NBER WORKING PAPER SERIES

LABOR MARKET NETWORKS AND RECOVERY FROM MASS LAYOFFS: EVIDENCE FROM THE GREAT RECESSION PERIOD

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Working Paper 21262
http://www.nber.org/papers/w21262

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2015, Revised June 2019

This research was supported by the Russell Sage Foundation. Opinions expressed in this paper are those of the authors and not necessarily those of the U.S. Census Bureau, the Federal Deposit Insurance Corporation, the Russell Sage Foundation, or the National Bureau of Economic Research. Much of the work for this analysis was done while Mark Kutzbach was an employee of the Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed. We thank seminar/conference participants at the Institute of Poverty Research, the Israel Real Estate and Urban Economics Symposium, the Urban Economics Association, Tufts, Uppsala University, the University of Washington, the All-California Labor Economics Conference, the NBER Urban Economics Summer Institute, and the BLS-Census Workshop, and Bob Edelstein, Erika McEntarfer, Lars Vilhuber, and Maury Gittleman for helpful comments. This research uses data from the Census Bureau's Longitudinal Employer Household Dynamics Program, which was partially supported by National Science Foundation Grants SES-9978093, SES-0339191, and ITR-0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation.

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NBER Working Paper No. 21262
June 2015, Revised June 2019
JEL No. J01,R12

ABSTRACT

We measure the changing efficacy of neighborhood-based labor market networks, across the business cycle, in helping displaced workers become re-employed, focusing on the periods before, during, and just after the Great Recession. Networks can only be effective when hiring is occurring, and hiring varied greatly between 2005 and 2012, the period we study. We therefore focus on a measure of the strength of the labor market networks that includes not only the number of employed neighbors of a laid off worker, but also the gross hiring rate at that person's neighbors' workplaces. Our evidence indicates that local labor market networks increase re-employment following mass layoffs, and in particular, that networks serve to markedly increase the probability of re-employment specifically at neighbors' employers. This is especially true for low-earnings workers. Moreover, although hiring and employment rates decreased during the Great Recession period, the productivity of labor market networks in helping to secure re-employment for laid off workers was remarkably stable during our sample period.

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I. Introduction

During the Great Recession and its immediate aftermath, the U.S. labor market experienced massive job losses not seen in at least three decades. Hiring slowed to extremely low levels, employers took longer to fill vacancies (Davis et al., 2012), and unemployment peaked at 10 percent.[1] In other words, the labor market was dysfunctional along most commonly measured metrics, so that displaced workers at that time faced large barriers to re-employment. In this paper, we examine whether labor market networks, which help to resolve information imperfections between job searchers and firms, assist in the re-employment for recently-displaced job searchers. We study the productivity of networks formed by residential neighbors. More specifically, we examine whether the productivity of these networks declined during the Great Recession, further exacerbating the already large challenges displaced workers faced in finding re-employment then.

Our particular focus on the job finding outcomes of displaced workers is natural given the outsized importance of the large number of displaced workers during the Great Recession, and also given the compelling existing evidence that job displacement is an extreme adverse event. Displaced workers on average suffer years of low (or no) earnings post-displacement (e.g. Jacobsen et al., 1993, hereafter JLS; Davis and von Wachter, 2011), and even experience higher mortality (Sullivan and von Wachter, 2009).

Our emphasis on labor market networks defined by residential neighborhoods arises because of prior research indicating that such networks play an important role in matching workers to employers (Bayer et al., 2008; Hellerstein et al., 2011 (HMN) and 2014 (HKN)), especially for lower skilled workers who arguably were hit hardest by job displacement during the Great Recession (e.g., Farber, 2015). Because networks can only be productive when hiring is occurring, we focus on a measure of the strength of labor market networks that incorporates not only the number of employed neighbors of a laid off worker, but also the gross hiring rate at that person's neighbors' workplaces – which we therefore view as characterizing how "active" the network is.

There are multiple reasons to think that the productivity of networks may have fallen during the Great Recession.[2] The theoretical model in Galenianos (2014) predicts that network

[1] Both the JOLTS data and the Job-to-Job flows data (or J2J, based on LEHD) report approximately a 25% drop in the seasonally-adjusted hiring rate from 2006 to 2009, measured as non-farm hiring in JOLTS and main job hires in J2J, and averaged across each year (U.S. Bureau of Labor Statistics 2019b; Census Bureau, 2019). The unemployment rate reached its peak in October 2009 (U.S. Bureau of Labor Statistics, 2019a).

[2] For the purposes of this paper we treat the Great Recession period as extending from 2008 through 2010, because even though the recession had formally ended in 2010, unemployment was still extremely high and payroll job growth was still very depressed.

productivity should have been lower during the Great Recession for two reasons: because higher unemployment rates among network members lead an unemployed worker to be less likely to find work via a network contact (as explicitly modeled in the paper); and because an economic contraction means that fewer employers will seek to expand employment by hiring referred workers (as implicitly suggested via the model parameters). Empirically, Davis et al. (2012) show that employers filled vacancies at a slower rate during and after the Great Recession, potentially because the transactions costs of hiring and firing weighed more heavily on hiring decisions in the face of unusually high uncertainty about product demand. This might also suggest that referrals were less productive in filling vacancies. Anecdotally, there were conflicting stories about whether network connections were more productive or less productive during and after the Great Recession.[3] For example, some stories claimed that network hiring became more important in the Great Recession as employers grew pickier about their hires, while others suggested that networks were less important during the recession because network connections were "severed" by the huge labor market upheavals.[4]

In the end, understanding the changing effects of networks during the Great Recession can only come via careful empirical analyses. To address this question, we quantify the effects of network strength on the employment recovery of displaced workers in the periods before, during, and right after the Great Recession, testing whether strong labor market networks formed by residential neighbors helped in the labor market recovery of displaced workers by facilitating re-employment overall, and re-employment specifically with hiring employers where neighbors in the network were already working. Following many previous studies on displaced workers, we focus on displaced workers who are displaced in mass layoffs. And although the data we use are by nature observational (as are virtually all studies of displaced workers), we harness the power of our detailed administrative data (the U.S. Census Bureau's Longitudinal Employer-Household Dynamics, or LEHD) to estimate heavily saturated regression models in order to account for differential selection into displacement and into different kinds of residential networks and labor markets, to make a causal interpretation of our evidence more credible.

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[3] For example: http://money.cnn.com/2009/03/27/news/economy/yang_jobhunters.fortune/index.htm?utm_source=feedburner&utm_medium=feed&utm_campaign=Feed%3A+rss%2Fmoney_latest+(Latest+News); http://abcnews.go.com/Business/jobs-outlook-college-graduates/story?id=16345862; http://www.jibberjobber.com/blog/2008/10/07/how-to-find-a-job-in-a-recession/ (all viewed May 30, 2014).

[4] For example: http://www.nytimes.com/2013/01/28/business/employers-increasingly-rely-on-internal-referrals-in-hiring.html?_r=0 (viewed May 14, 2014).

To summarize, we find that stronger residence-based labor market networks facilitate re-employment by matching displaced workers to vacancies, especially at neighbor's employers – as theory would suggest. These effects are driven by low earners, as might be expected given that the relevant labor markets for low-skilled workers tend to be more local. Importantly, while both employment and especially hiring dropped markedly during the Great Recession, severely disadvantaging the re-employment possibilities of displaced workers, we find no evidence of a decline in the *productivity* of residence-based labor market networks matching job searchers to their neighbors' employers. This suggests that labor market networks may still be an important tool for job searchers to activate, even during economic downturns.

II. Motivation and Relationship to Previous Research

Standard approaches to the search behavior of unemployed individuals (e.g., Ham and Rhea, 1987) model the probability that an unemployed worker becomes re-employed as a function of the unemployment rate, the vacancy rate, the worker's reservation wage, and the worker's preferences for non-work activity. Clearly, during economic downturns such as the Great Recession, unemployment rates rise and vacancy rates fall, hindering re-employment. Reservation wages may fall too, leading to faster re-employment, but recent evidence suggests that reservation wages for job searchers did not fall much during the Great Recession (Krueger and Mueller, 2016).

Theoretical models of labor market networks expand on standard search models by assuming that imperfect information hinders the search behavior of unemployed workers and/or firms, and that information flows through networks. These models generally fall into one of two categories that describe the information imperfections and how they are mitigated by networks. In models such as Calvó-Armengol and Jackson (2007) and Ioannides and Soetevent (2006), unemployed workers do not have full information about job vacancies. Job searchers can learn about job vacancies either directly from employers or indirectly via employed individuals among their network contacts. The probability that an unemployed worker learns of a job vacancy is generally positively related to the size of his/her network, but it is negatively related to the unemployment rate in his/her local labor market, so that networks themselves are less useful for job searchers during economic downturns.

In the other class of network models, the information imperfection is on the employer side, as employers do not have full information about the quality of job applicants or the job match that would arise if the applicant were hired. Specifically, in Montgomery (1991) firms learn about a potential worker's ability if the firm employs individuals from the potential worker's network. In equilibrium, individuals are more likely to receive and accept wage offers from businesses that employ others in their network, creating stratification across employers on the basis of these networks.[5,6]

These two classes of models essentially layer onto standard models of job search the additional implication that an unemployed individual will have better labor market outcomes if he or she searches for work in a local labor market (or markets) in which they have many network contacts who can pass along information on specific job vacancies to the unemployed individual, or who can provide employers with information about the productivity or match quality of the unemployed individual. In these models, network contacts serve as conduits for information only when they are employed, because only then are they willing to pass along information about job vacancies or able to provide a referral to their employer. Moreover, when network contacts are themselves employed, they do not "compete" with job searchers to get information about vacancies or to be referred to a hiring employer.

Estimating credible models of job search behavior is challenging due to data constraints in measuring key variables such as the size and scope of local labor markets, characteristics of individuals that affect their reservation wage, the availability and accessibility of job vacancies. The challenge is amplified when trying to account for networks, because of a dearth of data on who is connected to whom in labor market networks. Partially as a result, when it comes to research on the importance of labor market networks in job search, there is a large, earlier body of empirical research that documents the importance of informal contacts in finding jobs, but does not identify with whom workers are networked (Ioannides and Datcher Loury, 2004).

More recent empirical research suggests that labor market networks based on residential communities or neighborhoods are important. Using confidential Long-Form 2000 Census data (in Boston), Bayer et al. (2008) show that two individuals who live on the same Census block are about one-third more likely to work on the same block than are two individuals who live in the same block group but not on the same block. (The latter may be as alike as those who live on the same block, but are less likely to be networked.) HMN take this further by trying to capture connections between neighbors who work at the same business establishment, and not just in the

[5] Jackson (2008, Chapter 10) provides a transparent discussion and comparison of these models.
[6] Working with network members does not always lead to higher productivity, however. For example, Bandiera et al. (2005) show that working with peers can lead to lower productivity when an individual's compensation creates negative externalities for peers.

same location, consistent with the hypotheses that labor market networks mitigate employers' lack of information about workers or that these networks provide job searchers with information on vacancies at those establishments.

HMN develop a measure of the extent to which employees of a business establishment come disproportionately from people who live in the same neighborhood (defined as a Census tract), relative to the residential locations of other employees working in the same Census tract but in different establishments. They term this measure "network isolation," to capture how much workers from the same neighborhood are isolated or segregated from workers from other nearby neighborhoods. The concept underlying this measure parallels the well-known and influential work by Granovetter (1974), but extends it beyond a narrow (and by now old) case study to a very large national sample. HMN calculate network isolation using information on workers reporting to the 2000 Decennial Census Long Form who are matched to administrative information on establishments. The results indicate that local, residence-based labor market networks at the level of a Census tract appear to be quite important in influencing where people work, especially for less-educated workers, minorities, and immigrants.

Although we focus on residential labor market networks, one important caveat is that networks can be formed along many dimensions of society in which people interact – not just neighborhoods, but also workplaces, extended families, religious and civic institutions, etc. Aside from the past evidence that local labor market networks matter, another rationale for focusing on these networks is that we are able to construct measures of them in large-scale national data sets – in our case, on where people work, with whom they work, and where they live. Our caveat is illustrated in some related work on labor market networks and recovery from job displacement that focuses on potential network connections between former co-workers. Glitz (2017), Saygin et al. (forthcoming), and Cingano and Rosolia (2012) all find that network connections to co-workers (or former co-workers) are important in helping displaced workers find employment, and Saygin et al. also find some evidence that displaced workers are more likely to become re-employed at a firm that employs former co-workers of the displaced worker.[7] This research reinforces the idea that networks are not only local, based on geography, and that it is possible to measure at least some other types of potential network connections in datasets similar to those we use.

A second caveat, shared with much work on networks, is that we use our data to capture

[7] Saygin et al. (forthcoming) suggest that this implies that these former co-workers are referring the displaced worker to their employer, à la Montgomery (1991) and Simon and Warner (1992), but this evidence is equally consistent with former co-workers providing information about the availability of jobs at their firms.

people with potential network connections, but we do not observe the actual network connections – i.e., between which members of the network information about jobs actually flows; this could attenuate our estimates of the effects of networks. We regard the evidence we present on re-employment at neighbors' employers (which, as we explain, is conditional on other local hiring), as significantly bolstering a network interpretation of our evidence. On the other hand, evidence we find indicating that the effects of local labor market networks are larger for lower-skilled workers could reflect more attenuation of network effects in our estimation because the actual labor market networks of higher-skilled workers are less local.

Regardless of the kind of network considered, we are not aware of any previous papers that have directly examined empirical evidence on whether the productivity of networks varies with labor market conditions, and in particular how networks functioned during the Great Recession to help displaced workers recover from job loss.

III. Network Measures and Analysis

Consider a sample of workers who lose their jobs as part of a mass layoff. How quickly do these displaced workers find new jobs? And does the strength of their neighborhood networks affect whether these laid off workers find jobs quickly, and where they are re-employed?

The theoretical models of general job search described above tell us that a displaced worker's probability of finding work in a given period will be a positive function of the vacancy rate in their local labor market, a positive function of the employment rate in their local labor market (or a negative function of the unemployment rate), and a negative function of the worker's reservation wage. In all of the empirical models we estimate, therefore, we include measures of the local employment rate and the local vacancy rate, as well as measures that are meant to capture the reservation wage.

Network measure and related controls

The network models described above augment standard search models by positing an additional mechanism by which the employment rate and vacancy rate affect a displaced worker's probability of finding work. Specifically, employed network members are useful to job searchers not only because employed workers do not compete for vacancies, but also because, for any given vacancy, employed workers facilitate information transfers that increase the probability that a job searcher will be hired into that vacancy. In our empirical analysis of how networks matter for displaced workers, we therefore consider how the re-employment probability of a displaced worker is affected by the opportunities conveyed by his or her residential labor market network, examining first re-employment generally and then homing in specifically on re-employment at a neighbor's workplace.[8]

As in HKN, we operationalize the strength of a job searcher's network by developing a measure of residence-based hiring networks at the level of the Census tract of residence. Census tracts are a geographic definition with many features in common with standard conceptions of a neighborhood.[9] The Census Bureau defines tracts to be contiguous and clearly bounded geographic units with a target size of about 4,000 residents (ranging from 2,500 to 8,000), and tracts are designed to contain a population with similar housing and socioeconomic characteristics. We restrict the analysis to urban Census tracts, which are defined based on population density and may fall in both central cities and suburbs. In 2010, Census tracts defined as urban contained 81 percent of the U.S. population. Details on the LEHD data and our sample construction appear in the appendix.

We first examine the impact of our tract-level measure of network strength on the re-employment outcomes of displaced workers, and how the effect varied across the periods before, during, and just after the Great Recession. We estimate the effect for our whole sample of workers displaced in mass layoffs, and then for the sample of workers who we classify as lower-skilled. We then consider whether our measure of network strength leads to higher re-employment of these displaced workers specifically at the employers of their employed neighbors, as network theory would suggest. We are able to condition on an extremely large set of worker, employer, neighborhood, and job-related covariates (as explained below), in order to control for observable characteristics of workers and their residential neighborhoods on which job searchers might sort into displacement, networks, and labor markets.

We limit our analysis to examining outcomes in the quarter following displacement, partially for simplicity, but more so because workers with long durations of unemployment prior

[8] We do not report results for earnings as an outcome in our network analysis for a number of reasons. First, in HKN we found strong positive effects of networks on reducing turnover for employed workers, but less robust results for wages. Although network models predict better job matches that should lead to higher wages, the effect could go in the other direction either because people prefer to work with their neighbors, or because worker reliance on networks may signal high search costs enabling employers to offer lower wages. Second, in the context of the Great Recession's historically high unemployment rates and low labor force participation, re-employment for displaced workers is the first-order outcome of interest. Third, and relatedly, as we show below (Figure 2), the recovery of earnings in our sample is itself driven primarily by re-employment. As a result, although we did explore the impact of networks on the post-displacement earnings of displaced workers, these results are driven by re-employment.

[9] Indeed, the Census Bureau suggests that visitors to its website who are interested in learning about their neighborhoods do so by looking up Census-based statistics on their Census tract of residence. See: https://www.census.gov/newsroom/blogs/random-samplings/2013/12/discover-your-neighborhood-with-census-explorer.html (viewed June 28, 2019).

to the Great Recession were likely much more negatively selected than those with long durations during the Great Recession, whereas workers with short durations of unemployment were likely more similar in the two periods, making comparisons of network effects on re-employment before, during, and after the Great Recession more valid.

In order to explain our network strength measure and how we construct it using the LEHD data, consider the hypothetical case of a specific job searcher who is searching for a job after being displaced from his/her employer in a mass layoff in a given quarter. Given the detailed longitudinal nature of the LEHD, we observe the displaced worker's pre-displacement earnings on a quarterly basis, as well as his/her post-displacement employment and earnings (if any). We also have the location and industry of the establishment at which the job searcher last worked, as well as some demographic information about him/her. For employers with multiple establishments in the same state (accounting for about 40 percent of jobs), assignments are uncertain, so we assign a worker to an establishment that is drawn by a model designed to replicate the size distribution of establishments and the observed distribution of commute distances to workers' places of residence. In other words, workers tend to be assigned an establishment nearby their home. Critically, we also observe the Census tract in which the job searcher lives.

We also observe various characteristics of that Census tract, most importantly the number of adult neighbors that the job searcher has (defined as residents of that Census tract). For each of those neighbors, we know whether the neighbor is employed in the quarter following the job searcher's displacement. In addition, for each employed neighbor, we observe the establishment in which they work (their "employer"), as well as characteristics of the establishments, including where those establishments are located, establishment size, and, importantly, gross hiring (if any) at these and other nearby establishments in the post-displacement quarter.

We name our network measure the "active employer network" measure, denoted *AEN*. This measure is motivated explicitly by theoretical network models (such as Calvó-Armengol and Jackson (2005) and Montgomery (1991), as well as others) where – in our context of neighborhood-based labor market networks – a job searcher's employed neighbors transmit information about vacancies at the establishments where they work, and can transmit information about the job searcher to their employers who are looking to fill those vacancies. The "active" part of the name captures the idea that a job searcher's network may consist of all neighbors, but a network contact is only useful if the neighbor is employed at a firm that is hiring. That is, individual job seekers may have many network contacts, but unless these contacts can facilitate the transmission of information that is related to vacancies at their employers, they are not productive contacts.

We do not have direct measures of vacancies, but we do have information about gross hiring in all LEHD establishments in each quarter, which is a reasonable proxy for vacancies (especially ones that are "active" in the sense that employers are eager to fill them). Therefore, for each establishment at which a neighbor works, we calculate the gross hiring rate at that establishment in the quarter following the job searcher's displacement (defined as the gross number of new hires divided by the number of employees in the beginning of a quarter). Using a measure of the gross hiring rate rather than the absolute number of gross hires is a scaling measure intended to capture competition among job seekers for vacancies. That is, our job searcher's neighbor may have information on vacancies at his or her establishment to transmit to our job searcher, but that information is also transmitted by employees who live in other Census tracts back to the job searchers in their own Census tracts. In other words, a large number of gross hires at a neighbor's large employer does not necessarily imply that our job searcher learns about more potentially productive vacancies than from a small number of gross hires at a small employer.

We then calculate the average of this gross hiring rate across all of our job searcher's neighbors, where his/her unemployed neighbors contribute zeroes to this average. Thus, the "active employer network" measure for our job searcher is defined as:

$$AEN = \frac{1}{N}\sum_{i}^{N} I_i \cdot \frac{H_{ie}}{L_{ie}}$$

where N is the number of neighbors in our job searcher's Census tract at the time of his/her displacement (excluding the job searcher and any other displaced workers), I_i is an indicator for whether neighbor i is employed in the quarter following the job searcher's displacement, and $\frac{H_{ie}}{L_{ie}}$ (the "active" part of the name) is the ratio of new hires at the employer e of neighbor i in the first quarter following our job searcher's displacement, divided by the count of employees at that employer in the beginning of that quarter. Note that the neighbors who are not employed contribute zeroes to the measure; $\frac{H_{ie}}{L_{ie}}$ is undefined for these cases, but we have not introduced additional notation since this expression is multiplied by zero in these cases. We average across all of our job searcher's neighbors, N, rather than just over employed neighbors, to reflect that when more neighbors are searching the probability that our job searcher will obtain productive information on vacancies from his or her neighbors may be diluted, for two reasons. Either vacancy information is like a private good passed along by employed workers to only a subset (of perhaps one) of the job searchers in their network, or else our job searcher will have to compete with his/her neighbors when applying to job vacancies that are accessed through the neighborhood network.

Note also that if multiple neighbors work at the same employer, each of these contacts contributes to *AEN*. If we actually knew that every neighbor was in our job searcher's network, this might lead to double counting, because neighbors could be giving the job searcher redundant information about vacancies. However, it is more likely that information about job vacancies and referrals flows between our job searcher and only a subset of neighbors, in which case more neighbors working at an employer who is doing hiring makes it more likely that information about those vacancies reaches our job searcher (or that a referral is made). In addition, if there is some noise in the vacancy information that a given neighbor transmits, that noise can diminish relative to the signal if vacancy information is transmitted by multiple neighbors (and the noise is not perfectly correlated across them). For these reasons, we allow the network measure *AEN* to increase in the number of neighbors working at the same employer.

In all of our empirical specifications where we test the importance of *AEN* for re-employment outcomes, we also include both a measure of the local employment rate and a measure of the vacancy rate, thereby capturing the essential role that these local labor market characteristics play in job search even absent the existence of networks. Because these measures, like *AEN*, are also derived from information on neighbors, we define them here.

We calculate the local employment rate, *ER*, at the level of a job searcher's Census tract:

$$ER = \frac{1}{N}\sum_{i}^{N} I_i.$$

We use the gross hiring rate in all establishments located in Census tracts in which a displaced worker's neighbors (i) work (indexing these tracts by w) to construct a proxy measure for the local labor market vacancy rate. We denote this *HRT* (the "Hiring Rate in the Tract") and measure it as:

$$HRT = \left[\frac{\sum_i^N I_i \cdot \frac{H_{iw}}{L_{iw}}}{\sum_i^N I_i}\right].$$

For a job searcher in the quarter following displacement, $\frac{H_{iw}}{L_{iw}}$ is the hiring rate among *all* employers in the Census tract where neighbor i works. We sum these workplace Census tract hiring ratios across all employed neighbors and divide by the number of employed neighbors. Thus *HRT*, as a measure of the average gross hiring rate in Census tracts where neighbors work, captures the general strength of demand conditions in the local labor market, because neighbors' workplace locations likely represent the set of locations with economic opportunities that are accessible by transportation and where employers may have skill demands that match the skills of neighbors generally.[10]

Descriptive information on sample

Table 1 provides mean characteristics of our sample of 9.2 million workers displaced from 2005 to 2012, including the outcomes, the network measures and related controls, as well as additional controls we use in the regression models described in the next section. Among these, we link in the neighborhood (Census tract) poverty rate (from the 2000 Decennial Census), as well as numerous other tract characteristics pertaining to demography, education, and residential mobility, which control for longer-term labor market conditions of the worker's place of residence and characteristics of the worker's neighbors. Worker age is calculated for the quarter of displacement, and industry classification is the industry code of the establishment from which a worker was displaced.

Table 2 lists the distribution of our sample and some key characteristics across years. The sample share increases from 12.2 percent of displacements in 2005, to a peak of 17.6 percent in 2008, and then falls to 10.3 percent in 2011.[11] This pattern is what we would expect given the timing of the Great Recession, and is also reflected in the distribution of the number of layoff events (column (4)).[12] Column (7) shows that workers displaced in years encompassing the Great Recession (2007Q4-2009Q2) – especially 2009 – had higher pre-separation earnings at their main job. This evidence for earnings from the main job is consistent with mass layoffs falling across a broader swath of workers during the Great Recession.

Figure 1 displays various percentiles of the employer network measure (*AEN*), the employment rate (*ER*), and hiring rate (*HRT*). For some intuition about the value of *AEN*, consider

[10] Bayer et al. (2008) control for the strength of the local labor market by treating neighbors as those who live only on the same Census block in measuring network ties, and treating correlated outcomes among those who live in the same block group as (potentially) capturing local labor demand, job access, etc.
[11] The shortfall in 2006, compared to the surrounding years, is due to imprecision in Census Bureau geocoding of administrative records for residences in that year. The lower percentage of observations (7.5 percent) in 2012 occurs because we only use displacements up to and including the third quarter; data necessary for computing the network measures for those displaced in 2012Q4 were not available at the time of analysis.
[12] The distribution of displacement events has little seasonality, although there are slightly more in third quarters. During the recession, there are some years where displacements are more concentrated in a particular quarter, especially late 2008 and early 2009.

a job searcher residing in a tract with a median value of the network measure. Based on the median value of 0.108 in 2006, a random neighbor would be expected to have information on approximately one active job vacancy for every ten workers at an employer (with values for the first and third quartiles of 0.09 and 0.13). All three measures exhibit a clear pattern of decline associated with the Great Recession followed by some recovery, as we would expect from the changes in both the proportion of neighbors employed, and especially the hiring occurring at nearby employers. Note, in particular, that by 2009, the percentiles of *AEN* had fallen by more than one-third relative to their pre-recession levels.

<u>Analytical framework and identification</u>

We estimate linear probability models for re-employment in the quarter after displacement that are variants of the following form:

$$Emp_{jnkt} = \alpha^p + AEN_{nkt}\beta_1^p + ER_{nt}\beta_2^p + HRT_{nkt}\beta_3^p + X_{1jt}\gamma_1^p + X_{2n}\gamma_2^p + \varepsilon_{jnkt}. \quad (1)$$

The subscript *j* indexes the individual laid-off worker, *n* indexes residential neighborhood, *k* indexes the local labor market (which generally contains neighborhood *n*),[13] and *t* indexes the year/quarter in which the displaced job ended. X_{1jt} and X_{2n} are vectors of observable characteristics of individual *j* and his/her neighborhood *n*, respectively, which we include in some specifications.[14] The superscript *p* denotes the subperiods we consider: pre-Great Recession (2005-2007), Great Recession (2008-2010); post-Great Recession (2011-2012).[15] The variables *AEN*, *ER*, and *HRT* are as previously defined. Note that for job searcher *j* in year *t*, the set of persons displaced at the same time (including *j*) are excluded from the set of employed neighbors in the calculation of *AEN*, *ER*, and *HRT*.

As previously discussed, *AEN*, the key variable of interest, measures the strength of the neighborhood network. *ER* and *HRT* are local labor market characteristics relevant for job search (*ER* may also be a proxy for demographic neighborhood characteristics).

We think of the error term ε_{jnkt} as having three systematic components varying at the individual, local labor market, and neighborhood level, plus an idiosyncratic error term:

$$\varepsilon_{jnkt} = \eta_{jt} + \mu_{kt} + \omega_{nt} + \upsilon_{jnkt}. \quad (2)$$

[13] The Census tract, over which *ER* is calculated, is indexed by *n*. *AEN* and *HRT* are calculated over all tracts in which residents of a tract work (although *AEN* is computed only over the establishments in those tracts where neighbors work). This “local labor market” is indexed by *k*, the dimensions of which can vary across tracts depending on where residents of that tract work, so we also include an *n* subscript.

[14] The Census tract controls do not vary with time.

[15] We omit *p* from the subscripts; we just want to emphasize that we compute estimates of versions of equation (1) for different subperiods.

There are valid reasons to be concerned that the first three components of the error term in equation (2) are systematically correlated with the network measure *AEN*, in which case failure to account for these correlated unobservables could generate spurious evidence of effects of networks on re-employment. To account for this, we assume that the first two parts of the error term in equation (2), $\eta_{jt} + \mu_{kt}$, can be rewritten as:

$$\eta_{jt} + \mu_{kt} = E_{ct} + (\nu_{1jt} + \nu_{2kt}), \quad (3)$$

where E_{ct} represents a fixed effect that is uniquely defined by the year/quarter (designated by *t*) and the county location of the establishment. The establishment-county pair is indexed by *c*. Thus, E_{jct} is a fixed effect for a specific mass layoff in an establishment, or establishments of the same firm, in a geographic area. We include this fixed effect in all of the results we report, which implies that we identify the effect of neighborhood labor market networks on post-displacement employment from variation in the network measure *AEN* among individuals who are laid off in the *same* quarter, from the *same* firm, and from establishments of that firm in the *same* county. The identifying variation thus comes from co-workers who are laid off together but live in different neighborhoods.

Although these layoff fixed effects absorb a lot of the variation across workers that represent both their own characteristics and that of their local labor market, there may still be remaining variation that is not accounted for. As a result, while in our initial regressions we do not include the vector of control variables X_{1jt} and X_{2n}, we emphasize results from regressions that include detailed controls. The vector X_{1jt} includes controls for age, sex, race, and ethnicity from the LEHD, sourced from administrative data and Censuses. We can also control for annual earnings in the previous year from the displacement job as well as from all of a worker's other employers. These pre-layoff earnings measures are proxies both for the human capital of displaced workers and for their reservation wage, which can affect their job search behavior. We also include indicators for the industry of a worker's establishment, by seven high-level groupings (though in practice, there is little variation in industry for establishments in the same firm, in the same county). The vector X_{2n} contains a set of Census tract-level neighborhood characteristics that we construct from the 2000 Decennial Census, Summary File 3, including measures of the racial and ethnic composition, the share of residents in poverty, the share foreign born, the shares at different education levels, and the share of residents in the same home as five years ago. To the extent that individuals sort into neighborhoods based on shared preferences and characteristics, these neighborhood controls may also be proxies for individual-level characteristics. In principle the neighborhood characteristics could be time-varying, but we do not have access to them on an annual basis, and we fix them at year 2000 values.

Our complete specifications, then, include the detailed controls X_1 and X_2, as well as *ER* and *HRT*, and the layoff-specific fixed effects. Our identifying assumption is then that co-workers with the same values of these controls who lose their job in the same mass layoff face systematically different post-layoff employment outcomes associated with local labor market networks only because they have access to different neighborhood networks – that is, different *AEN*s.

To underscore the role of the layoff-specific fixed effects in the identification strategy, note first that workers who are laid off in the same mass layoff had previously been working for the same employer in the same county. To the extent that workers sort as a function of unobservable person-specific characteristics (or preferences for workplace amenities), the layoff fixed effects account for this. Note further (and importantly) that the period dimension of these layoff fixed effects captures both heterogeneity in the types of workers who are laid off in that quarter and in the strength of the local labor market at the time of the layoff. This worker heterogeneity was already noted in reference to Table 2, which showed that pre-displacement earnings were highest for those laid off at the height of the Great Recession, suggesting that in this period workers who experienced mass layoffs were on average higher quality than workers laid off when economic conditions were stronger, perhaps because mass layoffs during stronger economic conditions are more likely to be related to low productivity of the workforce. The workplace-by-year dimension of the fixed effects also controls for the generosity of time-varying state variables such as UI benefits during and after the Great Recession, which are another component of job searchers' reservation wages, and likely also capture any relevant local policy variation. Finally, note that the layoff-specific fixed effects lead to a highly saturated regression model; the number of these fixed effects is about one-quarter of the overall sample size. We cluster the standard errors at the same level as the fixed effects to account for common unobservables affecting outcomes of those experiencing the same mass layoff.

Returning to the error term expressions (equations (2) and (3)), from an operational standpoint, note that excluding the displaced individual in the construction of *AEN* avoids a mechanical correlation between *AEN* and η_{jt}. And excluding others displaced at the same time avoids a correlation between *AEN* and ω_{nt} owing to workers from the same neighborhood being laid off and searching for work together in particular periods. We implicitly treat the third term in equation (2) – the remaining neighborhood-specific error term ω_{nt} – as uncorrelated with *AEN*, conditional on the other observables of workers and neighborhoods and, importantly, the layoff fixed effects. This still leaves open the possibility that residential neighborhood sorting by unobservables (based, for example, on shared preferences for amenities) is correlated with our network strength measure, *AEN*. To explore this possibility, we conduct various robustness checks, as described below.

We estimate equation (1) for two different employment outcomes. *Emp* is first defined as whether the displaced worker is re-employed at all (observed in the LEHD to have positive earnings) in the post-displacement quarter under consideration. We then narrow the re-employment definition so that *Emp* is an indicator for becoming re-employed at the establishment of a neighbor. Because this measure captures employment at a neighbor specifically, the evidence using this re-employment definition speaks more directly to whether the employment effects of residence-based networks that we estimate actually reflect neighborhood networks, as the theoretical models of networks we have discussed would predict directly. It is also the case that any potential remaining role for correlations between the error components and *AEN* is reduced when we focus on re-employment at a neighbor's establishment, because generic sources of variation in re-employment per se play a much smaller role.

IV. Results

Earnings and employment loss and recovery

Because the central focus of studies of job displacement to date is the earnings recovery of displaced workers, we present, in the top panel of Figure 2, the standard depiction in this literature of the observed earnings shock associated with displacement. The panel depicts quarterly earnings (in levels) of the displaced workers, up to one year before and two years after the mass displacement, including workers with zero earnings in post-displacement quarters (all must work in the earlier quarters). Each line tracks the earnings of workers displaced in a given year, with quarter zero giving the average earnings of that cohort in the final quarter before displacement. Figure 2 shows that there is a drop in average earnings from approximately \$9,000 in the last quarter prior to displacement to average earnings of between \$3,800 and \$5,300 in the quarter following displacement, with those earnings rising to a range of about \$5,800 to \$7,100 by the eighth quarter, still remaining well below pre-displacement earnings.

Comparing the results by year, those displaced in 2005 and 2006 have the smallest average drop, and within two years they recover on average to within about \$1,900-\$2,200 of pre-displacement earnings. At the other extreme, those displaced in 2009 have the largest drop and remain on average about $3,500 (nearly 40 percent) below pre-displacement earnings two years post-displacement. The very sharp earnings losses and slow recovery for those displaced during the Great Recession suggest that if networks are helpful in the re-employment of workers displaced during a recession, the earnings effect could be pronounced.

One obvious question that arises is whether the drop in earnings is driven by those who have no post-displacement earnings, or whether it is driven by a drop in earnings for those who find new employment. The middle panel of Figure 2 uses the same sample of displaced workers but tracks quarterly employment (based on positive earnings). Because all the workers are employed up to and including the quarter of displacement by construction, the share employed for workers displaced in each of the years all overlap at a height of one until the post-displacement quarter. After that, the paths diverge, and then the figure closely parallels the results for earnings, implying that the earnings results are driven primarily by re-employment. In particular, around 64 percent of those displaced in 2005 or 2006 are re-employed in the first post-displacement quarter, but that percentage drops with each subsequent cohort of displaced workers through the 2009 displacements (and then rises beginning in 2010), and the re-employment rate in the quarter after displacement is only 48 percent for those displaced in 2009. In addition, those displaced in 2008 and 2009 have recovered the least two years after displacement – only 65 percent are employed by then. On the other hand, the recovery of employment appears steepest for those displaced in 2009, suggesting that re-employment of these displaced workers picked up as the economic recovery began; in contrast the pace of re-employment, was slower for those displaced earlier but still not employed as the Great Recession began to unfold.

We also confirm, in the bottom panel of Figure 2, that most of the earnings drop observed post-displacement (in the top panel) is, in fact, driven by those with zero post-displacement earnings, by producing an analog to the top panel of the figure, dropping observations from any quarter where earnings are zero. As expected, the pattern in this figure shows that post-displacement earnings if one works are not very different from pre-displacement earnings,[16] so what is most interesting to us – and perhaps more tied to network strength – is re-employment. We therefore focus the rest of our analysis on the re-employment margin.

[16] Our evidence that employment is the key driver of earnings losses is somewhat at odds with what was found in Davis and von Wachter (2011) for displaced workers. This is likely because our data are at a quarterly frequency whereas theirs are annual, implying that an employment shortfall for part of a year will show up as an earnings shortfall in annual data.

The effects of networks on re-employment

We now turn to our main analyses – the estimated effects of residence-based labor market network measures on various measures of employment. In Table 3, we report the results of the employment regressions represented by equation (1), where the dependent variable is a dummy variable indicating whether or not the laid off worker was re-employed in the quarter following the layoff. We report results separately for each period (2005-2007; 2008-2010; 2011-2012), allowing us to track the effects of networks on re-employment separately through the subperiods of our sample. Throughout the columns and panels of results representing different regressions and samples, we report the coefficients on *AEN*, *ER*, and *HRT*. In addition to reporting the estimated coefficients and their standard errors, we also provide, below the regression estimates, the implied effects of moving from the 25th to the 75th percentile of the distributions of these network measures.

The top panel of Table 3 reports estimated regression coefficients for the full sample of displaced workers in each period. The first three columns contain no controls aside from the fixed effects representing the worker's mass layoff; the second three columns contain the demographic and neighborhood controls.

In column (1) of the top panel, the results show that in the three years prior to the Great Recession (2005-2007), the estimated coefficient on the active network measure (*AEN*) is positive (0.161) and statistically significant. The implied interquartile change from the 25th to the 75th percentile of *AEN* raises the probability of re-employment in the quarter following displacement by 0.6 percentage point (compared to a mean job finding rate of 63.8 percent).[17]

The employment rate (*ER*) effect and the effect of local hiring (*HRT*) are also statistically significant and have relatively large interquartile effects, suggesting that standard measures of local labor market strength also impact re-employment, as would be expected in any job search model. This pattern persists throughout all of our results, so we focus our discussion, henceforth, on *AEN*.

Column (2) reports results from the Great Recession period where labor markets were clearly disrupted (2008-2010). The coefficient on (*AEN*), 0.068, is smaller than in column (1), suggesting perhaps that labor market networks were less productive in helping re-employed workers during the Great Recession, although the effect is still statistically significant. Moreover,

[17] We multiply the coefficient 0.161 for *AEN* from Table 3, column (1), by the range from 0.090 to 0.125, which gives an implied effect of 0.006 on the indicator for re-employment. See Appendix Table A2 for the percentiles of the three key components of local labor market strength, *AEN*, *ER*, and *HRT*.

during the Great Recession period the interquartile range of (*AEN*) is smaller than it was during the pre-recession period (see Appendix Table A2), so that the interquartile impact of (*AEN*) is quite a bit more muted than in column (1) – 0.2 percentage point versus 0.6. In the column (3) results for the post-Great Recession period (2011-2012), the estimated coefficient on (AEN) is 0.270, again statistically and economically meaningful through the interquartile range.

Although the regression specifications in columns (1)-(3) of Table 1 contain layoff fixed effects, which (as we explain in Section III) control for a lot of variation in local labor demand and in worker characteristics that affect the probability of re-employment, there is still a possibility that unobservables that are correlated with *AEN*, even conditional on *ER* and *HRT*, affect re-employment in a way that biases the estimated coefficients of *AEN*. In order to assess whether this is the case, in columns (4)-(6) we add into the regression a large set of control variables, reflecting both characteristics of the Census tract in which the displaced worker lives, as well as individual demographic characteristics of the worker. The full list of control variables is given both in the summary statistics in Table 1, and in the footnote to Table 3.

The estimated coefficients on *AEN* in columns (4)-(6), reflecting the impact of local neighborhood network strength on re-employment throughout the period, are remarkably similar to each other, at 0.268, 0.246, and 0.248, respectively, and the effects of the second and third periods are not statistically different from the first period (see Appendix Table A4).[18] The coefficient estimate of *ER* is also very stable across the three periods. In contrast, even with the controls, the estimated coefficient on *HRT* doubles during the Great Recession, suggesting that the local hiring rate is important for re-employment during an economic downturn, which we would expect given that there is local variation in labor demand conditions. This result helps highlight how our inclusion of *ER* and *HRT* control for sources of variation in re-employment that, if omitted, could lead to biased estimates of the effect of our measure of active labor market networks – given that our measure is also influenced by how much local hiring is occurring.

Of course, one can never fully test whether there is remaining unobserved variation in local labor markets that is conditionally correlated with both *AEN* and with re-employment in a way that generates spurious evidence of network effects. But given the fact that controlling for additional

[18] The same qualitative pattern of coefficients is observed when we estimate the regressions year-by-year; that is pooling the data into three periods does not affect the qualitative results at all. For parsimony, and because the three distinct periods we use in Table 3 reflect three distinct periods around the Great Recession years, we only report results separately for the three periods.

observables in the regressions in columns (4)-(6) increases the point estimate on *AEN* markedly in the first two periods (relative to the estimates in columns (1) and (2)), and leaves it close to unchanged in the last period, it is difficult to imagine a scenario where remaining unobservables are leading to large omitted variable bias in the other direction.[19] And it is not entirely surprising that the estimates change when we add the individual and tract controls, given that mass layoffs were likely to have affected a broader cross section of workers during the Great Recession.

Thus, our takeaway is that the productivity of networks, as captured in the estimated effects of *AEN*, did not vary substantively in the periods before, during, and just after the Great Recession. Still, although the coefficient estimates on *AEN* are very similar in the last three columns at the top of Table 3, the interquartile effects of *AEN* are somewhat different: 0.009 (for 2005-2007), 0.006 (for 2008-2010), and 0.005 (for 2010-2012). The higher interquartile effect in the first period mostly reflects the larger variation in *AEN* in that period, as seen in the top panel of Figure 1.

The bottom panel of Table 3 reports estimated regression coefficients for the sample of 7 million workers who are low-earning individuals (about three quarters of our total sample); in particular, those whose pre-displacement earnings were below $50,000. We assume that this sample is lower skilled based on lower income. We expect that for lower-skilled workers local neighborhood networks are more important for securing re-employment after layoff, based on evidence from HMN that local labor market networks are more important in determining who works where for lower-skilled (less-educated) workers than for higher-skilled workers. Indeed, the estimated coefficients on *AEN* in the bottom panel of the table, across all of our subperiods and specifications, are larger than in the full sample.[20] Once again, for this low-earning sample, when we include the full set of local labor market and demographic controls in columns (4)-(6), the estimated coefficients on *AEN* are remarkably stable – 0.307, 0.294, and 0.295 across the three subperiods of our sample (and are more stable in columns (1)-(3) as well).[21] Once again, however, because the interquartile range of *AEN* differs across the three periods, and in particular was higher prior to the Great Recession, the economic effect of *AEN* through this range is different across the three periods. In particular, our results suggest that local neighborhood networks were

[19] Altonji et al. (2005) formalize this argument, and Altonji and Mansfield (2014) present results from implementing this kind of approach.
[20] Although recall our earlier caveat that we may have more attenuation in estimating the effects of *AEN* on the re-employment of higher-skilled job searchers because their networks are less local.
[21] For the residual observations in the higher-earnings sample, the estimated effects of *AEN* on the probability of re-employment were small and statistically insignificant.

more effective in helping displaced workers find re-employment before the Great Recession, but this was due to the fact that neighbors' employers were doing more hiring, and not due to a higher inherent productivity of these networks when employers were filling vacancies.

Re-employment at a neighbor's employer

Because the outcome in Table 3 reflects re-employment at any employer in the quarter following displacement, it does not specifically address the network mechanism by which employed neighbors serve to connect displaced workers to vacancies at their own employers. In Table 4, therefore, we use a binary outcome variable that reflects re-employment in the quarter following layoff at a neighbor's employer (only). The estimated network effects in this table therefore capture the most direct implications of the network mechanisms we wish to test. In particular, if the employed members of our neighborhood networks serve directly as conduits for information about vacancies and/or worker quality between the establishments in which they work and displaced workers, these networks should yield higher probabilities of re-employment specifically at those establishments.

The results in Table 4 are reported in exactly the same way as in Table 3, so the only difference between the two tables reflects the results of changing the outcome variable. In the top panel, where we report results for the full sample, the coefficient estimates on *AEN* are substantially larger in magnitude than in Table 3, confirming that re-employment is happening at networked neighbors' employers. Here, the point estimates are relatively stable across the columns, whether or not we include the detailed controls; but the estimates are especially stable in columns (4)-(6) where we include the full set of controls. The interquartile effects of *AEN* are quite substantial, ranging across columns (4)-(6) from 1.5 percentage points in the pre-Great Recession period to 1 to 1.1 percentage points in the other two periods. Unlike in Table 3, here the interquartile effects of *AEN* are as large or greater than the interquartile effects of *ER* and *HRT*, which we interpret as demonstrating the importance of neighbors in helping job searchers to find employment specifically at a neighbor's employer, rather than to become re-employed more generally. To put these interquartile effects into perspective, note that the baseline probability of working with a neighbor's employer ranges from 12 to 14 percent.

The bottom panel of Table 4 reports results for the low-earnings sample. As in Table 3, the coefficients on *AEN* are larger than in the full sample; the interquartile effects of *AEN* are similar to that of the full sample.[22]

Additional results

We next turn to a series of robustness results. The results of the two previous tables show the stability of the coefficient of *AEN* across the three periods of the sample, even more so when the detailed controls are included. Because the models with the full controls are more compelling, here we proceed by simply pooling all years of the sample together and reporting aggregate results for 2005-2012 for these specifications. Table 5 reports results using the same dependent variable as Table 3 – whether the displaced worker became re-employed in the first quarter following displacement; Table 6 reports results using the same dependent variable as Table 4 – whether the displaced worker found work at a neighbor's employer. In the first column in each of Tables 5 and 6, we report estimated coefficients for the pooled sample from the regressions that parallel those of the last three columns of Tables 3 and 4, respectively. Not surprisingly, these results are very similar to those of the previous tables, and serve as a baseline for the columns that follow.

Turning to our additional analyses, in column (2) of Table 5 we consider the possibility that the impact on re-employment of our network measure, *AEN*, is nonlinear. This might be the case if, for example, high levels of *AEN* indicate that many neighbors in a tract are not just employed, but employed at the same employers, so that there is redundant and hence less-productive information being transmitted through the network, implying a smaller marginal effect of *AEN* at higher levels of *AEN*. Column (2) adds a quadratic in *AEN*. The linear coefficient on *AEN* is 0.192, somewhat smaller than that in column (1). The second-order term in *AEN*, however, is nowhere close to significant (and recall that there are over 9 million observations in this pooled sample), and the coefficients on *ER* and *HRT* are essentially unchanged. Thus, the effect of *AEN* on re-employment seems to be linear.

Although, as noted above, we only consider urban Census tracts in our analysis, there may still be variation in the residential population density across urban Census tracts. And when Census tracts are more densely populated, neighbors may be more connected to each other because of proximity. That is, when neighborhood population density is higher, our network measure *AEN* may actually reflect more network contacts. Of course, the opposite could be true.

[22] For the residual observations in the higher-earnings sample, the estimated effects of *AEN* on the probability of re-employment at a neighbor's employer were about 30 to 50 percent smaller. Given that there is no gross effect on re-employment probabilities for this sample, the implication is that the effect on the probability of re-employment at a different employer is negatively related to *AEN*, so the results for the higher-earnings sample has more to do simply with who gets which jobs, than with network connections facilitating re-employment overall.

Larger, denser cities may have less social capital, for example, if children are pooled into larger school districts with schools further from home (Asquith et al., 2019), or if individuals in apartment buildings as opposed to houses have fewer interactions with neighbors or are less social (Brueckner and Largey, 2008). In the third column of Table 5, we report results from a specification where we add to the baseline specification a measure of the population density in the Census tract measured as thousands of people per square mile, and an interaction of the population density measure with *AEN*. Although tract density is negatively and significantly related to re-employment,[23] the coefficient on *AEN* is essentially unchanged from column (1), and the interaction of *AEN* with the tract density measure is small and statistically insignificant.

We address a related possibility in column (4). Neighborhood networks may be stronger when neighbors' ties to their residences are strong, and when they have known their neighbors for longer, and therefore there may be an interaction between neighborhood network strength as we have constructed it and residential stability. One of our controls is the share of residents of a Census tract who, in the 2000 Census Long Form, reported living in the same residence where they resided five years ago. In column (4) we add an interaction between that variable and *AEN*. The coefficient on the neighborhood stability measure (not reported in the table) is negative and significant, but the interaction term is relatively small and statistically insignificant. Moreover, the coefficient on *AEN* in column (5) is, if anything, somewhat larger than in the baseline specification, and the coefficients on *ER* and *HRT* are essentially unchanged.

In the bottom panel of Table 5, we report estimates of all three of these variants to our baseline specification for the low-earnings sample. Across columns (2)-(4), the story is the same as for the full sample – the interaction terms of the new variables with *AEN* are statistically insignificant, and the coefficient on *AEN* is robust.

Table 6 repeats the robustness checks of Table 5, but using as the dependent variable whether the displaced worker is employed at a neighbor's employer (the same dependent variable as in Table 4). The full sample baseline results are in the top panel of column (1), and not surprisingly are very close to those the period-by-period results of the last three columns of Table 4. In the top panel of column (2), we explore for the full sample whether there is a non-linear effect of *AEN* on finding employment with a neighbor's employer. Here, unlike in Table 5, we do see some evidence of a nonlinear effect, with a second-order term that is negative and statistically

[23] As with the other demographic and neighborhood characteristics, we do not report the coefficient on the tract population density measure in Table 5.

significant. However, the nonlinearity is not quantitatively that important: for example, the marginal effects of *AEN* at the 25th and 75th percentiles of *AEN* are quite similar (0.270 and 0.244).

In the top panel of columns (3) and (4) we report full sample results where we introduce the interactions between *AEN* and the population tract density measure and the residential stability measure. The results in column (3) show that the interaction term between population density and *AEN* is negative and statistically significant. It is very small, though, and the implied marginal effects of *AEN* on re-employment with a neighbor at the 25th and 75th percentiles of the population density distribution are very similar to each other – 0.440 and 0.418, respectively – and similar to 0.421, the linear coefficient on *AEN* in column (1). The same holds in column (4) for residential stability: the interaction of *AEN* with share in the same residence is statistically significant, but the marginal effects at the 25th and 75th percentiles of the residential stability distribution are 0.421 and 0.443, respectively, and again are equal to or close to the column (1) linear coefficient. Once again, we find that these alterations to how we specify the effect of *AEN* do not impact the estimated coefficients on *ER* and *HRT*.

The bottom panel of Table 6 reports results for the low-earnings sample, again with the baseline, pooled results for re-employment with a neighbor's employer reported in column (1). As with the full sample, the results in column (2) suggest that there is a nonlinear effect of *AEN* on re-employment with a neighbor's employer – the linear term is positive and statistically significant while the quadratic term is negative and statistically significant. In this case this nonlinearity manifests in more variation in marginal effects across the *AEN* distribution – the marginal effect at the 25th percentile is 0.607, compared to 0.460 at the 75th percentile, providing more substantive evidence that the effect of *AEN* on re-employment with a neighbor's employer falls somewhat as *AEN* grows. In columns (3) and (4) of the bottom panel of the table, the results adding interactions with population density and residential stability are very similar to the results for the full sample in the top panel. The interaction terms are statistically significant but do not meaningfully affect the marginal effects; the marginal effects of *AEN* at the 25th and 75th percentiles of the tract density distribution are 0.454 and 0.434, respectively, and for the residential stability results, the marginal effects are 0.430 and 0.450.

All in all, the conclusion we draw from Tables 5 and 6 is that the productivity of our active network measure, *AEN*, in general is captured well by a linear specification with the detailed control variables we included in our baseline model, regardless of whether we are examining re-employment probabilities, or narrowing in on re-employment at a neighbor's employer. This, combined with the results in Tables 3 and 4 that demonstrate the consistent productivity of *AEN* before, during, and after the Great Recession period, provide what in our view is compelling evidence of the stable and important effect of networks for displaced workers, especially via the mechanism that theory suggests should be most potent – finding work at a neighbor's employer.

V. Conclusion

In this paper we develop a measure of residence-based labor market networks – which we refer to as *AEN*, for "active employer network" – and estimate the effect of this network measure on finding jobs. *AEN* captures gross hiring at the establishments of employed neighbors of a displaced worker, and hence can capture the effects of networks either via information passed along to job searchers about job vacancies or via referrals to employers about job searchers. The strength of *AEN* varies across residential neighborhoods and over time. We study workers who lost jobs in mass layoffs between 2005-2012, exploiting the detailed data including place of work and place of residence in the LEHD data, and the fact that *AEN* varies not only across residential neighborhoods, but also over time as the economy fell into the Great Recession and then began to recover.

We find strong evidence that this network measure increases the probability of re-employment for displaced workers, especially when this re-employment occurs at a neighbor's employer, exactly as network theory would suggest. Interestingly and importantly, although employment rates and hiring rates fell dramatically during the Great Recession, lowering the level of our network measure dramatically across the country, the productivity of these networks did not change across the period we study.

In our view, the estimated effect of networks is economically significant. As an illustrative example, the estimated effect of a change from the 25th to the 75th percentile of the tract-level distribution of our network measure is to increase the probability of re-employment at a neighbor's employer in the quarter after displacement by 1.1 to 1.5 percentage points (relative to a mean over our sample period of 12 to 14 percent). While we find strong evidence that local labor market networks are important in influencing the re-employment of workers displaced in mass layoffs – which were, of course, particularly pronounced during the Great Recession – we do not find evidence of changes in the productivity of labor market networks during the Great Recession.

Our evidence on the importance of residence-based labor market networks in securing the re-employment of workers displaced in mass layoffs complements a growing body of literature that, more generally, finds that labor market networks influence labor market outcomes along important dimensions. Evidence of labor market networks is always, in a sense, specific to the type of network connections that a researcher can measure, and there may be many kinds of connections among workers. Our research adds to the mounting evidence that network connections among neighbors – especially among lower-skilled workers – are an important source of such connections. The new evidence in this paper also suggests that these kinds of connections help mitigate the effects of mass layoffs, which – as other research has shown – can have adverse longer-run effects. However, our evidence is most clear when we examine the role of residence-based networks in generating re-employment at neighbors' employers rather than faster re-employment per se. We view the evidence on re-employment at neighbors' employers as strongly reinforcing a network interpretation of our evidence.

It remains an open question how much these network connections improve longer-run outcomes for displaced workers. Furthermore, the importance of neighborhood-based networks for re-employment after mass layoffs naturally raises the broader questions of the role of labor market networks in generating variation in longer-term labor market outcomes across neighborhoods, and what institutions or policies might be able to strengthen network connections to improve labor market outcomes in neighborhoods currently characterized by adverse labor market outcomes.

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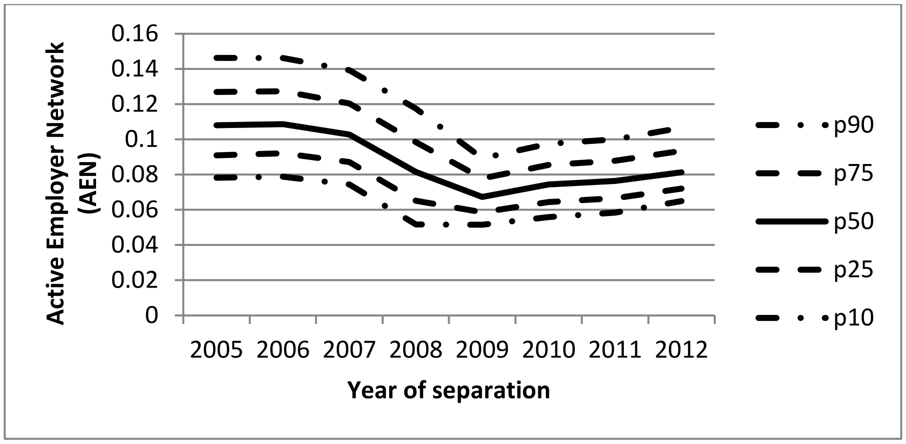
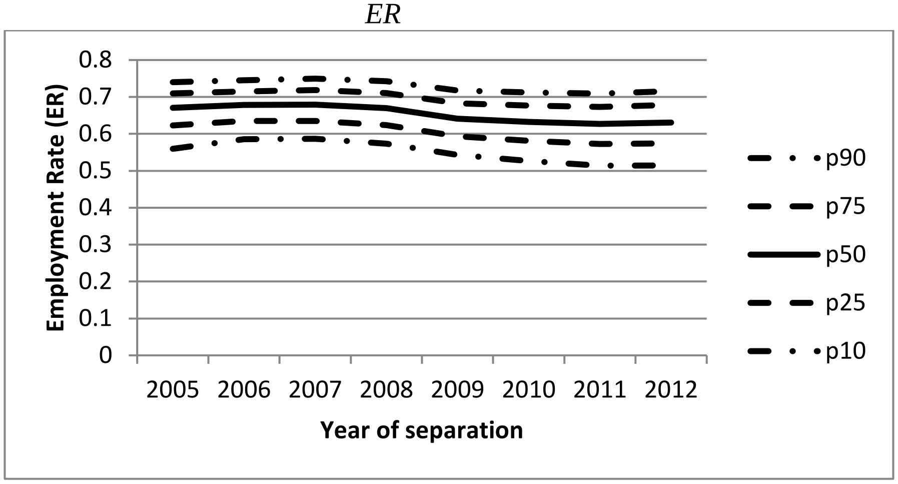
Figure 1: Percentiles of distributions of active employer network measure (*AEN*), employment rate (*ER*), and average gross hiring rate in Census tracts where neighbors work (*HRT*), by year

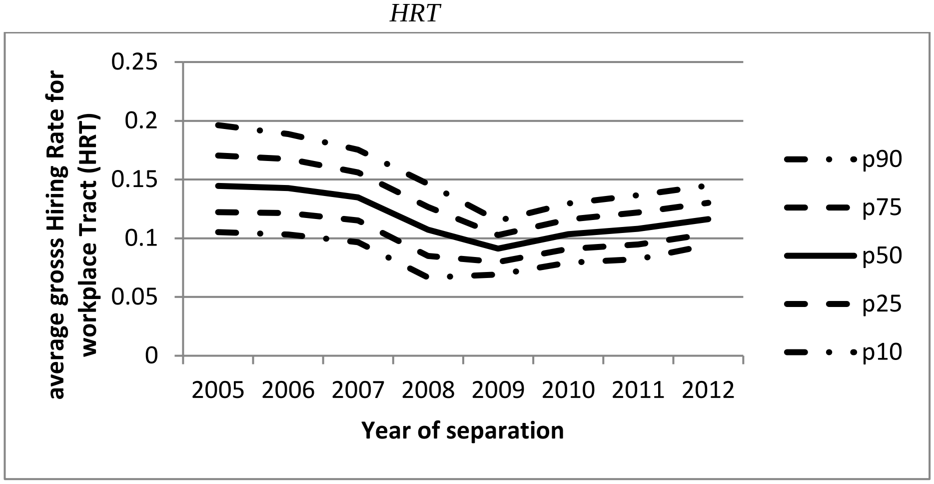
AEN



ER



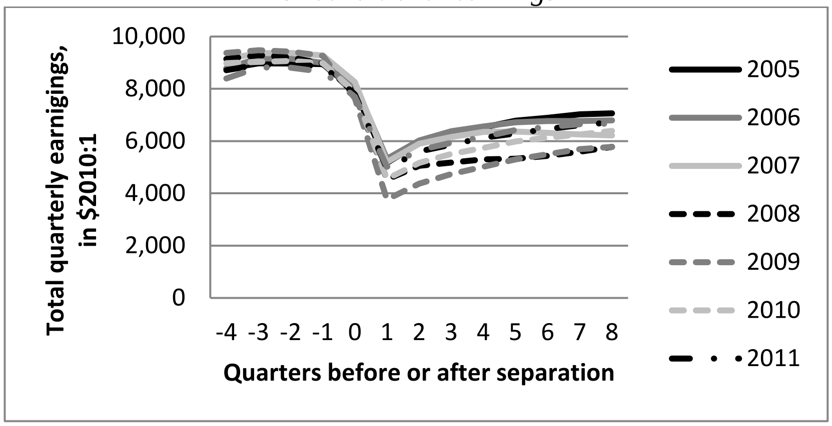
HRT



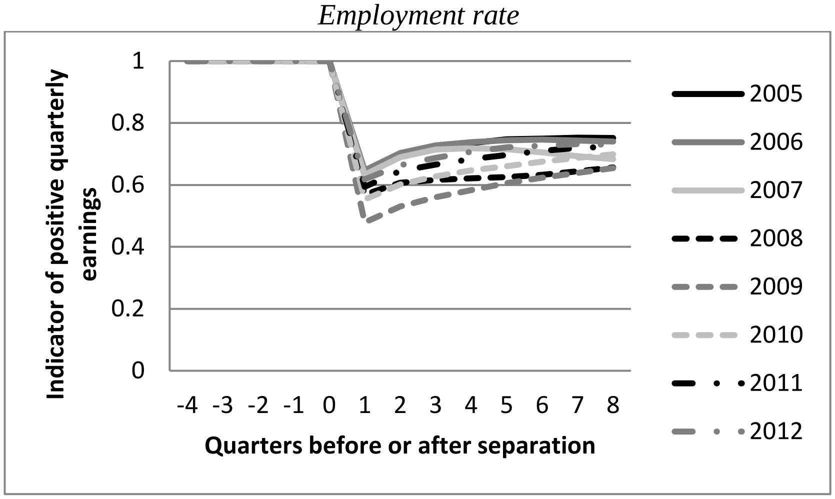
Notes: Calculations from LEHD data.

Figure 2: Earnings and employment of displaced workers, by year of displacement

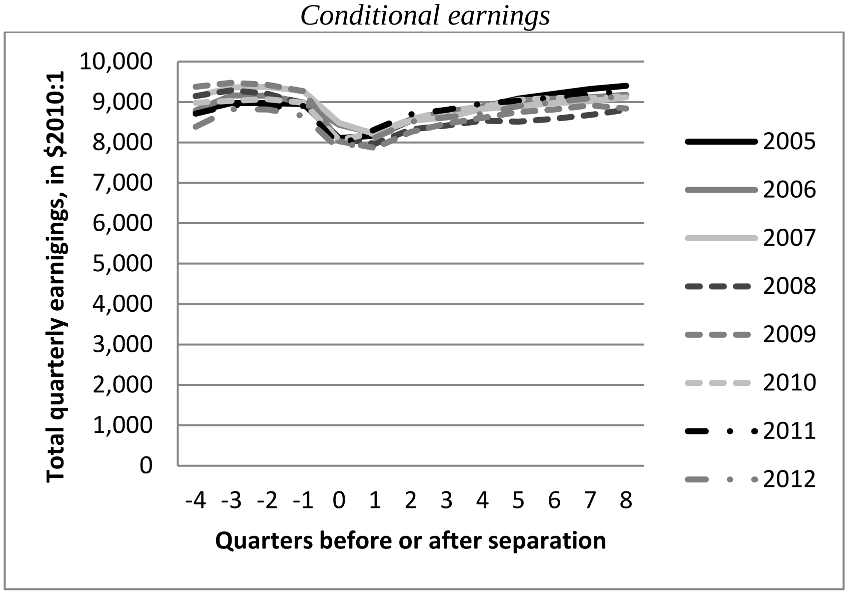
Unconditional earnings



Employment rate



Conditional earnings



Notes: Calculations from LEHD. Earnings are in 2010Q1 dollars. Earnings are top-coded at the 99th percentile for the displacement quarter and subsequent quarters. Employment status is defined as positive earnings during the quarter.

Table 1: Sample means

Variable	Mean	Variable	Mean
Employment indicator in quarter after displacement	0.585	White non-Hispanic	0.532
Employed at a neighbor's employer	0.122	Black non-Hispanic	0.190
Active employer network (*AEN*)	0.089	Other race non-Hispanic	0.016
Employment rate (*ER*)	0.648	Asian non-Hispanic	0.058
Average tract gross hiring rate (*HRT*)	0.119	Hispanic	0.204
Share in poverty rate in tract (2000)	0.132	Agriculture and mining (11,21)	0.008
Share in same house last year (2000)	0.509	Utility, wholesale, transportation (22,42,48-49)	0.083
Share foreign born (2000)	0.163	Construction (23)	0.096
Share less than high school (2000)	0.206	Manufacturing (31-33)	0.121
Share some college (2000)	0.282	Retail, administrative, other services (44-45,56,81)	0.258
Share college or more (2000)	0.247	Professional services (51-55)	0.199
Share white, not Hispanic (2000)	0.593	Education, health, public (61,62,92)	0.128
Share black, not Hispanic (2000)	0.158	Local services (71,72)	0.107
Earnings at employer in previous year (1,000s 2010Q1$) - mean	34.873	Displaced in 2005	0.122
Earnings at employer in previous year (1,000s 2010Q1$) - std. dev.	21.320	Displaced in 2006	0.118
Earnings from other jobs in previous year (1,000s 2010Q1$) - mean	1.460	Displaced in 2007	0.136
Earnings from other jobs in previous year (1,000s 2010Q1$) - std. dev.	4.665	Displaced in 2008	0.176
Age 19 to 24	0.143	Displaced in 2009	0.164
Age 25 to 34	0.297	Displaced in 2010	0.106
Age 35 to 44	0.231	Displaced in 2010	0.103
Age 45 to 54	0.204	Displaced in 2012	0.075
Age 55 to 64	0.125	Displaced in quarter 1	0.232
Female	0.461	Displaced in quarter 2	0.257
Male	0.539	Displaced in quarter 3	0.264
		Displaced in quarter 4	0.247

Notes: Observations (1,000s): 9,195. Calculations from the LEHD Infrastructure Files and from the 2000 Census Summary File 3. NAICS industry sector code ranges are listed.

Table 2: Longitudinal variation in sample

Displacement (year)	Observations (1,000s)	Percent sample observations	Layoff events (1,000s)	Percent layoff events	Average displaced workers per layoff event	Average earnings at displaced job in previous year	Average earnings at other jobs in previous year	Employment rate in quarter after job loss	Average earnings in quarter after job loss
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
2005	1,126	12.2	247	11.9	102.9	34,175	1,492	0.633	5,260
2006	1,086	11.8	254	12.3	91.3	34,474	1,626	0.647	5,423
2007	1,248	13.6	283	13.7	82.7	35,549	1,602	0.633	5,288
2008	1,620	17.6	365	17.6	75.4	35,061	1,540	0.569	4,614
2009	1,504	16.4	331	16.0	61.0	36,162	1,383	0.479	3,835
2010	978	10.6	223	10.8	96.5	34,760	1,292	0.553	4,650
2011	946	10.3	209	10.1	96.6	34,120	1,297	0.594	5,026
2012	686	7.5	159	7.7	49.9	33,347	1,333	0.618	5,106
All years	9,195	100.0	2,072	100.0	81.8	34,873	1,460	0.585	4,836

Notes: Calculations from LEHD data. Earnings are in 2010Q1 dollars.

Table 3: Estimated effect of network measures on re-employment in quarter following displacement, 2005-2007, 2008-2010, and 2011-2012

	2005-2007	2008-2010	2011-2012	2005-2007	2008-2010	2011-2012
	Omitting demographic and tract controls			Including demographic and tract controls		
	(1)	(2)	(3)	(4)	(5)	(6)
Full sample						
Active employer network (*AEN*)	**0.161***	**0.068***	**0.270***	**0.268***	**0.246***	**0.248***
	(0.022)	**(0.026)**	**(0.043)**	**(0.026)**	**(0.028)**	**(0.046)**
Employment rate (*ER*)	0.321***	0.378***	0.321***	0.231***	0.244***	0.244***
	(0.006)	(0.005)	(0.007)	(0.008)	(0.007)	(0.010)
Hiring rate (*HRT*)	0.183***	0.343***	0.155***	0.106***	0.209***	0.177***
	(0.027)	(0.030)	(0.045)	(0.027)	(0.030)	(0.045)
Controls included:						
Other demographic controls	No	No	No	Yes	Yes	Yes
Prior earnings measures	No	No	No	Yes	Yes	Yes
Industry dummy variables	No	No	No	Yes	Yes	Yes
SEIN/year/quarter/county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Interquartile effects						
Active employer network (*AEN*)	**0.006**	**0.002**	**0.006**	**0.009**	**0.006**	**0.005**
Employment rate (*ER*)	0.027	0.035	0.033	0.019	0.023	0.025
Hiring rate (*HRT*)	0.008	0.011	0.004	0.005	0.007	0.005
Number of fixed effects included (1,000s)	780	920	370	780	920	370
R-squared (within)	0.002	0.002	0.002	0.045	0.052	0.048
Observations (1,000s)	3,460	4,100	1,630	3,460	4,100	1,630
Mean of dependent variable	0.638	0.532	0.604	0.638	0.532	0.604
Low-earnings sample (pre-displacement earnings < $50,000)						
Active employer network (*AEN*)	**0.295***	**0.236***	**0.430***	**0.307***	**0.294***	**0.295***
	(0.025)	**(0.030)**	**(0.049)**	**(0.030)**	**(0.033)**	**(0.053)**
Employment rate (*ER*)	0.278***	0.335***	0.283***	0.202***	0.221***	0.232***
	(0.007)	(0.006)	(0.008)	(0.009)	(0.008)	(0.012)
Hiring rate (*HRT*)	0.169***	0.338***	0.171***	0.121***	0.231***	0.212***
	(0.031)	(0.036)	(0.052)	(0.031)	(0.036)	(0.052)
Controls included:						
Other demographic controls	No	No	No	Yes	Yes	Yes
Prior earnings measures	No	No	No	Yes	Yes	Yes
Industry dummy variables	No	No	No	Yes	Yes	Yes
SEIN/year/quarter/county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Interquartile effects						
Active employer network (*AEN*)	**0.010**	**0.006**	**0.009**	**0.011**	**0.008**	**0.006**
Employment rate (*ER*)	0.024	0.032	0.029	0.017	0.021	0.024
Hiring rate (*HRT*)	0.008	0.011	0.005	0.005	0.007	0.006
Number of fixed effects included (1,000s)	690	800	320	690	800	320
R-squared (within)	0.001	0.002	0.002	0.046	0.052	0.048
Observations (1,000s)	2,650	3,110	1,270	2,650	3,110	1,270
Mean of dependent variable	0.623	0.512	0.585	0.623	0.512	0.585

Notes: Employment estimates are from linear probability model for an indicator of employment. The demographic controls included are: share in poverty rate in tract (2000); share in same house last year (2000); share foreign born (2000); share less than high school (2000); share some college (2000); share college or more (2000); share white, not Hispanic (2000); share black, not Hispanic (2000); age 19-24; age 25-34; age 45-54; age 55-64; female; black non-Hispanic; other race non-Hispanic; Asian non-Hispanic; and Hispanic. Omitted reference indicators/variables are: age 35-44, share high school grads, male, and white non-Hispanic. Industry dummy variables (using the categories from Table 1) can vary within SEIN/year/quarter/county fixed effects for some multiple-establishment firms operating in more than one industry. The interquartile effects are computed using the percentiles of the distributions for the sample used in the corresponding regression. Robust standard errors are reported in parentheses, clustered by SEIN/year/quarter/county. ***p<0.01.

Table 4: Estimated effect of network measures on re-employment at a neighbor's employer in quarter following displacement, 2005-2007, 2008-2010, and 2011-2012

	2005-2007	2008-2010	2011-2012	2005-2007	2008-2010	2011-2012
	Omitting demographic and tract controls			Including demographic and tract controls		
	(1)	(2)	(3)	(4)	(5)	(6)
Full sample						
Active employer network (*AEN*)	**0.427*****	**0.457*****	**0.586*****	**0.418*****	**0.423*****	**0.481*****
	(0.017)	**(0.034)**	**(0.033)**	**(0.025)**	**(0.044)**	**(0.037)**
Employment rate (*ER*)	0.163***	0.136***	0.124***	0.144***	0.116***	0.138***
	(0.006)	(0.003)	(0.005)	(0.009)	(0.006)	(0.008)
Hiring rate (*HRT*)	0.139***	0.200***	0.204***	0.156***	0.212***	0.239***
	(0.030)	(0.026)	(0.038)	(0.032)	(0.028)	(0.038)
Controls included:						
Other demographic controls	No	No	No	Yes	Yes	Yes
Prior earnings measures	No	No	No	Yes	Yes	Yes
Industry dummy variables	No	No	No	Yes	Yes	Yes
SEIN/year/quarter/county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Interquartile effects						
Active employer network (*AEN*)	**0.015**	**0.012**	**0.012**	**0.015**	**0.011**	**0.010**
Employment rate (*ER*)	0.014	0.013	0.013	0.012	0.011	0.014
Hiring rate (*HRT*)	0.006	0.006	0.006	0.007	0.007	0.006
Number of fixed effects included (1,000s)	780	920	370	780	920	370
R-squared (within)	0.001	0.001	0.001	0.004	0.004	0.004
Observations (1,000s)	3,460	4,100	1,630	3,460	4,100	1,630
Mean of dependent variable	0.143	0.105	0.121	0.143	0.105	0.121
Low-earnings sample (pre-displacement earnings < $50,000)						
Active employer network (*AEN*)	**0.517*****	**0.532*****	**0.683*****	**0.435*****	**0.433*****	**0.507*****
	(0.020)	**(0.030)**	**(0.038)**	**(0.025)**	**(0.037)**	**(0.043)**
Employment rate (*ER*)	0.157***	0.125***	0.107***	0.148***	0.120***	0.141***
	(0.005)	(0.004)	(0.006)	(0.008)	(0.006)	(0.009)
Hiring rate (*HRT*)	0.153***	0.211***	0.248***	0.171***	0.224***	0.288***
	(0.025)	(0.028)	(0.041)	(0.026)	(0.029)	(0.042)
Controls included:	No	No	No	Yes	Yes	Yes
Other demographic controls	No	No	No	Yes	Yes	Yes
Industry dummy variables	No	No	No	Yes	Yes	Yes
SEIN/year/quarter/county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Interquartile effects						
Active employer network (*AEN*)	**0.018**	**0.014**	**0.015**	**0.015**	**0.011**	**0.011**
Employment rate (*ER*)	0.014	0.012	0.011	0.013	0.011	0.015
Hiring rate (*HRT*)	0.007	0.007	0.007	0.008	0.007	0.008
Number of fixed effects included (1,000s)	690	800	320	690	800	320
R-squared (within)	0.001	0.001	0.001	0.004	0.004	0.004
Observations (1,000s)	2,650	3,110	1,270	2,650	3,110	1,270
Mean of dependent variable	0.148	0.107	0.123	0.148	0.107	0.123

Notes: Employment estimates are from linear probability model for an indicator of employment at a neighbor's employer. See notes to Table 3. Robust standard errors are reported in parentheses, clustered by SEIN/year/quarter/county. ***p<0.01.

Table 5: Robustness results for estimated effect of network measures and interacted control variables on re-employment in quarter following displacement, 2005-2012

	Baseline results	Quadratic in AEN	Interaction of AEN with tract pop density	Interaction of AEN with housing stability
	(1)	(2)	(3)	(4)
Full sample				
Active employer network (*AEN*)	**0.274*****	**0.192*****	**0.273*****	**0.305*****
	(0.017)	**(0.058)**	**(0.019)**	**(0.030)**
AEN·AEN		0.373		
		(0.253)		
AEN·tract population density			0.0002	
			(0.001)	
AEN·share in same residence				-0.066
				(0.051)
Employment rate (*ER*)	0.241***	0.242***	0.240***	0.241***
	(0.005)	(0.005)	(0.005)	(0.005)
Hiring rate (*HRT*)	0.157***	0.159***	0.157***	0.156***
	(0.018)	(0.018)	(0.018)	(0.018)
R-squared (within)	0.048	0.048	0.048	0.048
Observations (1,000's)	9,200	9,200	9,200	9,200
Mean of dependent variable	0.585	0.585	0.585	0.585
Low-earnings sample (pre-displacement earnings < $50,000)				
Active employer network (*AEN*)	**0.312*****	**0.370*****	**0.314*****	**0.354*****
	(0.020)	**(0.069)**	**(0.022)**	**(0.035)**
AEN·AEN		-0.261		
		(0.294)		
AEN·tract population density			-0.0001	
			(0.0006)	
AEN·share in same residence				-0.089
				(0.060)
Employment rate (*ER*)	0.217***	0.216***	0.216***	0.217***
	(0.005)	(0.005)	(0.005)	(0.005)
Hiring rate (*HRT*)	0.179***	0.177***	0.179***	0.178***
	(0.021)	(0.021)	(0.021)	(0.021)
R-squared (within)	0.049	0.049	0.049	0.049
Observations (1,000's)	7,020	7,020	7,020	7,020
Mean of dependent variable	0.567	0.567	0.567	0.567

Notes: Employment estimates are from linear probability model for an indicator of employment. All specifications include SEIN/year/quarter/county fixed effects, and the worker control variables and Census tract control variables listed in Table 3. The specification in column (3) includes a control for the population density in the tract. The fraction of tract residents in the same home as five years ago (interacted in column (4)) is in the standard set of controls. Robust standard errors are reported in parentheses, clustered by SEIN/year/quarter/county. ***p<0.01.

Table 6: Robustness results for estimated effect of network measures and interacted control variables on re-employment at neighbor's employer in quarter following displacement, 2005-2012

	Baseline results	Quadratic in AEN	Interaction of AEN with tract pop density	Interaction of AEN with housing stability
	(1)	(2)	(3)	(4)
		Full sample		
Active employer network (*AEN*)	**0.421***** **(0.0209)**	**0.829***** **(0.052)**	**0.449***** **(0.023)**	**0.337***** **(0.025)**
AEN·AEN		-1.860*** (0.266)		
AEN·tract population density			-0.003*** (0.0005)	
AEN·share in same residence				0.175*** (0.046)
Employment rate (*ER*)	0.127*** (0.005)	0.121*** (0.005)	0.129*** (0.005)	0.127*** (0.005)
Hiring rate (*HRT*)	0.192*** (0.019)	0.180*** (0.019)	0.190*** (0.019)	0.192*** (0.019)
R-squared (within)	0.004	0.004	0.004	0.004
Observations (1,000's)	9,200	9,200	9,200	9,200
Mean of dependent variable	0.122	0.122	0.122	0.122
		Low-earnings sample (pre-displacement earnings < $50,000)		
Active employer network (*AEN*)	**0.435***** **(0.019)**	**0.902***** **(0.055)**	**0.463***** **(0.021)**	**0.380***** **(0.027)**
AEN·AEN		-2.110*** (0.262)		
AEN·tract population density			-0.003*** (0.0005)	
AEN·share in same residence				0.115** (0.049)
Employment rate (*ER*)	0.130*** (0.004)	0.124*** (0.004)	0.132*** (0.004)	0.131*** (0.004)
Hiring rate (*HRT*)	0.213*** (0.018)	0.200*** (0.017)	0.213*** (0.018)	0.213*** (0.017)
R-squared (within)	0.004	0.004	0.004	0.004
Observations (1,000's)	7,020	7,020	7,020	7,020
Mean of dependent variable	0.125	0.125	0.125	0.125

Notes: Employment estimates are from linear probability model for an indicator of employment at neighbor's employer. See notes to Table 5. Robust standard errors are reported in parentheses, clustered by SEIN/year/quarter/county. ***p<0.01; **p<0.05.

Appendix

The core dataset from which the samples we study are extracted is the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) Infrastructure Files. We use the LEHD datasets to identify a set of workers separating from jobs in mass displacement events, to measure the workers' pre-displacement characteristics and post-displacement labor market outcomes, and to characterize labor market networks in the neighborhood in which a displaced worker resides.

Input datasets

The LEHD consist of a frame of jobs produced from state Unemployment Insurance (UI) reporting systems, augmented with information on worker and employer characteristics.[24] The employer frame, for both jobs and employer characteristics, is the same as the Bureau of Labor Statistics' (BLS) Quarterly Census of Employment and Wages (QCEW), also known as the ES-202 program. The state data cover over 95 percent of wage and salary civilian jobs, including both private sector and state and local government workers. The data do not cover federal workers, the armed forces, or earnings through self-employment, the postal service, family workers, or some non-profit and agricultural workers (U.S. Bureau of Labor Statistics, 1997; U.S. Bureau of Labor Statistics, 2017). States provide the Census Bureau with two quarterly files. The earnings history (or UI covered jobs) file lists the quarterly earnings accruing to a worker from an employer. The employer file, like the QCEW, includes information on industry, ownership, size, and location of employer establishments. In order to disaggregate employment statistics by worker characteristics including age, sex, race, and ethnicity, and by home location, LEHD supplements the jobs data with demographic variables derived from the Social Security Administration's NUMIDENT file and the 2000 Census, as well as place-of-residence from federal administrative records (for details, see Graham et al., 2017).

The LEHD Infrastructure Files use unique person and establishment identifiers to merge worker and employer data. The LEHD reporting unit for Unemployment Insurance (UI) covered earnings is identified by a state UI account number, and can include multiple establishments, or worksites, within a state. This is referred to as the State Employer Identification Number (SEIN). Workers are identified with the Protected Identification Key (PIK), a unique mapping of person-level administrative records that is also assigned to survey responses (Wagner and Layne, 2014). Other survey and administrative person-level records are also identified with a PIK.

[24] See Abowd et al. (2009) for a summary of the various components of the LEHD Infrastructure Files. See Vilhuber (2018) for a detailed discussion of specific files as available in the Federal Statistical Research Data Centers.

One limitation of the LEHD Infrastructure Files for calculating the network measures is that, for most states, firms with multiple establishments (or units) in a state do not report the assignment of workers to establishments (about 40 percent of jobs are at such multi-unit firms). The LEHD program has developed an imputation model, known as Unit-to-Worker, to allocate establishments to workers based on establishment size during the worker's tenure at the employer and on the distance between the establishment and the worker's place of residence, favoring larger and closer establishments (Abowd et al., 2009).[25] For multi-unit firms, we use this imputed assignment to identify the establishment from which a worker was displaced as well as the location (county) and industry of that establishment, to determine whether a displaced worker was re-employed at a neighbor's establishment, to identify neighbors' establishments and the gross hiring rates at those establishments for our network measure, and to identify the workplace locations of neighbors' employers.[26] Reliance on this imputation for firms with multiple establishments in a state, when assigning workers to establishments in computing measures of network strength, as well as in determining where displaced workers become re-employed, leads to some bias towards zero in our estimated effects.

We supplement these person-level data with geographic information. We use the 2000 Census Summary File 3, tabulated from responses to the Long Form, which describes demographic characteristics of Census tracts. We also use Census block level data on urban status to define our sample. Our place of residence data is for 2000 tabulation geography before 2010 and for 2010 tabulation geography thereafter. We use Census block level relationship files to crosswalk neighborhood data from 2000 to 2010 geography for workers displaced in 2010 or later. Last, we construct a population density measure using 2000 Census population and land area from the 2000 and 2010 Census Gazetteer files. We measure population density as thousands of persons

[25] The state in which an employee works is indicated by the state to which a firm submits Unemployment Insurance earnings records. One exception to non-reporting is Minnesota, where firms report an establishment assignment along with earnings information for each worker. The LEHD program used the information from Minnesota to develop the imputation model that is applied to firms with multiple units in other states.

[26] The LEHD program actually takes ten independent draws from the Unit-to-Worker imputation model for the production of public-use statistics. For this study, in order to limit the computational burden, we use just the first of those imputation draws for most purposes. In particular, we link that drawn establishment (for jobs at multi-unit establishments) or the sole establishment (for jobs at single-unit establishments) to the Employer Characteristics File and assign industry and workplace location based on that establishment (employer size, used for determining mass displacement events, is measured at the firm level, combining all establishments in a state). The one exception is the gross hiring rate, where we use all ten draws with a weight of one-tenth assigned to each draw; the LEHD Infrastructure Files already include weighted aggregations of gross hires and employment (inputs to *AEN* and *HRT*) at the establishment level as inputs to the Quarterly Workforce Indicators.

per square mile, in 2000.

Sample of displaced workers

We begin with an extract of 1.7 billion jobs, or spells of earnings from an employer, held from 2004 through 2014 at employers located in 49 states.[27] From these data, we identify 136 million workers separated from their highest earning (dominant) job from 2005 through 2012, as defined below.[28] We observe a job separation in the LEHD as the end of a stream of quarterly earnings of a worker from an employer, and assume that the separation occurred at some time in the final quarter of earnings. Our definition is parallel to the Quarterly Workforce Indicators variable "Separations, Beginning-of-Quarter Employed," except that we also restrict attention to a set of attached workers, defined as having worked at an employer for four consecutive quarters before the separation; and we further require that the separated worker not return to the employer in the two years following the separation.[29] Last, we require that the separation was from the worker's main (i.e., highest-earning) job in the quarter prior to displacement, with the idea that the loss of a main job is likely to lead the worker to search for a new job. Note that some of the separated workers may hold a secondary job, and maintain that job following the separation.

Although all job searchers can potentially activate labor market networks as part of their search, we restrict attention to the outcomes of individuals who have experienced a separation as part of a mass layoff event. We do this in order to focus on workers who are exogenously displaced from their jobs due to labor force contractions (and thus not due to individual-specific unobservables that may affect post-displacement labor market outcomes and also may be correlated with our network measures). This is standard in the literature on displaced workers (e.g., JLS, 1993; Davis and Von Wachter, 2011). Consistent with past work on displaced workers, we define mass layoffs based on whether employers had a certain initial employment size that subsequently dropped by a minimum percentage. In particular, we define a mass layoff based on an initial employment level of at least 25 workers, which subsequently fell by at least 30 percent

[27] We include all states except for Massachusetts and also do not include the District of Columbia because LEHD earnings records were not available for the entire span of this study.

[28] We extract these records from the Person History File for each state. We use the Person History Enhanced Across SEIN and Non-SEIN Transitions (PHEASANT) process to consolidate state level Person History Files. The PHEASANT takes successor/predecessor transitions of employers into account when calculating a worker's job spell duration and earnings at an employer.

[29] For both separations and mass displacement events, we define employers at the SEIN level, and refer to the state-firm pair as the SEIN – the reporting entity for earnings and establishment records for most states. In requiring that displaced workers have no earnings at the downsizing SEIN for eight subsequent quarters, we include any other employers that the LEHD has linked to the downsizing SEIN using the Successor-Predecessor File. (The Successor-Predecessor File tracks worker flows across SEINs to identify spurious separations.) For more on the QWI variable definitions, see: http://lehd.ces.census.gov/doc/QWI_101.pdf.

over a period of one year (four quarters) during which we observe a worker leaving their employer. For 136 million separations, 78.5 percent of separations were at employers with 25 or more workers in the previous year, and 15.2 also had a drop of 30 percent or more that was not simply a restructuring. With this definition, we identify 20.7 million workers displaced from 2005 to the third quarter of 2012.

We define our labor market network measure for a set of urban Census tracts where these workers reside. The Census Bureau has developed standards to create and maintain Census tract definitions to promote consistency nationwide, with a target size of about 4,000 residents (ranging from 2,500 to 8,000). Most tracts follow permanent, visible features such as roads, rivers, and railroads, and in urban areas they often consist of a set of city blocks bounded by larger through streets. We use the Composite Person Record, an annual person-level file built from federal administrative data on residential addresses that contributes to the LEHD Infrastructure files (Abowd et al., 2009). We are able to assign a Census tract of residence in the year of displacement in one of the 49 states in our analysis to 89.1 percent of the sample. From among these locations, we require that the Census tract is entirely classified as urban in the 2000 Census and has at least 100 resident workers, which restricts attention to more densely populated areas in which neighbors are more likely to interact.[30] We drop a further 6.2 percent of the remaining workers who are not between 19 and 64 years old in the quarter in which they separated.

From the resulting sample of 10.2 million displaced workers, we retain those who had pre-displacement annual earnings from all jobs of between \$5,000 and \$100,000 (in 2010Q1\$).[31] Regarding the upper bound, the relevant labor market and network contacts of especially high earners are likely quite different from those of lower earners; in particular, high earners are likely to have networks and to engage in job search in a more national labor market and so residential network contacts are likely much less important. Regarding the lower bound, we exclude workers who, although they held a job for at least a year, were more likely to be a secondary earner or dependent, or otherwise not highly attached to the wage and salary labor market. The upper bound

[30] Using the 2000 Census definitions, urban areas must have at least 500 people per square mile and be in a geographic cluster that includes core Census blocks with a population density of at least 1,000 people per square mile. Our urban restriction is that all of the population in a tract resides in Census blocks (a sub-unit) classified as urban. As of the 2010 Census (contemporary with our sample), 81 percent of the U.S. population resided in an urban area, and the displaced worker extract has a mean urban share of 82 percent (based on the 2000 Census definitions). We only retain the 62 percent of displaced workers who reside in a 100-percent urban Census tract (urban status can range from 0 to 100 percent, and include suburban areas). The 100-resident worker restriction drops fewer than 1 percent of the displaced workers (for this sample, the average tract has a 2000 Census population of about 5,500).

[31] We use the urban Consumer Price Index, taking the average for each month in a quarter (because earnings are reported on a quarterly basis).

drops 7.7 percent of workers and the lower bound drops 2.2 percent, resulting in a final estimation sample of 9.2 million displaced workers. Our sample of 7 million lower-earning displaced workers further limits pre-displacement earnings to be less than $50,000.

Network measures

Using the same extract of 1.7 billion jobs from the LEHD Infrastructure Files spanning the study period, we construct the network measures of employment and hiring information in the quarter after each displacement cohort is separated (approximately 112 million jobs each quarter). The network measures described in the previous section are based on individuals aged 19 to 64 who reside in the same Census tract as the displaced worker. For a neighbor to be considered as "employed" in the network measures, the neighbor must have a job with positive earnings in the layoff quarter of a displaced worker as well as in the subsequent quarter. If a neighbor has more than one job spanning both quarters, we only use the job with the highest earnings in the subsequent quarter. All persons observed as neighbors in the residence data (employed or not) for the year of displacement contribute to the count of *N*. We subtract one, so a given displaced worker does not contribute to the count of neighbors. Additionally, the entire sample of workers laid off in the given quarter is excluded from being categorized as "employed," even if that laid off worker had some positive earnings in both periods. These conditions ensure that if an employer does a lot of hiring in the post-layoff quarter of displaced or unemployed workers who happen to be neighbors, these hires will not be considered as part of the network itself. Although these recent hires may in fact be influenced by networks among displaced workers, we want to avoid the possible influence on our network measures of employers located near the displaced workers simply doing a lot of hiring.

We use this set of employed neighbors, the total count of neighbors, the gross hiring rate at neighbor's establishments, and the gross hiring rate at all establishments in the same tract that a neighbor works, to compute the quarterly network measures for the beginning of the quarter after the layoff. The gross hiring rate at an establishment is the count of new (gross) hires at an establishment in a quarter divided by the count of employees at that establishment in the beginning of the quarter.[32] On average, employers hired about 13 new workers for each 100 they had at the beginning of the quarter, giving an average gross hiring ratio of 0.13 with a standard deviation of 0.64. For *AEN*, we calculate the gross hiring rate at the establishment of each employed neighbor

[32] We use the Quarterly Workforce Indicators definition of new hires (cannot have worked for an employer in the previous year) and beginning of quarter workers (those with earnings in the previous and current quarter).

(using zero for those not employed) and divide by the total count of neighbors. We calculate the employment rate (*ER*) as employed neighbors divided by the total count of neighbors. We calculate the hiring rate in the tract (*HRT*) by averaging (across employed neighbors) the gross hiring rate of all employers in neighbors' workplace Census tracts. For both *AEN* and *HRT*, we censor the Census tract level-average of gross hiring rates at the 99th percentile to avoid any influence of extreme outliers in hiring on our results. The estimates of network effects are similar with or without censoring, but there is more variability in the point estimates of some disaggregated results without censoring.

Supplemental tables

This appendix includes tables supplementing our main results. Appendix Table A1 describes how the composition of the estimation sample changes across years. Appendix Table A2 lists the mean, 25th percentile, median, and 75th percentile for each of the network measures as well as population density and the share of neighbors in the same house last year, both overall and for each time period. We compute interquartile ranges of the explanatory variables and use these, along with coefficients, to calculate interquartile effects. Appendix Table A3 gives the estimated coefficients for network, person, and neighborhood variables, pooling across all time periods. Appendix Table A4 reports estimates pooled across all years that interact dummies for the 2008 to 2010 and the 2011 to 2012 periods with *AEN*. The coefficient estimates for these interaction terms give the difference of the effect for each period from 2005 to 2007, along with standard errors for each of those differences.

Additional Appendix References

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Table A1: Sample composition by year, full sample

Displacement year	2005	2006	2007	2008	2009	2010	2011	2012	All
Sex									
Male	50.9	52.6	53.0	56.4	57.7	53.0	52.3	52.4	54.0
Female	49.1	47.4	47.0	43.6	42.3	47.0	47.7	47.7	46.1
Age									
19 to 24	16.0	15.9	14.9	14.3	12.9	13.6	13.5	13.9	14.3
25 to 34	29.6	29.6	30.0	29.6	28.9	30.0	30.2	30.2	29.7
35 to 45	24.1	23.8	23.5	23.3	23.1	22.5	22.2	22.2	23.2
45 to 54	19.4	19.6	19.9	20.6	21.5	20.7	20.5	20.1	20.4
55 to 64	10.9	11.2	11.6	12.3	13.6	13.3	13.7	13.7	12.5
Race/ethnicity									
White non-Hispanic	52.9	53.1	53.8	53.0	53.3	53.7	53.1	52.9	53.2
Black non-Hispanic	21.0	19.4	18.4	18.6	17.8	18.7	19.2	19.3	19.0
Other race non-Hispanic	1.6	1.6	1.6	1.7	1.6	1.7	1.7	1.7	1.7
Asian non-Hispanic	5.8	5.5	5.6	5.9	6.2	5.7	5.6	5.4	5.8
Hispanic	18.7	20.3	20.6	20.9	21.0	20.2	20.5	20.7	20.4
Industry (NAICS sectors)									
Agriculture and mining	0.7	0.7	0.6	0.7	1.0	0.8	0.8	1.1	0.8
Utility, wholesale, transportation	8.2	8.3	7.4	8.5	9.2	8.3	8.3	8.1	8.3
Construction	7.0	8.6	10.6	11.2	11.4	9.6	8.3	7.4	9.6
Manufacturing	11.7	11.9	12.2	14.3	15.6	9.6	8.2	9.0	12.1
Retail, administrative, other services	26.7	26.8	24.8	28.0	25.0	23.8	25.4	24.8	25.8
Professional services	18.7	19.8	21.5	19.1	20.2	20.8	19.9	18.9	19.9
Education, health, public	14.8	12.7	12.7	9.2	9.2	14.8	16.9	17.1	12.8
Local services	12.2	11.3	10.2	9.0	8.4	12.4	12.2	13.6	10.7
Previous year earnings (2010Q1$)									
< $25,000	37.9	36.9	34.4	35.7	34.0	38.2	39.8	41.2	36.8
$25,000 to $50,000	39.7	40.0	41.1	40.6	40.8	38.1	37.4	37.0	39.6
$50,000 to $75,000	15.7	16.3	17.1	16.5	17.4	16.1	15.6	15.1	16.4
> $75,000	6.8	6.9	7.4	7.2	7.9	7.6	7.3	6.8	7.3
Sample (thousands)	1,126	1,086	1,248	1,620	1,504	978	946	686	9,195
Sample share	12.25	11.81	13.57	17.62	16.36	10.64	10.29	7.46	100.00

Notes: Calculations from LEHD data. See Table 1 for NAICS industry code ranges.

Table A2: Network measure percentiles

		Full sample				*Low-earnings sample*			
Variable	Period	Mean	p25	p50	p75	Mean	p25	p50	p75
Active employer network (*AEN*)	2005-2012	0.089	0.070	0.085	0.105	0.091	0.071	0.086	0.107
	2005-2007	0.108	0.090	0.106	0.125	0.111	0.092	0.109	0.127
	2008-2010	0.077	0.062	0.073	0.088	0.078	0.063	0.075	0.089
	2011-2012	0.081	0.069	0.079	0.090	0.082	0.070	0.080	0.092
Employment rate (*ER*)	2005-2012	0.648	0.606	0.657	0.700	0.642	0.599	0.651	0.696
	2005-2007	0.666	0.631	0.676	0.714	0.662	0.625	0.672	0.711
	2008-2010	0.643	0.601	0.650	0.694	0.638	0.594	0.644	0.689
	2011-2012	0.620	0.573	0.629	0.675	0.613	0.565	0.622	0.669
Hiring rate (*HRT*)	2005-2012	0.119	0.095	0.114	0.139	0.120	0.096	0.116	0.141
	2005-2007	0.142	0.119	0.140	0.164	0.144	0.121	0.142	0.166
	2008-2010	0.101	0.084	0.099	0.116	0.102	0.085	0.100	0.117
	2011-2012	0.113	0.099	0.112	0.126	0.114	0.100	0.113	0.127
Tract population density	2005-2012	11.3	3.3	5.8	11.1	11.2	3.3	5.8	11.2
	2005-2007	11.1	3.0	5.5	10.9	11.1	3.0	5.5	11.0
	2008-2010	11.2	3.4	5.9	11.1	11.1	3.5	6.0	11.2
	2011-2012	11.9	3.4	5.9	11.5	11.9	3.4	6.0	11.6
Share in same residence	2005-2012	0.509	0.430	0.522	0.603	0.507	0.429	0.520	0.599
	2005-2007	0.509	0.430	0.523	0.604	0.507	0.429	0.520	0.601
	2008-2010	0.508	0.429	0.521	0.601	0.506	0.428	0.519	0.598
	2011-2012	0.510	0.430	0.524	0.605	0.508	0.429	0.521	0.601

Notes: Percentiles are calculated as an average of the closest observation to each percentile with the ten observations ranked above the percentile as well as the ten observations ranked below the percentile.

Table A3: Estimated effect of network measures and control variables on re-employment and re-employment at neighbor's employer in quarter following displacement, 2005-2012

	Re-employment		Re-employment at a neighbor's employer	
	Full sample	Low earnings	Full sample	Low earnings
	(1)	(2)	(3)	(4)
Active employer network (*AEN*)	**0.274*****	**0.312*****	**0.421*****	**0.435*****
Employment rate (*ER*)	0.241***	0.217***	0.127***	0.130***
Hiring rate (*HRT*)	0.157***	0.179***	0.192***	0.213***
Share in poverty rate in tract (2000)	0.015***	0.015***	-0.043***	-0.032***
Share in same house last year (2000)	-0.033***	-0.031***	-0.036***	-0.034***
Share foreign born (2000)	-0.0010	-0.007**	-0.020***	-0.017***
Share less than high school (2000)	-0.023***	-0.010*	0.003	0.0001
Share some college (2000)	0.025***	0.021***	-0.008**	0.003
Share college or more (2000)	-0.015***	-0.002	0.002	-0.016***
Share white, not Hispanic (2000)	0.009***	0.011***	-0.009***	-0.008***
Share black, not Hispanic (2000)	-0.004*	-0.002	-0.005***	-0.005***
Earnings ($1,000s) at employer in previous yr.	0.002***	0.004***	0.0001***	0.0003***
Earnings ($1,000s) from other jobs in previous yr.	0.016***	0.025***	-0.0004***	-0.0002***
Age 19 to 24	0.087***	0.096***	0.022***	0.023***
Age 25 to 34	0.040***	0.040***	0.006***	0.006***
Age 45 to 54	-0.040***	-0.039***	-0.010***	-0.010***
Age 55 to 64	-0.144***	-0.131***	-0.036***	-0.036***
Female	-0.009***	-0.008***	0.001***	-0.00004
Black non-Hispanic	-0.011***	-0.010***	0.006***	0.007***
Other race non-Hispanic	-0.009***	-0.009***	0.001	0.001
Asian non-Hispanic	-0.017***	-0.022***	0.004***	0.004***
Hispanic	-0.003***	-0.002***	0.008***	0.009***
Interquartile effects				
Active employer network (*AEN*)	**0.010**	**0.011**	**0.015**	**0.016**
Employment rate (*ER*)	0.023	0.021	0.012	0.013
Average gross hiring rate (*HR*)	0.007	0.008	0.008	0.010
Number of fixed effects included (1,000s)	2,070	1,810	2,070	1,810
R-squared (within)	0.048	0.049	0.004	0.004
Observations (1,000s)	9,200	7,020	9,200	7,020
Mean of dependent variable	0.585	0.567	0.122	0.125

Notes: Employment estimates are from linear probability model for an indicator of re-employment or re-employment with a neighbor's employer in the quarter after displacement. The full sample includes all separations from 2005 to 2012 (with pre-displacement earnings of between $5,000 and $100,000), while the low-earnings sample is for those with pre-displacement earnings < $50,000. Omitted reference indicators/variables are: age 35-44, share high school grads, male, and white non-Hispanic. Industry dummy variables (using the categories from Table 1) can vary within SEIN/year/quarter/county fixed effects for some multiple-establishment firms operating in more than one industry. The industry dummies and constant term are not reported. The interquartile effects are computed using the percentiles of the distributions for the sample used in the corresponding regression. Robust standard errors are reported in parentheses, clustered by SEIN/year/quarter/county. ***is p<0.01; **p<0.05; *p<0.10.

Table A4: Estimated effect of active employer network measure on re-employment and re-employment at neighbor's employer in quarter following displacement, 2005-2012, with interactions for the recession and post-recession periods

	Re-employment		Re-employment at a neighbor's employer	
	Full sample	Low earnings	Full sample	Low earnings
	(1)	(2)	(3)	(4)
Active employer network (*AEN*)	0.268^{***}	0.307^{***}	0.418^{***}	0.435^{***}
	(0.026)	(0.030)	(0.025)	(0.025)
Active employer network (*AEN*)·*I*(2008-2010)	-0.023	-0.013	0.006	-0.002
	(0.038)	(0.045)	(0.051)	(0.045)
Active employer network (*AEN*)·*I*(2011-2012)	-0.020	-0.012	0.063	0.072
	(0.053)	(0.061)	(0.045)	(0.050)
Number of fixed effects included (1,000s)	2,070	1,810	2,070	1,810
Observations (1,000s)	9,200	7,020	9,200	7,020
Mean of dependent variable	0.585	0.567	0.122	0.125

Notes: Employment estimates are from linear probability model for an indicator of employment or re-employment with a neighbor's employer in the quarter after displacement. See notes to Appendix Table A3. All included variables are interacted with indicators for whether the displacement was in the recession period (2008 to 2010) or the post-recession period (2011 to 2012). Only the estimates for *AEN* and its interactions are reported here. Robust standard errors are reported in parentheses, clustered by SEIN/year/quarter/county. ***is $p<0.01$.