtained through passed job information are lower on average than those obtained directly. This creates a second mechanism since a change in the size of the network also changes the fraction of network members who are employed through the network. This latter effect acts to offset the former. The empirical analysis therefore evaluates both the impact of an increase in network size on wage offers, including wages of zero for the unemployed, as well as the employed sample. Supplemental analysis using LAD estimation is also presented to check for sensitivity from censoring of wages. Comparing these two results provides some insight into the relative importance of these two effects.

In order to test this prediction empirically, I compiled a data-set on refugees resettled in the U.S. between 2001 and 2005 using administrative records from the International Rescue Committee (IRC), a large resettlement agency. A unique aspect of the resettlement process is that refugees without family in the U.S. do not choose their destination city. The resettlement of refugees is implemented by voluntary resettlement agencies that have been contracted by the State Department to provide initial services such as housing, financial assistance and job training/job referrals. The sample of refugees analyzed in this study comprises of those whose geographic location was selected by the IRC.

The empirical strategy exploits this institutional feature. The resettlement process precludes individuals from sorting into localities based on unobservable individual characteristics. Furthermore, all individual characteristics used by the IRC when placing refugees into particular cities are available in the data. This addresses the classic identification problem of sorting: when individuals select their place of residence, it is difficult to distinguish the role of networks from other common unobservable characteristics.[1] In the primary analysis of the paper, a social network is defined as the number of non-family reunification refugees from the same country of origin who are resettled by the IRC in the same city. Variation in network size is therefore uncorrelated with unobserved individual characteristics since I can control for factors that affect refugees' location. Variation in the relative size and structure of refugee social networks across cities and ethnic groups over time is then used to examine how networks affect labor market outcomes. To address the possibility that the resettlement agency makes placement decisions based on unobserved ethnic group and city level factors, I include in the empirical analysis city-year, ethnic group-year and city-ethnic group fixed effects.[2]

The empirical analysis using refugees resettled in the U.S. shows that an increase in network size has heterogeneous effects across network members, creating both negative and positive ramifications for

[1] This is one of the "Reflection" problems isolated by Manski (1993).

[2] For example, this unobserved match quality could be due to cross-city differences in returns to skills which are more common among certain groups. If the IRC knows this match quality and locates refuges accordingly, the estimates on the effect of changes in network size will be biased.

employment outcomes. I find that a one standard deviation increase in the number of network members who arrive in the U.S. one year prior lowers the probability of employment for a new arrival by 4.9 percentage points. This indicates that the within-network competition effect is economically significant. Conversely, as predicted by the model, an increase in the number of tenured network members improves the labor market outcomes for recently arrived refugees. An analogous increase in the number of network members who have two years tenure in the U.S. increases the employment probability by 4.6 percentage points.

The identifying assumption is that network size, conditional on individual characteristics, time variant city, time variant ethnic group and city-ethnic group controls, is uncorrelated with unobservable characteristics. The city-ethnic group fixed effect addresses the principle concern with the empirical strategy: comparative advantage. I additionally provide several robustness checks to address other concerns with this assumption. For example, I find that a refugee's labor market outcomes are unaffected by an increase in the number of family reunification refugees resettled in the same city.

The analysis also shows that average hourly wages follow the model's prediction.[3] A one standard deviation increase in the number of refugees who arrived one year prior decreases the average hourly wage of a new arrival by \$.70. An analogous increase in the number of refugees who arrived two years prior increases the wage by \$.50. Among those who are employed, there is a strong positive effect from an increase in the number of senior network members on wages but an insignificant impact from changes in the number of network members who arrived in the current or previous year. This suggests that by providing job information, networks affect wages both through the employment rate but also by affecting job quality once jobs are found.

An additional implication of the model is that a decline in the arrival rate of job opportunities weakens the influence of the network on labor market outcomes. I exploit the natural experiment of September 11, 2001 to evaluate the response of social networks to an exogenous negative labor market shock. The empirical analysis suggests that the 9/11 shock did in fact weaken the within-network competition effect and dampen the positive information effect from an increase in the size of a newly arrived refugee's social network.

Overall, this paper argues that evaluating the composition of a social network within a dynamic context is necessary to accurately assess the role of social networks in the labor market. A static analysis of network effects, as in using the stock of immigrants as the relevant network measure, is likely to miss important heterogeneity in the way network-based job information flows influence outcomes. In some cases, as demonstrated in this paper, estimating the static effect of an increase in the total size of the network, inclusive of all cohorts, will mask the presence of network effects completely. The dynamic approach also facilitates the interpretation of the results as evidence of job information transmission as it corresponds directly to a theoretical model predicting a specific non-monotonic relationship between network size and outcomes.

[3] The result is specifically for average hourly wages of the entire sample, including wages of zero for the unemployed.

2 Literature Review

Information transmission and job referrals are salient mechanisms discussed in both the theoretical and empirical literatures on social networks. Bayer, Ross, and Topa (2005) find that individuals who live on the same city block are at least 50% more likely to work together than those living on adjoining blocks. They argue this is evidence of network-based job referrals. Topa (2001) argues that information spill-overs explain the clustering of unemployment across Chicago neighborhoods.

Munshi (2003) motivates an empirical investigation of network effects among Mexican migrants to the U.S. with a model of employer referrals similar to that of Montgomery (1991). Using exogenous variation from rainfall shocks in Mexico to predict network size, he finds that a larger number of senior network members increase the probability of employment. However, the effect of recently arrived network members (those who arrived within the past 3 years) is small, positive and statistically insignificant. These results support the notion that network size is not the only relevant measure to capture how a network influences the economic outcomes of its members, and that social networks may influence the labor market differently depending on their structure. Moreover, the results suggest the possibility of a competition effect between very recent arrivals, resulting in an insignificant effect from an increase in the number of network members who arrived in the previous 3 years. This therefore serves as motivation for investigating the empirical relevance of within-network competition.

Also suggestive of competition is the work by Wabha and Zenou (2005). They show that among the employed, the probability of finding a job through a social network is concave with respect to population density, a proxy for the size of the social network. It is therefore possible to cross the threshold level of network size where the probability of finding employment through the network declines. My paper therefore seeks to provide direct evidence that an increase in the size of a network, even within a small network, can lead to a deterioration of labor market outcomes.

The availability of wage data is relatively rare in the empirical literature on social networks. For example, Laschever (2005)[4] only has data on employment outcomes, and Munshi (2003) only has coarse information on the occupation in which Mexican migrants were employed. While the literature has generally found positive effects on employment outcomes, the results are mixed for wages (Ioannides and Loury, 2004). The difficulty is that different models of networks will lead to different predictions on wages (Mortensen and Vishwanath, 1994; Bentolila et al., 2004). It is therefore important to specify the wage equation to correspond directly to the relevant theoretical model. My paper seeks to isolate the specific mechanism of job information transmission and is careful in distinguishing between the model's predictions for employment outcomes, wages conditional on employment, and wages in the full sample.

The estimation strategy in this paper contrasts with that used in some of the existing empirical literature. Edin et al. (2003), for example, estimate the impact of an increase in ethnic concentration, as

[4]Laschever (2005) provides evidence that social networks affect the post-war employment outcomes of American World War I veterans, using the war draft as an exogenous source of variation in an individual's social network.

measured by the stock of immigrants in a municipality, on earnings among refugees in Sweden. They find a positive association between concentration and earnings among less skilled immigrants. This effect is muted for individuals with "lower quality" networks, as measured by income. Borjas (2000), however, shows some evidence that residential segregation, as measured by the proportion of individuals within a city from the same country, has a negative impact on assimilation for refugees in the U.S. The static approach used in these two papers can provide a valuable estimate of the long run total effect from immigrant clustering, which I am unable to address, but can not distinguish between numerous mechanisms, including job information transmission and human capital externalities. This becomes problematic for external validity, as highlighted by the differing conclusions from these two papers.

In the following section, I present a theoretical framework of job information transmission within social networks. Details on the institutional background and data are provided in Section 4, and Section 5 discusses the empirical strategy. The results of the empirical analysis on both the probability of employment and wages are presented in section 6. Section 7 explores how the exogenous shock of 9/11 impacted the role of social networks in refugee communities. The paper concludes in section 8 and relates the findings to the broader literatures on the impact of immigration on wages and optimal refugee resettlement policy.

3 Theoretical Framework

3.1 A Model of Employment Rates

The theoretical framework is an extension of the model developed by Calvo-Armengol and Jackson (2004) and Boorman (1975), incorporated into an overlapping generations setting. The objective of Calvo-Armengol and Jackson's work is to show that in the steady-state, there is positive correlation of employment outcomes across time and across all agents within a network. By endogenizing labor market participation, they show that small initial differences in employment rates across networks can lead to persistent levels of inequality.

By embedding this model into an overlapping generations framework and analyzing the short-run dynamics from changes in cohort size, I generate concrete predictions which can be tested empirically. To do this, I make the simplifying assumption that all individuals within a network are connected, thereby eliminating the distinction made by Calvo-Armengol and Jackson between direct and indirect connections.

The basic structure and timing of the model is as follows: each agent lives and works for S periods, each cohort c has N_c agents. If agent i in cohort c is employed at the end of period t, then $s_{ic}^t = 1$ and accordingly $s_{ic}^t = 0$ if agent i is unemployed. Since all agents within a cohort are identical, it is preferable to work with the employment rate within the cohort at time t, denoted as s_c^t. There is a positive probability for any employed agent to lose his job at the very beginning of the period at the exogenous breakup rate b. Information about job openings then arrive: any agent hears about a job opening with probability a, and the job arrival process is assumed to be independent across agents.

Since each individual receives information directly with probability a, the total number of jobs available in the economy is scaled up as the size of the network increases. I therefore assume that the size of the network is small compared to the entire economy. The advantage of this approach is that it enables the model to isolate the network effect directly. This assumption also reflects the empirical setting in which the predictions will be tested. The average cohort size in the sample of refugees used in this paper is less than 30 (see Table (2)). Since the resettlement locations are medium-sized cities, including cities such as Atlanta, Phoenix, and Salt Lake City, a change in the number of refugees arriving in each city in a given year is unlikely to have any general equilibrium affect on the job arrival rate or the distribution of wages.

If an agent is unemployed and receives job information, he will fill the position. However, if the agent is already employed, he will pass along the information to a randomly selected network member who is unemployed. Job information is shared with equal probability to any unemployed member in the network, regardless of which cohort he belongs to. Once job information arrives and is referred to unemployed members where suitable, jobs are immediately accepted.

This structure can be formalized in the following way:

$$s_c^t = a + r^t \qquad \text{if } c = t \tag{1}$$

$$s_c^t = (1-b)s_c^{t-1} + (1-(1-b)s_c^{t-1})(a+r^t) \qquad \text{if } c \le t \le c + (S-1) \tag{2}$$

$$r^t = (1-b)\sum_{k=t-S+1}^{t-1} N_k s_k^{t-1} \frac{a}{\sum_{k=t-S+1}^{t} N_k - (1-b)\sum_{k=t-S+1}^{t-1} N_k s_k^{t-1}} \tag{3}$$

where r^t represents the probability of receiving job information through an employed network member. The probability of being employed for an individual entering the market is simply the probability of receiving job information directly, a, plus the probability an already employed network member passes him information, captured by the term r^t. The probability of receiving job information from the network, r^t, is the total number of jobs which are "available" in the network to be passed, divided by the number of potential recipients. The number of available jobs is the number of employed individuals in the network multiplied by the probability that each receives job information randomly. The number of recipients of job information in a given period is the number of individuals who are unemployed at the beginning of that period, after the exogenous break-up has occurred. For cohorts who have previously been in the market, the probability of being employed is the probability of having a job in the previous period and keeping it, at rate $1-b$, plus the same probability of becoming employed as the new cohort, weighted by the probability of being unemployed.

This simple model can be used to show a couple of predictions which can be tested empirically.

Proposition 1 *For all values $0 < a < 1$ and $0 < b < 1$, an increase in cohort size N_c decreases s_p^c for all p.*[5]

[5]This claim holds for all values of a and b such that $s_p^c \neq 1$ for all p and j.

Proof: See appendix.

The intuition is that since s_p^{c-1} does not change, increasing N_c only increases the number of unemployed individuals seeking job information from network members while leaving the number of employed members unchanged. The result is a decline in the employment rate for both the cohort which is made exogenously larger as well as all other cohorts in the market in that period. The effect is present despite the fact that any individual in the market has the same probability of hearing about a job directly as prior to the increase in N_c. This highlights the fact that this competition effect arises from the dynamics within the network since the larger market conditions remain constant.

Proposition 2 *The impact of an increase in* N_c *on* s_k^k *is monotonically increasing between* $k = c$ *and* $c + S - 1$.

Proof: See appendix.

Proposition 1 shows that the initial effect of increasing cohort c is to decrease the employment rate for that cohort. Proposition 2 shows that the impact on the subsequent cohort, $c+1$ is less negative than on c itself. Likewise, $\frac{\partial s_{c+2}^{c+2}}{\partial N_c} > \frac{\partial s_{c+1}^{c+1}}{\partial N_c} > \frac{\partial s_c^c}{\partial N_c}$ for the case when $S = 3$. The idea is that as cohort c gains experience in the labor market, its employment rate rises. Its negative impact on employment is then mitigated over time.

In fact, numerical analysis of the model shows $\frac{\partial s_k^k}{\partial N_c} > 0$ for at least $k = c + S - 1$ and usually earlier cohorts. That is, an increase in the size of cohort c first negatively impacts cohorts who arrive close in time to period c but then increases the employment rate for cohorts who arrive sufficiently later.[6] As cohort c's employment rate increases over time, its larger size becomes an asset to the entire network. To illustrate this and the predictions outlined in Propositions 1 and 2, Figure (1) provides an example. The graph shows a comparison in the employment rates of a control network with constant cohort size and that of a treatment network in which the size of cohort c is doubled. All subsequent cohorts after c have the same size as the control cohort. Each agent is in the market for 4 periods. The treated cohort, c, experiences a lower employment rate in their first period in the market, but by period 4, the larger cohort size leads to a slightly higher employment rate. s_{c+1}^{c+1} is represented as "Cohort c+1" in time period 1 in Figure (1). Similar to the pattern displayed by cohort c, the initial employment rate is lower than it would have been in the absence of the cohort size shock, but this effect is largely gone by cohort $c+1$'s second period in the market. In fact by time period 3, the cohort reaches a higher employment rate than the counterfactual cohort. The following cohorts, $c+2$ and $c+3$, both receive gains in the employment rates for all 4 periods these cohorts are in the market.

[6] The positive effect on later cohorts, according to the numeric analysis, holds for all parameter values even though the analytic results only show a monotonically increasing effect.

3.2 A Model of Employment Rates and Wages

Subsequent work by Calvo-Armengol and Jackson (2006) analyzes a more general model which includes stochastic wages. In this model, job information that arrives exogenously also includes a wage. This leads to different behavior than in the above model. An employed individual will now switch jobs if he receives job information with an offer wage higher than his current wage. The implications of this more general framework is that in the steady-state, information passing leads to positive correlation between the employment and wage status of agents who are connected by a social network. There is again, though, the possibility of a negative correlation in wages across certain agents due to within network competition.

I incorporate wages into the overlapping generations framework used above in the following way: with probability a, an individual receives job information which now also contains a wage. If the individual who receives the job information is unemployed, he takes the job. However, if the individual is employed, he accepts the job if $w^o_{ict} > w_{ict}$, where w^o_{ict} denotes the offer wage from the new job information received by employed individual i. Alternatively if $w^o_{ict} < w_{ict}$, the offer is passed to a randomly selected unemployed network member. Wages are *iid* draws from the uniform distribution $w \sim U[\underline{w}, \bar{w}]$. w^c_c denotes the average wage for employed network members in cohort c in period c.

The analysis of the model is done by simulation. I present here one numerical example. Figure (2) reflects the results of simulating the model with $a = .40$, $b = .05$ and agents working in the market for 5 periods, i.e. $S = 5$. Wages are distributed $w \sim U[5.15, 45.15]$, where $\underline{w} = 5.15$ reflects the minimum wage law. The thought experiment here is to triple cohort size N_c and evaluate the effect on employment rates and wages of cohorts c, $c+1$, $c+2$, and $c+3$. As in Figure (1), all cohorts except c are the same size. Both the employment rates and the average wages of cohorts c and $c+1$ are lower in the first period than the levels that would have been achieved under the counterfactual. The effect on cohort $c+2$ in its first period in the market, however, is close to zero while cohorts $c+2$ and $c+3$ show initial gains from the increase in cohort c.

In this model the effect on wages and employment is more subtle than in the simpler model with constant wages. For a given employment rate, the job information available in the network for unemployed members is diminished, since employed network members with low wages are unlikely to provide information to the unemployed. The only jobs which are passed are those with sufficiently low wages that the employed network member who initially receives the job information rejects the offer. This also implies that individuals who become employed through passed job information will have wages that are lower on average than those who become employed by receiving job information directly.

There are therefore two effects working in opposite directions on the average wages of the employed. The primary effect of an increase in network size is to change the number of job offers an individual receives, thereby affecting the wage. However, the offsetting effect is due to changes in the proportion of individuals who receive their jobs directly versus indirectly, i.e. through the network. Since these jobs have wages which are on average lower, this can counteract the primary effect.

There is thus the possibility that wages do not follow the same pattern as in the above example for all parameter values. An increase in N_c may lead to a lower proportion of jobs being attained through the network. Since these jobs have lower wages on average, the average wage of those who are employed may actually increase. It is possible to have within-period increases in the wage due to an increase in N_c.

There is therefore no general prediction with regards to wages of the employed. An analogous Claim to the Propositions shown for the model with constant wages does hold, however, for employment rates and wages in the entire network.

Claim 1 *For some values of (a,b) and an increase in N_c, there exists $\tilde{k}$ such that $\forall k \leq \tilde{k}$, $\frac{\partial w_k^k}{\partial N_c} < 0$ and $\frac{\partial s_k^k}{\partial N_c} < 0$. For $p > \tilde{k}$, $\frac{\partial w_p^p}{\partial N_c} > 0$ and $\frac{\partial s_p^p}{\partial N_c} > 0$.*

where here w_c^c represents the average hourly wage of the entire network, including a wage of 0 for those who are employed. The offsetting effect from changes in the composition of network members obtaining jobs through direct versus indirect channels is not strong enough to change the prediction regarding wages of the entire network.

The model therefore predicts that employment and wage rates will be inversely correlated with the number of recently arrived refugees, but positively correlated with the number of senior network members. The striking part of the prediction is that the deleterious effect from an increase in network size does not come from an increase in labor supply with a fixed labor demand. Instead, the negative effect comes from competition between network members for information provided by already employed individuals. It is a within-network information competition effect and not a result of an increase in labor supply driving down wages or employment rates in equilibrium. The assumption that each individual faces a constant rate a of hearing about a job directly ensures that labor demand is held fixed, so that the latter effect is not driving the model prediction.

4 Institutional Environment and Data

4.1 Refugee Resettlement Process

The United States has a long history of refugee resettlement, having accepted around 2.4 million refugees and asylees since 1975. Since 1996, over 500,000 refugees and asylees have been admitted. Refugees come from a wide variety of countries and flee their homes for different reasons, from war-related violence to religious persecution to retribution for political views. The process through which refugees gain access to the U.S. creates a unique opportunity to look at the role of ethnic networks. Limited research has looked at the economic performance of refugees in the U.S., largely due to data constraints.[7] Refugees are a well-defined

[7] Two exceptions are Cortes (2004) and Borjas (2000). Cortes (2004) uses Census data to create an approximate sample of refugees and argues that refugees perform worse relative to other immigrant groups in the short-run, but they eventually catch up and even surpass the other groups. Work on refugees outside the U.S. includes that of Gould, Lavy, and Paserman (2004), who use flows of Ethiopian refugees into Israel to estimate the impact of school quality on educational outcomes.

group. According to Immigration and Nationality Act (INA) Section 101: a refugee is

> any person who is outside any country of such person's nationality...who is unable or unwilling to return to...that country because of persecution or a well-founded fear of persecution on account of race, religion, nationality, membership in a particular social group, or political opinion.

Refugees are distinct from asylees in that refugees' status determination occurs overseas. Asylees, by contrast, travel by their own means to the United States and then apply for protected status upon arrival.

How does one become a refugee? The president, after consulting Congress, sets designated nationalities and processing priorities each year which fit into the predetermined ceiling for total refugee admissions levels. The Bureau of Population, Refugees, and Migration (PRM) of the State Department develops the application criteria and specific admission levels, while INS officers adjudicate individual cases in refugee processing centers around the world. Often these centers are within refugee camps, although individuals can also apply for refugee status in local U.S. embassies. Once the INS designates an individual as having refugee status, the PRM is responsible for overseas processing and transportation to the U.S.

The PRM's final role in the resettlement process is to allocate all accepted cases to one of ten contracted voluntary resettlement agencies. The resettlement agencies are responsible for acquiring housing, providing initial benefits including cash assistance and in-kind support, as well as providing access to resources such as ESL training and job assistance. This makes estimating the effects of social networks on labor market outcomes among refugees resettled in the U.S. a particularly interesting case, since the mechanism through which these networks operate can be pinpointed. Since refugees are provided with housing and some initial financial assistance, the potential intervention by the social network is more limited than for other groups. I use data from one voluntary resettlement agency, the International Rescue Committee (IRC), who resettles approximately 12 percent of all refugees and asylees.[8] In this paper I look specifically at individuals who are granted refugee status directly, excluding both asylum seekers and refugees who attained admittance via family reunification. For these individuals, the IRC has the sole discretion in determining where the refugee will be resettled among its 16 regional offices. The IRC receives information from the State Department about each refugee's characteristics, including basic information such as country of citizenship plus demographic information including age, gender, marital status and education. With this information, the IRC decides to send each refugee or refugee family to one of its 16 regional offices. It is important to note that no IRC employee meets the refugee or his family members until the allocation process has been completed, which is generally within one week of the State Department contacting the agency. The refugee travels directly from his place of residence overseas to the chosen IRC regional office within the U.S.

[8]The IRC is allocated refugees on a weekly basis. Each week, family reunification refugees are allocated to the resettlement agency which has an office closest to the receiving family. For non-family reunification refugees, the organizations are allowed to choose refugees or refugee families in a round-robin fashion. The order of selection each week is chosen at random. Only the most basic information, including the number of family members, age and country of origin, is provided during the selection process.

4.2 Placement Policy

The IRC does not have an explicit placement rule when distributing refugees across regional offices, although they do follow a few general guidelines. First, the IRC seeks to place refugees in locations where there is the presence of a pre-existing ethnic or nationality-based community. They also attempt to choose a regional office based on language competencies. The goal is to send each refugee to an office which has either a staff member of a volunteer who speaks the same language as the refugee. Individual refugees or refugee families who have special medical problems, such as HIV, are only sent to particular offices which specialize in such cases.

In addition to policies oriented towards achieving a good match between an individual refugee and a city, the IRC also budgets for the total number of refugees expected to arrive in each regional office. To do this, each regional office is budgeted a total number of people per year plus a target for non-family reunification refugees. These numbers are estimated using projected numbers from the State Department on how many refugees are expected to be admitted to the U.S. from each region of the world. Often the actual numbers can vary substantially from those anticipated, however, since the actual number of refugees who arrive from a region can be volatile. There is also a great deal of uncertainty about the number of family reunification cases arriving each year. Since family reunification cases are predestined for particular offices, this shifts the allocation of non-family reunification cases and often the total number of refugees sent to each city away from budgeted numbers. Finally, the overall number of refugees sent to a particular office is also a function of employment statistics at the regional office level.

There are three groups whose placement do not follow the above guidelines due to special circumstances: the Meskhetian Turks, the Somali Bantu and the Kakuma Youth. Each Meskhetian Turk is in fact reunited with family members, and therefore pre-destined, despite their official classification. The Somali Bantu and Kakuma Youth were resettled in coordination with all 11 other resettlement agencies. Sites were explicitly chosen and the flows to each site managed in a way outside the control of the IRC. These three groups are therefore excluded from the analysis.

As for the remaining information provided to the IRC by the PRM, the IRC reports using a limited amount of this information in the allocation process. Given that this is difficult to verify, the data set used in this analysis fortunately includes all information given to the IRC prior to each refugee's arrival. In fact, the data was compiled from the very forms provided to the IRC from the PRM. I can therefore control for individual characteristics which the IRC uses in the allocation process.[9] This is important since it removes the problem of sorting based on unobserved characteristics which exists in other studies estimating social network effects.[10]

[9]I make the distinction here between individual characteristics and those characteristics which will be shared by an entire ethnic group, for example. This issue will be discussed in the next section.

[10]Bertrand et al. (2000), for example, evaluate the role of networks in welfare participation. This study uses a similar empirical strategy with neighborhood and language group fixed effects, but there remains the possibility of differential selection of individuals into metropolitan areas based on unobserved preferences for work and welfare participation.

4.3 Data

The data from the IRC comprises of over 1,700 male adults who arrived in the U.S. between 2001 and 2005. All sample respondents did not have family members already in the U.S. to assist in their resettlement, and the IRC therefore placed all of these individuals using the placement policy described above. There are three components to this data. A fairly rich set of demographic variables which were compiled by the INS and the PRM prior to the refugee's arrival in the U.S. is available, including ethnicity, date of birth, country of first asylum, the size of the family being resettled, initial English language level and education received in the home country. This data is comprehensive of all individual characteristics known by the IRC at the time of placement and retried from the forms the IRC received from PRM. Labor market outcomes, in particular employment status and hourly wage, were collected by the IRC at 90 days after each refugee's arrival. For the period 2001-2003, industry and occupation codes are available for those employed. Finally, data on the total number of individuals (inclusive of all ages) placed in each IRC regional office by nationality from 1997 through 2004 were retrieved from archived aggregate reports. Unfortunately, individual-level data prior to 2001 are currently unavailable.

There is a wide variety of ethnic groups and nationalities in the data. The largest groups are from Afghanistan, Bosnia, Liberia, Somalia, and the Sudan, although there are in total 38 different ethnic groups represented. The IRC has 16 offices where they resettle non-family reunification cases.[11] The sample excludes those refugees who are HIV positive, which are less than 1% of the sample, since these refugees spend a substantial portion of their initial 90 days under medical supervision.

In order to get an estimate of the size of each ethnic group's network in a given geographic space, I will be using two different measures. The primary analysis will define the social network as non-family reunification refugees from the same nationality who were resettled in the same regional office. Since the aggregate data is available from 1997 onwards, this measure of network size for an individual will include fellow refugees resettled in the four years prior to that individual's arrival. The relevant network is defined to include only those individuals without family in the U.S. prior to their arrival. The reason for this restriction is twofold. First, while not modelled explicitly, an incentive for participation in the network is insurance: even if an individual is employed now, there is a positive probability of becoming unemployed in future periods and may then rely on the network to gain a job. Refugees with family members who are already established in the U.S. would need to depend less on the social network formed by refugees who largely have not known each other for more than 90 days. The second reason is that the resettlement experience is different across these two groups. Family reunification refugees can be located far away from the regional office but still "resettled" by the IRC. By contrast, since the IRC rents an apartment for each non-family reunification refugee, they tend to be clustered together spatially. Moreover, the two types of refugees are

[11] The offices are: Abilene, TX, Atlanta, Baltimore, Boston, Charlottesville, Dallas, New York, New Jersey, Phoenix, Salt Lake City, San Diego, Seattle, Tucson, Washington DC, and Worcester, MA. Atlanta, Baltimore, Dallas, Phoenix and Salt Lake City are the largest.

less likely to interact since family reunification refugees receive less resettlement services from the IRC and are accordingly less likely to meet fellow refugees in the office or at IRC-sponsored events. The data on the number of family reunification refugees resettled during this time period will also be used in the econometric analysis, as discussed in section 6.

The second measure of the size of the social network comes from the 2000 Census data available through IPUMS. I calculate the size of the network at level of the metropolitan statistical area (MSA). I define a social network by either nationality or ethnicity, depending on the availability of the relevant code in IPUMS.[12] IPUMS data also provides the age of each network member as well as the year of arrival in the U.S. Therefore I can create a network size variable which is specific to the year of arrival of the network members. The information on age also allows me to restrict the network to only prime age adults. Since the Census does not obtain information on the foreign born's visa type or residency status/citizenship, this measure will include all immigrant types, ranging from illegal immigrants to permanent residents and naturalized citizens.

Supplemental information is also available from a survey of refugees and asylees collected by the Department of Health and Human Services' Office of Refugee Resettlement (ORR) between 1993 and 2004. The survey is designed to be a panel study, where each respondent is interviewed for 5 years, and is intended to be representative of all refugees and asylees who were admitted to the U.S. in a given year. Unfortunately, there is no information available in the data indicating which refugees were family reunification cases. This sample may therefore not be precisely comparable to the IRC sample used in the majority of the analysis.

5 Empirical Strategy

The primary objective of this paper is to empirically test the predictions of a simple model of job-related information flows in social networks. The model corresponds nicely to the empirical setting. Propositions 1 and 2 predict that having a larger number of network members who arrived in the same year, corresponding to the N_c cohort, will decrease the probability of a new refugee obtaining employment. An increase in the number of refugees who arrived the year prior, analogous to a change in N_{c-1}, will impact employment outcomes more positively than the effect from an increase in N_c. In particular, a negative impact from an increase in N_c, N_{c-1} and a positive effect from N_{c-2} and N_{c-3} would be consistent with the model. Wages should exhibit the same differential pattern across network cohorts.

Using labor market outcomes as of 90 days after arrival and the aggregate data on IRC placements from 1997-2005, the model predictions will be tested using the following econometric specification:

$$Y_{ijkt} = \alpha + \gamma_1 N_{ijk(t)} + \gamma_2 N_{jk(t-1)} + \gamma_3 N_{jk(t-2)} + \gamma_4 N_{jk(t-3/t-4)} + X_{ijkt}\beta + \delta_{jt} + \phi_k + \epsilon_{ijkt} \tag{4}$$

for individual i in country of origin j in city k who arrived at time t .Y_{ijkt} represents either employment status or wages for individual i. $N_{jk(t-1)}$, $N_{jk(t-2)}$ are the number of refugees who arrived during the fiscal

[12]For example,I can identify some ethnic groups which cover multiple countries, such as the Kurds.

year one year, and two years prior to refugee i's arrival. $N_{jk(t-3/t-4)}$ is analogously the number who arrived three and four years prior. Therefore the network variables are the same for all refugees who arrive in the same fiscal year, are resettled in the same regional office and share the same country of origin/ethnicity.[13] N_{jkt} is the number of refugees from country of origin j resettled by the IRC in regional office k who arrived in fiscal year t up to i's specific date of arrival. Those individuals who arrived after i are excluded from N_{jkt} since they would be not be acting as competitors nor providing job information to individual i.[14] Negative point estimates of γ_1 and γ_2 and positive estimates of γ_2 and γ_3 would be consistent with the model.

The difficulty in consistently estimating γ_1, γ_2, γ_3 and γ_4 is separating the network effect from common unobservable attributes which are shared by members of a group. Networks which are defined by group identity and geography are particularly susceptible to bias from sorting. If individuals choose their locations based on factors which are not observable to the econometrician, and these factors are common among group members, then it is difficult to separate the effect of having a larger number of network members in a city on labor market outcomes from the impact of common characteristics network members share. In the case of refugee resettlement, the institutional environment provides a strategy to mitigate this problem of correlated unobservables.

There are two main threats to identification in this environment. The first originates from sorting along unobservable individual characteristics. The second class of correlated unobservables is omitted city and ethnic group characteristics. The first is addressed by including a flexible functional form to span the information set available to the IRC at the time of placement. In this case, X_{ijkt} captures individual characteristics which are correlated with network size. The remaining individual attributes in ϵ_{ijkt} can not be a source of bias since they are not known by the IRC at the time of placement and accordingly are uncorrelated with N_{jk}.

Adding structure to the error term ϵ_{ijkt} makes the potential sources of bias from omitted city and ethnic group factors more concrete. Therefore let

$$\epsilon_{ijkt} = \delta_{jt} + \xi_{kt} + \tau_{jk} + \nu_{jkt} + \omega_{ijkt} \tag{5}$$

Since the IRC resettles multiple ethnic groups across multiple cities, there is variation in social network size across cities, ethnic groups and over time. This facilitates a fixed effects strategy to minimize the unobservable factors common among ethnic groups or within cities which are correlated with network size.

In my preferred specification, I include only δ_{jt} and ϕ_k controls. Time variant heterogeneity at the nationality/ethnic group level is captured by δ_{jt}. Thus if one particular ethnicity has lower human capital on

[13]Since network size comes from aggregate data, this measures is the total number of refugees by nationality including children. Unfortunately, without individual records prior to 2001, this is as precise measure as available.

[14]For example, an individual who arrived in December could not influence the 90 day labor market outcomes of a refugee who arrived in January. An alternative measure would be the number of refugees who arrived during same year of arrival up until i's date of arrival plus 90 days.

average or if the types of people who become refugees vary across sending countries, these common factors within a group are captured. By varying across years of arrival, this term also allows there to be unobservable differences within a group across cohorts. Comparing labor market outcomes across cities is a challenge since there are numerous differences, such as macroeconomic variation, which are difficult to quantify completely. If the IRC uses this information when allocating refugees across cities, then these factors would be correlated with network size. Therefore unobservable factors at the city level are controlled for using metropolitan-area fixed effects, ϕ_k. Thus ϕ_k absorbs variations in the local labor market which affect all ethnic groups equally.

As equation (5) suggests, however, there are three additional sources of bias. There may be shocks at the city level, ξ_{kt}, a match quality between ethnic groups and cities, τ_{jk}, and finally shocks to match quality, ν_{jkt}. The first two can be addressed through fixed effects and will be discussed further in section 6.2.

Could systematic variation in factors which affect particular ethnic groups in certain cities generate a relationship between network size and labor market outcomes as predicted by the model? In order to do so, there must be a very special sequence of shocks which is identical in all comparative advantage industries in all cities. Furthermore, these shocks must be known and anticipated by the IRC in order to be correlated with the time variation in network size. This is unlikely to occur. During extensive discussions with the IRC, they stated that they believe that all refugees can succeed in each of their resettlement locations, and therefore do not make allocation decisions to maximize match quality. There is also a stochastic time lag between when a refugee is assigned to the IRC by the State Department, and accordingly allocated to a regional office, and the actual arrival date. In some cases, there can be a full year between these two events. This would make it extremely difficult for the IRC to exploit time variant shocks of this nature. More concretely, if this was the strategy used by the IRC, we would observe the flows of refugees from a particular group to exhibit an oscillating pattern. Table (1) shows that the number of refugees from a nationality group who are resettled in the same city are positively correlated across all four year time periods. The table presents the correlation matrix of the number of people the IRC allocated to each nationality/regional office pair, i.e. the size of each cohort across 4 year periods from 1997-2005. The strongest correlated is between time t and $t-1$ and is thereafter monotonically decreasing in the time elapsed between cohorts. A correlation between ν_{jkt} and network size therefore would not generate estimates of γ_1, and γ_2 which are negative and positive estimates of γ_3 and γ_4 under the null hypothesis of no network effects.

The employment analysis will be done using a linear probability model. There are a large number of categorical control variables to capture the unobserved heterogeneity across ethnic groups and cities, and an LPM is therefore easy to estimate. Furthermore, since the mean employment rate is 64%, the LPM should perform well. The error term is clustered at the nationality group/regional office level.

5.1 Alternative Network Measure

To further test the model, I also use Census data to construct a measure of network size which includes all individuals from a country of origin group in a given metropolitan area.[15] This measure will include all immigrants groups, not only refugees. Since the data structure differs from the network measure used above, the empirical specification varies as well. In order to test the hypothesis using the 2000 Census data, the size of the network is restricted to those who arrived most recently in the U.S., specifically those who arrived in 1999. I then look for a differential effect of this network for refugees who arrived in 2001 and 2002.

$$Y_{ijkt} = \alpha + \phi_1 N_{jk(t=1999)} + \phi_2 N_{jk(t=1999)} * \lambda_{2001} + X_{ijkt}\beta + \delta_j + \phi_k + \lambda_{2001} + \epsilon_{ijkt} \quad (6)$$

Y_{ijkt}, X_{ijkt}, δ_j, ϕ_k, and ϵ_{ijkt} are defined as above. As described above, $N_{jk(t=1999)}$ is the size of the network for those immigrants who arrived in 1999 according to the Census, and λ_{2001} is an indicator for those refugees who arrived in 2001. Estimates showing ϕ_1 to be positive and ϕ_2 to be negative would be consistent with the model. The differential network effect across the two cohorts is therefore captured by ϕ_2: an increase in the number of network members who arrived in 1999 would have a smaller or negative impact on labor market outcomes for those who arrived in 2001 than for those who arrived in 2002. By 2002, network members who arrived in 1999 would have acquired additional job information, becoming employed themselves, and be better able to provide referrals to newly resettled refugees. These network members would, however, be more likely to be competitors for job information with those who arrived more closely to them in time, namely refugees in the 2001 cohort. In this specification, the error term is again corrected for clustering at the nationality group/regional office level.

6 Empirical Results

6.1 Probability of Employment

To begin the analysis of the effect of networks on labor market outcomes among refugees resettled in the U.S., I follow much of the existing empirical literature and estimate the effect of the stock of network members on the probability of employment. Table (3) shows that this analysis leads to puzzling results. In Columns 1 and 2, an increase in the number of refugees from country j resettled in city k from years t though $t-4$ increases the probability of employment for a new arrival. This specification includes nationality-year, and city controls. However, once city-nationality and city-year controls are included, the effect becomes insignificant and the point estimates are negative. In this case, the analysis would be inconclusive regarding the existence of social networks providing job information to newly arrived refugees.

[15]Most networks are defined at the level of the MSA, however some include multiple MSAs. For example, refugees resettled in the New York office can be resettled in either New York-Northeastern NJ MSA or the Nassau Co., NY MSA. Thus the network size includes both MSAs since there is likely to be contact between individuals across this geographical space.

By contrast, the results from analyzing employment in a dynamic context consistently confirm the predictions of the information transmission model. Table (4) shows that a larger number of network members who arrived in the current and prior year strongly decreases the probability of employment for a new entrant. A one standard deviation increase in $t-1$ network size decreases the probability of employment by 4.9%. Given that the mean level of employment in the sample is 64%, this constitutes a decline of over 7%. Analysis done with the ORR data shows that an additional year in the U.S. lead to an increase in the employment rate of 3%.[16] Therefore this negative network effect is an economically significant factor in determining refugee labor market unemployment. As seen in Table (3), this component is lost in an analysis of the stock of the network, instead of looking for the dynamic component of social network behavior. The approach used in this paper therefore sheds new light on how networks function and affect on the labor market outcomes of network members.

As is consistent with the model, however, a larger number of refugees with two to four years of experience living in the U.S. prior to a new refugee's arrival has a positive and statistically significant effect on employment. The number of refugees resettled in year $t-2$ has the largest effect on the probability of employment. In this case a one standard deviation increase in $t-2$ network size raises the probability of employment by 4.6%. In this specification, the number of refugees who arrived in the prior 3 and 4 years are combined, and the estimates of γ_3 and γ_4 are jointly significant at the 5% level. One-sided tests indicate that while $\hat{\gamma}_1$ is not statistically more negative than $\hat{\gamma}_2$, $\hat{\gamma}_2 < \hat{\gamma}_3$ as predicted by the model. It is fairly surprising that the coefficient on the number of refugees who arrived in years $t-3$ and $t-4$ is smaller than that of the $t-2$ network, although it is still positive and statistically significant. One reason for this is that out migration is likely to be higher for refugees who had been resettled 3 or more years prior to the new arrival.[17] Out migration within the first 90 days is 6.96%, and therefore it is quite plausible that the smaller coefficient reflects the fact that this variable has more measurement error in representing the true number of network members currently available to the new arrival. Attenuation bias would then push down the size of the coefficient compared to that of the $t-2$ cohort. An alternative explanation, although less likely, is that if refugees remain in the social network for only 3 years, then the effect from an increase in the number of refugees who arrived 4 years prior would have a positive but smaller effect on 90 day outcomes.

The coefficients on the control variables are as expected, although the interpretation is unclear given that the coefficients are a mixture of the causal relationship and the selection rule used by the IRC. Age displays a concave relationship with the employment rate, increasing at a decreasing rate. Household size is negative, reflecting that a larger household may also contain more potential workers, thereby diminishing the incentive to work or providing the opportunity to invest in human capital for any given individual.

Table (4) includes city, and nationality group-year indicator variables. If individuals are able to influence the way the INS adjudicates individual cases or the timing between when an individual is granted

[16] See Table (18).

[17] The data used to measure network size is the total number of refugees who were placed in a given city in a given year, and I do not know if those individuals continue to live in their initial location.

refugee status and when he is allowed to travel to the U.S., there could be differences in the quality of cohorts over time. If the IRC is aware of these differences and changes its placement policy accordingly, then network size would be correlated with the error term. For example, individuals who enter the U.S. after a large cohort may differ in an unobservable way from those who enter at the same time as the large cohort. In that case, τ_{jt} would be correlated with $N_{jk(t)}$ for all t. To address this concern, all estimates include fixed effects for nationality-year.

The specification estimated in Column 1 contains a limited number of demographic covariates. There are accordingly individual characteristics which may have been used by the IRC when choosing an individual's location, thereby being in ϵ_{ijkt} and correlated with network size. Column 2 addresses this potential concern by including a wider range of individual characteristics. The estimates of $N_{jk(t)}$ for all t are robust to the inclusion of these variables as can be seen in Column 2 of Table (4). The coefficients are largely unchanged and continue to be significant.[18]

To return to the correlation matrix presented in Table (1), is the covariance structure between the network variables themselves causing the observed pattern? The positive correlation across all periods indicates that this is not the case. Further confirmation that the correlation between the network variables is not creating a spurious pattern will be shown in section 6.3 where an alternative measure of network size is used.

6.2 Probability of Employment: Robustness Analylsis

The above analysis relies on the assumption that factors other than individual characteristics influencing labor market outcomes are equal across refugees of the same nationality group who were sent to different cities. In this section, I discuss and address the most important of these factors. First, individuals in an ethnic group may perform better in particular cities if the return to that group's skill set varies across localities. Second, the number of family reunification refugees resettled in a city may both influence the labor market outcomes of non-family refugees and reflect unobservable characteristics of a city. Third, I evaluate whether the number of refugees who are not in the relevant social network but are resettled in the same city has an impact on the probably of employment of recently resettled refugees. Additionally, IRC's placement policy of matching language competencies could create a correlation between network size and factors which are not controlled for in the above analysis. The last section analyzes whether the network effect estimated in the previous section is driven by the network's ability to provide translation services to members, instead of the job information mechanism outlined in the theoretical framework.

[18] Another placement criteria used by the IRC is to send each refugee to an office where someone can speak the same language as the refugee. Including two discrete variables indicating whether the refugee was placed in an office with either a staff member or a volunteer who speaks at least one of the languages spoken by the refugee does not result in significant changes to the coefficients of interest. Having a volunteer who speaks the same language positively impacts employment while there is no impact from staff members.

6.2.1 City-Ethnic Group Comparative Advantage

An alternative hypothesis and a common problem in identifying network effects based on geographic variation is that there may be city-ethnic group specific matches which are both unobserved and correlated with network size. This would arise if, for example, there are characteristics or skills which are common to all individuals in ethnic group j which receive a higher return in particular cities k. Thus if particular immigrant groups receive differentially higher wages in a particular city and the IRC uses this information while making decisions on how to distribute refugees, network size would be endogenous. There are two reasons this is unlikely to be the case. First, the IRC themselves state that they take no position on whether certain cities are preferable for particular ethnic groups. Not unlike the canonical pool player subconsciously calculating angles while playing, though, this alone does not definitively rule out the possibility that there are not other unobservable characteristics of a city-ethnic group pair which are being used to determine placement. The second argument is that a comparative advantage story alone would not generate a negative $\hat{\gamma}_1$ and $\hat{\gamma}_2$ and positive $\hat{\gamma}_3$ and $\hat{\gamma}_4$. It would create a uniformly upward bias, not a differential effect between recently arrived refugees and tenured refugees.

The structure of the data also allows me to include a richer set of fixed effects than used in Columns 1 and 2 of Table (4). Specifically I can include nationality-city, nationality-year and city-year controls since the network variables varies at the nationality-city-year level. The results of this specification are shown in Columns 3 and 4 of Table (4). Despite the large number of additional controls this requires, Column 3 shows that the results are robust to this richer specification.[19] The estimates are in fact larger than in the specification used in Columns 1 and 2, although only the coefficient on $N_{jk(t)}$ is statistically different. One reason for this change is that controlling for city-year and nationality-city year better capture group and city-specific resources available to refugees from the IRC during their first 6 months in the U.S. Column 4 includes the full set of control dummies plus a wider set of individual characteristics. In this case, $\hat{\gamma}_4$ is statistically insignificant but qualitatively similar in size. $\hat{\gamma}_3$, $\hat{\gamma}_1$ and $\hat{\gamma}_2$ remain strongly significant and of the same sign and magnitude.

This specification removes the possible bias originating from time invariant unobserved city-ethnic group match quality. The city-year dummy variables also remove the possibility that city-level employment shocks are influencing the estimates. These additional controls help to identify the causal effect of network size on employment as long as there are not year-specific shocks which vary at the city-ethnicity level and are used by the IRC to determine placement.

In contrast to the analysis of the stock of network size shown in Table (3), the results are not sensitive to the inclusion of additional fixed effects. This highlights the problem in that estimation. By properly structuring the network variables to reflect the dynamic relationship between network size and labor market outcomes, the presence of network-based job information transmission is easily detected and not sensitive to the specification used.

[19] There are 198 nationality-city, 91 city-year and 149 nationality-year pairs.

6.2.2 Role of Family Reunification Refugees

In the primary analysis of this paper, I define the relevant network as being the number of non-family reunification refugees resettled in a city from the same country of origin. However, do family reunification refuguees also influence labor market outcomes? Despite a larger geographic distance and less frequent interaction, the two groups may form a larger social network.

Since these refugees are immediately reunited with their family already in the U.S., their placement in the U.S. is chosen by their family members. If there were specific match qualities at either the jk or jkt level, then the family members of these refugees would exploit this and self-select into preferable cities. If that were the case, a larger number of family reunification refugees would be associated with higher employment rates for all refugees from that ethnic group j in city k at time t. The analysis in Table (5) includes the number of refugees who arrived via family reunification and were resettled by the IRC. It therefore tests the joint hypothesis that family reunification refugees are not the relevant social network group for non-family reunification refugees and that unobserved factors at the jkt are not correlated with network size. If family reunification refugees do participate in the same social networks as the refugees in my sample, then the coefficients on those variables should be similar to those on the non-family reunification refugee variables: negative for periods t and $t-1$ and positive for period $t-2$ and $t-3/t-4$. If there is sorting among family reunification refugees, however, then we expect these coefficients to all be biased upwards.

There is no evidence that the number of family reunification refugees resettled in a city impacts the labor market outcomes of non-family reunification refugees of the same nationality. All specifications used in Table (5) show that these coefficients are statistically insignificant both independently and jointly. Moreover, the sign of the coefficients are not consistent with sorting nor social network participation.[20] The coefficients of interest are also largely unchanged from their counterparts in Table (5). In fact, the coefficients in Column 1 of each table are not statistically different from one another.

6.2.3 Impact of Non-network Refugees: Employment

The above section showed that refugees who are reunited with family already in the U.S. do not strongly impact labor market outcomes of non-family reunification refugees. What about refugees from other countries of origin? The objective of this section is twofold. First, given that some of the refugee groups in the sample have similar linguistic backgrounds, there may be a larger network based on language. Second, I test whether the number of refugees resettled in a city over time from dissimilar ethnic groups has an impact on the probably of employment. This serves as an additional specification test, since variation in the number

[20]Edin et al. (2003), however, find evidence of refugees sorting across locations in their study in Sweden. While the analysis here does not show sorting along factors which are common to an entire nationality group-city pair, there is still the possibility of sorting based on characteristics which vary within an ethnic group, such as education level. Since I do not currently have individual data with labor market outcomes for family reunification refugees, I can not evaluate whether there is in fact sorting along these other dimensions.

of non-network refugees should not demonstrate the same heterogeneous effect on newly arrived refugees' outcomes as the number of network refugees.

Table (6) shows the estimates from the analysis using an alternative measure of social networks based on linguistic similarities. Groups are categorized using regional similarities and former colonial heritage. For example, the former Soviet states form one group and the francophone West Africa countries another.[21] The estimates follow the model predictions: an increase in the number of network members who arrived in the same year or year $t-1$ negatively impacts the employment rate. As seen in section 6, an increase in the number of network members who arrive in years $t-2$ and $t-3/t-4$ raises the probability of employment. Columns 1 and 2 reflect the baseline specification using nationality group-year and city fixed effects, and Columns 3 and 4 use the full set of controls. The pattern of heterogeneous network effects is observed in all specifications.

Using the newly specified grouping of similar social networks, Table (7) tests whether an increase in the number of refugees from linguistically dissimilar groups affect the probability of employment for a new refugee. Column 1 includes nationality group-year and city fixed effects. The point estimates on the number of network members in years t through $t-4$ are consistent with Table (6), although the estimate on year t is smaller and more noisily estimated. An increase in The number of refugees from dissimilar groups have a positive effect on the probability of employment, with statistically significant effects on years t, $t-2$ and $t-3/t-4$. These effects are not consistent with the model of job information transmission. These results also suggest that the number of refugees from other groups do not negatively impact employment rates by increasing labor supply. This is consistent with the assumption in the model that the number of refugees resettled in a given year is sufficiently small as not to impact overall labor market conditions. The positive effect may reflect time variant city-wide factors, which can not be separately identified from the effect of variation in the number of non-network refugees sent to a city.

6.2.4 Heterogeneous Effects by Initial English Level

Social networks play an important role in many aspects of a refugee's life. In particular, network members may be helpful in providing translation services either during the job search process or on-the-job. Does the network effect estimated in this paper reflect job information transmission or simply the ability of network members to translate for network members at the work site? A larger number of network members available to translate at the workplace is likely to increase the probability recently arrived refugees find employment at those sites. This benefit of the network should be stronger for those individuals who arrive in the U.S. with no or low English knowledge. I therefore test to see if there are heterogeneous network effects depending on refugees' initial English levels. Table (8) shows results of including $N_{jk(t)}$ for all t and $N_{jk(t)}$ for all t interacted with initial English ability. The level effects are consistent with the previous analysis and the

[21] Table (6) provides the details on the linguistic groupings. All other social networks remained the same as in the previous analysis if they did not fit one of these 5 categories.

interaction terms are insignificant. This indicates that the coefficients on $N_{jk(t)}$ for all t do not reflect the ability of social networks to provide English language services but is instead more consistent with the model of job information transmission.

6.3 Probability of Employment: Census Data Specification

Using the 2000 Census to create a second network measure allows for flexibility in the definition of the social network. This measure expands the potential network members to those who come from the same country of origin or ethnic group but who may have different immigration status. In this case, individual network members have self-selected into their preferred location based on a number of unobserved factors. So while this measure of the network is more susceptible to selection bias due to comparative advantage, it does test the generality of the job information sharing effect across two independently constructed network measures. Table (9) shows that the estimates are as expected from the model. The effect of a larger number of network members from 1999 increases the probability of employment for those refugees who arrived in 2002. More specifically, increasing network size by one standard deviation increases the probability of employment for the 2002 cohort by 6.7%. The interaction term between network size and the indicator for arrival in 2001 is negative. This shows that relative to those refugees who arrived in 2002, an increase in the network size has a smaller effect on the probability of employment. The sum of the two coefficients is negative but small and statistically insignificant. This is consistent with the information transmission model: those refugees who arrive less than 2 years after the network members do not gain from an increase in network size while those who arrived sufficiently later do experience the positive influence of the network in terms of job information. While not shown in Table (9), these results are also robust to the inclusion of a richer set of demographic variables.

6.4 Job Quality: Wages

Both measures of network size provide complementary evidence on the importance of job information flows for employment within social networks. I now turn to the role of networks in determining hourly wages. Table (10) shows the effect of network size on wages for the employed sample. Recall that the theoretical predictions regarding these effects were ambiguous. There are two offsetting factors: on one hand, an increase in $N_{jk(t)}$ will decrease wages since an individual will receive less job offers, thereby reducing the ability to choose the highest paying offer. However, the proportion of individuals who receive job information indirectly, through other employed network members, will decline. Since these wages are lower on average, the average wages of those who are employed rises. Columns 1 and 2 of Table (10) are broadly consistent with the model's prediction. The size of the network in periods $t-2$ and $t-3/t-4$ are positive and statistically significant. There is no evidence that junior network members, those who arrived in years t and $t-1$, impact average hourly wages. These results constitute weak evidence of the information transmission model. The more senior network members are having a strong, positive effect on hourly wages while those network members who arrived more recently have no discernable effect. This is also suggestive that the effect of the network in changing the number of wage offers an individual receives is stronger than the compositional effect.

The inclusion of additional demographic information on individual refugees in Column 2 does not have a large effect on the estimates. The initial English level has a large impact on the average hourly wage. Again, this must be interpreted with caution since the estimate may not reflect the causal relationship between English level and wages since the IRC may use this information when making geographic placement decisions.

In order to test Claim 1, I estimate equation (4) with the full male sample and impute wage offers for the unemployed as zero. Claim 1 argues that variation in network size will have heterogeneous effects on hourly wages (unconditional on employment) due to the dynamic relationship between network size and wages. An increase in the number of network members who arrive in the same period will have a negative impact on wages. The impact from an increase in the number of more senior members on a new arrival i's wages will be monotonically increasing in the time elapsed between the additional network member's arrival in the U.S. and individual i. The estimates are consistent with the model's prediction. As shown in Column 1 of Table (11), one standard deviation increase in the number of network members who arrived in time $t-1$ decreases the wage by \$.70. An increase of one standard deviation in $N_{jk(t-2)}$ increases hourly wage by \$.50. Using this measure of wages, these results reflect both the effect of the network on employment and the direct effect on wages. These results support the intuitive notion that network members become increasingly valuable to new arrivals as their exposure in the labor market in the U.S. increases.

Including a wider range of demographic and other control variables as in Column 2 of Table (11) leads to little change in the network coefficients. Additionally, the estimates on the control variables are consistent with their effects on employment. Age is again concave: wages increase with age at a decreasing rate as is observed in numerous settings in the U.S. labor market. Household size is again negative, and IRC exemption from employment is negatively correlated with wages.

However, there remains a potential problem in estimating the wage equation with the full sample. The model predictions imply that the effect of network size should have an effect on *offer* wages. However, the data provided by the IRC only provides wages for those individuals who are employed, and as such offer wages for those who are unemployed are unknown. According to the model, individuals who are unemployed have received no job offers. Therefore, a wage of zero for these individuals is the correct offer wage. However, there may be a censuring problem if some individuals reject an offer because their reservation wage is higher than the wage offer. While this is not in the model, I address this concern in the empirical analysis. In this case, the interpretation of coefficients on the network variables is unclear when wage equations (4) and (6) are estimated by OLS. Estimates based on imputing a zero wage for those individuals who rejected positive wage offers due to their high reservation wage will be biased.

The classic solution to this problem is to estimate a structural model of wage offers and labor market participation. Without a suitable exclusion restriction, however, classic selection models are not necessarily identified (Heckman, 1974). One alternative solution, as in Neal and Johnson (1996), is to impute unobserved wages as zero and estimate the wage equation using least absolute deviations (LAD). Following Johnson, Kitamura, and Neal (2000), consider the following model:

$$w_i = X_i'\beta + \epsilon_i$$

where w_i is wage offer, X_i are observed characteristics and ϵ_i are unobserved traits for individual i. However, w_i is unobserved if i is unemployed. Let I_i denote individual i's employment status, where $I_i = 1$ implies that i is employed. We can therefore create another variable y_i such that $y_i = w_i$ if $I_i = 1$ and $y_i = 0$ if $I_i = 0$. The key assumption is that all unemployed individuals receive wage offers below the median offer made to employed workers with comparable skills:

$$w_i < X_i'\hat{\beta} \qquad if \quad I_i = 0$$

Under this assumption, LAD estimation is unaffected by imputing unobserved wage offers as zero. In response to criticism of this technique by Altonji and Blank (1999), Johnson, Kitamura, and Neal show that in the NLSY, the assumption is confirmed in the vast majority of cases. They use panel data to follow up on those individuals who were unemployed in 1990 and 1991 to show that this method is a fairly accurate way to get unbiased estimates in the face of censured wage problems.

The analysis of wages using LAD estimation shows results consistent with OLS results. As shown in Columns 3 and 4 of Table (11), a larger number of refugees who arrived in years t and $t-1$ negatively impact the average hourly wage of a new arrival. The point estimates are, however, smaller than in the OLS specification and have larger standard errors. The analysis does not include the full set of control variables for city-time, nationality-time and city-nationality since LAD estimation is difficult with large numbers of dummy variables. This additional specification does suggest that the main results in Columns 1 and 2 are not driven by wage censuring.

6.5 Wages: Robustness Analysis

6.5.1 Impact of Non-network Refugees

Similar to the employment analysis, Table (12) shows the results using the alternative definition of network size based on linguistic similarities. Again, this measure of the wage imputes a wage of zero for those who are unemployed. The results are consistent with the model predictions and highlight the heterogeneity in consequences from an increase in network size. However, the model's prediction does not hold when estimating the impact of variation in the size of cohorts of dissimilar groups on newly arrived refugees' wage outcomes. Column 2 of Table (13) indicates that an increase in the number of non-network refugees in years t has the opposite sign than predicted by the job information transmission model. Increases in the number of non-network refugees in years $t-2$ and $t-3/t-4$ also have a positive impact effect on wages. The positive effect, as found in the employment analysis, is likely to reflect time variant city-wide shocks, which can not be separately identified in this specification. This provides further evidence that the baseline analysis identifies a network effect and not unobservable characteristics common to all refugees resettled in a particular city in a given year.

6.5.2 Evidence using Fraction Employed

Given that the model's predictions are driven by the distinction between employed and unemployed network members, ideally the size of the network would be broken down along those lines. Unfortunately, since there are no individual records prior to 2001, and the IRC only collects employment and wage data as of 90 days after arrival, this is not possible. However, restricting the sample to those refugees who arrived between 2003 and 2005 allows for an analysis which is at least suggestive of this preferable specification. By making the assumption that there is persistence in employment outcomes over time, i.e. that the probability of employment at 90 days is a good predictor of whether that individual will be employed later, I can construct the number of network members who arrived during the previous two years who are likely to be employed at time t. Indeed, Table (14) confirms that an increase in the number of individuals who were unemployed as of 90 days after their arrival in the U.S. is negatively associated with employment rates of refugees who arrive in time t, up to two years after the arrival of the network members. Conversely the number of refugees who were employed as of 90 days after arrival is positively correlated with employment outcomes. This same pattern is found for wages for the sample who are employed.

6.6 Wages: Census Data Specification

Changing the estimation approach to use Census data as in equation (6) provides qualitatively similar results. Column 1 of Table (15) indicates that the OLS estimates show no significant effect of network size on wages of those employed although the signs of the point estimates are as expected from the model. The coefficients in Columns 2 and 3 from using the full sample with LAD estimation are more informative. The network effect for refugees who arrived in 2002 is positive and statistically strong. A one standard deviation in network size wages by \$0.56. The interaction of network size with the dummy indicating arrival in 2001 is negative and statistically significant as in the employment regression. The sum of the two network coefficients is negative but statistically insignificant. This closely parallels the results found in the employment results as well as those predictions of the theoretical model.

The results of the LAD estimation in sections 6.4 and 6.5 depend on the assumption that unemployed refugees receive offer wages which are below the median wage of those employed with similar observable characteristics. While it is difficult to provide direct evidence on the validity of this assumption for the sample of refugees used in this study without panel data, I will note that the majority of refugees in the IRC sample come to the U.S. with very low levels of education and often little to no English skills. As can be seen in Table (2), 47% of men in the sample arrived in the U.S. with no English ability. The refugees in sample generally find employment in low skilled service positions, such as housekeepers, in low-skilled industries. [22] It is therefore likely that those who are unable to gain employment in the initial 90 days after arrival are those with limited skills, beyond which is observed by the econometrician, who would otherwise have low wage offers.

The OLS results in Columns 2 and 3 of Table (10) show that tenured network members positively influence hourly wages of recently arrived refugees. Taken in conjunction with the LAD estimates, the analysis provides consistent evidence supporting the job information transmission model.

7 Natural Experiment: September 11, 2001

Anecdotal evidence from the International Rescue Committee suggests that the terrorist attack on September 11, 2001 had a tremendous impact on refugees' labor market outcomes. The number and variety of opportunities available to refugees diminished significantly. Two potential channels for this effect include, first, an increase in xenophobia may have decreased the ability of refugees to gain employment. Second, as seen in Table (17), many refugees are employed in the traveller accommodation industry. Since the tourism industry was negatively impacted after 9/11, this would have reduced labor market opportunities for refugees. Furthermore, it would have diminished the ability of senior network members to provide job information since they were suddenly less likely to hear about new opportunities. I exploit this natural experiment to investigate how refugee social networks respond to an exogenous negative shock to employment opportunities and evaluate whether the response is consistent with the job information transmission model.

In terms of the model presented in section 3.1, the 9/11 shock can most simply be analyzed in the model as a shock to the arrival rate, a.[23] Figure (3) shows how the treatment effect from an increase in the size of cohort c, more specifically $\frac{\partial s_k^k}{\partial N_c}$ for some k, varies with a. The treatment effect is the impact from a change in the size of one specific cohort, c, on the probability of employment of an arriving cohort k. The figure represents the specific case where $b = .20$, each individual lives for 4 periods, and the treated cohort is doubled in size. The first panel demonstrates how the treatment effect of doubling cohort c varies with a: for values of a between 0 and .40, the treatment effect becomes stronger, i.e. more negative, as a increases. However, the relationship between a and $\frac{\partial s_c^c}{\partial N_c}$ is nonlinear. In particular, for values of a such that the network is close to full employment, in this case approximately $a = .40$, the treatment effect quickly converges to zero. The intuition for this nonlinearity is that for low levels of a, there are few referrals that can be made by the network since so few individuals are employed and also receive job information randomly. As a rises, there is more available job information and therefore more scope for competition. However, for high levels of a such that employment is almost 100%, there is little competition between unemployed network members since all are likely to receive job information and become employed directly. Since the average employment

[22] See Table (17).

[23] Alternatively, the negative 9/11 shock could also have increased the exogenous break-up rate b. The prediction is in fact the same.

rate in the sample is 66%, however, it is unlikely that the portion of the model in which an increase in a lowers competition is relevant empirically. Therefore, the conclusion from the model is that for a given change in network size, an increase in a will exacerbate the competition effect, making $\frac{\partial s_c^c}{\partial N_c}$ more negative.

The second panel in Figure (3) provides an example of the model's prediction regarding an increase in N_c on the employment rate of the cohort which enters two periods afterwards, $c + 2$. The analysis of the model in section 3 showed that an increase in the size of cohort c would have a more positive effect on the employment rate of cohort $c + 2$ than on cohort c. In the example presented here, the impact on the employment rate of cohort $c + 2$ is positive for all values of a, b and N_c. An increase in a strengthens this effect for values of a up to approximately .40. Again, as discussed above, for values of a greater than .40, the network is close to full employment and the treatment effect quickly goes to zero. When all network members are likely to receive job information directly, there is limited benefit from having a larger network.

By interpreting the effect of 9/11 as an exogenous shock which decreased a, the model would predict that after September 11, 2001, an increase in the size of the network would have a more dampened effect on labor market outcomes. The corresponding prediction in terms of the econometric specification used in the empirical analysis is that the effect of an increase in N_{jkp} for all p will be closer to zero after 9/11 than prior. Table (16) uses the following specification:

$$Y_{ijkt} = \alpha + \Sigma_{p=1}^{4}(\gamma_p N_{ijk(t-p+1)} + \pi_p N_{ijk(t-p+1)} * Post9/11) + X_{ijkt}\beta + \delta_{jt} + \phi_k + \epsilon_{ijkt} \tag{7}$$

If Post 9/11 represents a decline in the arrival rate a in the model, then π_p should be of the opposite sign of γ_p. In particular, we would expect $\pi_p > 0$ for $p = t$ and $p = t - 1$ since these are the cohorts which were estimated to have a negative impact on the employment probability of a new arrival. Conversely, $\pi_p < 0$ for $p = t - 2$ and $p = t - 3/t - 4$. Table (16) shows the results of estimating equation 7 using two different functional forms. Columns 1 and 2 estimate the effect of an increase in network size captured by two variables: the number of network members who arrived in years t and $t - 1$ and the number of members who arrived in years $t - 2$, $t - 3$ and $t - 4$.[24] The interaction term between the number of refugees who arrived in years t and $t - 1$ and the post 9/11 indicator is positive as predicted by the model but statistically insignificant. The coefficient is smaller than the main effect, .023 compared to -.326, such that the total effect from an increase in the number of junior network members continues to be negative after September 11, 2001. An increase in the number of senior network members, captured by an increase in $N_{jk(t-2/t-3/t-4)}$, has a negative impact as in the previous analysis, but the interaction with the post 9/11 indicator is positive. The interaction term is significant at the 13% level; Column 2 shows that the effect is slightly larger with the inclusion of additional demographic characteristics and accordingly the effect is significant at the 10% level.

Columns 3 and 4 test for a differential network effect after 9/11 from an increase in the number of

[24] There are a limited number of observations, 472, prior to September 11, 2001. I therefore combine the variables of interest to maximize statistical power.

network members who arrived in the same period, N_{jkt}, and two or more years prior, $N_{jk(t-2/t-3/t-4)}$.[25] In this specification, the interaction term between the number of network members resettled in the same city in the same year and the post 9/11 indicator is precisely estimated and positive. This implies that while still present, the competition effect is weaker after the exogenous shock of 9/11 than before. Furthermore, the effect from an increase in $N_{jk(t-2/t-3/t-4)}$ is more muted after September 11, 2001. The interaction term is estimated with a p-value of .11 in Column 4, just outside the range of classical significance levels, and is estimated across all specifications to be around .0004 in size.

The analysis therefore suggests that after September 11, 2001, the influence of social networks on newly arrived refugees' employment outcomes diminished. This is as predicted by the simple job information transmission model but in some sense the opposite of what may be expected. We could also imagine that the effect of September 11 would be to draw ethnic communities together more tightly.[26] In this case, the network could become more valuable, causing the interaction between $N_{jk(t-2/t-3/t-4)}$ and post 911 to be positive. If this effect is present, it is likely to bias the analysis against finding the result consistent with the model's prediction. The exogenous shock of September 11, 2001 therefore provides additional support of the job information transmission model: while the specific pattern of network effects as a function of network members' tenure in the U.S. are present both before and after the incident, the network's impact on employment outcomes of new arrivals is dampened afterwards, consistent with a decline in the arrival rate of jobs.

8 Conclusion

This paper presents evidence on the importance of ethnic networks in influencing access to local labor markets for refugees recently resettled in the U.S. The empirical results support a model of job information transmission within a social network. Both the size and the structure of the network, as measured by length of tenure of network members in the U.S., influence the labor market outcomes of newly arrived refugees. This provides an insight into the functioning of social networks and provides empirical evidence that within-network competition over job information can lead to an economically sizable negative impact on labor market outcomes. This result tempers the previous findings in the empirical literature on social networks which show that networks play a beneficial role in overcoming market frictions. The existence of costs to having a larger social network is difficult to identify unless the dynamic relationship between employment,

[25] The previous analysis showed that an increase in N_{jkt} and $N_{jk(t-1)}$ lead to a negative effect on employment rates. However, I specifically estimate the impact from an increase in N_{jkt} in Columns 3 and 4 since an increase in N_{jkt} is unambiguously negative in the model. An increase in $N_{jk(t-1)}$, however, can be of either sign in the model. Decreasing a or increasing b could shift the effect from being negative to positive. For this reason the interaction between post 9/11 and network size in year t is the easiest to interpret.

[26] The model presented here does not capture whether a network is densely or close knitted. The work by Calvo-Armengol and Jackson (2004), however, does show that these dimensions can have an impact on how effective networks are in providing job information to members.

wages and social network structure is taken into account. Using network variables which capture the tenure composition of social networks is therefore crucial to accurately asses the full network effect. A static analysis of the effect of total network size on labor market outcomes, for example, conflates these two opposite effects and could erroneously fail to identify the presence of social networks in the labor market.

Evidence of social networks providing labor market information suggests that there are spill-overs from policy interventions. The negative estimated effect from an increase in social network size is driven by an increase in the proportion of unemployed network members. Therefore, a job training program which increases the employment rate of some individuals in a social network will generate a positive externality to other network members. According to the model, such an intervention would create a net increase in the information available to unemployed network members who were not directly exposed by the program. This makes the returns to programs providing employment and training services to refugees even higher than otherwise measured by looking at program participants alone. The model's prediction is general to any social network providing job information to its members. Accordingly, we would expect there to be spill-overs in policy interventions which improve labor market performance more generally, particularly for other immigrant groups. Interventions to improve the employment rate of some immigrants within a community would have a positive impact throughout the network, thereby reducing the need for social services from local communities.

The results are also relevant for understanding recent research on the impact of immigration on wages. Cortes (2005) presents a puzzle: according to her structural estimates and theoretical predictions, increased immigration should lead to a depression of wages for other immigrants. However, she does not find direct evidence of this in the data. Neither Card (1990) nor Borjas (2003) find a significant negative effect of an increase in immigration on immigrant wages. My findings provide a potential explanation for this: social networks influence labor market participation and wages by providing job information. This suggests a difficulty in estimating the effect of increased labor supply due to immigration on wages in the U.S. An increase in the number of immigrants in a city also implies an increase in the size of the social network for some groups. This can be an offsetting factor, depending on the tenure structure of the network, to the downward pressure on wages due to the increase in labor supply. Alternatively, the larger social network may put further downward pressure on wages if the additional network members are largely unemployed. The estimates from regressing wages on the number of immigrants in a city may conflate the two effects, that of increased competition due to larger labor supply and the increase in social network size, potentially leading to ambiguous results. The relative magnitudes and directions of these two effects will depend on the size of the city of interest, overall labor market conditions, and the size and tenure structure of immigrant social networks.

The evidence in this paper sheds some light onto the debate over the optimal resettlement of refugees. Large numbers of refugees and asylum seekers are permanently resettled in Europe and North America due to prolonged and protracted conflicts around the world. During 2004, for example, 676,400 people applied for asylum and over 83,000 refugees were permanently resettled to third countries through UNHCR resettlement programs (UNHCR, 2005). However, there is no consensus on the optimal method of resettlement within the new destination country. Policies vary widely from the dispersal policies in some European countries to the clustering method used by at least some American resettlement agencies (Edin et al., 2001).

By showing empirical evidence that refugee social networks provide labor market information to its members, this paper suggests a drawback to immigrant dispersal policies. Sending refugees to areas with a community of tenured network members, who have achieved relatively high employment rates, could improve short-run labor market outcomes. Improving the short-run labor market outcomes of refugees would ease the fiscal burden refugees put on local municipalities, one motivation for dispersal policies. However, this analysis only looks at very short-run outcomes and therefore can not provide an estimate of the total costs of dispersal policies. In the long-run, there are additional considerations such as the way the network affects individuals' incentives to invest in learning the host country language or other types of human capital. Similarly, it is difficult to conclude whether other immigrant groups in the U.S. should be encouraged to cluster together. There is not only the above concern, but an analysis of larger immigrant groups in the U.S. would also have to address the general equilibrium consequences of an increase in social network size. Identifying one specific mechanism through which networks affect labor market outcomes, job information transmission, does however provide one piece of the puzzle. However, the long run role of social networks in creating incentives or disincentives for integration and investments in host country-specific human capital remains an open question and an area of future research.

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A Appendix

A.1 Proposition 1

Proposition 1 *For all values* $0 < a < 1$ *and* $0 < b < 1$*, an increase in cohort size* N_j *decreases* s_c^j *for all* c*.*[27]

Proof:

For cohort j: If N_j increases, s_j^j decreases. This is simple since the previous periods' employment rate, s_c^{j-1}, will be unchanged for all c. Since $s_j^{j-1} = 0$, s_j^j can be written as:

$$s_j^j = \frac{a(N_j + \sum_{c' \neq j} N_{c'})}{N_j + \sum_{c' \neq j} N_{c'}(1 - (1-b)s_{c'}^{j-1})}$$

Differentiating with respect to N_j gives:

$$\begin{aligned}\frac{\partial s_j^j}{\partial N_j} &= \frac{a}{N_j + \sum_{c' \neq j} N_{c'}(1 - (1-b)s_{c'}^{j-1})} - \frac{a(N_j + \sum_{c' \neq j} N_{c'})}{[N_j + \sum_{c' \neq j} N_{c'}(1 - (1-b)s_{c'}^{j-1})]^2} \\ &= \frac{-a(1-b)\sum_{c' \neq j} s_{c'}^{j-1}}{[N_j + \sum_{c' \neq j} N_{c'}(1 - (1-b)s_{c'}^{j-1})]^2} < 0\end{aligned}$$

For cohorts $c > j$: Similarly, if N_j changes, the employment rate for all other cohorts in time period j, s_c^j, decreases as well. Consider cohort $j-1$, although this holds for all other cohorts in the market at time j:

$$s_{j-1}^j = (1-b)s_{j-1}^{j-1} + (1 - (1-b)s_{j-1}^{j-1})\frac{a(N_j + \sum_{c' \neq j} N_{c'})}{N_j + \sum_{c' \neq j} N_{c'}(1 - (1-b)s_{c'}^{j-1})}$$

Since s_c^{j-1} is unaffected by change in N_j for all c,

$$\begin{aligned}\frac{\partial s_{j-1}^j}{\partial N_j} &= \frac{(1 - (1-b)s_{j-1}^{j-1})a}{N_j + \sum_{c' \neq j} N_{c'}(1 - (1-b)s_{c'}^{j-1})} - \frac{a(1 - (1-b)s_{j-1}^{j-1})(N_j + \sum_{c' \neq j} N_{c'})}{[N_j + \sum_{c' \neq j} N_{c'}(1 - (1-b)s_{c'}^{j-1})]^2} \\ &= \frac{-a(1 - (1-b)s_{j-1}^{j-1})(1-b)\sum_{c' \neq j} s_{c'}^{j-1}}{[N_j + \sum_{c' \neq j} N_{c'}(1 - (1-b)s_{c'}^{j-1})]^2} < 0\end{aligned}$$

since $(1 - (1-b)s_{j-1}^{j-1}) > 0$.

[27] This claim holds for all values of a and b such that $s_c^j \neq 1$ for all c and j.

A.2 Proposition 2

Proposition 2 *The impact of an increase in N_c on sk^k is monotonically increasing between $k = c$ and $c + S - 1$.*

Proof:

Assume $S = 3$ and $N_k = 1\ \forall k \neq j$.

Step 1: $s^{j+1}_{j+1}(N_j) > s^j_j(N_j)$

$$s^j_j(N_j) = \frac{a(2+N_j)}{2+N_j-(1-b)(s^{j-1}_{j-1}+s^{j-1}_{j-2})}$$

$$s^{j+1}_{j+1}(N_j) = \frac{a(2+N_j)}{2+N_j-(1-b)(N_j s^j_j+s^j_{j-1})}$$

Therefore, need to show: $N_j s^j_j + s^j_{j-1} > s^{j-1}_{j-1} + s^{j-1}_{j-2}$

$$\frac{aN_j(2+N_j)}{2+N_j-(1-b)(s^{j-1}_{j-1}+s^{j-1}_{j-2})} + (1-b)s^{j-1}_{j-1} + \frac{a(2+N_j)(1-(1-b)s^{j-1}_{j-1})}{2+N_j-(1-b)(s^{j-1}_{j-1}+s^{j-1}_{j-2})} > s^{j-1}_{j-1} + s^{j-1}_{j-2} \tag{8}$$

Using the steady-state properties of the economy,[28] equation (8) will hold with equality if $N_j = 1$.

Since (8) holds with equality when $N_j = 1$, inequality holds if expression is increasing in N_j.

$$\frac{a(2+N_j)(1+N_j-(1-b)s^{j-1}_{j-1})}{2+N_j-(1-b)(s^{j-1}_{j-1}+s^{j-1}_{j-2})} + (1-b)s^{j-1}_{j-1} \tag{9}$$

Differentiating equation (9) with respect to N_j gives:

$$\frac{\partial}{\partial N_j} = \frac{a(3+2N_j-(1-b)s^{j-1}_{j-1})}{2+N_j-(1-b)(s^{j-1}_{j-1}+s^{j-1}_{j-2})} - \frac{a(2+N_j)(1+N_j-(1-b)s^{j-1}_{j-1})}{[2+N_j-(1-b)(s^{j-1}_{j-1}+s^{j-1}_{j-2})]^2}$$

$$= \frac{a(3+2N_j-(1-b)s^{j-1}_{j-1})(2+N_j-(1-b)(s^{j-1}_{j-1}+s^{j-1}_{j-2}) - a(2+N_j)(1+N_j-(1-b)s^{j-1}_{j-1})}{[2+N_j-(1-b)(s^{j-1}_{j-1}+s^{j-1}_{j-2})]^2}$$

Since denominator is greater than zero, if numerator is greater than zero, then equation (8) holds.

$$= a[(2+N_j)(1-(1-b)s^{j-1}_{j-2}) + (1+N_j)(2+N_j-(1-b)(s^{j-1}_{j-1}+s^{j-1}_{j-2})) - (1-b)s^{j-1}_{j-1}(2+N_j-(1-b)(s^{j-1}_{j-1}+s^{j-1}_{j-2}))] > 0$$

since $1 + N_j - (1-b)s^{j-1}_{j-1} > 0$

Step 2: $s^{j+2}_{j+2}(N_j) > s^{j+1}_{j+1}(N_j)$

$$s^{j+2}_{j+2}(N_j) = \frac{a(2+N_j)}{2+N_j+(1-b)(s^{j+1}_{j+1}+N_j s^{j+1}_j)}$$

$$s^{j+1}_j(N_j) = (1-b)s^j_j + (1-(1-b)s^j_j)\frac{a(2+N_j)}{2+N_j+(1-b)(s^{j+1}_{j+1}+N_j s^{j+1}_j)}$$

Need to show:

$$s^{j+1}_{j+1} + N_j s^{j+1}_j > N^j_j s^j_j + s^j_{j-1}$$

Let $N_j = 1 + x$. Rearranging equation (9) gives:

$$(s^{j+1}_{j+1} - s^j_j) + (s^{j+1}_j - s^j_{j-1}) + x(s^{j+1}_j - s^j_j) > 0 \tag{10}$$

[28] Employment status reflect a finite-state irreducible and aperiodic Markov process as in Calvo-Armengol and Jackson (2004). Then by Freidlin and Wentzell (1984) and Young (1993), there exists a unique steady-state distribution associated with this process.

We can write s_j^{j+1}, s_{j-1}^j and s_{j-2}^{j-1} in the following way:

$$s_j^{j+1} = (1-b)s_j^j + [1-(1-b)s_j^j]s_{j+1}^{j+1}$$
$$s_{j-1}^j = (1-b)s_{j-1}^{j-1} + [1-(1-b)s_{j-1}^{j-1}]s_j^j$$
$$s_{j-2}^{j-1} = (1-b)s_{j-1}^{j-1} + [1-(1-b)s_{j-1}^{j-1}]s_{j-1}^{j-1}$$

The left hand side (LHS) of equation (10) is then:

$$LHS = (s_{j+1}^{j+1} - s_j^j)(2-(1-b)s_j^j) + x(s_j^{j+1} - s_j^j) + (1-b)(s_j^j - s_{j-1}^{j-1})(1-s_j^j)$$

Using the fact that $N_j s_j^j + s_{j-1}^j > s_{j-1}^{j-1} + s_{j-2}^{j-1}$ as shown above, this implies

$$s_j^j - s_{j-1}^{j-1} > +s_{j-2}^{j-1} - s_{j-1}^j - xs_j^j > (s_{j-1}^{j-1} - s_j^j)(1-(1-b)s_{j-1}^{j-1}) - xs_j^j$$

Substituting the above gives:

$$LHS > (s_{j+1}^{j+1} - s_j^j)(2-(1-b)s_j^j) + x(s_j^{j+1} - s_j^j) + (1-b)(1-s_j^j)[(1-(1-b)s_{j-1}^{j-1})(s_{j-1}^{j-1} - s_j^j) - xs_j^j]$$
$$= (s_{j+1}^{j+1} - s_j^j)(2-(1-b)s_j^j) + (1-b)(1-s_j^j)(1-(1-b)s_{j-1}^{j-1})(s_{j-1}^{j-1} - s_j^j) + x[(1-(1-b)s_j^j)(s_{j+1}^{j+1} - s_j^j)] > 0$$

since $s_{j+1}^{j+1} > s_j^j$ as shown above and $s_{j-1}^{j-1} > s_j^j$ as in Claim 1.

B Tables

Table 1: Correlation Coefficients of Refugee Cohort Sizes: 1997-2005

	Current Year	Prior Year	2 Years Prior	3 Years Prior
Num Refugees Resettled in Current Year	1			
Num Refugees Resettled in Prior Year	0.5394	1		
Num Refugees Resettled in 2 Years before	0.2859	0.4744	1	
Num Refugees Resettled in 3 Years before	0.2711	0.3794	0.5892	1
Num Refugees Resettled in 4 Years before	0.2399	0.3473	0.3971	0.5815

Table 2: Summary Statistics

	Mean	Std. Dev.	No. Obs
IRC Data:			
Age	33.99	11.05	1720
HH Size	2.76	2.04	1720
Employment rate	0.66		1720
Wage (conditional on employment)	7.48	1.36	1125
Spoke No English Upon Arrival	0.466		1453
Primary School	0.180		1720
Secondary School	0.464		1720
University or Above	0.202		1720
None, vocational or adult education	0.153		1720
Muslim	0.251		1720
IRC Exemption from Employment	0.059		1720
# Refugees Resettled in Year t	10.32	13.47	1720
# Refugees Resettled in Year $t-1$	29.47	34.13	1720
# Refugees Resettled Year $t-2$	25.16	43.82	1720
# Refugees Resettled Years $t-3$ and $t-4$	65.80	102.63	1720
# Family Reunification Refugees Resettled in Year t	15.45	27.24	1720
# Family Reunification Refugees Resettled in Year $t-1$	18.77	48.25	1720
# Family Reunification Refugees Resettled in Year $t-2$	19.50	58.55	1720
# Family Reunification Refugees Resettled in Years $t-3$ & $t-4$	48.89	149.99	1720
2000 Census Data:			
Network Members who Arrived in 1999	150.83	267.84	753

Table 3: Employment Probability on Composite Network Size

	Column 1	Column 2	Column 3	Column 4
# Refugees Resettled Years t to $t-4$ [e]	0.028 ***	0.028 ***	-0.035	-0.040
	(0.011)	(0.011)	(0.047)	(0.047)
Age	0.022 ***	0.021 ***	0.025 ***	0.024 ***
	(0.006)	(0.006)	(0.006)	(0.007)
Age Squared	0.000 ***	0.000 ***	0.000 ***	0.000 ***
	(0.000)	(0.000)	(0.000)	(0.000)
HH Size	-0.015 **	-0.013 *	-0.020 ***	-0.018 **
	(0.007)	(0.007)	(0.007)	(0.008)
IRC Exemption from Employment	-0.540 ***	-0.543 ***	-0.552 ***	-0.552 ***
	(0.051)	(0.050)	(0.057)	(0.056)
p-value of education		0.387		0.116
p-value of Initial English Level		0.002		0.0003
p-value of Religion		0.365		0.593
No obs	1720	1720	1720	1720
Adjusted R squared	0.224	0.229	0.273	0.280

a SE are in parentheses and clustered by city-ethnicity.

b Columns 1 and 2 include fixed effects for nationality-year and regional office.

c Columns 3 and 4 include fixed effects for nationality-year, regional office-year and nationality-city.

d Columns 2 and 4 include additional individual covariates including: education, initial English level, religion.

e Row 1 is multiplied by 100.

Table 4: Linear Probability Model of Employment Probability on Network Size

	Column 1	Column 2	Column 3	Column 4
# Refugees Resettled in Year t [e]	-0.236 **	-0.245 **	-0.340 **	-0.345 **
	(0.118)	(0.124)	(0.164)	(0.173)
# Refugees Resettled in Year $t-1$ [e]	-0.140 **	-0.117 *	-0.257 ***	-0.232 **
	(0.069)	(0.070)	(0.100)	(0.100)
# Refugees Resettled in Year $t-2$ [e]	0.104 ***	0.100 ***	0.113 **	0.105 **
	(0.039)	(0.039)	(0.054)	(0.054)
# Refugees Resettled in Years $t-3$ and $t-4$ [e]	0.037 **	0.034 **	0.056	0.044
	(0.017)	(0.017)	(0.043)	(0.042)
Age	0.023 ***	0.022 ***	0.026 ***	0.025 ***
	(0.005)	(0.006)	(0.006)	(0.007)
Age Squared	-0.0003 ***	-0.0003 ***	-0.0004 ***	-0.0004 ***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
HH Size	-0.015 **	-0.013 **	-0.021 ***	-0.019 ***
	(0.007)	(0.007)	(0.007)	(0.008)
IRC Exemption from Employment	-0.540 ***	-0.543 ***	-0.557 ***	-0.557 ***
	(0.051)	(0.050)	(0.056)	(0.054)
p-value of education variables		0.449		0.250
p-value of initial English level variables		0.002		0.000
p-value of religion variable		0.318		0.581
No obs	1720	1720	1720	1720
Adjusted R squared	0.231	0.236	0.282	0.288

a SE are in parentheses & clustered by city-ethnicity.

b Columns 1 and 2 include fixed effects for nationality-year and regional office.

c Columns 3 and 4 include fixed effects for nationality-year, regional office-year and nationality-city.

d Columns 2 and 4 include additional individual covariates including: education, initial English level, religion.

e Coefficients in row are multiplied by 100.

Table 5: Employment Robustness Analysis with Family Reunification Refugees

	Column 1	Column 2	Column 3	Column 4
# Refugees Resettled in Year t [e]	-0.245 **	-0.255 **	-0.348 **	-0.355 **
	(0.118)	(0.125)	(0.164)	(0.172)
# Refugees Resettled in Year $t-1$ [e]	-0.159 **	-0.135 *	-0.301 ***	-0.275 **
	(0.074)	(0.075)	(0.114)	(0.114)
# Refugees Resettled Year $t-2$ [e]	0.101 **	0.097 **	0.064	0.040
	(0.051)	(0.051)	(0.065)	(0.067)
# Refugees Resettled Years $t-3$ and $t-4$ [e]	0.053 **	0.053 **	0.049	0.027
	(0.023)	(0.023)	(0.053)	(0.055)
Age	0.023 ***	0.022 ***	0.026 ***	0.025 ***
	(0.005)	(0.006)	(0.006)	(0.007)
Age Squared	-0.0003 ***	-0.0003 ***	-0.0004 ***	-0.0004 ***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
HH Size	-0.0152 **	-0.0137 **	-0.0210 ***	-0.0193 ***
	(0.0066)	(0.0071)	(0.0071)	(0.0076)
IRC Exemption from Employment	-0.543 ***	-0.547 ***	-0.559 ***	-0.560 ***
	(0.050)	(0.049)	(0.057)	(0.054)
# Family Reunification Refugees Resettled in Year t [e]	0.075	0.086	-0.055	-0.011
	(0.105)	(0.108)	(0.158)	(0.147)
# Family Reunification Refugees Resettled in Year $t-1$ [e]	0.001	-0.007	0.131	0.127
	(0.063)	(0.061)	(0.111)	(0.107)
# Family Reunification Refugees Resettled in Year $t-2$ [e]	-0.040	-0.042	0.069	0.095
	(0.073)	(0.074)	(0.085)	(0.089)
# Family Reunification Refugees Resettled in	-0.008	-0.009	-0.081	-0.078
Years $t-3$ and $t-4$ [e]	(0.025)	(0.025)	(0.060)	(0.068)
No obs	1720	1720	1720	1720
Adjusted R squared	0.230	0.235	0.281	0.287

a SE are in parentheses & clustered by city-ethnicity.

b Columns 1 and 2 include fixed effects for nationality-year and regional office.

c Columns 3 and 4 include fixed effects for nationality-year, regional office-year and nationality-city.

d Columns 2 and 4 include additional individual covariates including: education, initial English level, religion.

e Coefficients in row are multiplied by 100.

Table 6: Employment Effects within Language Groups

	Column 1	Column 2	Column 3	Column 4
# Network Members Resettled in Year t [f]	-0.084 ***	-0.090 ***	-0.095 ***	-0.101 ***
	(0.033)	(0.035)	(0.037)	(0.040)
# Network Members Resettled in Year $t-1$ [f]	-0.166 ***	-0.148 **	-0.253 ***	-0.228 **
	(0.063)	(0.064)	(0.096)	(0.095)
# Network Members Resettled in Year $t-2$ [f]	0.109 ***	0.106 ***	0.112 **	0.103 **
	(0.042)	(0.042)	(0.052)	(0.053)
# Network Members Resettled in Years $t-3$ and $t-4$ [f]	0.027 *	0.025	0.069	0.056
	(0.015)	(0.015)	(0.043)	(0.043)
Age	0.023 ***	0.022 ***	0.026 ***	0.025 ***
	(0.005)	(0.006)	(0.006)	(0.007)
Age Squared	-0.0004 ***	-0.0003 ***	-0.0004 ***	-0.0004 ***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
HH Size	-0.014 **	-0.013 *	-0.020 ***	-0.018 **
	(0.007)	(0.007)	(0.007)	(0.008)
IRC Exemption from Employment	-0.539 ***	-0.543 ***	-0.560 ***	-0.560 ***
	(0.051)	(0.050)	(0.056)	(0.054)
p-value of education variables		0.384		0.189
p-value of initial English level variables		0.002		0.000
p-value of religion variable		0.308		0.564
No obs	1720	1720	1720	1720
Adjusted R squared	0.231	0.236	0.280	0.287

a SE are in parentheses & clustered by city-ethnicity.

b Network members are grouped linguistically: Balkans: Bosnia and Croatia; Spanish: Argentina, Cuba, Columbia; Anglophone West Africans: Liberia, Sierra leone; Russian: Azerbaijan, Kyrgystan, Russia, Soviet Union, Turkmenistan, Ukraine, Uzbekistan; Francophone: Cameroon, Central African Rep, Chad, Congo, Braz., Congo, DR, Rwanda, Burundi, Togo.

c Columns 1 and 2 include fixed effects for nationality-year and regional office.

d Columns 3 and 4 include fixed effects for nationality-year, regional office-year and nationality-city.

e Columns 2 and 4 include additional individual covariates including: education, initial English level, religion.

f Coefficients in row are multiplied by 100.

Table 7: Employment Effects across Language Groups

	Column 1	Column 2
# Network Members Resettled in Year t [d]	-0.046	-0.052
	(0.034)	(0.036)
# Network Members Resettled in Year $t-1$ [d]	-0.188 ***	-0.173 **
	(0.072)	(0.073)
# Network Members Resettled in Year $t-2$ [d]	0.042	0.037
	(0.040)	(0.039)
# Network Members Resettled in Years $t-3$ and $t-4$ [d]	0.044 ***	0.042 ***
	(0.015)	(0.015)
# Refugees Outside Network Resettled in Year t [d]	0.039 ***	0.037 ***
	(0.013)	(0.013)
# Refugees Outside Network Resettled in Year $t-1$ [d]	0.001	0.002
	(0.025)	(0.025)
# Refugees Outside Network Resettled in Year $t-2$ [d]	0.042 ***	0.045 ***
	(0.014)	(0.014)
# Refugees Outside Network Resettled Years $t-3$ and $t-4$ [d]	0.025 ***	0.026 ***
	(0.008)	(0.008)
Age	0.023 ***	0.022 ***
	(0.005)	(0.006)
Age Squared	-0.0003 ***	-0.0003 ***
	(0.0001)	(0.0001)
HH Size	-0.015 **	-0.014 **
	(0.007)	(0.007)
IRC Exemption from Employment	-0.525 ***	-0.531 ***
	(0.050)	(0.049)
0 yr network coef ¡= 0 yr non-network coef (p-value of one-sided test)	0.007	0.007
1 yr network coef ¡= 1 yr non-network coef (p-value)	0.003	0.007
2 yr network coef ¿= 2 yr non-network coef (p-value)	0.501	0.571
3 and 4 yr network coef ¿= 3 and 4 yr non-network coef (p-value)	0.136	0.169
No obs	1720	1720
Adjusted R squared	0.251	0.256

a SE are in parentheses and clustered by city-ethnicity.

b Also includes fixed effects for nationality-year and regional office.

c Column 2 includes additional individual covariates: education level, initial English level, religion.

d Coefficients in row are multiplied by 100.

Table 8: Employment Robustness Analysis with Heterogeneous English Effects

	Column 1	Column 2
# Refugees Resettled in Year t [d]	-0.203	-0.253
	(0.164)	(0.172)
# Refugees Resettled in Year $t-1$ [d]	-0.219 **	-0.192 **
	(0.094)	(0.096)
# Refugees Resettled in Year $t-2$ [d]	0.111	0.104
	(0.075)	(0.076)
# Refugees Resettled in Years $t-3$ and $t-4$ [d]	0.077 **	0.072 **
	(0.033)	(0.033)
# Refugees Resettled in Year t * No English [d]	-0.005	0.066
	(0.235)	(0.230)
# Refugees Resettled in Year $t-1$ * No English [d]	0.135	0.107
	(0.087)	(0.095)
# Refugees Resettled in Year $t-2$ * No English [d]	-0.067	-0.059
	(0.096)	(0.098)
# Refugees Resettled in Years $t-3$ and $t-4$ * No English [d]	-0.027	-0.028
	(0.031)	(0.032)
No English	-0.077	
	(0.048)	
Age	0.019 ***	0.017 ***
	(0.007)	(0.008)
Age Squared	-0.0003 ***	-0.0003
	(0.0001)	(0.0001) ***
HH Size	-0.012	-0.011
	(0.008)	(0.008)
IRC Exemption from Employment	-0.531 **	-0.536 ***
	(0.058)	(0.058)
p-value of F test for No English Interactions	0.320	0.320
No obs	1471	1471

a SE are in parentheses and clustered by city-ethnicity.

b Also includes fixed effects for nationality and regional office-year.

c Column 2 includes additional individual covariates: education level, initial English level, religion.

d Coefficients in row are multiplied by 100.

Table 9: Employment Effects Using Census Data for Network Measure

	Column 1	Column 2
Network size which arrived in 1999 [f]	0.0284 **	0.0220 *
	(0.0121)	(0.0126)
Network size which arrived in 1999 * Refugee arrived in 2001 [f]	-0.0275 **	-0.0253 **
	(0.0114)	(0.0121)
Age	3.388 ***	3.020 ***
	(0.912)	(1.002)
Age Squared	-0.0530 ***	-0.0482 ***
	(0.0122)	(0.0129)
HH Size	-1.789 *	-1.474
	(1.093)	(1.046)
p-value of education variables		0.014
p-value of initial English level variables		0.066
p-value of religion variables		0.027
p-value of occupation variables		0.757
No obs	753	753
Adjusted R squared	0.187	0.199

a Standard errors are in parentheses and clustered by city-ethnicity.

b Sample restricted to refugees who arrived in 2001 and 2002.

c Columns 1 and 2 include fixed effects for nationality-year and regional office.

d Column 2 also includes: education, initial English level, religion and occupation variables.

e Network Size is number of individuals in the 2000 Census who arrived in 1999 by place of birth/MSA.

f Coefficients in row are multiplied by 100.

Table 10: Wages on Network Size

	Column 1	Column 2
# Refugees Resettled in Year t [b]	0.161	0.230
	(0.372)	(0.369)
# Refugees Resettled in Year $t-1$ [b]	0.000	0.034
	(0.246)	(0.256)
# Refugees Resettled in Year $t-2$ [b]	0.576 ***	0.537 ***
	(0.214)	(0.211)
# Refugees Resettled in Years $t-3$ and $t-4$ [b]	0.440 ***	0.394 ***
	(0.137)	(0.134)
Age	0.083 ***	0.078 ***
	(0.021)	(0.021)
Age Squared	-0.001 ***	-0.001 ***
	(0.000)	(0.000)
Case Size	0.032	0.033
	(0.020)	(0.021)
IRC Exemption from Employment	0.708	0.708
	(0.645)	(0.578)
p-value of education variables		0.000
p-value of initial English level variables		0.097
p-value of religion variables		0.516
No obs	1127	1127
Adjusted R squared	0.311	0.328

a SE are in parentheses and clustered by city-ethnicity.

b Coefficients in row are multiplied by 100.

c Columns 1 and 2 include fixed effects for nationality-year, regional office-year and nationality-city.

d Column 2 includes additional individual covariates including: education, initial English level, religion.

Table 11: Wages on Network Size: Full Sample and LAD

	Column 1	Column 2	Column 3	Column 4
# Refugees Resettled in Year t	-0.024 **	-0.023 *	-0.005	-0.006
	(0.012)	(0.013)	(0.005)	(0.006)
# Refugees Resettled in Year $t-1$	-0.021 ***	-0.018 **	-0.005 **	-0.004
	(0.008)	(0.008)	(0.002)	(0.003)
# Refugees Resettled Year $t-2$	0.011 **	0.010 **	0.005 ***	0.006 **
	(0.005)	(0.005)	(0.002)	(0.002)
# Refugees Resettled Years $t-3$ and $t-4$	0.006 *	0.005	0.001	0.001
	(0.004)	(0.003)	(0.001)	(0.001)
Age	0.245 ***	0.231 ***	0.133 ***	0.095 ***
	(0.039)	(0.046)	(0.029)	(0.038)
Age Sq	-0.004 ***	-0.003 ***	-0.002 ***	-0.001 ***
	(0.001)	(0.001)	(0.000)	(0.001)
HH Size	-0.130 ***	-0.114 **	-0.045	-0.034
	(0.053)	(0.056)	(0.028)	(0.036)
IRC Exemption from Employment	-4.11 ***	-4.09 ***	-6.15 ***	-6.04 ***
	(0.423)	(0.407)	(0.229)	(0.294)
p-value of Education		0.207		0.000
p-value of Initial English Level		0.000		0.030
p-value of Religion		0.463		0.084
No obs	1706	1706	1706	1706
Adjusted R squared / Pseudo R squared	0.300	0.309	0.183	0.190

a SE are in parentheses and clustered by city-ethnicity.

b Columns 1 and 2 include fixed effects for nationality-year, regional office-year and nationality-city.

c Columns 3 and 4 are LAD estimates with FE for nationality group, year of arrival, and city.

d Columns 2 and 4 include additional individual covariates including: education, initial English level, religion.

Table 12: Wage Effects within Language Groups

	Column 1	Column 2	Column 3	Column 4
# Network Members Resettled in Year t [f]	-0.673 ***	-0.707 ***	-0.656 **	-0.699 **
	(0.240)	(0.258)	(0.281)	(0.306)
# Network Members Resettled in Year $t-1$ [f]	-1.364 ***	-1.185 **	-1.994 ***	-1.769 **
	(0.536)	(0.537)	(0.770)	(0.750)
# Network Members Resettled in Year $t-2$ [f]	0.842 **	0.822 **	1.068 **	0.965 **
	(0.374)	(0.371)	(0.481)	(0.486)
# Network Members Resettled in Years $t-3$ and $t-4$ [f]	0.234 *	0.214	0.707 **	0.559
	(0.144)	(0.145)	(0.360)	(0.347)
Age	0.216 ***	0.203 ***	0.248 ***	0.232 ***
	(0.037)	(0.042)	(0.039)	(0.044)
Age Squared	-0.0032 ***	-0.0031 ***	-0.0036 ***	-0.0033 ***
	(0.0005)	(0.0006)	(0.0005)	(0.0006)
HH Size	-0.097 *	-0.080	-0.125 **	-0.109 *
	(0.051)	(0.054)	(0.053)	(0.057)
IRC Exemption from Employment	-3.970 ***	-4.001 ***	-4.140 ***	-4.117 ***
	(0.382)	(0.373)	(0.421)	(0.405)
p-value of education variables		0.337		0.159
p-value of initial English level variables		0.000		0.000
p-value of religion variable		0.243		0.444
No obs	1706	1706	1706	1706
Adjusted R squared	0.239	0.246	0.298	0.308

a SE are in parentheses & clustered by city-ethnicity.

b Network members are grouped linguistically: Balkans: Bosnia and Croatia; Spanish: Argentina, Cuba, Columbia; Anglophone West Africans: Liberia, Sierra leone; Russian: Azerbaijan, Kyrgystan, Russia, Soviet Union, Turkmenistan, Ukraine, Uzbekistan; Francophone: Cameroon, Central African Rep, Chad, Congo, Braz., Congo, DR, Rwanda, Burundi, Togo.

c Columns 1 and 2 include fixed effects for nationality-year and regional office.

d Columns 3 and 4 include fixed effects for nationality-year, regional office-year and nationality-city.

e Columns 2 and 4 include additional individual covariates including: education, initial English level, religion.

f Rows 1-4 are multiplied by 100.

Table 13: Wage Effects across Language Groups

	Column 1	Column 2
# Network Members Resettled in Year t [d]	-0.379	-0.425
	(0.248)	(0.265)
# Network Members Resettled in Year $t-1$ [d]	-1.495 ***	-1.346 ***
	(0.609)	(0.610)
# Network Members Resettled in Year $t-2$ [d]	0.274	0.239
	(0.343)	(0.334)
# Network Members Resettled in Years $t-3$ and $t-4$ [d]	0.385 ***	0.366 ***
	(0.135)	(0.134)
# Refugees Outside Network Resettled in Year t [d]	0.276 ***	0.263 ***
	(0.103)	(0.101)
# Refugees Outside Network Resettled in Year $t-1$ [d]	0.036	0.029
	(0.212)	(0.212)
# Refugees Outside Network Resettled in Year $t-2$ [d]	0.337 ***	0.362 ***
	(0.111)	(0.114)
# Refugees Outside Network Resettled Years $t-3$ and $t-4$ [d]	0.247 ***	0.248 ***
	(0.063)	(0.062)
Age	0.213 ***	0.204 ***
	(0.037)	(0.041)
Age Squared	-0.0032 ***	-0.0031 ***
	(0.0005)	(0.0005)
HH Size	-0.104 **	-0.089 *
	(0.052)	(0.054)
IRC Exemption from Employment	-3.858 ***	-3.904 ***
	(0.377)	(0.366)
0 yr network coef ¡= 0 yr non-network coef (p-value of one-sided test)	0.004	0.005
1 yr network coef ¡= 1 yr non-network coef (p-value)	0.005	0.010
2 yr network coef ¿= 2 yr non-network coef (p-value)	0.566	0.632
3 and 4 yr network coef ¿= 3 and 4 yr non-network coef (p-value)	0.165	0.200
No obs	1706	1706
Adjusted R squared	0.260	0.269

a SE are in parentheses and clustered by city-ethnicity. b Also includes fixed effects for nationality-year and regional office.

c Column 2 includes additional individual covariates: education level, initial English level, religion.

d Coefficients in row are multiplied by 100.

Table 14: Network Size Using Within Sample Employment Info

	Employment	Wage
# Refugees Unemployed during 2 Prior Years	-0.038 ***	-0.028 ***
	(0.005)	(0.009)
# Refugees Employed during 2 Prior Years	0.018 ***	0.014 ***
	(0.003)	(0.004)
Age	0.025 ***	0.068 ***
	(0.006)	(0.022)
Age Sq	0.000 ***	-0.001 ***
	(0.000)	(0.000)
HH Size	-0.013 **	0.032
	(0.006)	(0.021)
Constant	0.463 ***	6.702 ***
	(0.166)	(0.504)
No obs	1487	848

a Network variables indicate employment status as of 90 days after the network member's arrival.

Therefore assuming persistence in employment outcomes over time.

b SE are in parentheses and are clustered by city-ethnicity-arrival year pairs.

c Also included are fixed effects for nationality, regional office and year of arrival.

d Sample includes only refugees resettled from 2003-2005.

Table 15: Wage Effects Using Census Data for Network Measure

	Column 1	Column 2	Column 3
Network size which arrived in 1999 [g]	0.013	0.249 ***	0.172 *
	(0.055)	(0.088)	(0.091)
Network size which arrived in 1999 * Refugee arrived in 2001 [g]	-0.043	-0.238 ***	-0.201 **
	(0.058)	(0.095)	(0.097)
Age	0.073 **	0.270 ***	0.229 ***
	(0.034)	(0.068)	(0.072)
Age Squared	-0.0009 **	-0.0042 ***	-0.0037 ***
	(0.0005)	(0.0009)	(0.0009)
HH Size	-0.017	-0.162 **	-0.122
	(0.028)	(0.078)	(0.076)
p-value of education variables			0.038
p-value of initial English level variables			0.003
p-value of religion variables			0.002
p-value of occupation variables			0.736
No obs	523	742	742
Adjusted R squared	0.300	0.183	0.207

a Standard errors are in parentheses and clustered by city-ethnicity.

b Sample restricted to refugees who arrived in 2001 and 2002.

c All columns include fixed effects for nationality-year and regional office.

d Column 1 uses only the employed sample. Columns 2 and 3 use the full sample.

e Column 3 also includes: education, initial English level, religion and occupation variables.

f Network Size is number of individuals in the 2000 Census who arrived in 1999 by place ofvbirth/MSA.

g Coefficients in row are multiplied by 100.

Table 16: Employment: Shock of 9/11

	Column 1	Column 2	Column 3	Column 4
# Network Members Resettled in Year t [d]			-0.708 ***	-0.720 ***
			(0.183)	(0.183)
# Network Members Resettled in Year t * Post 9/11 [d]			0.530 **	0.536 **
			(0.256)	(0.258)
# Network Members Resettled in Year t and $t-1$ [d]	-0.326 **	-0.315 **		
	(0.136)	(0.139)		
# Network Members Resettled in Year t and $t-1$ * Post 9/11 [d]	0.023	0.020		
	(0.143)	(0.144)		
# Network Members Resettled in Year $t-1$ [d]			-0.289 ***	-0.265 ***
			(0.105)	(0.105)
# Network Members Resettled in Years $t-2$, $t-3$ and $t-4$ [d]	0.080 *	0.069	0.077 *	0.066
	(0.046)	(0.044)	(0.043)	(0.041)
# Network Members Resettled in Years $t-2$, $t-3$	-0.043	-0.046 *	-0.039	-0.041
and $t-4$ * Post 9/11 [d]	(0.028)	(0.028)	(0.026)	(0.026)
Age	0.026 ***	0.025 ***	0.026 ***	0.025 ***
	(0.006)	(0.007)	(0.006)	(0.007)
Age Squared	-0.0004 ***	-0.0004 ***	-0.0004 ***	-0.0004 ***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
HH Size	-0.021 ***	-0.019 ***	-0.021 ***	-0.019 ***
	(0.007)	(0.008)	(0.007)	(0.008)
IRC Exemption from Employment	-0.558 ***	-0.558 ***	-0.554 ***	-0.554 ***
	(0.056)	(0.055)	(0.056)	(0.055)
Post 9/11	0.069	0.093	0.059	0.083
	(0.201)	(0.202)	(0.174)	(0.177)
p-value of education variables		0.281		0.294
p-value of initial English level variables		0.000		0.000
p-value of religion variable		0.627		0.623
No obs	1720	1720	1720	1720
Adjusted R squared	0.281	0.287	0.284	0.290

a SE are in parentheses and clustered by city-ethnicity.

b All columns include fixed effects for nationality-year, regional office-year and nationality-city.

c Columns 2 and 4 include additional individual covariates including: education, initial English level, religion.

d Coefficients in row are multiplied by 100.

Table 17: Largest Industries from 2001-2003

Construction	.03
Animal slaughtering and processing	.05
Grocery Stores	.07
Misc general merchandise stores	.40
Misc Retail Stores	.04
Employment services	.04
Services to buildings and dwellings	.02
Colleges, including junior colleges	.04
Hospitals	.06
Traveller Accommodation	.24
Restaurants and other food services	.09
Other	.28

Above reflect the major industries in which the IRC sample gained employment from 2001-2003.

Table 18: Probability of Employment Using ORR Data

Number of years in U.S. since Resettlement	0.034 ***
	(0.002)
Female	-0.098 ***
	(0.005)
Age	-0.008 ***
	(0.000)
Married	0.156 ***
	(0.006)
Years of Schooling Prior to Arrival in U.S.	0.029 ***
	(0.001)
Constant	0.695
	(0.440)
No obs.	30,906

a Standard errors are in parentheses.

b Also included are fixed effects for nationality, survey year, and initial resettlement state.

Table 19: Probability of Outmigration

	Column 1	Column 2
# Refugees Resettled in Year t [d]	-0.064	-0.086
	(0.076)	(0.090)
# Refugees Resettled in Year $t-1$ [d]	-0.050	-0.059
	(0.047)	(0.067)
# Refugees Resettled Year $t-2$ [d]	0.013	-0.046
	(0.037)	(0.050)
# Refugees Resettled Years $t-3$ and $t-4$ [d]	-0.001	0.003
	(0.014)	(0.032)
Age	-0.002	-0.002
	(0.003)	(0.004)
Age Squared	0.0000	0.0000
	(0.0000)	(0.0000)
HH Size	-0.005 *	-0.007 **
	(0.003)	(0.003)
IRC Exemption from Employment	-0.024	-0.038
	(0.026)	(0.032)
No obs	1886	1886
R squared	0.116	0.236

a SE are in parentheses and clustered by city-ethnicity.

b Columns 1 includes fixed effects for nationality-year and regional office.

c Column 2 includes fixed effects for nationality-year, regional office-year and nationality-city.

d Coefficients in row are multiplied by 100.

C Figures

Figure 1: Graphical Example of Model with Constant Wages

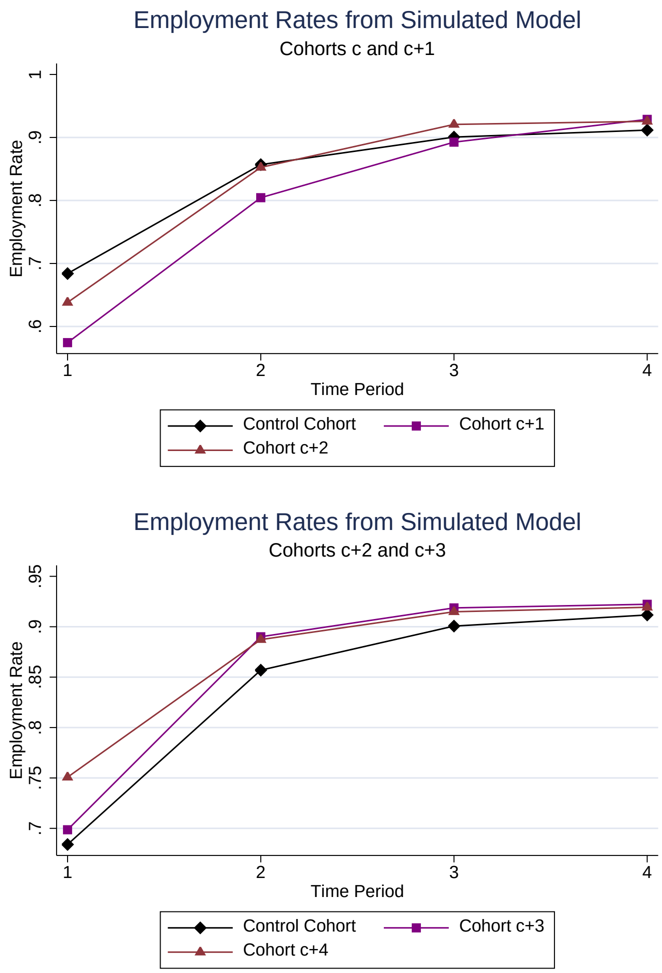

Figure 2: Graphical Example of Model with Wages

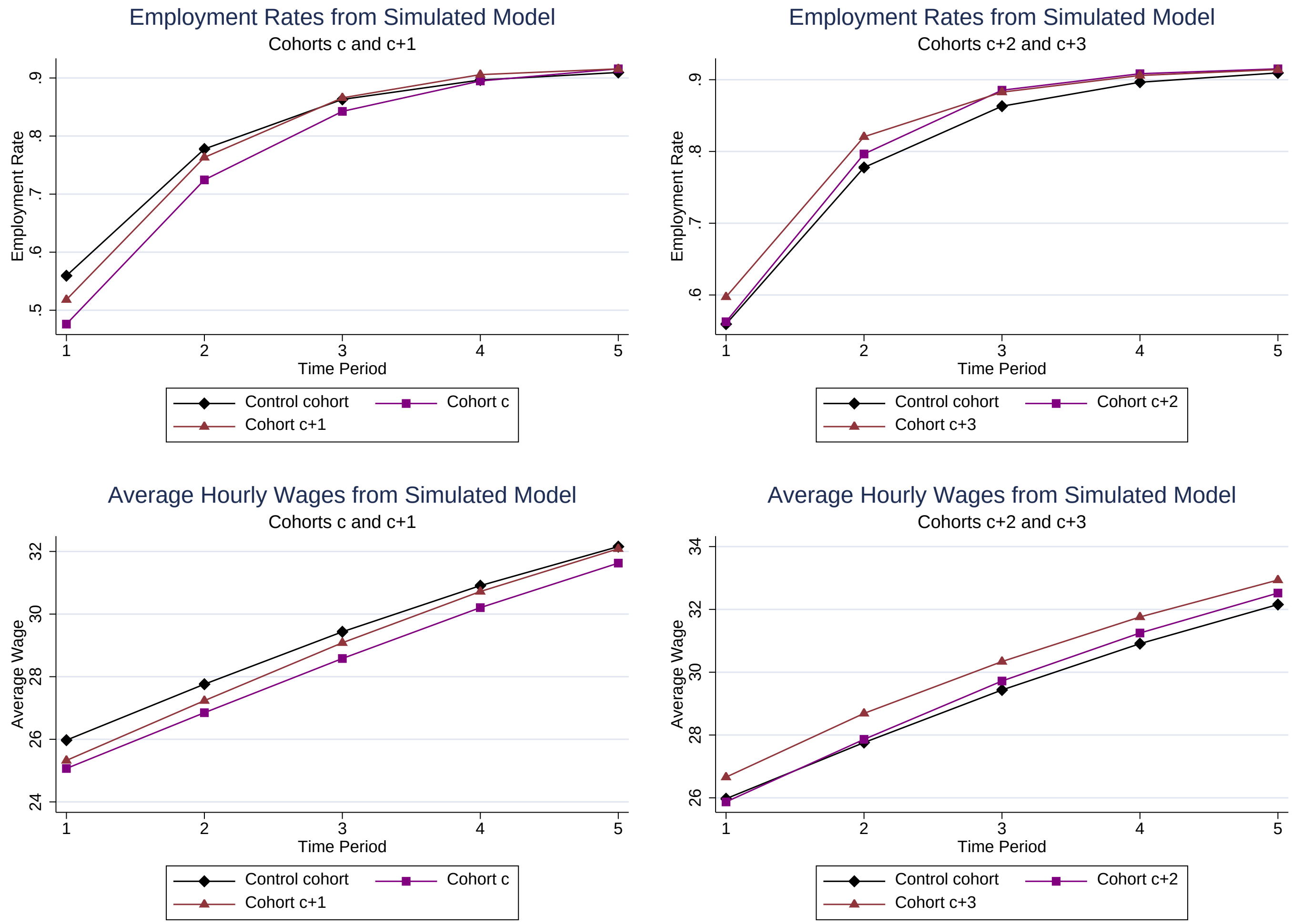

Figure 3: Graphical Example of Model Varying a