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Labor Market Networks and Recovery from Mass Layoffs: Evidence from the Great Recession Period

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ABSTRACT

We measure the impact of labor market referral networks defined by residential neighborhoods on re-employment following mass layoffs. Because networks can only be effective when hiring is occurring, we focus on a measure of the strength of the labor market network 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, as network theory suggests that employed neighbors in a network serve to increase the probability that, for any given job opening, an unemployed job searcher will be hired into that vacancy. We find some evidence that local labor market networks are linked to re-employment following mass layoffs for lower-earning workers, but our strongest evidence shows that networks serve to markedly increase the probability of re-employment specifically at neighbors' employers, both conditional and unconditional on re-employment itself. This finding is consistent with the specific role that networks play in reducing frictions in the transmission of information in hiring. Moreover, additional evidence provides confirmation of a network interpretation of this evidence: jobs found at neighbors' employers lead to more persistent employment, higher earnings, and higher tenure. Finally, although overall employment and gross hiring both declined markedly during the Great Recession, we find little evidence of changes during this period in the productivity of networks in helping displaced workers find new jobs.

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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. We know that involuntary job displacement has long-term adverse consequences on employment and earnings (e.g. Jacobsen et al., 1993, hereafter JLS; Davis and von Wachter, 2011), and even on mortality (Sullivan and von Wachter, 2009). Because of this, it is important to identify factors that can help facilitate the re-employment of displaced workers.

In this paper, we explore the role of labor market networks in the re-employment process. We focus on labor market networks defined by residential neighborhoods, based on 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)). 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. In particular, we test the hypothesis that strong labor market networks formed by residential neighbors help 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 already are working.

In empirical tests of the importance of labor market networks, it is a challenge to identify exogenous sources of variation in networks because individual-level unobservables may be correlated with both the outcomes studied (e.g., employment or re-employment) and with sorting into networks. In our view, we generate particularly compelling evidence on the role of labor market networks for six reasons. First, we study workers who lost jobs because of mass layoffs that are quite likely exogenous with respect to other characteristics of workers. Second, we use observational data derived from administrative records of displaced workers and their neighbors, and so our results are broadly representative of an important population of workers. Third, by using matched employer-employee data, we are able to estimate highly-saturated models that include layoff-specific fixed effects. This allows us to identify the effects of networks using only variation within a given mass layoff in the strength of networks in the neighborhoods across which laid off workers live. We argue that this within-mass layoff variation in network strength, especially when coupled with other controls for local labor market strength, is very unlikely to be correlated with remaining unobserved determinants of re-employment probabilities of the workers themselves. Fourth, our specification of network effects allows the inclusion of key controls for local labor market conditions that should capture remaining variation in relevant labor market characteristics on which workers sort across neighborhoods. Fifth, for those displaced workers who are re-employed, we observe whether re-employment occurred specifically at the employer of a neighbor, as most network models would suggest. By restricting our sample to only those displaced workers who are subsequently re-employed, and by examining whether network strength is related to the likelihood that they are re-employed alongside a neighbor, we effectively eliminate any remaining unobservables that are correlated with network strength and that also determine re-employment itself. In addition, finding that stronger labor market networks increase the probability of re-employment at the employers of employed network members is an especially compelling result given that it is the outcome predicted by leading theories of the exact mechanism(s) by which labor market networks operate. Finally, focusing more on predicted behavior than on further refinements of the empirical approach, we move beyond increasingly demanding tests for the baseline effect of networks, and instead test for specific implications of network models that relate to employment, tenure, and earnings in network-related jobs.

To briefly summarize our evidence, we find that stronger residence-based labor market networks facilitate re-employment by matching displaced workers to vacancies, especially at neighbor's employers – just as theory would suggest. These effects are substantially larger for low earners than for high earners, as might be expected given that the relevant labor markets for low-skilled workers tend to be more local. While both employment and especially hiring dropped markedly during the Great Recession, we find little evidence of a drop in the productivity of residence-based networks matching job searchers to their neighbors' employers. Finally, we find evidence consistent with network connections leading to better jobs, as displaced workers who are re-employed at their neighbors' employers experience more persistent employment, higher earnings, and higher tenure, compared to displaced workers re-employed elsewhere.

II. Motivation and Previous Research

Theoretical models of general job search 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, and a positive function of the employment rate in their local labor market (or a negative function of the unemployment rate). When vacancies go up, a job searcher is more likely to (perhaps randomly) match to the vacancy. When employment goes up, a job searcher is more likely to match to a vacancy because competition for that vacancy is lower. Moreover, job search models predict that the probability of successful re-employment is a negative function of the job searcher's reservation wage, and a negative function of the length of time the person has been unemployed (assuming there is negative duration dependence, as suggested in recent work by Kroft et al., 2013). In models of spatial mismatch such as Kain (1968) (or more nuanced versions, such as Hellerstein et al., 2008), the probability of finding employment is also a function of job accessibility, which itself is related to factors such as commuting costs and information about vacancies in very local labor markets such as neighborhoods.

Theoretical models of labor market networks expand on these standard models by assuming that there is imperfect information that 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, and negatively related to the unemployment rate in his/her local labor market. In equilibrium, better connected job searchers are more likely to find employment (and to have higher wages).

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.[1] This network model, based on referrals, predicts better job matches for hiring that results from network connections, which should be reflected in longer tenure on the job, and may be reflected in higher wages as well.[2] The first model, based more on information about job vacancies, can also lead to better job matches if a faster job arrival rate allows workers to be more selective in their search.

These two classes of models both layer onto standard models of job search the additional

[1] Jackson (2008, Chapter 10) provides a transparent discussion and comparison of these models.
[2] 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.

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) where he or she has 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 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 models of job search behavior that incorporate all of these features 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, and, most important, 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, there is a large, earlier body of empirical research that documents the importance of informal contacts in finding jobs.

Survey evidence summarized in Ioannides and Datcher Loury (2004) establishes some reliance on friends and relatives to find jobs; in particular, they report that 15.5 percent of the unemployed and 8.5 percent of the employed contact friends and relatives as part of their job search. Other evidence suggests that these contacts may be productive, with those who use friends to search for jobs reporting more offers and more accepted offers per contact (Blau and Robins, 1990). And a survey by Bewley (1999) estimates that 30-60 percent of jobs were found through relatives and friends, although the evidence on the effects of these contacts on match quality is more ambiguous. These results partly echo the findings in Granovetter's famous book (1974), which is widely viewed as having launched the literature on networks. He interviewed people in Newton, Massachusetts about how they found their jobs, finding that about half of workers (among technical, professional, and managerial workers) found their jobs through a social contact. However, many also found jobs through a work contact, emphasizing that friends and relatives are not the only potential source of information about jobs or referrals to jobs.

While this type of survey evidence provides important support to the hypothesis that job market information in part flows through networks, the kind of evidence we explore in this paper (and other evidence in the more recent literature on labor market networks) is more decisive and informative for a number of reasons. First, and most important, the differences in job finding rates between those who report using friends and relatives (e.g., Holzer, 1987) is descriptive evidence that does not try to rigorously compare similar workers who face plausibly exogenous variation in the network contacts available to them. This is a central challenge undertaken in our work and a good deal of the recent work on networks. Second, a good deal of the survey evidence asks workers about all of the methods they used to search for work (see the PSID evidence in Appendix Table 1 in Ioannides and Datcher Loury, 2004, and the CPS evidence in, e.g., Kuhn and Skuterud, 2000), so the results on job finding may simply reflect more intensive search using more job search methods. Third, the survey evidence tends not to cover many of the potential links between workers that have been uncovered in recent research on labor market networks. In our view, information on the different kinds of links between workers can be critical to thinking about how policy might leverage network connections.

Most pertinent to this paper, 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 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 – termed "network isolation" to capture how much workers from the same neighborhood are isolated or segregated from workers from other nearby neighborhoods. This concept parallels the well-known and influential work by Granovetter (1974), extending beyond a very 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 and immigrants.

In this paper we turn our attention to the effects of residence-based labor market networks in helping non-employed workers in general, and displaced workers in particular, find work. This issue is especially important within the context of the large job losses that accompanied the Great Recession and the ensuing high rates of unemployment and low rates of labor force participation, so our analysis estimates network effects on re-employment for workers displaced right before, during, and just after the Great Recession.

There is some related work on labor market networks and recovery from displacement. This work focuses on potential network connections between former co-workers – reinforcing the point that labor market networks are not limited to connections between neighbors. Glitz (2014) suggests that network connections to co-workers (or former co-workers) may be more important because those co-workers should know more about a person's work abilities, and also should be likely to know each other (although that may not be true in larger firms). Using German data, he finds that displaced workers within the same "origin" establishment have a higher probability of re-employment when the employment rate among former co-workers is higher, using exogenous variation (as an instrumental variable) in that employment rate driven by mass layoffs among those co-workers. Saygin et al. (2014) report similar results for Austria, although without the advantage of the mass layoff instrumental variable. They 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.[3] And Cingano and Rosolia (2012) present related evidence for Italy, finding that unemployment durations of displaced workers are shorter when the current employment rate among their former co-workers is higher.[4]

Whereas these other recent papers focus on network links to former co-workers, we study residential labor market networks. Without in any way implying that network links among co-workers are not operative or important, the "urban" flavor of residence-based labor markets is potentially important for at least two reasons. First, if there are network links among neighborhood residents, policymakers may be able to exploit the "multipliers" that networks can generate to

[3] Saygin et al. (2014) suggest that this hiring outcome 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 simply providing information about the availability of jobs at their firm, à la Calvó-Armengol and Jackson (2007).

[4] Other empirical papers test explicitly for the importance of referrals in the job finding process, but do not focus on displaced workers per se. These include Beaman and MacGruder (2012), Brown et al., (2014), Pallais and Sands (forthcoming), and Burks et al (2015).

enhance the impact of place-based policies. For example, if enterprise zones are designed to encourage hiring of residents of disadvantaged locations, then referrals or job leads from those hired could lead to increased employment among these residents.[5] Conversely, dependence on labor market networks could explain why place-based policies sometimes fail to generate jobs among residents of the targeted locations, as when enterprise zones incentivize hiring per se, but the hiring comes from outside the areas, or employers relocate to the area without increasing hiring of local residents (see the discussion in Neumark and Simpson, 2015). Second, residence-based labor markets can help explain concentrations of low employment and poverty in particular local areas, and can also – if these networks are racially- or ethnically-stratified – help explain pockets of poor economic performance in minority, segregated neighborhoods. At the same time, paralleling the argument with respect to place-based policies, such networks may provide scope for enhanced efforts to increase employment in these areas, as well as improving job matches.[6]

III. Network Measures and Analysis

Consider a sample of workers who lose their jobs as part of a mass layoff. We address a number of questions, including: Do these displaced workers find jobs quickly? How does the strength of their neighborhood networks affect whether these laid off workers find jobs quickly, and how does it affect where they are re-employed? Do workers with better network connections find better jobs?

In our empirical analysis of how networks matter for displaced workers (described in Section V), we consider how the re-employment probability of a displaced worker is affected by the strength of his or her residential labor market network, examining first re-employment generally and then honing in specifically on re-employment at a neighbor's workplace. Our baseline estimates focus on examining outcomes in the quarter following displacement, partially for simplicity, but more so because workers with long durations of unemployment prior 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

[5] Neumark and Simpson (2015) also discuss efforts to exploit network effects in the "Jobs-Plus" program (Riccio, 1999). Jobs-Plus aimed to increase labor supply incentives for public housing residents by reducing the rent increases that accompany increases in earnings. In addition to including employment-related activities and services, Jobs-Plus tried to encourage the formation of labor market networks. Most sites had "job developers" on staff whose responsibilities included providing outreach to local employers, cultivating relationships with them in an effort to place Jobs-Plus participants in employment (Kato et al., 2003). The program also employed residents as "court captains" or "building captains" who maintained contact with other participants, including sharing information about employment opportunities.

[6] Hellerstein and Neumark (2012) discuss this in the context of the Jobs-Plus experiment.

similar in the two periods, making comparisons of network effects on re-employment before and during (and after) the Great Recession more valid. However, we also present evidence on longer-run effects in assessing evidence on whether network connections lead to better jobs.

We operationalize the strength of a job searcher's network by developing a measure of the strength 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. The U.S. 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 socio-economic characteristics.[7] We restrict the analysis to urban Census tracts, which are defined based on population density and may fall in both central cities and suburbs.[8] In 2000, urban areas accounted for 79.5 percent of the U.S. population and 2.6 percent of land area.

We then empirically examine whether and how our tract-level measure of network strength affects the re-employment outcomes of displaced workers, conditional on an extremely large set of worker, employer, neighborhood, and job-related covariates that we are able to use given the considerable detail and size of the Longitudinal Employer-Household Dynamics (LEHD) Infrastructure Files that we use. Finally, we explore additional implications of network models to better assess whether our findings are likely generated by the effects of networks.

In order to explain our network strength measure and how we construct it using the LEHD data, consider the hypothetical case of one specific job searcher who is searching for a job after being displaced from his/her employer in a mass layoff in a given quarter. To clarify terms, we generally use "employer" and "establishment" interchangeably. (The majority of jobs are at are stand-alone employers, and use of the word "employer" is more natural in discussing labor market models. In contrast, the word "firm" always refers to companies, whether single-establishment or multi-establishment entities.)[9] Given the detailed longitudinal nature of the LEHD data, we

[7] The Census Bureau has developed standards to create and maintain Census tract definitions to promote consistency nationwide. 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.

[8] 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.

[9] As discussed below, the LEHD reporting unit for Unemployment Insurance covered earnings is identified by a state UI account number, referred to as the State Employer Identification Number (SEIN). As part of the Quarterly Census of Employment and Wages, the Bureau of Labor Statistics requires that employers with multiple locations within a state report complete a Multiple Worksite Report listing all their establishments. Because in most states firms with multiple establishments do not report establishment assignments of workers, the analysis uses the LEHD unit-to-

observe the displaced worker's pre-displacement earnings, 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. Critically, we observe the Census tract in which he or she lives. We also can 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, as well as characteristics of the establishments, including its location, and, importantly, gross hiring (if any) at these establishments in the post-displacement quarter.

We term our core network measure the "active employer network" measure, denoted *AEN*. It is a Census-tract-level measure that is motivated explicitly by the fact that theoretical network models (such as Calvó-Armengol and Jackson (2005) and Montgomery (1991), as well as others) predict that employment outcomes of job searchers will be better when both the employment rate of network contacts is higher *and* when there are more vacancies available so that employed network contacts can facilitate information transmission. *AEN* is the product of these factors and can be written as:

$$AEN = ER \times HR,$$

where *ER* measures the neighborhood employment rate and *HR* is a proxy for vacancies.

In both models, 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. As such, *AEN* captures the amplification effect that is provided by the interaction between the employment rate and the vacancy rate, so that, for example, while an increase in the vacancy rate should lead to better employment outcomes for all job searchers, such an increase will have an even larger impact on the re-employment of job searchers who are networked to many employed neighbors who can connect them to those vacancies.[10] The "active"

worker imputation model (discussed later) to assign workers to establishments.

[10] One paper where this idea is developed explicitly in a theoretical treatment of networks is Calvó-Armengol and Zenou (2005). The authors use the micro-foundations of a network model to generate the results that an individual's probability of being hired via a network contact is a non-linear function of the unemployment rate and the vacancy rate, and that there is an aggregate matching function that itself is a function of the unemployment rate, the vacancy rate, and the strength of the social network. For an excellent review of theoretical and empirical treatments of networks, see Topa and Zenou (2015).

part of the name references the fact that this measure only counts connections where there is gross hiring occurring. That is, individual job seekers may have many network contacts, but unless these contacts can facilitate the transmission of information about vacancies (which are unobserved, but for which gross hiring is a proxy), they are not productive contacts.

We now describe our empirical implementation of the two components that make up *AEN*. First, recall that for each of the displaced worker's neighbors in our data, we observe whether or not the neighbor is employed. We therefore can calculate the employment rate in the Census tract as

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

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), and I_i is an indicator for whether neighbor i is employed in the quarter following the job searcher's displacement. Because the employment rate is obviously the complement of the non-employment rate, the employment rate in the local labor market will both control for local labor market conditions that affect search outcomes for the unemployed (as in a standard search model), *and* contribute to multiplier-like effects that occur in network models when employed contacts facilitate information transmission about job vacancies.[11]

Second, we observe not only whether any given neighbor works, but also where he or she works (if employed). Therefore, for each establishment at which a neighbor works, we can 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 quarter). We therefore can calculate the overall average gross hiring rate among employed neighbors' employers as:

$$HR = \frac{\sum_i^N I_i \cdot \frac{H_{ie}}{L_{ie}}}{\sum_i^N I_i}$$

where $\frac{H_{ie}}{L_{ie}}$ is the ratio of new hires at the employer e of neighbor i in the first quarter following our

[11] Aside from having a role in job search, *ER*, along with other Census tract controls introduced later, may control for neighborhood characteristics and sorting by neighborhood, including cultural norms of working that can generate peer effects (Mota et al., 2016).

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 both the numerator and denominator. $\frac{H_{ie}}{L_{ie}}$ is undefined for these cases, but we have not introduced additional notation since this expression is multiplied by zero in these cases; we also require that each tract has a minimum count of employed neighbors, so the denominator is never zero.)

We use this overall average gross hiring rate as a proxy for the vacancy rate, which given the periodicity of our data (quarterly) is reasonable. As such, this gross hiring rate belongs in our empirical analysis both because it affects the probability of re-employment in a standard job search model, and because, in any network model, the re-employment probability is further amplified when employed network contacts hear about vacancies and transmit information to job searchers about the vacancies (as in models like Calvó-Armengol and Jackson, 2007) or about the quality of job searchers to hiring employers (as in models like Montgomery, 1991).

Our active employer network measure, or *AEN*, therefore explicitly captures the idea that re-employment probabilities for job searchers are increased when both the employment rate of network contacts is high and the gross hiring rate is high in the post-displacement quarter.[12]

<u>Interpretation of the network measure</u>

We note a few specific aspects of how this measure operates, to aid in interpretation. First, *AEN* is lower when the employment rate, *ER*, is lower. This reflects the fact that as the rate of job seekers in a neighborhood increases, the probability that any one job searcher will obtain productive information on vacancies from his or her neighbors is lower, either because 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 because our job searcher will have to compete with his/her neighbors when applying to job vacancies that are accessed through neighborhood contacts.

Second, the component of *AEN* that comes from the gross hiring rate, *HR*, averages across the gross hiring rate in each establishment rather than the absolute number of gross hires to calculate *HR*. Using a measure of the gross hiring rate rather than the absolute number of gross hires is a scaling measure that is meant to capture competition across networks 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

[12] *AEN* can alternatively be written as: $AEN = \frac{1}{N}\sum_i^N I_i \cdot \frac{H_{ie}}{L_{ie}}$.

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 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. Similarly, a large employer with a lot of vacancies does not necessarily gain proportionately more information about potential hires from its employees than a small employer with a small number of vacancies.

Third, because the hiring rate, *HR*, is calculated across all employed neighbors, 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 from neighbors giving the job searcher redundant information about vacancies. However, it is more likely that our job searcher learns of labor market information only from 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.[13] In addition, if there is some noise in the 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 employed neighbors, regardless of the number of establishments at which they are employed.

As we explain in further detail below, in our baseline empirical analysis of re-employment outcomes for displaced workers, we estimate regressions where we include as covariates *ER, HR,* and *AEN*, interpreting the coefficient on *AEN* as the effect of active residential network strength on re-employment.

<u>Additional controls for local labor markets</u>

We include many other controls in these regressions, to capture individual characteristics (such as age, sex, and race/ethnicity) and neighborhood characteristics (such as the poverty rate and the shares with different levels of education). Two other controls closely related to *HR* and *AEN* are included in some specifications, and require additional explanation.

First, for a given displaced worker, the gross hiring rate measure, *HR*, only measures hiring occurring at neighbors' employers. Hiring at these establishments may properly reflect the hiring

[13] This discussion emphasizes that our empirical network variable measures with error the corresponding metric for the neighbors that are actually in each worker's network. To the extent that this measurement error is classical, which may well be a reasonable assumption if information on vacancies arrives via some stochastic process, we would expect attenuation bias, suggesting that the effects of networks we find would be larger absent the measurement error.

rate in the local labor market generally, especially if residential sorting by Census tract leads neighbors with the same kinds of skills to live in the same neighborhoods. However, the LEHD data also allow us to construct a more general measure of the gross hiring rate in the local labor market. Specifically, in addition to considering the gross hiring rate among neighbors' employers, we can also consider the gross hiring rate in all establishments located in Census tracts (*w*) in which a displaced worker's neighbors (*i*) work. We denote this "tract-level" hiring rate as *HRT*, and measure it as:

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

HRT is measured in the Census tract *w* in which worker *i* works, rather than just at their employer. Specifically, we calculate an aggregate H_{iw} and L_{iw} across all establishments within each workplace tract where neighbors work to measure the overall hiring ratio in that location, and then sum the workplace tract ratios across all employed neighbors. Therefore, *HRT*, as a measure of the average gross hiring rate in Census tracts where neighbors work, can additionally capture the general strength of demand conditions in the local labor market, because neighbors' workplaces likely represent the set of locations with economic opportunities that are easily accessible by transportation.[14]

Second, we can construct a Census tract-level analog to *AEN*, which we denote as *ATC*, for "active tract control."[15] While *AEN* captures the notion that a job searcher's employed neighbors can serve as a conduit for information when there are vacancies in their own establishments, neighbors may also serve as a conduit for information when there are vacancies in establishments near to their own, rather than just in their own establishments. This is the conceptualization of networks used in Bayer et al. (2008).[16] *ATC* is defined as the product of the employment rate (*ER*) and *HRT*:

$$ATC = ER \cdot HRT$$

In some of our empirical specifications, we include as covariates *HRT* and *ATC* in addition

[14] Bayer et al. (2008), in their measure of network ties, are able to control for the strength of the local labor market by treating neighbors only as those who live on the same Census block, and treating correlated outcomes among those who live in the same block group as (potentially) capturing local labor demand, job access, etc.
[15] The use of "active" in the name reminds the reader that the construction parallels *AEN*.
[16] Bayer et al. (2008) use the word "referrals" in the context of co-residents providing information to each other about jobs near where they work (p. 1152).

to *ER*, *HR*, and *AEN* in order to test the robustness of our results to the inclusion of another, more general measure of the gross hiring rate and another type of potential mechanism for networks to impact re-employment.

IV. Data

The core dataset from which the samples we study are extracted is the Census Bureau's LEHD Infrastructure Files.[17] The files consist of a frame of jobs produced from state Unemployment Insurance (UI) reporting systems, augmented with information on worker and employer characteristics. The state data cover the universe of wage and salary workers in the private sector as well as state and local government workers, but do not include federal workers or earnings through self-employment. States provide the Census Bureau with two quarterly files. The earnings history file lists the quarterly earnings accruing to a worker from an employer. The employer file 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. The LEHD Infrastructure Files use unique person and establishment identifiers to merge worker and employer data.[18] We use the LEHD Infrastructure Files 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.

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 44 percent of jobs are at multi-unit firms). The LEHD program has developed an imputation model to allocate establishments to workers in a manner that is meant to replicate the distribution of establishment sizes within a firm and the general distribution of commute distances.[19] While workers are more likely to have larger and

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[17] See Abowd et al. (2009) for a summary of the various components of the LEHD Infrastructure Files.

[18] Employer identifiers may change over time, which may lead to false inferences about the continuity of employers or jobs. Throughout our analysis, we make use of employer identifier mappings based on worker flows to track employer size, hiring, and job tenure (see Benedetto et al., 2007).

[19] The state in which an employee works is indicated by the state to which a firm submits UI earnings records. In the LEHD Infrastructure Files, a unique SEIN is assigned to each firm in each state. One exception to non-reporting is

closer establishments imputed to them, they could potentially be allocated any establishment at the firm. For multi-unit firms, we use this imputed assignment to identify the establishment from which a worker was displaced (as well as the 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.[20] In general, we expect the use of imputed establishments for jobs at multi-unit employers to impose no systematic bias toward finding network effects (placing neighbors at the same employer), and if anything, to attenuate the magnitudes of estimates for working at a neighbor's employer.[21]

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.[22] From these data, we identify 136 million workers separated from their highest earning (dominant) job from 2005 through 2012, as defined below. 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.[23] Last, we require that the separation was from the

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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, incorporating establishment size and commute distance that is applied to firms with multiple units in other states.

[20] 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. The one exception in this study 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 these weighted aggregations of gross hires and employment (inputs to *HR*) at the establishment level as inputs to the Quarterly Workforce Indicators.

[21] For the location of a displaced worker's former employer, our use of county-level geography aggregates across the set of nearby workplaces to which a worker would likely have been imputed, minimizing the role of the imputation draw. For identifying re-employment at a neighbor's employer, the imputation model makes neighbors no more likely to be co-workers in the same establishment of the firm beyond what would be expected from the size distribution of establishments and the similarity of local commute distances (a tract-level calculation). The imputation model will tend to underrepresent the tendency of neighbors to be co-workers, which we find some evidence of in HKN (2014). Finally, for measuring the hiring rate at neighbor's employers, our averaging across all neighbors in a tract and the use of all ten implicates from the imputation model for calculating the hiring rate will downplay the contribution of any particular imputation draw among all neighbors.

[22] 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.

[23] For both separations and mass displacement events, we define employers at the SEIN level, and refer to the state-

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 more 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 over a period of one year (four quarters) during which we observe a worker leaving the employer. For this sample, 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 2012.

We apply several additional restrictions to the set of displaced workers based on data availability constraints and suitability for our research focus. 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.[24] From among these locations, we require that the Census tract is entirely classified as urban and has at least 100 resident workers, which restricts attention to more densely populated areas in which neighbors are more likely to interact.[25] 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

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 (viewed February 27, 2017).

[24] We use the Composite Person Record, an annual file built from federal administrative data on residential addresses that contributes to the LEHD Infrastructure Files (Abowd et al., 2009).

[25] In the 2010 Census, 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 includes 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).

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 $100,000 (in 2010Q1$), for two reasons.[26] First, 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 (consistent with the evidence in HMN). Second, the lower restriction excludes workers who, although they held a job for at least a year, were more likely to be a dependent, or otherwise not highly attached to the wage and salary labor market. The upper bound 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.

Using the data on 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) contribute to the count of N. 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 and the total count of neighbors to compute the quarterly employment rate ER for the beginning of the quarter after the layoff. We calculate the

[26] We use the urban Consumer Price Index, averaging the three months in a quarter (because earnings are reported on a quarterly basis).

average gross hiring rate (*HR*) for the same quarter by averaging (across employed neighbors) the count of new (gross) hires by a neighbor's employer at an establishment in a quarter divided by the count of employees at that establishment in the beginning of the quarter.[27]

Table 1 provides mean characteristics of our worker sample, 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 is displaced.[28]

For some intuition about the value of *AEN*, based on the means in Table 1 a displaced worker would typically live in a neighborhood where six or seven out of every ten neighbors are employed (0.648), with a gross hiring rate of one to two new hires for every ten workers at their firms (0.140). The product of these specific estimates is 0.091, close to the mean *AEN* of 0.090. 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.[29] 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)).[30] 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.

[27] 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).
[28] In Appendix Table A1 we provide sample means for these variables for each year separately. Some of the patterns in this table are consistent with what we would expect – for example, the much higher share of mass layoffs in manufacturing and construction around the Great Recession. We verified that our results were qualitatively similar if we reweighted the data to hold the sample composition fixed (relative to 2006Q1) in terms of industry, the factor that varied most across the recession years.
[29] The shortfall in 2006, compared to the surrounding years, is due to an imprecision of Census Bureau geocoding of administrative records for residences in that year. Also note that in 2012, 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. This explains the lower percentage of observations (7.5 percent) in 2012.
[30] 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.

Figure 1 displays various percentiles of the employer network measure (*AEN*), employment rate (*ER*), and hiring rate (*HR*). All three measures exhibit a clear pattern of decline and some recovery associated with the Great Recession, as we would expect from the changes in both the proportion of neighbors employed, and especially the hiring occurring at their employers. Note, in particular, that by 2009, the percentiles of *AEN* had fallen by more than one-third relative to their pre-recession levels.[31]

V. Empirical Analysis

Having defined our measures of the employment rate (*ER*), the gross hiring rate (*HR*) (which, recall, serves as a proxy for the vacancy rate), and the strength of the network (*AEN*), we proceed to explain our empirical analysis.[32] To answer the question of whether and where a displaced worker is re-employed following a mass layoff, we conduct a series of regression-based analyses where, for our sample of displaced workers, our first set of outcomes of interest are shorter-run post-layoff re-employment outcomes. We consider three of these: (1) employment in the first quarter following layoff; (2) employment in a neighbor's establishment in the first quarter following layoff; and (3) employment in a neighbor's establishment in the first quarter following layoff conditional on any employment in that quarter (i.e., re-employment).

As foreshadowed above, we view the data generating process as a linear relationship between employment outcomes and four related (observable and unobservable) factors that affect the productivity of job search: characteristics of the laid off worker and his/her employer; characteristics of the worker's residential neighborhood; local labor market characteristics; and residential network strength.

We estimate linear probability models for re-employment in the first quarter following layoff that have the following form:

$$Emp_{jnkt} = \alpha + X_{1jt}\beta_1 + X_{2nt}\beta_2 + ER_{nt}\beta_3 + HR_{nkt}\beta_4 + AEN_{nkt}\gamma + \varepsilon_{jnkt}. \quad (1)$$

The subscript j indexes the individual laid-off worker, n indexes residential neighborhood, k indexes the local labor market (which contains neighborhood n) and t indexes the year/quarter in which the displaced job ended. X_{1jt} and X_{2nt} are vectors of observable characteristics of individual j

[31] Appendix Figure A1 shows that these measures track other published measures of hiring and vacancies. For an analysis of the co-movement of job turnover dynamics in the Quarterly Workforce Indicators and survey-based measures, see Hyatt and Spletzer (2013).

[32] In this section we describe our core analysis used to test for network effects. Some additional analyses intended to better understand the effects of networks or to test implications of network models are described in the results section (Section VI) that follows; these additional analyses follow straightforwardly from our core analysis.

and his/her neighborhood *n*, respectively. As previously discussed, *ER* and *HR* are local labor market characteristics (*ER* may also be a neighborhood characteristic), and *AEN*, the key variable of interest, measures the strength of the neighborhood network. 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:

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

Although the LEHD has limited demographic information as compared to, say, the Current Population Survey, we are still able to include in the vector X_{1jt} controls for age, sex, race, and ethnicity. We also control for annual earnings in the previous year from the displacement job as well as from all 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. The vector X_{2nt} contains a set of neighborhood characteristics that we construct from the 2000 Decennial Census, including measures of the racial and ethnic composition of the neighborhood, the share of residents in poverty, and the share of residents who did not move in the previous year. In principle these characteristics could be time-varying, but we do not have access to them on an annual basis, and so we fix them at the year 2000.[33]

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,* even conditional on the other observables in Equation (1), and failure to account for these correlated unobservables could then 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:

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

where E_{jt} is a layoff fixed effect that is uniquely defined by SEIN, year, quarter, and county location of the establishment.[34] The remaining sum, $(v_{1jt} + v_{2kt})$, is assumed to reflect idiosyncratic unobservables of the individual and the local labor market. That is, we assume in Equation (3) that the systematic individual and local labor market components of the error term that are correlated with the network measure *AEN* can together be accounted for by one fixed effect reflecting the specific mass layoff in which the worker lost his or her job. We therefore identify the effect of

[33] 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.

[34] We have not added a layoff index. *E* is indexed by *j* and *t*, but is equal for each person in the same layoff as defined here, except as noted below.

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* SEIN, and from establishments of that SEIN in the *same* county. The variation thus arises when co-workers who are laid off together live in different neighborhoods.[35] In total, then, the identifying assumption is that, conditional on the observables X_1, X_2, *ER,* and *HR*, co-workers who lose their job in the same mass layoff face systematically different post-layoff employment outcomes only because they have access to different neighborhood networks – that is, different *AEN*s.

To underscore the role of the layoff 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. We saw earlier 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. Finally, note that 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.[36]

In our baseline specifications, we implicitly treat the third term in Equation (2) – the 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

[35] Ideally one might want to further distinguish layoffs that happen simultaneously across establishments of a given employer within a county if, for example, one establishment houses managerial workers and another houses production workers. However, because of the limits of the LEHD in identifying individual establishments of multi-establishment employers, we do not take this extra step. We thus interpret our employer-by-year-by-quarter-by-county fixed effects as layoff-specific fixed effects. When we disaggregate our sample into higher- and lower-earning individuals, both across the whole sample and (separately) among workers who share the same layoff-specific fixed effect, we may be implicitly distinguishing between these kinds of establishments even within the same county.
[36] 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.

leaves open the possibility that residential neighborhood sorting by unobservables (based, for example, on shared preferences for amenities) is correlated with *AEN*. To the extent that common unobservables among residential neighbors are also driving the neighborhoods in which they work, when we include the workplace controls *ATC* and *HRT* in the regressions, we capture this heterogeneity. That is, while *ATC* and *HRT* reflect local labor market conditions more generally, they also help control for the part of ω_{nt} that reflects systematic variation that sorts workers both into residential neighborhoods and workplace neighborhoods (and that remains even after including layoff-specific fixed effects).

Finally, from an operational standpoint, note that excluding the 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.

If, despite including these extensive and varied controls, there is still a concern about unobserved heterogeneity across workers laid off from the same establishment that is correlated with neighborhood network strength and that affects re-employment per se, we estimate Equation (1) for different outcomes and samples that successively narrow the scope for bias from this source. In particular, 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. This specification is clearly the most prone to biases because anything at the individual-, local labor market-, or neighborhood-level that is correlated with *AEN* and is not accounted for by the layoff-specific fixed effects will generate a spurious relationship.

We then narrow the re-employment definition so that *Emp* is an indicator for becoming re-employed at the employer 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 employment at a neighbor, because generic sources of variation in re-employment per se do not play a role.

Finally, our strongest evidence comes from looking at this latter outcome – employed at a neighbor's employer – but only for the subsample of those who become re-employed. If there were still unobserved heterogeneity across workers laid off from the same establishment that is correlated with neighborhood network strength and that affects re-employment per se, it is by definition eliminated in our specifications where the outcome is re-employment at a neighbor's employer and where we restrict the sample to those who are re-employed at any employer.

We focus on the average effect of network strength in facilitating labor market recovery for displaced workers across all of our sample years. But we also explore differences in the effects of network strength on the employment recovery of displaced workers in the periods prior to, during, and coming out of the Great Recession, asking whether positive effects of network strength, if they exist, are stronger or weaker during the recession. Hence, in addition to estimating models for the full sample of 2005-2012, we also explore separate estimates for each year in the time span 2005-2012.

VI. 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 first present, in the top panel of Figure 2, the standard depiction in this literature of the observed earnings shock associated with displacement. Although previous analyses have focused on annual earnings over a long horizon, we present the data quarterly both because we only have recent data and (relatedly) because in our empirical analysis we examine a quarterly employment outcome following displacement. The top panel of Figure 2 therefore 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 the first quarter of 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 by the end of 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,[37] so what is most interesting to us – and perhaps more tied to network strength – is re-employment. We therefore focus most of our analysis on the re-employment margin.

Other determinations of employment and earnings recovery after displacement

In Table 3 we report the results of the employment regressions represented by Equation (1), and in this sub-section we discuss the estimates of the coefficient vectors, β_1 and β_2, for the

[37] Our evidence that employment is the key driver of earnings losses is somewhat at odds with what Davis and von Wachter (2011) found 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.

control vectors of covariates, X_1 and X_2. We report results for three measures of re-employment: in Column (1) we show the estimates from a regression where the dependent variable captures whether a displaced worker is re-employed in the quarter following displacement; in Column (2) the dependent variable is a dummy variable capturing whether the displaced worker is re-employed at the establishment of a neighbor; in Column (3) we restrict the sample to those who are re-employed, and the dependent variable is a dummy variable that captures whether, for the re-employed worker, he or she is working at the establishment of a neighbor.

The results in Column (1) show that workers who we know tend to be advantaged in the labor market generally are also advantaged when it comes to the probability of re-employment in the quarter following displacement. Workers who had higher earnings in the previous year, both from the employer from whom they were displaced, and from other employers, had higher re-employment probabilities, as did younger workers, whereas older workers, minorities, and women generally had lower post-displacement employment rates, conditional on previous earnings and the other controls. Many of the neighborhood characteristics are correlated, so we would not necessarily expect to see the anticipated sign of the effect of each of these characteristics on re-employment of the displaced worker.

When the outcome variable is not re-employment alone, but re-employment at a neighbor (Column (2)) and re-employment at a neighbor's employer conditional on re-employment (Column (3)), the results do not mirror those in Column (1) in magnitude or even in direction. The coefficient estimates on earnings in the previous year across Columns (2) and (3) are small, and if anything suggest that higher-earning workers are slightly less likely to find employment in a neighbor's establishment. Women are more likely to become re-employed in a neighbor's establishment in both Columns (2) and (3), as are blacks, Asians, and Hispanics. These results are fully consistent with the results in HMN and HKN, who find that less-skilled workers and non-white workers are more likely to work with their residential neighbors, which in turn suggests that residential neighbors may serve as important network contacts when it comes to re-employment, especially for certain groups of workers.

<u>The effects of networks on re-employment</u>

We now turn to our main analyses – the estimated effects of residence-based labor market network measures on various measures of employment. In Table 4 we report the coefficients on *AEN*, *ER,* and *HR* from a number of regressions and for different samples. All of these regressions also include (but we do not report) all of the control variables listed in Table 3, as well as the layoff fixed effects. 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 percentiles of the distributions of these three measures – most importantly the active employer network measure, *AEN*.

The top panel of the table reports estimated regression coefficients for the full sample of displaced workers – repeating the results reported in Table 3. In Column (1) of the top panel, where the dependent variable is simply re-employment, the results show that both the employment rate and the gross hiring rate have positive and statistically significant impacts on the probability of re-employment. The economic magnitude of the employment rate (*ER*) effect, as reported in the table, is relatively large, with an estimated coefficient of 0.270; the implied effect of the interquartile change is to raise the probability of re-employment in the quarter following displacement by 2.54 percentage points (compared to a mean job finding rate of 58.5 percent, reported in Table 1).[38] In contrast, the estimated coefficient on the active network measure (*AEN*), while positive at 0.022, is statistically insignificant, and its implied interquartile effect of 0.08 percentage points is economically small.

However, this evidence does not address the explicit network mechanism that potentially links displaced workers to vacancies at their neighbors' employers. In Columns (2) and (3), therefore, we turn to estimates for re-employment at a neighbor's employer, both unconditionally and then conditional on re-employment. The estimated network effects in these columns 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 the displaced workers, these networks should yield higher probabilities of re-employment specifically at those establishments.

The estimates in Columns (2) and (3) provide strong and more convincing evidence of the importance of residential networks on re-employment outcomes. As reported in both columns, the estimated coefficients on (*AEN*) are positive and statistically significant. In Column (2), the implied interquartile range of the coefficient estimate of 0.513 is 1.84 percentage points. Given that the mean of the dependent variable in Column (2) is 0.122, or 12.2 percent, we view this as an

[38] We multiply the coefficient 0.270 for *ER* from Table 4, Column (1), by the range from 0.606 to 0.700, which gives an implied effect of 0.0254 on the indicator for re-employment. See Appendix Table A2 for the percentiles of each of the network variables.

economically meaningful effect, whereby networks formed by residential neighbors successfully serve to help job searchers become re-employed at neighbors' employers. This effect is mirrored in Column (3), where we measure the interquartile impact of *AEN* on re-employment at a neighbor, only for those who become re-employed, as 2.55 percentage points (relative to a mean of 20.9 percent).

The coefficient estimates on *ER* in the top panel of Columns (2) and (3) are positive and statistically significant at 0.100 and 0.106, respectively, with interquartile ranges less than half as large as *AEN*. The negative coefficient estimate on *HR* does not imply an overall negative effect of *HR* on the re-employment probability; because *AEN* is a function of *HR*, the overall effect of an increase in the hiring rate on re-employment at a neighbor includes the effect through *AEN*.[39] Moreover, to the extent that *ER* and *HR* may also serve as controls for (unobservable) local labor market conditions, it is not a priori clear that their effects on re-employment, particularly at a neighbor's employer, should be positive, especially conditional on all the other covariates in the model. As a result, moving forward, while we always report the coefficient estimates on *ER* and *HR* in the tables, we focus the discussion on the estimated coefficients of *AEN*.

While the top panel of Table 4 reports estimates for the full sample, the middle panel reports estimates for those with pre-displacement earnings below $50,000, and the bottom panel for those with pre-displacement earnings of $50,000 or higher.[40] Our conjecture is that local labor market networks are more important for lower-skilled than higher-skilled workers, because these low-skilled workers are more likely to search for jobs in local labor markets. Conversely, we would not be surprised to find much less or no evidence of effects of local labor market networks for higher-skilled workers. Given that we do not have extensive skill measures in the LEHD data, we use pre-displacement earnings as a proxy for skill.[41]

In the middle panel, for those earning less than $50,000, we find that the coefficient on the active employer network (*AEN*) is positive across all three columns of the table, but strongly

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[39] When we estimate the model excluding *AEN*, the estimated coefficients of *HR* and *ER* are positive and significant.

[40] The means of the re-employment rate for these samples, pooled across all years, are 0.59, 0.57, and 0.64 respectively.

[41] As noted earlier, a stronger network could increase the reservation wage by increasing the job offer arrival rate, implying that a worker with a strong network searches for longer. Our results generally imply that, over the quarterly time range our data cover, this reservation wage effect is not dominant. In principle, with much higher-frequency data on search duration (and perhaps intensity), as well as an hourly wage measure, one could test more directly for the reservation wage effect. It is also possible that this reservation wage effect is stronger for more-skilled workers, perhaps because leaving high-skilled jobs is costlier, and this partly explains our differences for more-skilled (higher-pay) versus less-skilled workers.

statistically significant, as well as economically large, only in the last two columns. The interquartile ranges in Columns (2) and (3) are sizable, implying increases in employment at a neighbor, respectively, of 2.24 percentage points and 3.25 percentage points.

For the high-earnings sample, as reported in the bottom panel of Table 4, the coefficient on *AEN* is actually negative in the first column, which explains why the parallel coefficient in the top panel of the table, for the full sample, was close to zero. In contrast, the coefficient estimates in Columns (2) and (3) for the high-earnings sample are both positive, although both their magnitudes and the implied interquartile effects are around half as large as for the low-income sample. These latter results may imply that, even for higher-skilled workers, when residential networks are strong they increase the probability that a displaced worker will become re-employed at a neighbor, although the importance of these network links in connecting high-earning job seekers to these employers is less important than these links are for low-earning job seekers. However, our view is that high earners are more likely to rely on networks that extend well beyond their residential neighbors (consistent with the evidence in HMN). Thus, going forward, we report results only for the full sample and the low-earnings sample.[42]

<u>Robustness to controls</u>

We have already discussed how our extensive controls, including the highly-detailed SEIN/year/quarter/county fixed effects, go a long way toward mitigating the possibility that our estimated effects of networks instead reflect sorting of workers based on unobserved factors that affect both re-employment probabilities and the network measures. Moreover, for the specifications in the final columns of Tables 3 and 4, where the outcome is re-employment at a neighbor's employer conditional on becoming re-employed anywhere, such a selection or sorting story seems even less plausible. Nonetheless, it is still possible that the coefficient estimates are biased by unobservable heterogeneity. We address this in three different ways.

First, in Table 5, we replicate the structure of Table 4, but we report results from

[42] In our tables, 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, 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. Earnings results for our baseline specification (paralleling Table 4 for employment) are available in an on-line appendix at (***).

regressions that add to the specification the control variables *ATC* and *HRT*, which, as previously described, are meant to capture other features of the local labor market for job searchers, including the potential that neighborhood networks serve to inform job searchers of the availability of local jobs generally, not just of specific jobs with neighbors' employers.

In the first column of Table 5, where the dependent variable is simply re-employment (anywhere) in the first quarter following employment, the results show that for the full sample (top panel), and especially for the low-earnings sample (bottom panel), the inclusion of the two extra tract-level control variables results in the coefficient on *AEN* becoming larger (and strongly statistically significant), suggesting that there is a net positive effect on overall employment of displaced workers arising from neighborhood networks. In Columns (2) and (3) of Table 5, the estimated coefficients for *AEN* for the full sample and for the low-earnings sample are very similar to those in Table 4. These columns provide evidence of a robust finding that neighborhood networks help displaced workers, and in particular less-skilled displaced workers, find jobs at neighbors' employers. In contrast, had the addition of the *ATC* and *HRT* controls substantially diminished the estimated effects of residence-based labor market networks, there would be a greater concern that unobservables drive our estimated effects.[43]

A second way to ask whether there is (presumably upward) bias from unobservables in Tables 3 and 4 is to test to what extent the estimated coefficients are attenuated by having already included a large set of controls, rather than by including additional controls. Such an analysis is also not decisive, of course, but evidence that the added controls tend *not* to reduce the estimated network coefficients, and the evidence (in Table 5) that the addition of *ATC* and *HRT* did not much affect the results, bolsters the argument that there are not important (and less obvious) unobservables that, if observable and included in the regression, would reduce the estimated

[43] A common approach to establishing that a result is causal is to conduct placebo tests in which similar relationships could be driven by unobservables (sorting), but not by a causal mechanism. In our case, there are not natural placebo tests. One might consider estimating models like those in Column (3) of Table 4, but for being re-employed *not* at a neighbor's employer. However, we would not expect a zero effect, but instead would just get opposite-signed estimates from those in Table 4. One might also consider evidence that the apparent network effects do not apply to displaced workers becoming re-employed in different occupations (at least occupations for which their skills do not transfer). However, we have no occupation data in the LEHD. Alternatively, we considered doing this for industry. This is not as compelling a priori since a worker's skills or lack thereof do not tie that worker to a specific industry; consistent with this, we found considerable inter-industry mobility for re-employed workers. In addition, network connections could be more important for inter-industry changes, if workers do not have as much information about labor markets in industries in which they were not working, so that there is not a clear prediction of weaker (let alone zero) network effects for cross-industry moves.

effects of *AEN*.[44]

To that end, Table 6 reports the influence on the estimates from excluding the worker controls, for both the full sample (odd-numbered columns) and the low-earnings sample (even-numbered columns). We report results for what we regard as the most rigorous evidence of network effects – the estimates paralleling the last column of Table 4, where the outcome is re-employment at a neighbor's employer conditional on re-employment. Columns (1) and (2) of the table repeat the estimates from Table 4. The key result, shown in Columns (3) and (4), is that when we drop the worker controls the estimates are very similar to those in Columns (1) and (2). These findings for the worker-level controls parallel the findings from Table 5 in suggesting that there is unlikely to be upward bias in our main estimates (Columns (1) and (2)) from remaining unobserved worker or neighborhood heterogeneity.[45]

Still, it remains possible that there is additional sorting for which we have not controlled. In particular, workers at different skill levels (different occupations, different education, etc.) – even within the same layoff – could be sorted into different residential neighborhoods, either because of income differences or different tastes for neighborhood amenities (which could be driven by income, if, say, the income elasticity of demand varies across amenities). If variation in hiring at neighbors' employers reflects demand variation across skill levels (or perhaps across jobs with different types of amenities), then this type of sorting could, in principle, generate the kind of evidence we find; in particular, controlling for overall labor demand variation might not rule out this more "occupation-specific" demand variation.

The LEHD does not contain information on occupation or education. However, we do have earnings data, and hence we explore the robustness of our estimates to expanding our fixed effects by splitting them into layoff-specific fixed effects for above- and below-median earnings workers within each establishment (actually, each SEIN/year/quarter/county) and layoff. The idea is that within an establishment, lower-paid workers are likely to be similar in skill and preferences – and presumably occupation, since we are looking within an establishment – relative to higher-paid workers.[46] The results are reported in Table 7. A comparison of the estimates with the comparable

[44] Altonji et al. (2005) formalize this argument, and Altonji and Mansfield (2014) present results from implementing this kind of approach.

[45] We also estimated the specifications in Columns (3) and (4) using the other two dependent variables – just re-employment, and re-employment at a neighbor's employer (without conditioning on re-employment). The qualitative results were similar. An on-line appendix (available at ***) also provides some estimates, including dropping the layoff-specific fixed effects, which leads to qualitatively similar (but larger) estimates of the effects of *AEN*.

[46] Note that when we focus on those with annual earnings < $50,000, this can imply a quite fine split of workers at the

ones in Table 4 reveal the estimates to be very robust – if anything, the results are slightly stronger in Table 7 than in Table 4. This bolsters the interpretation of our evidence as reflecting a causal effect of networks, rather than sorting, because even among much more homogeneous workers, variation across neighborhoods in our network measure affects re-employment (and re-employment at a neighbor's employer) – with estimates of similar magnitude.

Estimates by year

The potential impact of the Great Recession, or economic downturns more generally, on the productivity of residential networks is theoretically ambiguous. On the one hand, as in Galenianos (2014), a theoretical model of job search over the business cycle that incorporates labor market networks predicts fewer referrals when the unemployment rate is higher, therefore reducing the productivity of networks during recessions. On the other hand, when search intensity by firms in filling vacancies decreases during economic downturns (as in Davis et al., 2013), firms may rely more on referrals to fill vacancies as firms become more selective in hiring.[47] Indeed, anecdotal reports in the media during the Great Recession pointed to these contradictory theoretical predictions, sometimes claiming that network hiring became more important as the economy recovered, and other times suggesting that networks were less important during the recession, because network connections were "severed."[48] Ultimately, whether the productivity of networks changes across the business cycle is an empirical question. Therefore, in Table 8, we estimate the regression models year by year throughout our sample years of 2005-2012. We report results only for the low-earnings sample for brevity. In the top panel, we report results where the outcome is employment; in the middle panel, the outcome is re-employment at a neighbor's employer; in the bottom panel, the outcome is re-employment at a neighbor's employer conditional on re-employment. To interpret these in light of the Great Recession, the recession began in December 2007 and officially ended in June 2009. However, as is usual in recessions, the labor market lagged in the Great Recession; payroll employment did not start growing consistently until about the second quarter of 2010,[49] and the unemployment rate did not reach its peak until

establishment prior to the layoff.

[47] Davis et al. (2012) provide empirical evidence of procyclical recruiting intensity. Hershbein and Khan (2015) and Modestino et al. (2015) provide evidence that the skill requirements of vacancy postings were higher during the Great Recession.

[48] For example: http://www.nytimes.com/2013/01/28/business/employers-increasingly-rely-on-internal-referrals-in-hiring.html?_r=0; and http://usatoday30.usatoday.com/news/nation/2011-05-17-unemployment-economy-benefits_n.htm (both viewed February 27, 2017).

[49] See http://www.nber.org/cycles/cyclesmain.html (viewed June 5, 2014) and http://data.bls.gov/pdq/SurveyOutputServlet?request_action=wh&graph_name=CE_cesbref1 (viewed April 15, 2015).

October of 2009.[50]

The coefficient estimates on *AEN* in the top panel of Table 8 are not robust in sign or in magnitude. Given our findings in the earlier tables for re-employment per se, this is not particularly surprising. In the middle and bottom panels of Table 8, where we hone in on re-employment at a neighbor's employer, although the coefficient estimates on *AEN* do vary across years, they are always positive and, with one exception, always statistically significant, with interquartile ranges that generally (with the possible exception of a couple of estimates) imply an important effect of networks on re-employment at a neighbor's employer. That said, although employment rates and especially gross hiring rates clearly declined during the Great Recession, there appears to be little evidence that residential networks became less productive during the Great Recession.

Additional evidence interpreting and testing the network mechanism

In this final subsection of the paper, we present evidence that aims to better understand how the network effects we study impact labor market outcomes, in part to provide more explicit assessments of whether the empirical evidence presented thus far is best interpreted as reflecting the effects of labor market networks.[51] In particular, we ask whether the jobs laid off workers found via networks at neighbors' employers are good, long-lasting jobs, or whether they are jobs that are not particularly high-quality or persistent, but simply help bridge the displacement period (or whether network connections just lead to one job in the network versus another equivalent job). Of course network models of hiring – especially the referral model – predict that networks result in better job matches that should last longer and may pay more.

To address this question, we examine the relationship between re-employment at a neighbor's employer in the quarter after layoff and a number of outcomes, focusing on those laid off workers who were re-employed in the quarter after displacement. We begin by testing whether the re-employed workers who found jobs at a neighbor's establishment are more likely to be working 4 or 8 quarters post-displacement, and whether they have more quarters of total employment 4 or 8 quarters post-displacement (including the immediate post-displacement quarter). We explore this question only for the subsample of workers laid off in 2005 sample, so that we do not have to be concerned with the onset of the Great Recession leading jobs to end for other reasons.

[50] See http://data.bls.gov/timeseries/LNS14000000 (viewed March 26, 2015).
[51] Some of the analyses in this subsection were suggested by anonymous referees.

These estimates, reported in the top panel of Table 10, indicate that re-employed workers who became re-employed at a neighbor's employer – which we interpret as a job more likely found through the local labor market network – are more likely to still be employed 4 (or 8) quarters after displacement, by 1 (1.1) percentage points, or 1.2 (1.2) percent, based on the estimates in Columns (1) and (2) of Table 10 and the means in Table 9. Similarly, they have accumulated more quarters of experience since displacement (and re-employment) as of 4 or 8 quarters after the displacement (a little less than 1 percent, based on the estimates in Columns (3) and (4) of Table 10 and the means in Table 9). These results suggest some impact of networked jobs on overall employment post-layoff, although the effects are economically small.

More directly related to the quality of the jobs found is how long these jobs last. Hence, in the middle panel of Table 10, we examine whether jobs that re-employed workers find at neighbors' employers last longer. Specifically, the outcome is now whether the worker has worked at the same employer for 4 (8) post-displacement quarters, and then, relatedly, the number of quarters (that is, tenure) at the employer beginning with the post-displacement quarter. These estimates are all positive and statistically significant, and in this case point to sizable effects. For example, re-employed workers at a neighbor's employer are 5.7 percentage points (11.9 percent) more likely to still be at the same employer in the 4^{th} quarter after displacement. The corresponding figures in the 8^{th} quarter after displacement are 4.6 percentage points (14.9 percent). The estimates for numbers of quarters at that job are similarly in the range of a bit over 10 percent (using the means in Table 9).

The estimates in the bottom panel of Table 10 provide additional evidence that the jobs at neighbors' employers are better jobs. In this panel, we look at various earnings measures, including earnings in the quarter after displacement and total earnings since displacement. We estimate these regressions both for the level of earnings (in $1,000s), and for the inverse hyperbolic sign (IHS) of earnings – the latter leads to coefficients interpretable in the same way as for log earnings, but allows the inclusion of zero values (Andersson et al., 2016), which are important in this case. All of the estimated earnings effects are statistically significant, and the IHS estimates indicate that total earnings are higher by about 7 percent in the quarter after employment, declining a bit by the 8^{th} quarter to 4.5 percent (consistent with the possibility that re-employed workers *not* initially in jobs at their neighbors' employers do eventually move on to better jobs).[52]

VII. 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 where employed neighbors of a displaced worker themselves work, and hence can capture the effects of networks either via information passed along by employees to job searchers about job vacancies or via referrals by employees to employers about job searchers. The strength of *AEN* varies across residential neighborhoods and over time. By studying the labor market outcomes of workers who lost jobs in mass layoffs, and exploiting the detailed data – including place of work and place of residence – in the LEHD, and by considering different refinements of the measurement of re-employment (and earnings), we are able to address multiple potential threats to the identification of productive network effects on post-layoff outcomes.

We find strong evidence that this network measure increases the probability of re-employment for displaced workers when this re-employment occurs at a neighbor's employer, exactly as network theory would suggest. Moreover, this result is also maintained when we restrict our sample to only those workers who become re-employed, and estimate the impact of networks on re-employment at a neighbor's employer conditional on re-employment. We show that the results for the full sample are driven by low earners for whom local labor market networks should be more important. While we do a number of checks to confirm that we are isolating the exogenous variation in our network measure, we regard the analysis of re-employment at a neighbor's employer, conditional on re-employment, as the most compelling and, conversely, the least likely to be affected by sorting on unobservables.

In our view, the estimated effect of networks is economically significant. As an illustrative

[52] We also explored what might be considered an implication of the referral model of Montgomery (1991). In particular, the estimated effect of *AEN* should be smaller if the share of workers with very low tenure is high, because an employer may regard referrals from very new workers as unreliable because they do not yet know what leads to a good match. We tested this by reverting to our baseline model in Table 4, but adding an interaction between *AEN* and a measure of the share of employed neighbors with 1+ year of tenure (as well as the main tenure effect). (We find similar using 2+ years of tenure.) In results reported in the on-line appendix (at ***), we find consistent evidence that the positive effect of our network measure (*AEN*) on employment at a neighbor's employer is much stronger when the share of employed neighbors with more than 1 (or 2) years of tenure is higher – and indeed when the share with low tenure is very high, there is little evidence of a positive effect of *AEN*. While this evidence may further confirm a network interpretation of our findings – and in particular, an interpretation based on the referral model of networks – we do not regard it as decisive, because there is not necessarily a sharp theoretical prediction that the effects of networks are weaker for low-tenure workers; for example, low-tenure workers may know more about low-skill jobs for which more hiring is occurring, or may know more younger workers likely to be looking for jobs.

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 1.84 percentage points (relative to a mean of 12.2 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 clear evidence of changes in the productivity of labor market networks during the Great Recession. Finally, we find evidence consistent with network connections leading to better jobs: displaced workers who are re-employed at their neighbors' employers experience somewhat more persistent employment, and substantially higher earnings and higher tenure as compared to displaced workers re-employed elsewhere.

Our evidence on the importance of residence-based labor market networks in securing re-employment in stable jobs for workers displaced in mass layoffs complements a growing body of literature finding that labor market networks positively influence labor market outcomes along important dimensions. More specifically, 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. Our evidence is most clear when we examine the role of residence-based networks in generating re-employment at neighbors' employers – leading to more stable jobs and higher earnings in those jobs – rather than only faster re-employment per se. The importance of neighborhood-based networks for re-employment after mass layoffs naturally raises the question of whether these networks can continue to yield productive labor market outcomes in the long run (longer than two years), 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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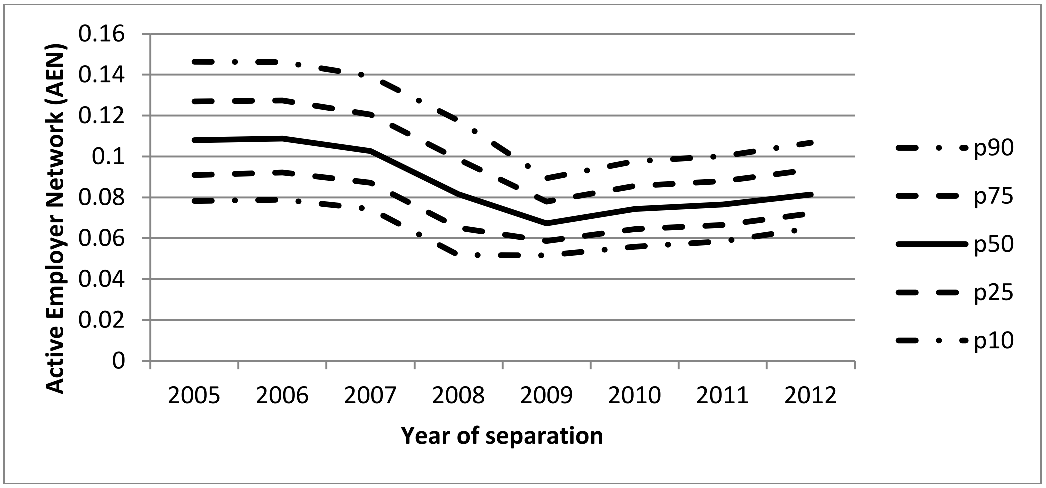
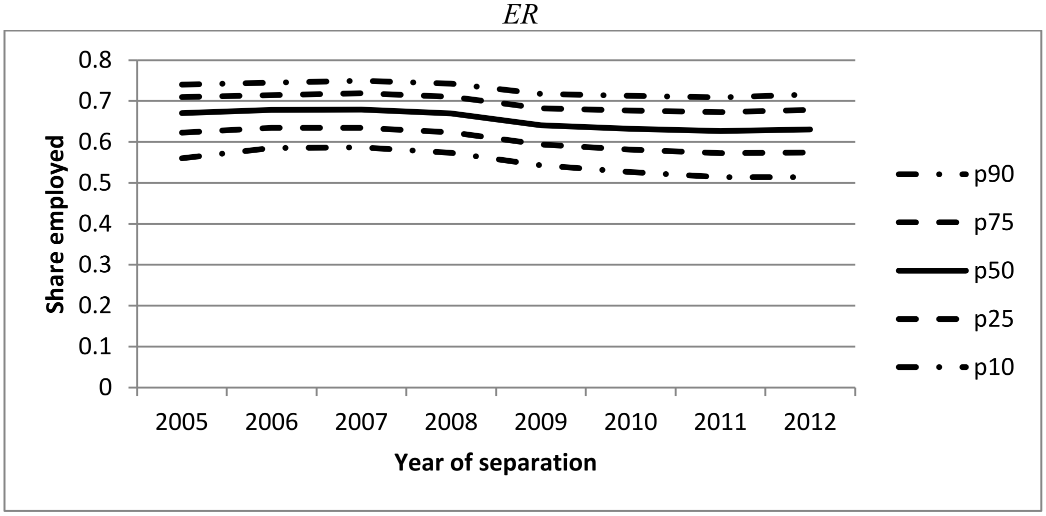
Figure 1: Percentiles of distributions of active employer network measure (*AEN*) and components (*ER, HR*), by year

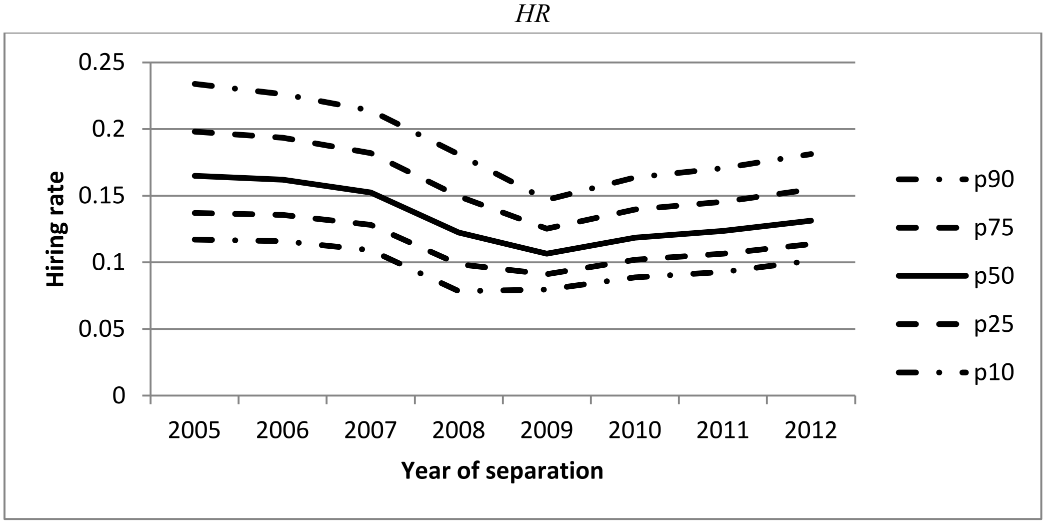
AEN



ER



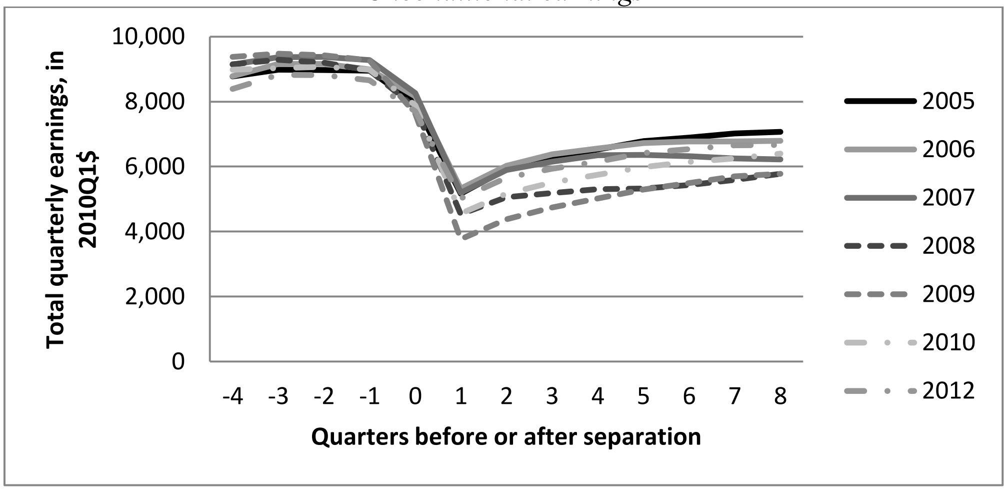
HR



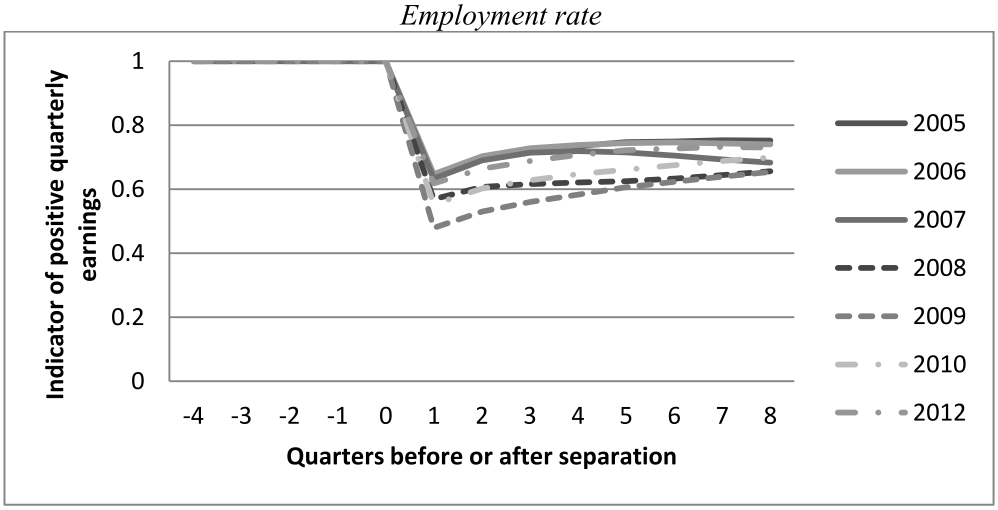
Notes: Calculations from LEHD data.

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

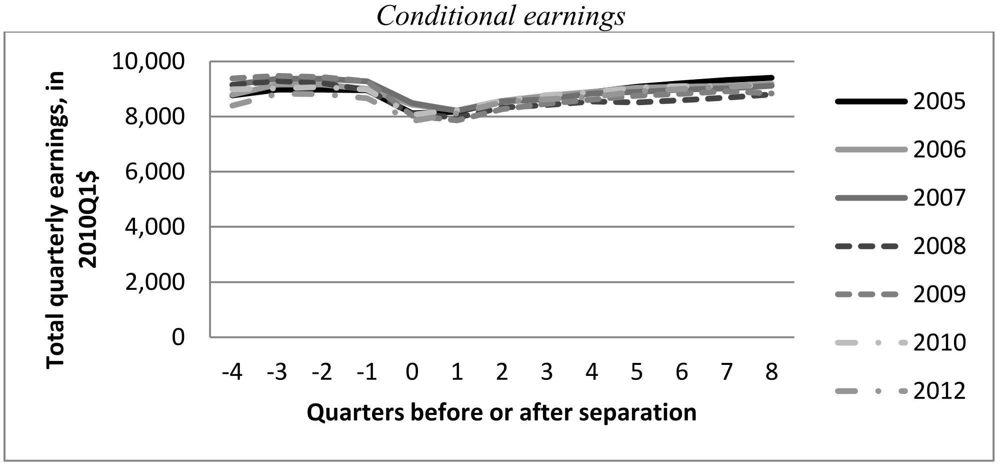
Unconditional earnings



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	0.53
Employed at a neighbor's employer	0.122	Black non-Hispanic	0.19
Employed at a neighbor's employer, conditional on re-employment	0.209	Other race non-Hispanic	0.02
Active employer network (*AEN*)	0.090	Asian non-Hispanic	0.06
Employment rate (*ER*)	0.648	Hispanic	0.20
Average gross hiring rate (*HR*)	0.140	Agriculture and mining (11,21)	0.01
Active tract network control (*ATC*)	0.077	Utility, wholesale, transportation (22,42,48-49)	0.08
Average tract gross hiring rate (*HRT*)	0.119	Construction (23)	0.10
Share employed (layoff quarter)	0.65	Manufacturing (31-33)	0.12
Share in poverty rate in tract (2000)	0.13	Retail, administrative, other services (44-45,56,81)	0.26
Share in same house last year (2000)	0.51	Professional services (51-55)	0.20
Share foreign born (2000)	0.16	Education, health, public (61,62,92)	0.13
Share less than high school (2000)	0.21	Local services (71,72)	0.11
Share some college (2000)	0.28	Displaced in 2005	0.12
Share college or more (2000)	0.25	Displaced in 2006	0.12
Share white, not Hispanic (2000)	0.59	Displaced in 2007	0.14
Share black, not Hispanic (2000)	0.16	Displaced in 2008	0.18
Earnings at employer in previous year (1,000s 2010Q1$)	34.87	Displaced in 2009	0.16
Earnings from other jobs in previous year (1,000s 2010Q1$)	1.46	Displaced in 2010	0.11
Age 19 to 24	0.14	Displaced in 2010	0.10
Age 25 to 34	0.30	Displaced in 2012	0.08
Age 35 to 44	0.23	Displaced in quarter 1	0.23
Age 45 to 54	0.20	Displaced in quarter 2	0.26
Age 55 to 64	0.13	Displaced in quarter 3	0.26
Female	0.46	Displaced in quarter 4	0.25
Male	0.54		

Notes: Observations (1,000s): 9,195 for all job searchers, and 5,377 conditional on re-employment in the quarter after displacement. 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 and control variables on employment outcomes in quarter following displacement, 2005-2012

	Employed	Employed at a neighbor's employer	Employed at a neighbor's employer, conditional on re-employment
	(1)	(2)	(3)
Active employer network (*AEN*)	**0.022**	**0.513*****	**0.693*****
Employment rate (*ER*)	0.270***	0.100***	0.106***
Average gross hiring ratio (*HR*)	0.128***	-0.126***	-0.151**
Share in poverty rate in tract (2000)	0.016***	-0.038***	-0.074***
Share in same house last year (2000)	-0.036***	-0.041***	-0.057***
Share foreign born (2000)	-0.002	-0.021***	-0.037***
Share less than high school (2000)	-0.022***	0.005	0.016**
Share some college (2000)	0.024***	-0.008**	-0.027***
Share college or more (2000)	-0.020***	-0.005*	0.001
Share white, not Hispanic (2000)	0.008***	-0.009***	-0.020***
Share black, not Hispanic (2000)	-0.005***	-0.007***	-0.010***
Earnings ($1,000s) at employer in previous yr.	0.002***	0.000***	-0.001***
Earnings ($1,000s) from other jobs in previous yr.	0.016***	-0.000***	-0.004***
Age 19 to 24	0.087***	0.022***	0.004***
Age 25 to 34	0.040***	0.006***	-0.004***
Age 45 to 54	-0.041***	-0.010***	-0.001
Age 55 to 64	-0.144***	-0.036***	-0.009***
Female	-0.009***	0.001***	0.007***
Black non-Hispanic	-0.011***	0.006***	0.014***
Other race non-Hispanic	-0.009***	0.001	0.005***
Asian non-Hispanic	-0.017***	0.004***	0.013***
Hispanic	-0.003***	0.009***	0.015***
Constant term	0.306***	0.061***	0.183***
Interquartile effects			
Active employer network (*AEN*)	**0.0008**	**0.0184**	**0.0255**
Employment rate (*ER*)	0.0254	0.0095	0.0097
Average gross hiring rate (*HR*)	0.0071	-0.0070	-0.0085
SEIN/year/quarter/county fixed effects	Yes	Yes	Yes
Number of fixed effects included (1,000s)	2,072	2,072	1,611
R-squared (within)	0.048	0.004	0.005
Observations (1,000s)	9,195	9,195	5,377
Mean of dependent variable	0.585	0.122	0.209

Notes: Employment estimates are from linear probability model for an indicator of employment. All specifications include industry dummy variables (using the categories from Table 1), which can vary within SEIN/year/quarter/county fixed effects for some multiple-establishment firms in more than one industry; the estimated coefficients were very small and generally insignificant, and are not reported. The omitted indicators for the variables reported in the table are for age 35 to 44, male, and white non-Hispanic. The share variables are proportions. The interquartile effects are computed using the percentiles of the distributions for the sample used in the corresponding regression. Robust standard errors, clustered by SEIN/year/quarter/county, are computed. Standard errors are not reported here, given that nearly all of the estimated coefficients are statistically significant at the one-percent level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effects of networks on employment outcomes in quarter following displacement, 2005-2012

	Employed	Employed at a neighbor's employer	Employed at a neighbor's employer, conditional on re-employment
	(1)	(2)	(3)
Full sample			
Active employer network (*AEN*)	**0.022**	**0.513*****	**0.693*****
	(0.070)	**(0.060)**	**(0.110)**
Employment rate (*ER*)	0.270***	0.100***	0.106***
	(0.011)	(0.009)	(0.017)
Average gross hiring rate (*HR*)	0.128***	-0.126***	-0.151**
	(0.045)	(0.031)	(0.059)
Interquartile effects			
Active employer network (*AEN*)	**0.0008**	**0.0184**	**0.0255**
Employment rate (*ER*)	0.0254	0.0095	0.0097
Average gross hiring rate (*HR*)	0.0071	-0.0070	-0.0085
Number of fixed effects included (1,000s)	2,072	2,072	1,611
R-squared (within)	0.048	0.004	0.005
Observations (1,000s)	9,195	9,195	5,377
Low-earnings sample (pre-displacement earnings < $50,000)			
Active employer network (*AEN*)	**0.133***	**0.609*****	**0.856*****
	(0.070)	**(0.049)**	**(0.096)**
Employment rate (*ER*)	0.235***	0.090***	0.096***
	(0.012)	(0.008)	(0.015)
Average gross hiring rate (*HR*)	0.074*	-0.178***	-0.248***
	(0.044)	(0.026)	(0.053)
Interquartile effects			
Active employer network (*AEN*)	**0.0049**	**0.0224**	**0.0325**
Employment rate (*ER*)	0.0228	0.0087	0.0091
Average gross hiring rate (*HR*)	0.0042	-0.0102	-0.0144
Number of fixed effects included (1,000s)	1,813	1,813	1,358
R-squared (within)	0.049	0.004	0.006
Observations (1,000s)	7,025	7,025	3,983
High-earnings sample (pre-displacement earnings ≥ $50,000)			
Active employer network (*AEN*)	**-0.305****	**0.319****	**0.472***
	(0.129)	**(0.156)**	**(0.277)**
Employment rate (*ER*)	0.282***	0.120***	0.134***
	(0.019)	(0.020)	(0.036)
Average gross hiring rate (*HR*)	0.235***	-0.066	-0.082
	(0.085)	(0.082)	(0.151)
Interquartile effects			
Active employer network (*AEN*)	**-0.0098**	**0.0103**	**0.0154**
Employment rate (*ER*)	0.0232	0.0099	0.0109
Average gross hiring rate (*HR*)	0.0111	-0.0031	-0.0039
Number of fixed effects included (1,000s)	756	756	579
R-squared (within)	0.047	0.004	0.006
Observations (1,000s)	2,170	2,170	1,394

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 interquartile effects are computed using the percentiles of the distributions for the sample used in the corresponding regression. Robust standard errors in parentheses, clustered by SEIN/year/quarter/county. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effects of networks on employment outcomes in quarter following displacement, with tract hiring/network controls added, 2005-2012

	Employed	Employed at a neighbor's employer	Employed at a neighbor's employer, conditional on re-employment
	(1)	(2)	(3)
Full sample			
Active employer network (*AEN*)	**0.241*** **	**0.442*** **	**0.695*** **
	(0.063)	**(0.078)**	**(0.154)**
Employment rate (*ER*)	0.339***	0.116***	0.140***
	(0.011)	(0.010)	(0.018)
Average gross hiring rate (*HR*)	-0.046	-0.144***	-0.246***
	(0.039)	(0.040)	(0.084)
Active tract network control (*ATC*)	-0.835***	-0.009	-0.236
	(0.111)	(0.106)	(0.205)
Average tract gross hiring rate (*HRT*)	0.701***	0.291***	0.566***
	(0.071)	(0.066)	(0.130)
Interquartile effects			
Active employer network (*AEN*)	**0.0086**	**0.0158**	**0.0255**
Employment rate (*ER*)	0.0319	-0.0003	0.0128
Average gross hiring rate (*HR*)	-0.0025	-0.0080	-0.0138
R-squared (within)	0.048	0.004	0.005
Low-earnings sample (pre-displacement earnings < $50,000)			
Active employer network (*AEN*)	**0.373*** **	**0.536*** **	**0.832*** **
	(0.065)	**(0.063)**	**(0.128)**
Employment rate (*ER*)	0.312***	0.108***	0.129***
	(0.013)	(0.010)	(0.018)
Average gross hiring rate (*HR*)	-0.115***	-0.200***	-0.336***
	(0.039)	(0.033)	(0.070)
Active tract network control (*ATC*)	-0.932***	-0.024	-0.187
	(0.122)	(0.099)	(0.191)
Average tract gross hiring rate (*HRT*)	0.778***	0.327***	0.584***
	(0.077)	(0.063)	(0.122)
Interquartile effects			
Active employer network (*AEN*)	**0.0137**	**0.0197**	**0.0316**
Employment rate (*ER*)	0.0302	0.0104	0.0121
Average gross hiring rate (*HR*)	-0.0066	-0.0114	-0.0195
R-squared (within)	0.049	0.004	0.006

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. Numbers of observations and fixed effects are the same as in Table 4, for the corresponding samples. The interquartile effects are computed using the percentiles of the distributions for the sample used in the corresponding regression. Robust standard errors in parentheses, clustered by SEIN/year/quarter/county. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Effects of network on employment at a neighbor's employer in quarter following displacement, conditional on re-employment, pooled years 2005-2011, different controls

	All controls (Table 4)		Excluding worker-level controls	
	Full sample	Pre-displacement earnings < $50,000	Full sample	Pre-displacement earnings < $50,000
	(1)	(2)	(3)	(4)
Active employer network (*AEN*)	**0.693***** **(0.110)**	**0.856***** **(0.096)**	**0.750***** **(0.108)**	**0.870***** **(0.095)**
Employment rate (*ER*)	0.106*** (0.017)	0.096*** (0.015)	0.090*** (0.016)	0.085*** (0.015)
Average gross hiring rate (*HR*)	-0.151** (0.059)	-0.248*** (0.053)	-0.175*** (0.058)	-0.252*** (0.053)
Interquartile effects				
Active employer network (*AEN*)	**0.0255**	**0.0325**	**0.0275**	**0.0330**
Employment rate (*ER*)	0.0097	0.0091	0.0082	0.0080
Average gross hiring rate (*HR*)	-0.0085	-0.0144	-0.0098	-0.0147
R-squared (within)	0.005	0.006	0.001	0.001

Notes: Employment estimates are from linear probability model for an indicator of employment. The specifications include the same controls as in other tables, except that in Columns (3) and (4) the worker-level neighborhood controls listed in Table 3 are omitted; these include the demographic and earnings controls and the Census tract controls. Numbers of observations and fixed effects are the same as in Table 4, for the corresponding samples. The interquartile effects are computed using the percentiles of the distributions for the sample used in the corresponding regression. Robust standard errors in parentheses, clustered by SEIN/year/quarter/county. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Effects of networks on employment outcomes in quarter following displacement, with separate layoff-specific fixed effects for above- and below-median earnings workers, 2005-2012

	Employed	Employed at a neighbor's employer	Employed at a neighbor's employer, conditional on re-employment
	(1)	(2)	(3)
		Full sample	
Active employer network (*AEN*)	**0.017**	**0.533*****	**0.732*****
	(0.062)	**(0.052)**	**(0.101)**
Employment rate (*ER*)	0.257***	0.094***	0.099***
	(0.010)	(0.008)	(0.015)
Average gross hiring rate (*HR*)	0.119***	-0.144***	-0.180***
	(0.039)	(0.027)	(0.054)
Interquartile effects			
Active employer network (*AEN*)	0.0006	0.0191	0.0269
Employment rate (*ER*)	0.0243	0.0089	0.0091
Average gross hiring rate (*HR*)	0.0066	-0.0080	-0.0101
Number of fixed effects included (1,000s)	3,292	3,292	2,354
R-squared (within)	0.046	0.003	0.005
Observations (1,000s)	9,195	9,195	5,377
		Low-earnings sample (pre-displacement earnings < $50,000)	
Active employer network (*AEN*)	**0.139****	**0.637*****	**0.923*****
	(0.066)	**(0.048)**	**(0.096)**
Employment rate (*ER*)	0.225***	0.084***	0.086***
	(0.011)	(0.008)	(0.015)
Average gross hiring rate (*HR*)	0.062	-0.197***	-0.292***
	(0.041)	(0.025)	(0.052)
Interquartile effects			
Active employer network (*AEN*)	0.0051	0.0234	0.0351
Employment rate (*ER*)	0.02189	0.0081	0.0081
Average gross hiring rate (*HR*)	0.0035	0.0234	-0.0170
Number of fixed effects included (1,000s)	2,741	2,741	1,894
R-squared (within)	0.048	0.004	0.005
Observations (1,000s)	7,025	7,025	3,983

Notes: Employment estimates are from linear probability model for an indicator of employment. All specifications include fixed effects defined to be unique for each SEIN/year/quarter/county as well as whether the worker's earnings were above or below median earnings for that cell, and the worker control variables and Census tract control variables listed in Table 3. The interquartile effects are computed using the percentiles of the distributions for the sample used in the corresponding regression. Robust standard errors in parentheses, clustered by SEIN/year/quarter/county and above or below median. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Estimated effect of network measures and control variables on employment outcomes in quarter following displacement, low-earnings sample (pre-displacement earnings < $50,000), by year

Displacement years	2005-2012	2005	2006	2007	2008	2009	2010	2011	2012
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Employed				
Active employer network (*AEN*)	**0.133***	**0.393*****	**-0.102**	**-0.264**	**0.041**	**0.027**	**-0.012**	**0.314**	**0.296**
	(0.070)	**(0.099)**	**(0.159)**	**(0.163)**	**(0.155)**	**(0.212)**	**(0.211)**	**(0.230)**	**(0.242)**
Employment rate (*ER*)	0.235***	0.188***	0.262***	0.282***	0.240***	0.255***	0.236***	0.219***	0.231***
	(0.012)	(0.022)	(0.032)	(0.031)	(0.024)	(0.028)	(0.032)	(0.034)	(0.038)
Average gross hiring rate (*HR*)	0.074*	-0.099*	0.197**	0.307***	0.151	0.175	0.111	-0.002	0.015
	(0.044)	(0.057)	(0.099)	(0.104)	(0.099)	(0.125)	(0.122)	(0.133)	(0.137)
Interquartile effects									
Active employer network (*AEN*)	**0.0049**	**0.0143**	**-0.0036**	**-0.0089**	**0.0014**	**0.0005**	**-0.0003**	**0.0068**	**0.0065**
Employment rate (*ER*)	0.0228	0.0169	0.0214	0.0241	0.0213	0.0232	0.0229	0.0225	0.0245
Average gross hiring rate (*HR*)	0.0042	-0.0062	0.0115	0.0170	0.0079	0.0061	0.0043	-0.0001	0.0006
Observations (1,000s)	7,025	873	835	942	1,237	1,125	746	730	536
					Employed at a neighbor's employer				
Active employer network (*AEN*)	**0.609*****	**0.471*****	**0.563*****	**0.259***	**0.883*****	**0.503*****	**0.733*****	**0.634*****	**1.051*****
	(0.049)	**(0.060)**	**(0.121)**	**(0.137)**	**(0.246)**	**(0.137)**	**(0.153)**	**(0.167)**	**(0.227)**
Employment rate (*ER*)	0.090***	0.112***	0.108***	0.177***	0.064**	0.091***	0.048**	0.098***	0.071**
	(0.008)	(0.015)	(0.026)	(0.027)	(0.032)	(0.018)	(0.023)	(0.025)	(0.033)
Average gross hiring rate (*HR*)	-0.178***	-0.143***	-0.207***	0.058	-0.346***	-0.090	-0.208**	-0.112	-0.263**
	(0.026)	(0.028)	(0.074)	(0.086)	(0.129)	(0.080)	(0.089)	(0.097)	(0.124)
Interquartile effects									
Active employer network (*AEN*)	**0.0224**	**0.0172**	**0.0200**	**0.0088**	**0.0299**	**0.0098**	**0.0158**	**0.0138**	**0.0230**
Employment rate (*ER*)	0.0087	0.0101	0.0088	0.0151	0.0057	0.0083	0.0047	0.0101	0.0075
Average gross hiring rate (*HR*)	-0.0102	-0.0089	-0.0122	0.0032	-0.0180	-0.0032	-0.0081	-0.0045	-0.0111
Observations (1,000s)	7,025	873	835	942	1,237	1,125	746	730	536
					Employed at a neighbor's employer, conditional on employment				
Active employer network (*AEN*)	**0.856*****	**0.790*****	**0.809*****	**0.347**	**1.308*****	**0.870*****	**1.166*****	**0.722****	**1.502*****
	(0.096)	**(0.126)**	**(0.201)**	**(0.215)**	**(0.441)**	**(0.318)**	**(0.289)**	**(0.302)**	**(0.393)**
Employment rate (*ER*)	0.096***	0.090***	0.099**	0.232***	0.053	0.119***	0.046	0.138***	0.050
	(0.015)	(0.029)	(0.043)	(0.039)	(0.059)	(0.042)	(0.044)	(0.045)	(0.058)
Average gross hiring rate (*HR*)	-0.248***	-0.271***	-0.339***	0.081	-0.530**	-0.129	-0.262	-0.003	-0.364
	(0.053)	(0.070)	(0.125)	(0.135)	(0.236)	(0.189)	(0.173)	(0.176)	(0.223)
Interquartile effects									
Active employer network (*AEN*)	**0.0325**	**0.0286**	**0.0287**	**0.0119**	**0.0439**	**0.0173**	**0.0253**	**0.0158**	**0.0335**
Employment rate (*ER*)	0.0091	0.0077	0.0079	0.0195	0.0046	0.0107	0.0044	0.0138	0.0052
Average gross hiring rate (*HR*)	-0.0144	-0.0167	-0.0199	0.0045	-0.0274	-0.0046	-0.0101	-0.0001	-0.0154
Observations (1,000s)	3,983	539	529	583	679	515	398	418	323

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 interquartile effects are computed using the percentiles of the distributions for the sample used in the corresponding regression. Robust standard errors in parentheses, clustered by SEIN/year/quarter/county. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Sample means for longer-run outcomes, 2005 displacements (low-earnings sample--pre-displacement earnings < $50,000)

Variable	Unconditional	Conditional on re-employment in quarter following displacement
AEN	0.113	0.115
ER	0.652	0.658
HR	0.177	0.177
Outcomes in Table 10, top panel		
Employed in 4th quarter post-displacement	0.722	0.894
Employed in 8th quarter post-displacement	0.739	0.865
Quarters of employment since displacement, 4 quarters post-displacement	2.728	3.733
Quarters of employment since displacement, 8 quarters post-displacement	5.677	7.232
Outcomes in Table 10, middle panel		
At employer through 4th quarter post-displacement	0.295	0.479
At employer through 8th quarter post-displacement	0.190	0.308
Quarters at employer, 4 quarters post-displacement	1.600	2.594
Quarters at employer, 4 quarters post-displacement	2.489	4.035
Outcomes in Table 10, bottom panel		
Total earnings ($1,000) in quarter after displacement	3.767	6.108
Total earnings since displacement, 4 quarters post-displacement	17.838	25.601
Total earnings since displacement, 8 quarters post-displacement	39.172	52.508
Total earnings (IHS) 1 quarter after displacement	5.582	9.050
Total earnings (IHS) since displacement, 4 quarters post-displacement	8.340	10.562
Total earnings (IHS) since displacement, 8 quarters post-displacement	9.628	11.287

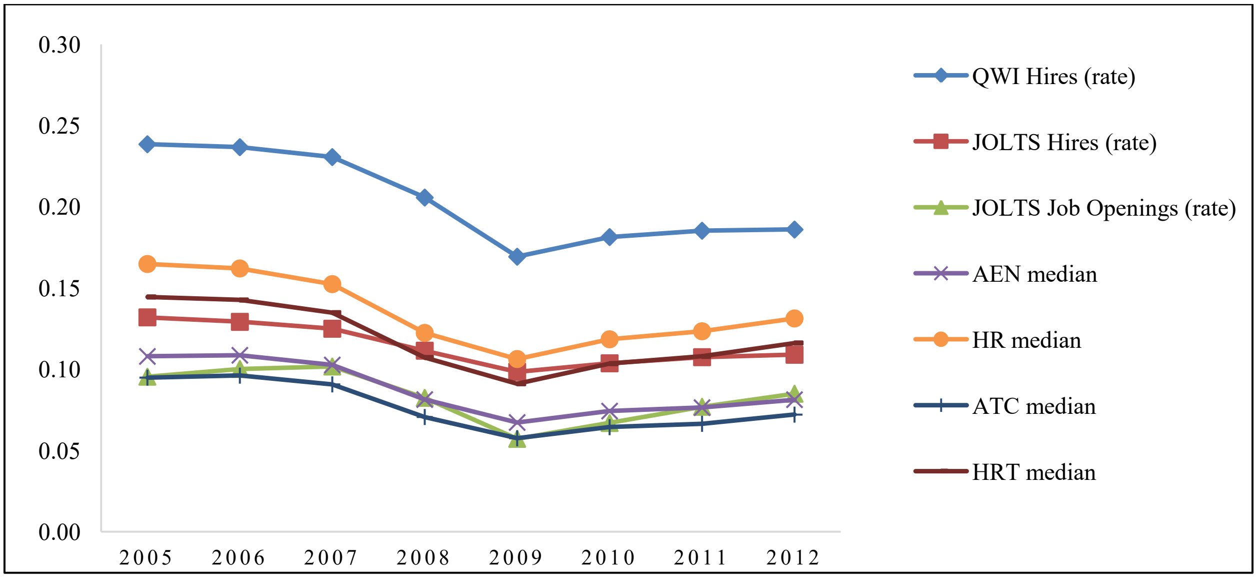
Notes: For numbers of observations, see Table 10.

Table 10: Effects of networks on longer-term employment outcomes following displacement, estimated effect of being re-employed at neighbor in quarter following displacement, conditional on re-employment in quarter following displacement, on longer-term outcomes, low-earnings sample (pre-displacement earnings < $50,000), 2005 displacements

			Employment outcomes			
Outcome	Employed in 4th quarter post-displacement	Employed in 8th quarter post-displacement	Quarters of employment since displacement, 4 quarters post-displacement	Quarters of employment since displacement, 8 quarters post-displacement		
	(1)	(2)	(3)	(4)	(5)	(6)
Re-employed at neighbor	0.011***	0.010***	0.030***	0.069***		
	(0.001)	(0.001)	(0.003)	(0.006)		
			Job-related tenure outcomes			
Outcome	At employer through 4th quarter post-displacement (all quarters)	At employer through 8th quarter post-displacement (all quarters)	Quarters at employer, 4 quarters post-displacement	Quarters at employer, 8 quarters post-displacement		
Re-employed at neighbor	0.057***	0.046***	0.280***	0.479***		
	(0.002)	(0.002)	(0.007)	(0.015)		
			Earnings-related outcomes			
Outcome	Total earnings ($1,000) 1 quarter after displacement	Total earnings ($1,000) since displacement, 4 quarters post-displacement	Total earnings ($1,000) since displacement, 8 quarters post-displacement	Total earnings (IHS) 1 quarter after displacement	Total earnings (IHS) since displacement, 4 quarters post-displacement	Total earnings (IHS) since displacement, 8 quarters post-displacement
Re-employed at neighbor	0.147***	0.568***	0.929***	0.066***	0.053***	0.045***
	(0.015)	(0.061)	(0.121)	(0.004)	(0.003)	(0.003)

Notes: There are approximately 539,000 observations for every estimation. Employment and "at employer" 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, as well as the employment rate (*ER*) and the average gross hiring rate (*HR*) controls. The earnings units for earnings specifications labeled IHS are based on the inverse hyperbolic sine function, which permits the inclusion of zeros but is still interpretable as percentage effects, like the log specification. Robust standard errors in parentheses, clustered by SEIN/year/quarter/county. *** p<0.01, ** p<0.05, * p<0.1.

Figure A1: Dynamics of Hires and Vacancies



Notes: All values are annual averages across quarterly statistics. Quarterly Workforce Indicators (QWI) and Job Openings and Labor Turnover Survey (JOLTS) values are derived from public-use statistics. Network-related measures are calculated from LEHD data. The QWI Hires rate for private, U.S. employers is the quarterly ratio of all accessions in a quarter (*HirA*) to the count of jobs at the beginning of each quarter (*Emp*). The JOLTS Hires rate for private, U.S. employers (JTU10000000HIR) is the monthly ratio of new hires in a month to employment on the 12th of the month. The JOLTS Job Openings rate for private, U.S. employers (JTU10000000JOR) is the monthly ratio of active job openings in a month to employment on the 12th of the month.

Table A1: Sample composition by year

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 notes for NAICS industry code ranges.

Table A2: Network measure percentiles

Network measure	p10	p25	p50	p75	p90
Active employer network (*AEN*)	0.058	0.070	0.085	0.105	0.127
Employment rate (*ER*)	0.550	0.606	0.657	0.700	0.734
Average gross hiring rate (*HR*)	0.091	0.108	0.132	0.163	0.198
Active tract network control (*ATC*)	0.049	0.060	0.073	0.092	0.110
Average tract gross hiring rate (*HRT*)	0.080	0.095	0.114	0.139	0.166

Notes: Calculations from LEHD data.