Labor Market Networks and Recovery from Mass Layoffs

Before, During, and After the Great Recession*

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Abstract

We test the effects of labor market networks defined by residential neighborhoods on re-employment following mass layoffs. We develop two measures of labor market network strength. One captures the flows of information to job seekers about the availability of job vacancies at employers of workers in the network, and the other captures referrals provided to employers by other network members. These network measures are linked to more rapid re-employment following mass layoffs, and to re-employment at neighbors' employers. We also find evidence that network connections – especially those that provide information about job vacancies – became less productive during the Great Recession.

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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 job displacement of this type 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). As a result, it is important to identify factors that might 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 (HNM) and 2014 (HKN)). In particular, we test the hypothesis that the labor market networks formed by residential neighbors help in the labor market recovery of displaced workers.

We examine two types of questions about labor market networks. First, we explore differences in the effects of network strength on the employment recovery of displaced workers in the period 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.[1,2] It is not clear that economic theory makes any strong prediction about the answer to this question. But the press was replete with anecdotal evidence (and advice) on the importance of network connections in finding jobs during the Great Recession.[3] Of course such anecdotes prove nothing. Networks may be more productive for displaced workers when the economy is not in recession (or its aftermath), but the media are less likely to focus on how workers found jobs in such periods. Moreover, media stories tell a contradictory story, sometimes claiming that network hiring has become more important as the economy has recovered, while suggesting that networks were less important

[1] For the purposes of this paper we treat Great Recession period as extending into 2010 when the recession had formally ended but unemployment was still extremely high.
[2] As we make clear below, our measure of network strength not only captures how many members are in a displaced worker's network, but also the potential connections these members have to existing vacancies.
[3] For example:
http://money.cnn.com/2009/03/27/news/economy/yang_jobhunters.fortune/index.htm?utm_source=feedburner&utm_medium=feed&utm_campaign=Feed%3A+rss%2Fmoney_latest+(Latest+News);
http://abcnews.go.com/Business/jobs-outlook-college-graduates/story?id=16345862;
http://www.jibberjobber.com/blog/2008/10/07/how-to-find-a-job-in-a-recession/ (all viewed May 30, 2014).

during the recession, because network connections were "severed."[4]

Second, we use the context of job displacement to garner evidence on the two main theoretical frameworks in which labor market networks have been analyzed. The first emphasizes the role of networks in providing information about job vacancies to job searchers (Calvó-Armengol and Jackson, 2007; Ioannides and Soetevent, 2006). The second emphasizes the role of networks in providing information about workers to firms (Montgomery, 1991). We construct measures of residential labor market network strength intended to capture these two different dimensions of labor market networks, and estimate and compare the effects of both of them on recovery from job displacement before, during, and coming out of the Great Recession.

In the network literature, 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 generally) and with sorting into networks. In our view, we get particularly compelling evidence on the roles of labor market networks for three reasons. First, we study workers who lost jobs because of mass layoffs, which are quite likely exogenous with respect to other characteristics of workers that might be correlated with network measures. Second, we 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 in which laid off workers live. This within-mass layoff variation in network strength is very unlikely to be correlated with unobserved determinants of re-employment probabilities of the workers themselves. And third, we observe whether re-employment occurred at the employer of a neighbor, even conditional on re-employment, which further rules out a role for unobservables that both determine re-employment and are correlated with the sorting of workers across neighborhoods with varying network strength.

To briefly summarize our evidence, we find that our first measure of residence-based labor market network strength – which is intended to capture the flows of information to job seekers about the availability of vacancies at neighbors' employers – is linked to more rapid re-employment generally, and

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

especially in re-employment at neighbors' employers. These effects are substantially larger for low earners than for high earners. However, it appears that this type of network connection proved to be less useful in the recovery of employment during the Great Recession, when re-employment rates were lower (and hence earnings losses larger). The evidence of the importance to re-employment of our second measure of residence-based network strength – which is intended to capture referrals provided by neighbors to hiring employers – is somewhat less robust. Nonetheless, we find that this measure consistently aids in the re-employment of displaced workers at the workplaces of neighbors, again more so for low earners.

Motivation and Previous Research

Standard approaches to the search behavior of unemployed individuals (e.g., Ham and Rhea, 1987) generally model the probability that an unemployed worker becomes re-employed as a function of the unemployment rate, the vacancy rate, the worker's reservation wage, and the worker's preferences for non-work activity. 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. These 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 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, where employers do not have full information about the quality of job applicants or the job match that would arise if the applicant were hired. Specifically, in Montgomery (1991), firms learn about a potential worker's ability if the firm employs individuals from the potential worker's network. In equilibrium, individuals are more likely to receive and accept wage offers from businesses that employ others in their network, creating stratification across employers on the basis of these networks.[5]

These two classes of models both imply that an unemployed individual will have better labor market outcomes if he or she searches for work in a local labor market (or markets) with a high vacancy rate(s) and a low unemployment rate(s), and if he or she has many network contacts that can pass along information on specific job vacancies to the unemployed individual, as well as network contacts that can potentially pass along information to an employer about the productivity of the unemployed individual. 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 strength and nature of labor market networks, the size and scope of local labor markets, characteristics of individuals that affect their reservation wage, and the availability and accessibility of job vacancies.

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, but which does not identify with whom workers are networked (Ioannides and Datcher Loury, 2004). However, 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 alike, but are less likely to be networked.)

Taking this further, HNM test whether neighbors are more likely to work at the same business establishment, consistent with the hypothesis that labor market networks mitigate employers' lack of information about workers or that these networks provide job searchers with information on vacancies at

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

those businesses. We provide a bit of detail on the measure of labor market networks used in HNM to help understand the measures used in this paper – one of which is closely related.

The idea in HNM is to capture 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. 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. To construct the network measure, HNM first identify all establishments within each Census tract, and the workers in these establishments. They then compute for each worker in the sample the percentage of his or her co-workers who come from the same residential neighborhood. For worker *i* this observed network index is:

$$NI_i = \frac{\sum_{j \neq i} I^R(i,j) \cdot I^E(i,j)}{\sum_{j \neq i} I^E(i,j)},$$

where $I^R(i, j)$ is an indicator for whether co-worker *j* of worker *i* also lives in the same residential neighborhood as *i*, and $I^E(i, j)$ is an indicator for whether *i* and *j* work in the same establishment. The sums in the numerator and denominator are taken over all workers (other than the worker *i*) who work in worker *i*'s establishment. Their ratio is the share of co-workers with whom each worker is co-resident. The index requires a definition of residential neighbors; Census tracts are used, as the tract is a reasonable definition of a neighborhood in which co-residents are likely to interact, more so because most Census tracts are relatively small, facilitating contact at schools, churches, community organizations, etc. The observed network index for an individual, NI_i, is averaged over all workers in the establishment, to characterize the level of residential network connectedness that characterizes each employer.[6]

As described in HNM, two other adjustments are made to this index. The first corrects for the fact that some clustering of residential neighbors by establishment can occur even if workers are assigned

[6] As explained below, in the present paper one of our network measures (*ADE*) is a function of this network index aggregated at the level of the residential neighborhood, intended to characterize the extent to which neighbors are clustered at the employers who are hiring.

randomly to establishments, because people often work close to home or near transportation infrastructure that connects to their place of residence. Second, there is a maximum value that *NI* can take on, which can only be approximated, but which then permits the calculation of how much clustering of neighbors in the same establishments occurs relative to the maximum amount of clustering that could occur, so that the network measure is ultimately reported in percentage terms.

HNM calculate the network isolation measures using the 2000 DEED, a dataset that matches workers reporting to the 2000 Decennial Census Long Form to administrative information on establishments at the U.S. Census Bureau. The results indicate that community-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, likely because residence-based networks are bound to be more important for local than national labor markets, and labor markets for less-skilled workers are more local. Residence-based labor market networks are even more important for Hispanics than for whites, and especially for Hispanic immigrants and those with poor language skills, likely because informal labor market networks are particularly important for workers who are not as well-integrated into the labor market, and for whom employers may have less reliable information.[7]

Although the Bayer et al. (2008) and HNM papers are similar in focusing on residence-based networks, one key difference is that the measure that Bayer et al. implicitly use may mainly captures information about jobs in the locations where one's neighbors work, whereas the HNM measure captures connections between neighbors who work at the same business establishment. The "clustering" of neighbors in work locations or business establishments that both papers document could reflect something similar – specifically the transmission to job searchers of information about job vacancies either near where employed neighbors work or specifically at neighbors' employers. However, the HNM measure, by linking

[7] The paper also tries to rule out alternative explanations of the findings. Neighbors may tend to work in the same establishments because they share the same skills, reflecting residential sorting. But HNM get similar values for *NI* when looking *within* schooling or occupation groups, focusing only on the network connections between those at a particular schooling level or in a particular occupation. They also rule out reverse causality, where co-workers choose to move to the same residential neighborhoods. When restricting attention to residents who have *not* moved in the past five years who work in establishments that are fewer than five years old – for whom residential location was determined prior to workplace location – the analysis yields very similar results.

neighbors to employment at the same establishment, may also or instead reflect network connections associated with referrals of neighbors to one's employer – which is why neighbors end up working in the same establishment.

In general it is hard to distinguish empirically between these two channels of network connections, because explicit referrals are generally unobserved. One exception is a recent paper by Brown et al. (2014) that uses a company-level dataset with explicit information on which job candidates were referred by an employee, and finds important referral effects on early wages and on job tenure, especially for workers at lower skill levels (in particular, support staff).

These findings are related to our most recent work (HKN), in which we first revisit the strength of residential networks using data on essentially the universe of workers from a different data set, and also the one we will use in this paper – the Longitudinal Employer Household Dynamics (LEHD) database – finding similar evidence that workers are substantially more likely to work at the same employer as their residential neighbors than what would be expected by random assignment of workers to an employer within a Census tract.[8] More important, paralleling Brown et al. (although without explicit information on referrals), we test theoretical predictions about the effects of labor market networks that act by providing employers with information about the quality of job applicants (Simon and Warner, 1992; Dustmann et al., 2011). If networks facilitate good job matches, wages should be higher and turnover should be lower (particularly at low levels of tenure) for workers who are better networked at the time they are hired. We find robust evidence that workers who are more residentially networked to their co-workers at the time of hire have lower rates of turnover.[9] When we use wages as an outcome, we find evidence for blacks, Hispanics, and Asians that wages are higher when workers are more networked to neighbors, although the

[8] Something approximating the random assignment of workers to employers in the Census tract could arise if networks serve to give job seekers information about vacancies in the Census tract where neighbors work. In the present paper, in which we focus on the effect of labor market networks connecting workers to the establishments where their neighbors work rather than measuring such networks, we account for this potential "tract-level" network effect differently – through the introduction of a control variable to isolate the effect of networks operating at the establishment level.

[9] Reflecting the difficulty of distinguishing between the different models or hypotheses regarding networks, it is possible that better matches result simply from potential workers having information about more job matches, enabling them to choose a better one.

result is less clear for whites.[10]

In this paper we focus more explicitly on trying to distinguish between the potential effects of labor market networks identified in the theoretical literature, as explained more fully in the next section of the paper. In addition, while HNM and HNK focused on the matches of employed individuals, 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 uses other dimensions of labor market networks, emphasizing that network connections that may be productive in the labor market – whether or not in the context of displacement – need not arise only through 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.[11] And Cingano and Rosolia (2012) present related evidence for Italy, finding that for networks defined by "co-displaced" workers, employment (re-employment) of other co-displaced workers in the network reduces

[10] Schmutte (2015) finds a different kind of evidence of a matching role of networks – finding that workers are more likely to move to a higher-paying job when their neighbors are employed in high-paying firms, and that these local networks match high-ability workers to high-paying firms.
[11] Saygin et al. (2014) suggest that this implies that these former co-workers are referring the displaced worker to their employer, à la Montgomery (1991) and Simon and Warner (1992), but this evidence is equally consistent with former co-workers simply providing information about the availability of jobs at their firm.

unemployment duration.

However, these other recent papers focus on network links to former co-workers, whereas 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 a few reasons. First, if there are network links among neighborhood residents, policymakers may be able to exploit the "multipliers" that networks can generate to enhance the impact of place-based policies, but conversely it is also possible that place-based policies that fail to generate jobs among residents of those places can, for the same reason, cause those policies to underperform in generating employment among local residents (see the discussion in Neumark and Simpson, 2014). And 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.[12]

Network Measures and Analysis

Consider a sample of workers who lose their jobs as part of the huge number of layoffs that accompanied the Great Recession. How quickly are these displaced workers able to find jobs? And how does the strength of their neighborhood networks affect post-layoff re-employment?

Thinking back to the theoretical work summarized above, a displaced worker's probability of finding work in a given period of time will be a positive function of the vacancy rate in her local labor market, a negative function of the unemployment rate in her local labor market, a negative function of her reservation wage, a negative function of the length of time she has been unemployed (assuming there is negative duration dependence, as suggested in recent work by Kroft et al., 2013), and of course will also be related to her preferences for non-work activities like leisure. In addition, depending on the nature of imperfect information in the labor market, a displaced worker's probability of finding work may well be a

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

function of how many network contacts she has that will pass along to her information on vacancies, and alternatively (or in addition) may be a function of how "deep" those network contacts are in the sense of transmitting information via multiple network contacts employed at the same business, which could convey more reliable information to the employer about the quality of the job searcher.

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 both re-employment generally and re-employment specifically at a neighbor's workplace.[13] We limit our analysis to examining outcomes in the quarter following displacement, partially for simplicity and also because the frequency of unemployment durations lasting past one quarter were at more "historically normal" levels prior to the Great Recession, so that this window probably gives us the best comparison to the pre-recession period.[14]

We operationalize the strength of an unemployed worker's network by developing multiple measures of community-based networks at the level of the Census tract of residence. We then empirically examine whether and how these measures of network strength affect post-displacement employment, 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 LEHD dataset.

In order to explain our specific network strength measures and the data in the LEHD from which they come, consider the hypothetical case of John Jones, who is displaced from his employer in a mass layoff in the first quarter of a sample year. Given the detailed longitudinal nature of the LEHD, we observe John's pre-displacement earnings, as well as his post-displacement earnings (if any). We also have an

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[13] We do not report results for earnings as an outcome in our network analysis for a number of reasons. First, as noted above, in HKN we found strong positive effects of networks on reducing turnover for employed workers in our sample, but less robust results for wages. Although network models predict better job matches that should lead to higher wages, the effect could go in the other direction either because people prefer to work with their neighbors, or because worker reliance on networks may signal high search costs enabling employers to offer lower wages. Second, in the context of the Great Recession's historically high unemployment rates and low labor force participation, re-employment for displaced workers is the first-order outcome of interest. Third, and relatedly, as we show below, 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 also driven primarily by re-employment, and results for earnings conditional on re-employment are very noisy. These results are available upon request.

[14] For example, in the first quarter of 2005, 36.6 percent of unemployed workers had been unemployed for more than 15 weeks. In 2010 that number was 59.2 percent. See (http://research.stlouisfed.org/fred2/graph/?id=LNU03008276,LNS13008517 (viewed June 3, 2014).

indicator for the establishment from which John is displaced, as well as some demographic information about him. Critically, we observe the Census tract in which John lives. We also can observe various characteristics of that Census tract, most importantly the number of adult neighbors that John has (defined as residents of that Census tract). For the each of the neighbors, we know whether he/she is employed in the quarter following John's displacement. In addition, for each of John's employed neighbors, we observe the establishment in which they work, as well as important characteristics of those establishments, including whether they are observed to make any gross hires in that post-displacement quarter.

To the extent that we are considering networks defined by residential proximity, the size of John's network is the number of (adult) residents of his network. But size is not necessarily synonymous with the strength of John's network – that is, with its ability to be productive in aiding John's job search. First, the productivity of John's network depends crucially on how much information his neighbors have to transmit to him about the existence of vacancies. Second, and just as important at least conceptually, the productivity of John's network may also depend on how "deep" the network connections are, providing more valuable or reliable information about John to employers at which many neighbors work. Indeed, we think that these two ways of characterizing networks can be usefully thought of as corresponding to the two types of models of labor market networks discussed above – one in which network members transmit information about the existence of vacancies to searching workers, and one in which network members convey information about searching workers to employers.

If John's network is productive because employed neighbors provide him with information on vacancies, we might expect that the network is more productive the more employers John is connected to through his network, which we characterize as the "breadth" of his network.[15] One way to think about this is that when two (or more) neighbors who are employed in the same establishment give John information on a vacancy in the establishment, they are transmitting redundant information (as in Ioannides and Soetevent, 2006), whereas two (or more) neighbors who are employed in separate establishments have

[15] The breadth of the network may also matter because it is likely to be related to greater diversity in job vacancies John learns about through his network (for example, by industry), and hence a broader network may be more likely to lead to a good match for John.

unique vacancy information to transmit.[16] It is more complicated than that, of course. First, unemployed neighbors will not contribute to the strength of John's network; indeed, his unemployed neighbors would be expected to compete with him for jobs and even for information on vacancies to the extent that they are passed as private information. In addition, John's employed neighbors can only provide him with information on the existence of vacancies in their establishments if there are, in fact, vacancies. If there is no hiring occurring where John's neighbors work, his network will not be productive in helping him secure employment. We take account of these issues in our broad establishment-level network measure described below.

Alternatively, suppose that John's network is productive because John's employed neighbors give their employers information about John's quality (or the quality of a potential match between John and the firm). As before, it is only John's employed neighbors who can do that productively, and again, this is likely to affect John's job search only if their employers are hiring. In this case, however, it seems more likely that having multiple neighbors work for the same employer is more productive for John than having neighbors work for separate employers. In particular, multiple neighbors who provide a signal to the same employer about John's (match) quality serve to increase the signal of the quality of a potential match between John and their shared employer than the signals provided by multiple workers each to unique employers.

Because these two mechanisms for information transmission in networks operate along different (and in some ways opposing) dimensions, we construct two distinct types of network measures in order to try to distinguish between them in the empirical work. In contrast, the network measures used in previous research, described above, do not try to distinguish these two dimensions.

The first network measure we construct is one we term the "active broad employer network" measure, denoted *ABE*.[17] *ABE* is meant to capture networks that provide information on the existence of vacancies through employed workers to job seekers, where (we hypothesize) the breadth of the network

[16] We recognize, however, that this does not necessarily have to be true. Since some of John's neighbors may not actually be in his network, the likelihood that his network passes on information about a vacancy may be increasing in the number of neighbors who work in the establishment.

[17] "Employer" throughout refers to the establishment of employment.

across multiple potential employers increases John's probability of employment. Recall that for each of John's neighbors (that is, for each member of his network as we define it), we observe not only whether the neighbor works, but also where he or she works (if employed). Therefore, we know from how many unique employers John may receive information on vacancies.

Moreover, we know the identity of the employers at which John's neighbors work. To capture the fact that John will not receive information from his neighbors on vacancies if there is no hiring going on at their employers, we construct measures of the extent to which hiring is occurring in the establishments of John's neighbors. In particular, for each unique employer for which one of John's neighbors works, we calculate the gross hiring rate of that establishment in the quarter following John's displacement, defined as the gross number of new hires divided by the number of employees in the quarter. 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 among job seekers for vacancies. That is, John's neighbor may have information on vacancies at his or her establishment to transmit to John, but that information is also transmitted by employees who are not John's neighbors 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 John learns about more potentially productive vacancies than from a small number of gross hires at a small employer.

The "active broad employer" network measure (*ABE*) for our displaced worker John (and, of course, all other displaced workers as well – we just continue to use John as an example for simplicity), then becomes just the average of the hiring rates for all John's neighbors' employers, giving an equal weight to each employer. Neighbors who are not employed effectively contribute zero to the measure, and neighbors who work in the same establishment only contribute once to the measure:

$$ABE = \frac{1}{N}\sum_{e}^{E}\frac{H_e}{L_e}$$

where *N* is the number of neighbors in John's Census tract at the time of his displacement, *E* is the number of unique employers for whom John's neighbors work, $\frac{H_e}{L_e}$ is the ratio of new hires at the employer *e* in the quarter following John's displacement, divided by the count of employees at that employer in the beginning of the post-displacement quarter. We take the average across all of John's neighbors, *N*, rather than just across employers (or just across John's employed neighbors), to reflect the fact that the more of John's neighbors who are not employed, the less likely it is that any given neighbor he speaks with will have job information. In addition, the more neighbors who are job searchers like he is, the lower is the probability that he will obtain productive information on vacancies from his neighbors, 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 John will have to compete with his neighbors if he applies to job vacancies that he accesses through his network.[18]

The network strength measure *ABE* captures the notion that John's employed neighbors have information about vacancies in their establishments that can be transmitted to him. But John's neighbors may also have more general information about vacancies in establishments near to their own, rather than just in their establishment. (This is the conceptualization of networks implicitly used in Bayer et al., 2008.) We therefore construct a parallel network measure to account for information on vacancies in a geographic area (denoted *ABT*, where the *T* stands for "tract," and referred to as our "active broad tract" network measure) as:

$$ABT = \frac{1}{N}\sum_{c}^{C}\frac{H_c}{L_c}$$

where the hiring rates are summed across unique Census tracts (indexed by *c*) in which any of John's *N* neighbors are employed.

Although *ABT* can be interpreted as an alternative network measure wherein one's neighbors may provide information on the existence of vacancies generally in the locations where they work, this is not the

[18] Because the magnitude of *ABE* is affected both by the number of its members that are employed and by the hiring rate at establishments of employed members, *ABE* is a really a measure of the strength of the network, rather than its size per se; nonetheless, as a shorthand we sometimes refer to *ABE* and our other measures of network strength as simply "network measures."

only interpretation of its role in job finding. Because we cannot control completely for the strength of the local labor market generally in which a displaced worker is searching for work using standard observable measures,[19] a high *ABT* may also simply capture more employment and hiring in John's local labor market (that is, in the tracts where John's neighbors work, which are likely to be nearby or easily accessible by public transportation). Viewed that way, *ABT* may more appropriately be interpreted as a control variable for unobserved local labor market strength that, by its inclusion in the regressions, can sharpen the interpretation of the estimated effect of *ABE* as capturing a more specific form of residence-based networks connecting people to the employers where their neighbors work. We do not take a strong stand on these two interpretations of *ABT*, nor on the interpretation of the related measure discussed below, but we do think that *ABE* more reliably identifies a network effect when we condition on *ABT*.

The alternative network measure we construct accounts for the "depth" of networks that John's neighbors have with employers – which we hypothesize is related to the strength or quality of information that can be transmitted from John's neighbors to a potential employer about match productivity. This measure is most closely related to the previous work (HNM, HKN) examining the importance of networks and their role in increasing tenure and wages for working individuals. We construct a measure of what we term "active deep employer" network strength (*ADE*) for John by building up from the observed network index NI_i (defined in the previous section) for each of John's neighbors. For individuals living in John's Census tract who are not employed (including those who have been recently displaced), NI_i is, by definition, equal to zero since they have no network contacts at their place of employment.[20] We define John's active deep employer network measure as a weighted average of the observed network indexes for his neighbors:

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

[19] In contrast, Bayer et al., (2008) are able to control for the strength of the local labor market by treating neighbors as those who live only on the same Census block in measuring network ties, and treating correlated outcomes among those who live in the same Census tract as (potentially) measuring local labor demand, job access, etc.
[20] We also define workers at single-employee firms (who have no co-workers) as having an NI_i of zero.

Here, we sum across all of John's neighbors, rather than across employers. I_i is an indicator for whether neighbor *i* is employed, and $\frac{H_{ie}}{L_{ie}}$ is the ratio of new hires at the employer *e* of neighbor *i* in the quarter immediately following John's displacement, divided by the count of employees at that employer in the beginning of the quarter. John's neighbors who are not employed contribute zeroes to the measure; $\frac{H_{ie}}{L_{ie}}$ is undefined for these cases, but we have not introduced additional notation since this expression is multiplied by zero in these cases. If the individual has many neighbors who are not employed, or who are employed but in establishments where no other neighbors are employed, *ADE* will be low. Conversely, if the individual has many neighbors who are employed in workplaces that are doing a lot of hiring, *and* whose co-workers consist largely of other neighbors, *ADE* will be high.

Finally, we build a parallel measure of the depth of the network at the level of the workplace Census tract, which we term the "active deep tract" network measure, as follows:

$$ADT = \frac{1}{N}\sum_{i}^{N} TI_{ic}\,\frac{H_{ic}}{L_{ic}} \cdot I_i.$$

This measure is a weighted average of what in HKN we termed each neighbor's observed Transport Isolation index (*TI*, which is the same for all neighbors working in the same workplace Census tract), where the weights are the hiring rate in that Census tract of employment. For each worker, *TI* gives the share of total workers in a workplace Census tract who reside in the same tract as that worker – i.e., having the same origin and destination tracts in their commute. It is constructed in an identical manner as *NI* (which is why we use a similar notation), except that we use the workplace Census tract rather than the establishment. Neighbors with no employment effectively contribute zero to the deep tract measure, as do neighbors working in Census tracts that are doing no gross hiring.

The name "Transportation Isolation index" is so coined because of the possibility, as discussed in HKN, that residential neighbors may work in the same Census tract because shared transportation opportunities lead to shared access to jobs in that Census tract, rather than a mechanism by which shared information via networks leads to shared access to jobs in that Census tract.[21] Under this interpretation, paralleling the discussion of *ABT* above, *ADT* serves as a control variable for estimating the effect of *ADE*; it could also reflect network contacts that connect a worker to other employers in the tracts where their neighbors work, although this interpretation seems less tenable with regard to a network mechanism whereby workers refer neighbors to their employers.

Thus, to summarize, we view the employer-level (establishment-level) network measures – *ABE* and *ADE* – as the cleanest measures of the importance of residence-based network strength in re-employment outcomes of displaced workers, especially when we control for other potential confounders (including the other tract-level network measures), because *ABE* and *ADE* measure the distribution and concentration of neighbors at specific establishments, conditional (given our data) on an extremely rich set of other determinants of labor market outcomes.

Data

The core dataset from which the samples we study are extracted is the Census Bureau's LEHD Infrastructure files.[22] The files consist of a frame of jobs produced from state unemployment insurance 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 employer identifiers to merge worker and employer data.

[21] As explained in HKN, the inclusion of *ADT* along with *ADE* parallels the adjustment of *NI* in HNM for the clustering that can occur randomly.
[22] See (Abowd et al., 2009) for a summary of the various components of the LEHD Infrastructure files.

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. We begin with an extract of 1.4 billion jobs, or spells of earnings from an employer, held from 2004 through 2013 at employers located in 49 states.[23]

From these data, we identify 106 million workers separated from their highest earning (dominant) job from 2005 through 2011, as defined here. 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 been employed at a firm 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.[24] Last, we require that the separation was from the worker's main (i.e., highest-earning) job, with the idea that the loss of a main job is likely to lead the worker to search for a new job. Note that some of the separated workers may hold a secondary job, and maintain that job following the separation.

We want to restrict attention to workers who experience mass layoffs 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 establishments 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

[23] We include all states except for Massachusetts, which (along with the District of Columbia) did not begin submitting data to the Census Bureau until after 2005.
[24] We require that displaced workers have no earnings at the downsizing employer for eight subsequent quarters. Included in that definition of employer are any other employers that the LEHD has linked to the downsizing employer using the Successor Predecessor File. For more on the QWI variable definitions, see: http://lehd.ces.census.gov/doc/QWI_101.pdf.

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 establishment. For this sample, 78 percent of separations were at employers with 25 or more workers in the previous year, and 17 percent were both of that size and had a drop of 30 percent or more that was not simply a restructuring.[25] With this definition, we identify 18.4 million workers displaced from 2005 to 2011.

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 88 percent of the sample.[26] 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.[27] We drop a further 6.5 percent of the remaining workers who are not between 19 and 64 years old in the quarter in which they separated.

From the resulting sample of 9.3 million displacements, we retain those who had pre-displacement annual earnings from all jobs of between $5,000 and $100,000 (in 2010:Q1 $), for two reasons.[28] 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. Second, the lower restriction excludes workers who, although they held a job for at least a year, were more likely to be a secondary earner or dependent, or otherwise not highly attached to the wage and salary labor market. The upper bound drops 7.5 percent of workers and the lower bound drops 2.5 percent, resulting in a final

[25] The Successor Predecessor File tracks worker flows across employers to identify spurious separations.
[26] We use the Composite Person Record, an annual file built from federal administrative data on residential addresses that contributes to the LEHD Infrastructure files.
[27] The urban status of a Census tract is based on Census Bureau classifications of the 2000 Decennial Census. The Census Bureau defines urban areas using population total and density rules. Urban areas typically include suburbs, but may not include some "exurbs." Approximately 80 percent of the U.S. population resides in an urban area, and the displaced worker extract has a mean urban share of 82 percent. We only retain the 62 percent of displaced workers who reside in a 100-percent urban Census tract (urban status can range from 0 to 100 percent, and include suburban areas). The 100-resident worker restriction drops fewer than 1 percent of the displaced workers, as Census tracts have a target population of approximately 4,000.
[28] We use the urban Consumer Price Index, taking the average for each month in a quarter (because earnings are reported on a quarterly basis).

estimation sample of 8.4 million displaced workers.

Table 1 provides mean characteristics of our worker sample, including the variables we will use as controls in the regression models described in the next section. We normalize all earnings variables to 2010:Q1 dollars. We link in neighborhood (Census tract) poverty rate (from the 2000 Decennial Census), which we use as a basic control for labor market conditions of the worker's place of residence and characteristics of the worker's neighbors. Age is calculated for the quarter of displacement, and industry affiliation is the industry of the establishment from which a worker is displaced.[29]

Table 2 lists the distribution of our sample across years. The sample share increases from 13.0 percent of displacements in 2005, to a peak of 18.8 percent in 2008, and then falls to 10.8 percent in 2011. 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) show that workers displaced in years encompassing the Great Recession (2007-2009) – 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.

Using the data on 1.4 billion jobs from the LEHD Infrastructure files spanning the study period, we construct the network measures using employment and hiring information in the quarter after each displacement cohort is separated, and residence information from the same year. This timing is intended to capture the jobs to which workers' networks may connect them in the period following displacement. 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 quarter of layoff of a displaced worker and in the subsequent quarter. If a neighbor has more than one job spanning both quarters,

[29] 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 in terms of the variables shown in the table.
[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.

we only use the job with the highest earnings in the subsequent quarter. All neighbors (employed or not) contribute to the count of neighbors, or *N*. Additionally, the entire sample of laid off workers 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.

One limitation of the LEHD Infrastructure files for calculating the network measures is that employers with multiple units in a state do not report the assignment of workers to establishments (this happens in about 44 percent of jobs).[31] The LEHD program has developed an imputation model to allocate establishments to workers based on establishment size during the worker's tenure at the employer and on the distance between the establishment and the workers place of residence. We use this imputed establishment assignment to calculate our network isolation measure, to identify employers in the same workplace Census tract as a neighbor's employer, and to characterize the pre-displacement industry of displaced workers.[32] Uncertainty due to lack of definite geography adds noise to NI (see HKN) and may be expected to add noise to our estimates.

We calculate the hiring rate used in the employer-level network measures – *ABE* and *ADE* – as the number of new hires at an employer in a quarter divided by the count of employees at the beginning of that quarter.[33] On average, employers hired about 13 new workers for each 100 they had at the beginning of the quarter, giving an average hiring ratio of 0.13 with a standard deviation of 0.64. The hiring rate used in the

[31] At a minimum, the state in which an employee works is indicated by the state to which an employer submits unemployment insurance earnings records. In the LEHD Infrastructure files, each employer in a state has a unique State Employer Identification Number. One exception is that multi-unit employers in Minnesota report an establishment assignment along with earnings information for each worker.
[32] The LEHD program actually takes ten independent draws from the "unit to worker" allocation model for the production of public use statistics. For this study, we use just the first of those imputation draws.
[33] 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).

tract-level measures is calculated similarly, but at the level of the tract where neighbors work, rather than their specific employers.

Figures 1a and 1b displays various percentiles of the network measures across the distribution of displaced workers, by year.[34] Comparing Figures 1a and 1b makes it clear that the scale of the active deep network measures is much different than that of the active broad measures – by an order of magnitude at the median – because while the broad measures are weighted averages of hiring rates, the deep measures are weighted averages of hiring rates multiplied by the network isolation measure. This scaling differential needs to be kept in mind when interpreting the regression results.[35]

The effects of the Great Recession on the network measures are made clear by the drop from 2007 to 2009. By 2009 the network measures have fallen by about one-third to nearly one-half of their pre-recession levels.

Analysis

With the network measures defined, the analysis is relatively straightforward. To answer the question of how quickly a displaced worker is re-employed, we conduct a series of regression-based analyses where, for our sample of displaced workers, we regress post-layoff re-employment on our network measures and a host of variables that control for observable characteristics of the neighborhood as well as the individual.

Focusing on the first quarter after experiencing a mass layoff between 2005 and 2011, we estimate linear probability models for employment of the following form:

$$Emp_{it} = \alpha + X_{it}\beta + Net_{it}\gamma + \varepsilon_{it} \ .$$

The subscript *i* indexes individuals, *t* indexes time, and *X* is a series of controls for the individual and his/her neighborhood and employer. *Net* is the vector of the four network measures discussed earlier (*ABE, ABT, ADE, ADT*).

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[34] Appendix Table A2 gives the percentiles of the network measures pooled across all years.
[35] We have top-coded the establishment-based network measures to the 99th percentile of the distribution, because there were some extreme outliers that went up to an order of magnitude higher than the 99th percentile. (This occurred in the establishment measures but not the tract measures, presumably because the latter average over many workers.)

Models are estimated for two different employment outcomes. First, *Emp* is 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. Second, to gauge whether the employment effects of residence-based networks that we estimate actually reflect neighborhood networks, we narrow the re-employment definition to an indicator of becoming employed at the employer of a neighbor. We look at this latter outcome for the full sample, and for the subsample of those re-employed.

Although the LEHD has limited demographic information as compared to, say, the Current Population Survey, we are still able to control for age, sex, race, and ethnicity, and for earnings and industry affiliation in the year prior to displacement at the primary employer from which the worker is displaced. 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 human capital of the displaced workers as well as controls for at least part of their reservation wage, which can affect their job search behavior. The industry controls also may be thought of as accounting for unobserved human capital characteristics of workers, as well as for variation in labor demand across industries.

One key factor for which we want to control is local, time-varying labor demand conditions. This is critical because our network measures are influenced by employment rates in each person's tract and by hiring rates at employers and in tracts where neighbors work. In addition, we want to allow for heterogeneity across workers laid off at different points of time or from different employers. For example, we saw 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. To control for this heterogeneity, we include in our regressions layoff fixed effects that are uniquely defined by employer-by-year-by-quarter-by-county. As a result, we identify the effect of neighborhood labor market networks on post-displacement employment from variation in the network measures within individuals who are laid off in the same quarter, from the same employer, and from establishments in the same county. This variation arises when workers laid off from the same establishment (or set of establishments within a county of a given employer), who therefore are likely very similar, live in different neighborhoods.[36]

Thus, these highly-detailed fixed effects account in a non-parametric fashion for labor market conditions that vary spatially,[37] as well as varying over time as the Great Recession and recovery unfolds, and for differences across workforces experiencing each mass layoff. The workplace-by-year dimension of the fixed effects also controls for the generosity of time-varying state variables such as Unemployment Insurance 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. Hence, we can be more confident that the estimated impacts of the residence-based network measures are not confounded with other policy differences, and, more important, are not confounded with unobservable characteristics of the local labor market or of the displaced worker that are correlated with our network measures. The only remaining possible confounder to our identification is if workers who worked together prior to being subject to the same mass layoff are sorted across neighborhoods with different network strength based on factors that affect their re-employment probabilities. This seems unlikely. Moreover, unobserved heterogeneity that is correlated with network strength and that affects re-employment per se by definition is eliminated in our specifications where the outcome is re-employment at a neighbor's employer conditional on becoming re-employed anywhere.

In addition to the highly-detailed fixed effects, we include the poverty rate in the neighborhood as a catch-all proxy for economic conditions and demographic characteristics of the tract.[38] Finally, as discussed earlier, the tract-level network measures *ABT* and *ADT* may also capture local labor market conditions, and

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[36] 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.

[37] As discussed below, other controls also capture variation in local labor demand conditions.

[38] In unreported results we also included other controls at the tract level estimated using 2000 Census data, including measures of the share living in the same home since 1995, the share foreign born, education levels, and race. The results for the coefficients of our network measures were qualitatively similar to those we report below.

in some sense have the advantage of capturing these in the same functional form via which local labor market conditions enter the establishment-level network measures on which we focus attention – *ABE* and *ABT*.[39]

We are also interested in exploring whether the ability of networks to help displaced workers changed during and after the Great Recession. Hence, we estimate our models for the full sample, as well as separately for each year in the time span 2005-2011. 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.

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 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. 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 with the downsizing employer. 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 $3,700 and $5,300 in the quarter following displacement, with those earnings rising to a range of about $5,700 to $7,100 by the 8th 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 $1,700-$2,000 of pre-displacement earnings.

[39] As discussed above, the estimated coefficients of *ABT* and *ADT* could also reflect, in part, the effects of network connections to establishments in the tracts where neighbors work.

At the other extreme, those displaced in 2009 have the largest drop and recover only about 40 percent of the loss on average, remaining about $3,300 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 in Figure 2 is whether or not 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. Figure 3 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 75 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 in 2010 and 2011), and the re-employment rate in the quarter after displacement is only 47 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 recovery began; in contrast the recovery was slower for those displaced earlier but still not employed as the Great Recession began to unfold.

We also confirm in Figure 4 that most of the earnings drop observed post-displacement in Figure 2 is, in fact, driven by those with zero post-displacement earnings, by producing an analog to Figure 2 where we dropped 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,[40] so what is most interesting to us – and perhaps more tied to network strength – is re-employment. We therefore focus the rest of our analysis on the re-employment margin.

<u>Other determinations of employment and earnings recovery after displacement</u>

As a preliminary to our core analyses, Table 3 displays the full set of coefficient estimates for our baseline employment regression when estimated by pooling all the years of our sample together. Here we do not focus on the estimated effects of the network measures, to which we will turn in depth just below. Rather, we report these to display the estimates of the effects of the demographic, earnings history, and other control variables on the employment status and earnings outcomes in the first quarter after the displacement. The first column displays OLS results with year controls, and the next column displays results with the employer-by-year-by-quarter-by-county fixed effects. As noted above, the latter are always included in our main analyses. But the specification in column (1) that includes only year dummy variables is useful for seeing how re-employment varies across the years of our sample, conditional on the other controls.

As Table 3 shows, the estimated coefficients on variables meant to capture characteristics of the displaced workers or of their neighborhood (defined as Census tracts) have the expected signs and magnitudes. For example, re-employment probabilities are lower in neighborhoods with a higher poverty rate. For example, a 0.1 (10 percentage point) higher poverty rate is associated with slightly less than a 0.014 (one-and-a-half percentage points) lower probability of re-employment in the first quarter after 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. Older workers, minority workers, and women generally had lower post-displacement employment rates, conditional on previous earnings and the other controls. Column (1) shows that workers displaced from manufacturing had particularly poor re-employment probabilities. The year dummy variable coefficients indicates that – as we

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

saw in the graphs – re-employment deteriorated as the economy moved into the Great Recession, and was worst for those displaced in 2009, followed by those displaced in 2008 and those displaced in 2010. By 2011, conditional re-employment probabilities had returned to their pre-recessions levels.[41]

The effects of networks on re-employment

We now turn to our main analyses – the estimated effects of residence-based network measures on employment. Tables 4a-4c report results for employment. All specifications (as well as those in subsequent tables) include the controls in column (2) of Table 3.

Table 4a reports the re-employment results. Column (1) reports pooled results for the full sample period; these are the same coefficient estimates that are reported in column (2) of Table 3. We include both network measures (*ABE* and *ADE*) and their census tract-level counterparts (*ABT* and *ADT*), because by including them simultaneously we are best able to distinguish the different mechanisms by which networks might matter.

In column (1) of Table 4a, we find positive and significant estimates of both of our key establishment-level network measures: the "broad" measure *ABE* that corresponds more to the role of networks in providing information about jobs to other workers in the networks; and the "deep" measure *ADE* which we argue is more reflective of the provision of information about potential hires to employers of workers in the network. To interpret the magnitudes, we also provide, below the regression estimates, the implied effects of moving from the 25th to the 75th percentiles of the distribution of each of our network-related measures (or controls). For *ABE*, the estimates in column (1) indicate that the effect of an interquartile change is to raise the probability of re-employment in the quarter after displacement by 1.9 percentage points (compared to a mean job finding rate of 57.2 percent). This may seem like a small magnitude on the face of it, but based on the estimated coefficient on the Census tract poverty rate (see Table 3), a move from the 75th percentile of the poverty rate to the 25th percentile (a change from 18.1

[41] The specification in column (2) excludes includes the industry dummy variables, even though these vary for a handful of observations within employer/year/quarter/county cells (when an employer has establishments in different industries in the same cell). The estimates were identical to three or more digits to the right of the decimal point with the industry dummy variables included; the industry dummy variables are also excluded from the fixed-effects specifications in the tables that follow.

percent in poverty to 5.0 percent in poverty) would entail an increase in post-displacement employment of a somewhat smaller magnitude – 1.26 percentage points. Meanwhile, a change in *ABT* from the 25th to 75th percentile would imply a reduction in employment probability that is about one-fifth as large in absolute magnitude as that of *ABE* – about 0.4 percentage point. Recall that we were agnostic about whether the active broad tract measure (*ABT*) represents a network measure or a control variable. It is not obvious why as a control variable for whether neighbors tend to work at many employers within the same Census tracts its effect would be negative, but the lack of a positive effect should not be taken to imply a negative network effect. One possibility is that, conditional on the other network-related variables we include, a higher value of *ABT* – which means that neighbors work in more tracts – mainly reflects that jobs are farther away on average, raising commuting costs and lowering the net wage and hence employment.

The estimated effect of *ADE* is also positive and significant. The estimate implies that a change from the 25th to the 75th percentile is associated with a re-employment probability that is higher by 0.07 percentage point, much smaller than the implied effect of the broad measure *ABE*. Finally, the estimated effect of *ADT* is also positive and significant, and of larger magnitude. As noted earlier, however, this may or may not reflect a network effect. Going forward, therefore, we focus on the estimated effects of the explicit measures of network strength, *ABE* and *ADE*.

In columns (2)-(8) we split the sample by year. To interpret these in light of the Great Recession, the recession began in December 2007 and officially ended in June 2009. However, as usual the labor market lagged; payrolls did not start growing consistently until about the second quarter of 2010,[42] and the unemployment rate did not reach its peak until October of 2010.[43] The results show that the coefficient on the broad employer network measure (*ABE*) was very stable through 2008, fluctuating only between 0.76 and 0.85. It then fell sharply to 0.65 and 0.61, respectively, in 2009 and 2010, exactly when job losses fell the most and payroll employment reached its lowest level.[44] Then in 2011 the estimated coefficient of *ABE*

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[42] 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).
[43] See http://data.bls.gov/timeseries/LNS14000000 (viewed March 26, 2015).
[44] We verified that these declines in the estimated coefficient of *ABE* in the years during and after the Great Recession are generally statistically significant, by pooling the data, interacting all variables with year dummy variables, and testing the significance of differences in the estimated coefficient of *ABE* relative to the 2005 estimate.

rebounded to 0.73. All of these estimates are statistically significant. The interquartile effects reported in the lower panel of the table tell a similar story, although the decline in the effect begins earlier – in 2008 – and the decline from earlier years to 2009-2010 is sharper than for the estimated coefficient. This reflects the changes in the network measures, depicted in Figures 1a and 1b, which fell during and immediately after the Great Recession, with the distribution (and interquartile range) narrowing.

The estimated effects for *ADE* are less robust. They are positive and significant in 2007 and 2008, but for the other years the coefficients are generally much smaller and statistically insignificant. The interquartile effects reported in the lower panel tell a similar story. There is a hint of a decline in the effect during the Great Recession, but this pattern is much less distinct than for *ABE*.[45,46]

Overall, these estimates suggest that labor market networks that link workers to their neighbors' employers are effective at helping displaced workers become re-employed, especially the broad network measure *ABE* that captures the scope of vacancies at neighbors' establishments. That said, networks that provide information about job vacancies to job searchers (captured in *ABE*) were quite clearly less effective in aiding the transition back to work for displaced workers during the depths of the labor market disruptions of the Great Recession (2009-2010), as compared to the period either before or after. In terms of changes in the data, this happened for two reasons. First, the distributions of the employer network measures *ABE* (and *ADE*) shifted to the left and narrowed during the Great Recession, so that network connections to vacancies themselves diminished – more so in the more networked neighborhoods. Second, the fact that the estimated coefficients on *ABE*, in particular, fell during the recession suggests that, conditional on the network measures, the productivity of these networks in helping displaced workers find work also fell during the recession.

So, for example, consider a hypothetical worker laid off in the pre-recession period. Conditional on

[45] Although some of the analyses reported later show larger declines in the estimated coefficient of *ADE* during and after the Great Recession, these differences are not statistically significant, based on tests described in the previous footnote.

[46] In the specifications reported in Table 4a, and those reported in the additional tables that follow, the network measures enter linearly. We also estimated all specifications with linear and quadratic terms in the network measures. There was frequently evidence of diminishing effects of the network measures at higher values, but the implied partial derivatives of the outcomes with respect to the network measures were virtually always positive throughout the distributions of the network measures.

other regression controls, we estimate that if her active broad employer network (*ABE*) were at the median of the distribution instead of zero, this would boost her re-employment probability by 6.3 percentage points or 10.1 percent ({0.85·0.074}/0.624, using the column (2) 2005 regression estimate, the 2005 median for *ABE*, and the mean re-employment rate for 2005 displacements in Table 2), relative to having no network linking her to job vacancies. In contrast, the same worker, laid off in 2009 with *ABE* at the median of the distribution in that year, would only have a re-employment probability rate of 3.1 percentage points or 6.5 percent higher than if she had no such network. This strikes us as an economically significant change.

We can only offer possible explanations for the decline of the productivity of employer networks – as reflected especially in the decline in the estimated coefficients of *ABE* – during the recession. With regard to *ABE*, one possible explanation is that neighbors may have alerted job searchers to fewer vacancies, either because they had less actual knowledge of whether there were vacancies during the recession (perhaps because employers did not have to do as much advertising for applicants), or perhaps because they were only willing to do so for the very highest quality neighbors during the Great Recession since employers were doing little hiring. Another possible explanation for the fall in the productivity of *ABE* as a result of the Great Recession may be related to the evidence in Davis et al. (2012), who show that employers filled vacancies at a slower rate during and after the Great Recession, potentially because the transactions costs of hiring and firing weighed more heavily on hiring decisions in the face of unusually high uncertainty about product demand.

The results thus far on *ADE* do not provide clear evidence of a decline in the importance of networks during the Great Recession; results presented below provide more such evidence, although it is always much less clear than for *ABE*. The productivity of deep networks could have declined because neighbors were only willing to refer to their employers their very highest quality job-seeking neighbors. Alternatively, although networks can facilitate a "foot in the door" for displaced workers at employers who are hiring, the fact that hiring rates were low meant that employers could be very picky about who they actually hired, so that the signal given to an employer from a network referral may have been less valuable.[47] On the other hand – and this could explain the more ambiguous results for changes in the estimated coefficient of *ADE* over the course of the Great Recession – when the number of applicants soared, employers might have increased reliance on referrals to identify better applicants.

Tables 4b and 4c report employment results for subsamples based on earnings: first those with pre-displacement earnings below $50,000, and then those with pre-displacement earnings of $50,000 or higher. Our conjecture – which is consistent with the evidence in HNM – is that local labor market networks are more important for lower-skilled than higher-skilled workers, because these workers are more likely to search for jobs in local labor markets. Given that we do not have extensive skill measures in the LEHD data, we use pre-displacement earnings as a proxy that can help account for sorting on skill. Since the structure of these two tables is the same as Table 4a, the results can be described succinctly.

For the lower-earnings/lower-skilled group, the evidence reported in Table 4b that labor market networks helped displaced workers become re-employed, but that this effect weakened during and immediately after the Great Recession, is as strong as or stronger than the full sample results. The evidence for *ABE* is quite similar to that in Table 4a. The evidence for *ADE* is now somewhat more consistent with the evidence for *ABE*. In particular, there is more consistently positive evidence of the effect of *ADE* on re-employment in the years prior to the Great Recession. Moreover, for *ADE* specifically, the interquartile effect is larger (when it is positive) – for some years about twice as large – for the lower-earning group, compared to the estimated effects for the full sample in Table 4a.

The stronger evidence of network effects in Table 4b, for the low-earnings subsample, is mirrored in much weaker effects for the higher-earnings subsample, reported in Table 4c. For the broad establishment-level network measure, *ABE*, the estimates are still positive and statistically significant, although only at the ten-percent level in 2010. And the estimated interquartile effects are smaller. Still, for

[47] There is a potentially related finding in the affirmative action literature. Holzer and Neumark (2000) find that among firms that report using affirmative action in hiring, there is less reliance on conventional and perhaps cheap screens, like education, and more willingness to invest resources in identifying qualified minority applicants who are less qualified in terms of these screens (education, in particular). Relying on referrals when the applicant pool may on average be of lower quality may similarly reflect tapping other sources of information to find good employees in this pool.

this measure, we see the same drop-off in the effect in 2009 and 2010. For the deep network measure, *ADE*, there is no consistent evidence of a positive effect for the high-earnings group. [48]

To provide additional information on the sources of changes in the effects and importance of network strength over the course of the Great Recession, Figure 5a plots three graphs – the upper panel corresponds to *ABE.* The black solid line plots the interquartile effects in each year; these estimates directly match those in Table 4b. For *ABE* we see a clear decline as we move into the Great Recession. We then use the estimated effects of network strength, and the estimated changes in the distribution of the network strength measures, to gauge how much of the change in the overall effect is attributable to changes in the estimated effect of the network measures, as opposed to changes in the distribution. For the latter, we focus on the sharp decline in the interquartile range depicted in the top panel of Figure 1a (which is, for example, more pronounced than the decline in the median). The dotted line in the top panel of Figure 5a holds the interquartile range constant at its 2005 level, and plots the product of this with the estimated coefficient in each year, hence capturing only the change in the estimated effect of the broad network measure. In contrast, the dashed line holds the estimate network effect constant at its 2005 value, and uses the observed changes in the interquartile range of the network measure. As the graph shows, both types of changes generate declines in the estimated impact of network strength, but the change in the distribution accounts for much more of the overall change.[49] For *ADE*, in contrast, it is the change in the estimated effect of networks that more closely mirrors the pattern of overall changes, although there is less clear evidence of a decline during the Great Recession.

<u>The effects of networks on re-employment at a neighbor's employer</u>

In Tables 5a and 5b we focus on whether the re-employment occurred at an employer of a worker in the neighborhood network. This provides more direct evidence on whether our network measures are in fact picking up referrals of neighbors to employers or the provision to neighbors of information about jobs.

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[48] The much weaker effect of *ADE* for higher earners could reflect employers' ability to get more reliable information about higher-skilled workers without having to rely on referrals from other workers who know and can refer potential hires.

[49] The lower panel presents similar results for *ADE*, for the estimates from Table 4a. But as already noted, in this case the evidence of changes over the course of the recession are less clear-cut.

The evidence provides striking confirmation of the network interpretation of the evidence. In Table 5a we report models for the full sample, so the outcome is hiring at an employer of a neighbor, versus re-employment at some other employer or no re-employment. For the broad measure *ABE*, every coefficient estimate is larger than in Table 4a, but with the same pattern of smaller effects on re-employment at a neighbor's employer in 2009 and 2010, the worst years of the labor market's downturn resulting from the Great Recession. The results for *ADE* are positive in all years, and statistically significant in most, but the pattern does not as unambiguously suggest a decline during the worst of the Recession's impact on labor markets (although the point estimate in 2010 is the lowest (0.67) and is statistically insignificant).

The evidence in Table 5b, which conditions on re-employment by excluding from the sample those who did not have positive earnings in the quarter following displacement, is similarly strong and exhibits the same pattern for both *ABE* and *ADE*. Moreover, the interquartile effects are strongest in this case. Because the sample used in this table consists only of those re-employed in the quarter after displacement, it provides perhaps the clearest evidence that neighborhood labor market networks affect the re-employment of displaced workers by leading to new jobs where neighbors are employed.

Figure 5b presents similar information to earlier on the role of changes in the estimated effects of network strength versus changes in the distributions of the network measures themselves, in this case for the regression estimates in Table 5a, and Figure 5c does the same for the regression estimates in Table 5b (which condition on re-employment). Again, in both cases the decline in the effect of networks during the Great Recession is much more apparent for *ABE*, and changes in the distribution of this network measure play a larger role (especially in Figure 5c).

Finally, based on the earlier evidence that the two network measures were more important in determining re-employment for the lower-skilled/lower-earnings sample, we re-estimated for this subsample the models for re-employment at a neighbor's employer. The results are reported in Tables 5c and 5d. In both cases, we find strong evidence of the effects of both network measures, although perhaps less distinct patterns of declines in these impacts during the Great Recessions. Figures 5d and 5e present the corresponding evidence on the role of changes in the estimated effects of network strength as compared to changes in the distributions of the network measures. The results are qualitatively similar to those described above, exhibiting a decline in the effect of *ABE* during the Great Recession driven in part by changes in the coefficient, but more so by changes in the distribution.

Conclusion

In this paper we develop two measures of what we refer to as active residence-based labor market networks, which vary over time and across residential neighborhoods. These measures of active networks are largely informed by theoretical models of how networks work, with our broad measure *ABE* intended to capture networks that connect workers with information about job vacancies, and our deep measure *ADE* capturing primarily networks that provide employers with information about workers ("referrals").

By studying workers who lost jobs in mass layoffs, and using the detailed spatial data on place of work and place of residence in the LEHD data, we are able to address multiple potential threats to the identification of network effects on finding jobs. We also test for the effects of these measures of network strength against the backdrop of the Great Recession, estimating their impact on the re-employment of displaced workers prior to, during, and after the Great Recession. In so doing, we provide the first evidence, to our knowledge, of changes in the role of labor market networks over the course of entry intro and recovery from the Great Recession.

Interpreting our active network measures in terms of these two models, for our "deep" active network measure (*ADE*) that is intended to capture referrals of job searchers to hiring employers, we find some evidence of the productivity of this dimension of networks. In particular, we find positive effects of this network measure on re-employment generally when we focus on low earners for whom local labor market networks should be more important. We also find strong effects of this network measure on re-employment at a neighbor's employer, which is more direct evidence of network effects. For our "broad" measure (*ABE*) that captures the provision of information about jobs to job searchers, we find consistent evidence that this network measure aids in the general re-employment of displaced workers, especially for low earners. And again we find stronger effects on re-employment at employers of neighbors. Moreover, when we find clear evidence of positive network effects on re-employment after mass layoffs in the pre-

Great Recession period, we also find that the effect of networks fell – sometimes substantially – during the weak labor market of the Great Recession and its immediate aftermath, when neighbors lost their jobs in larger numbers and employers slowed their hiring substantially or stopped hiring altogether.

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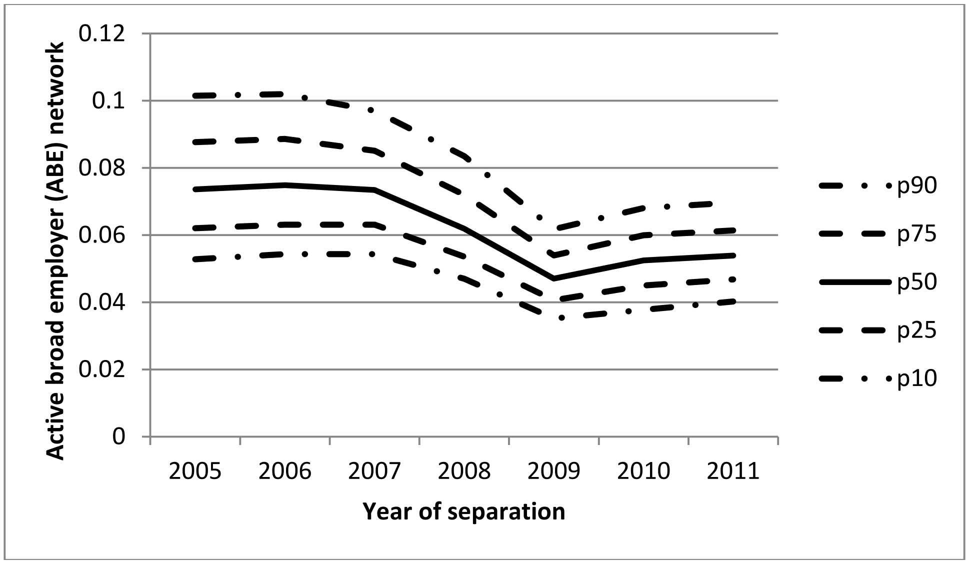
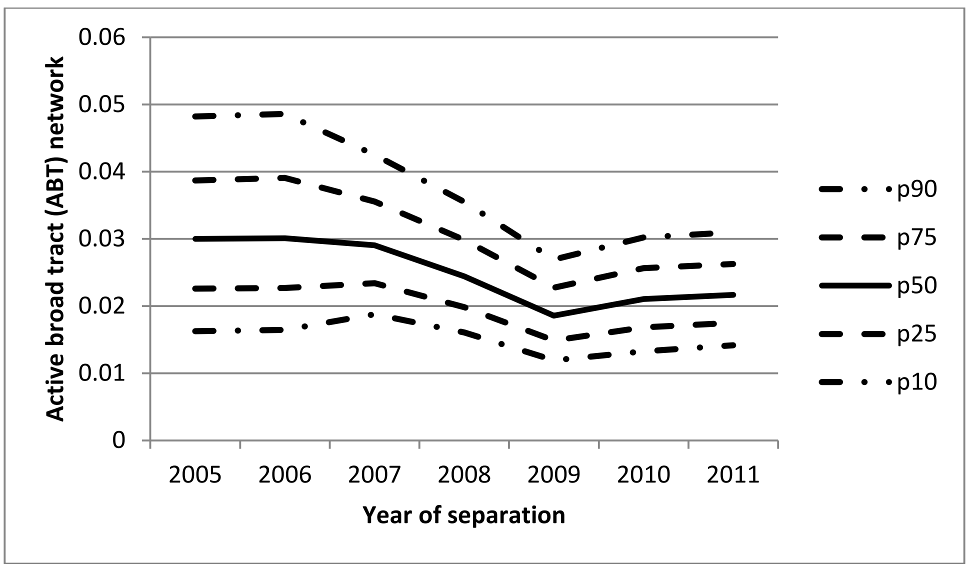
Figure 1a: Percentiles of distributions of broad network measures, by year

ABE



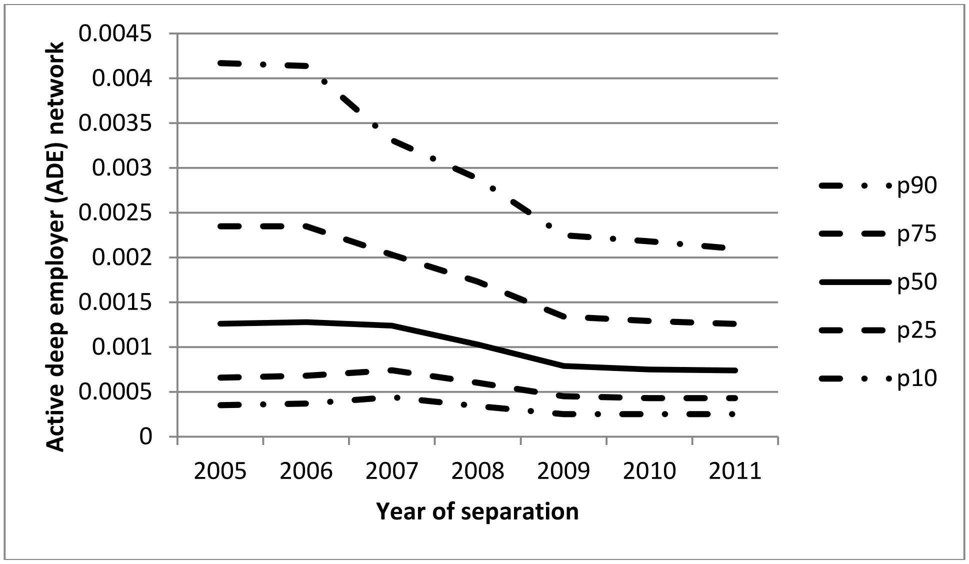
ABT



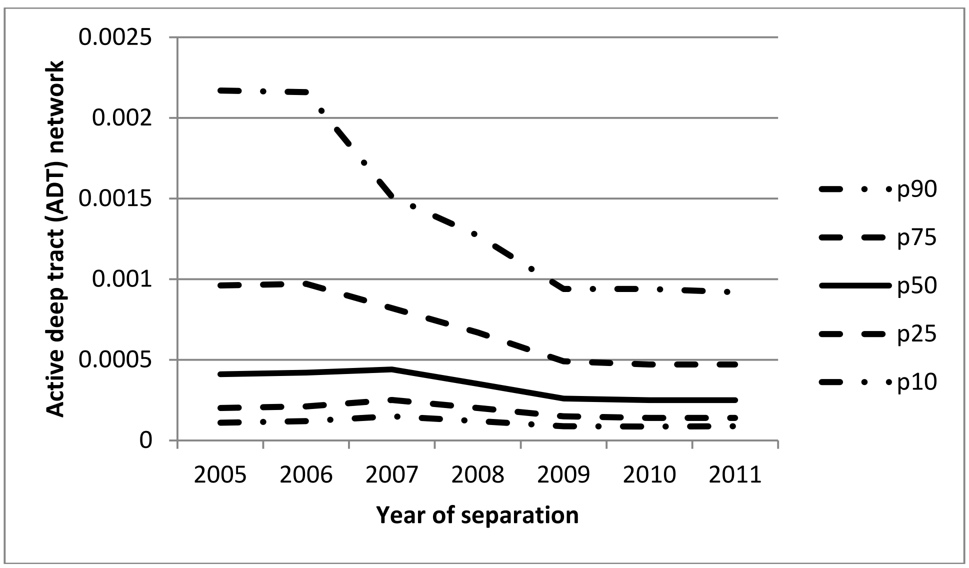
Notes: Calculations from LEHD data.

Figure 1b: Percentiles of distributions of deep network measures, by year

ADE

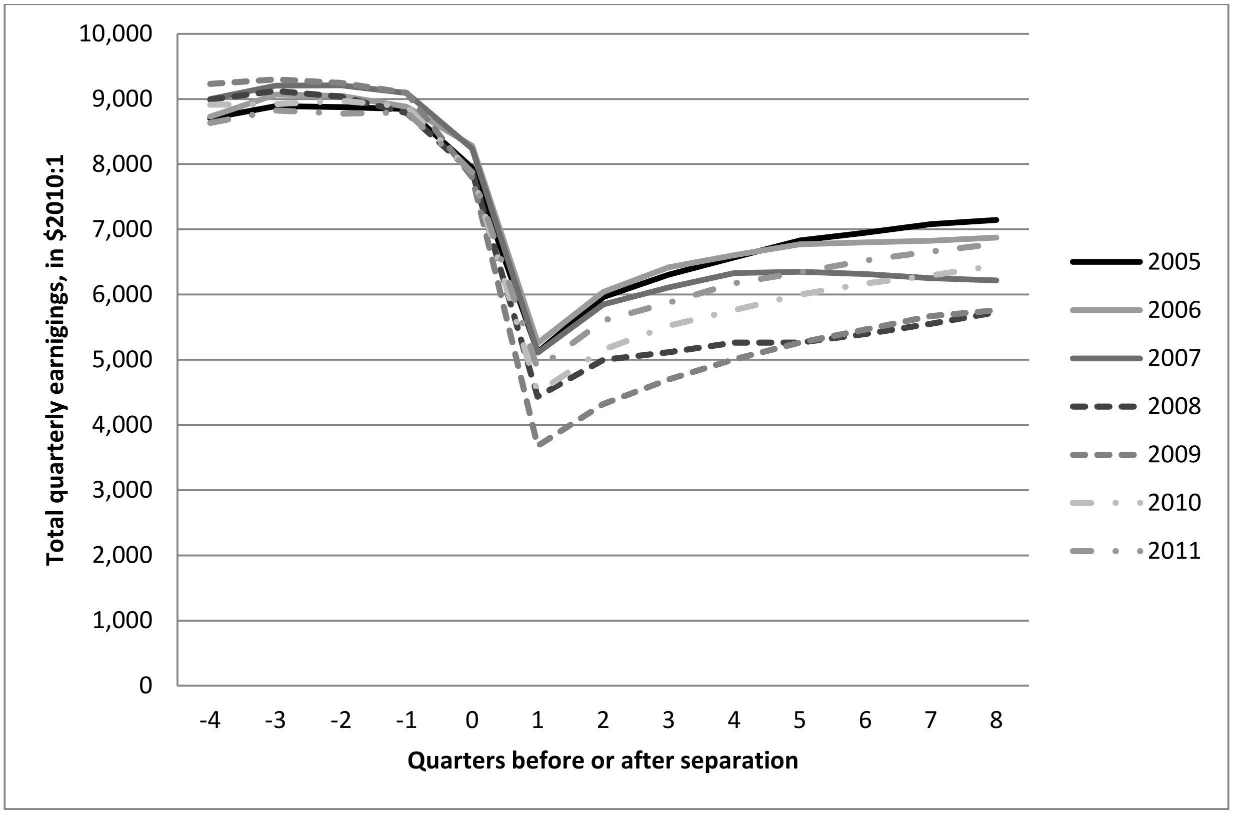


ADT



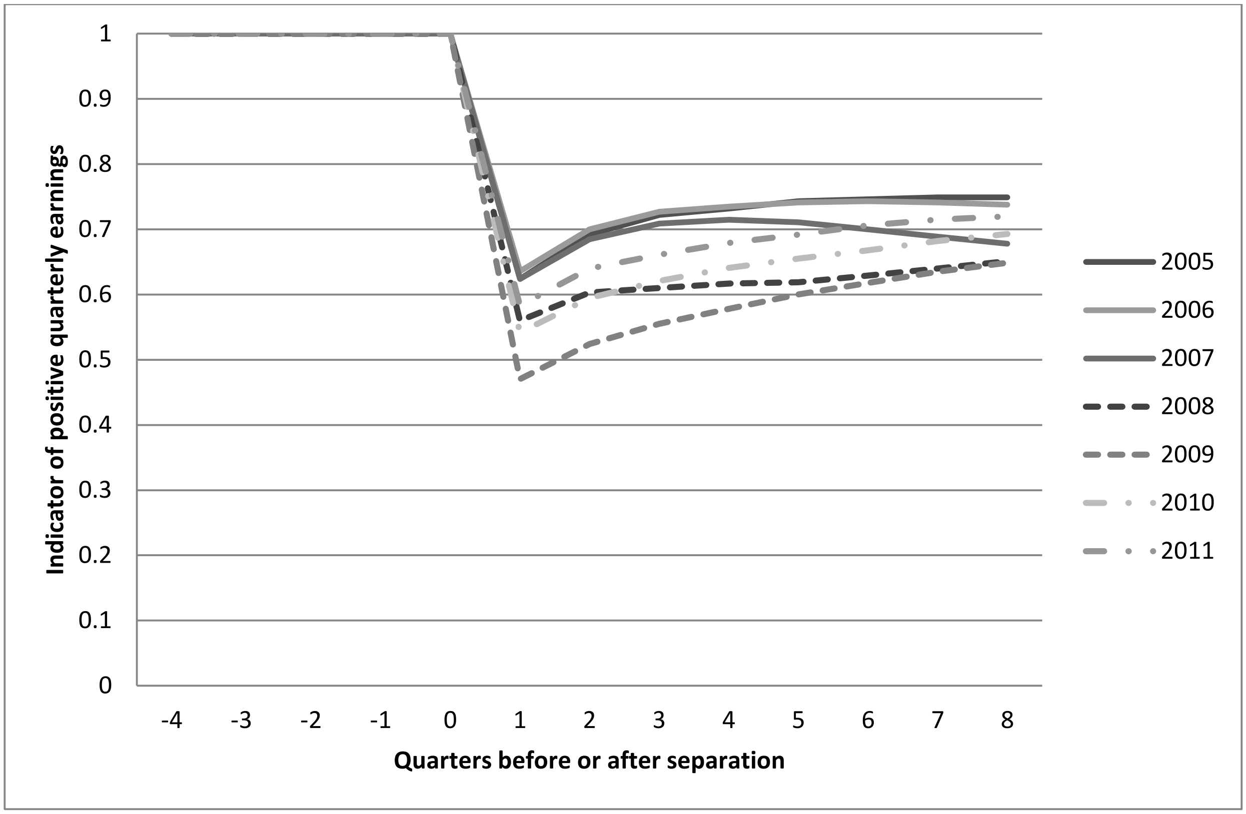
Notes: Calculations from LEHD data.

Figure 2: Earnings for displaced workers, by year of displacement



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

Figure 3: Employment rate for displaced workers, by year of displacement



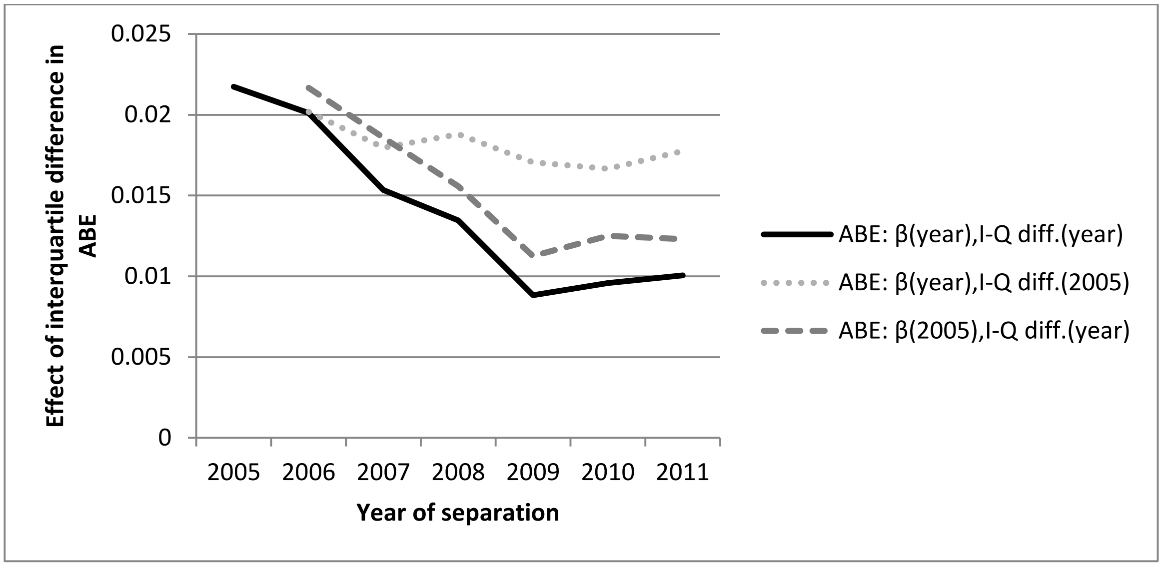
Notes: Calculations from LEHD. Employment status is defined as positive earnings during the quarter.

Figure 4: Earnings for displaced workers conditional on employment, by year of displacement

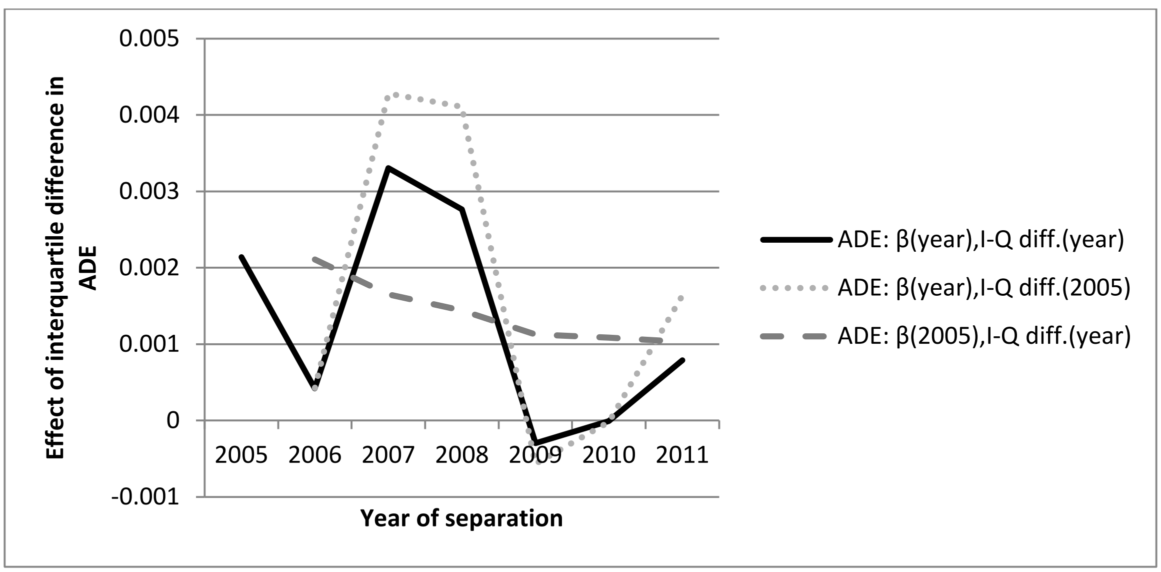
Notes: Calculations from LEHD. Earnings are in 2010:Q1 dollars.

Figure 5a: Effects of networks on employment status in quarter following displacement, low-earnings sample (pre-displacement earnings < $50,000), estimated interquartile effects and effects with network measures held fixed or network effects held fixed

ABE



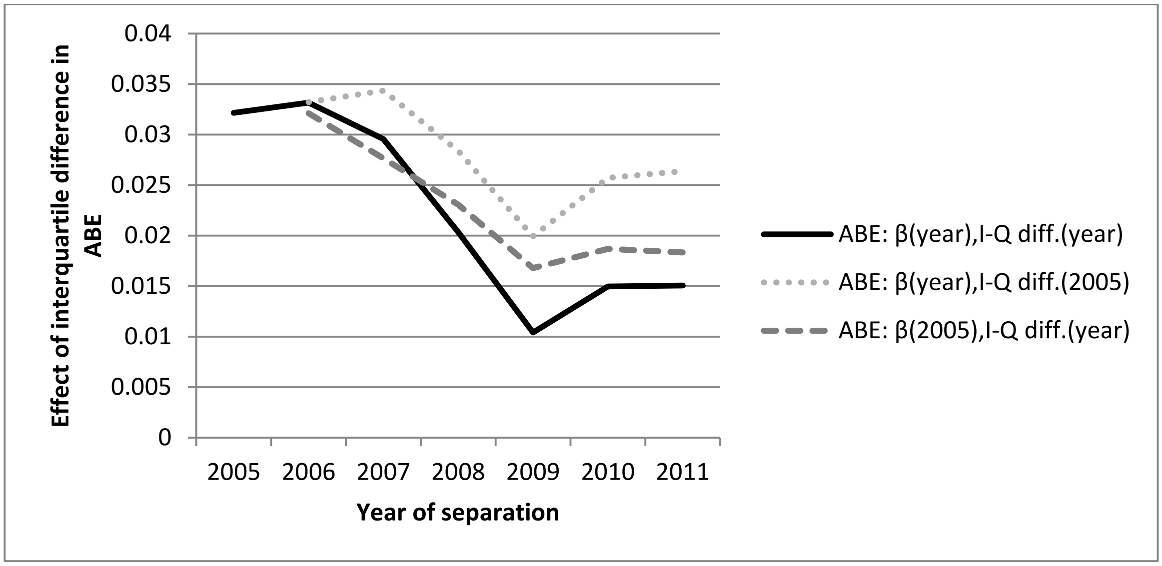
ADE



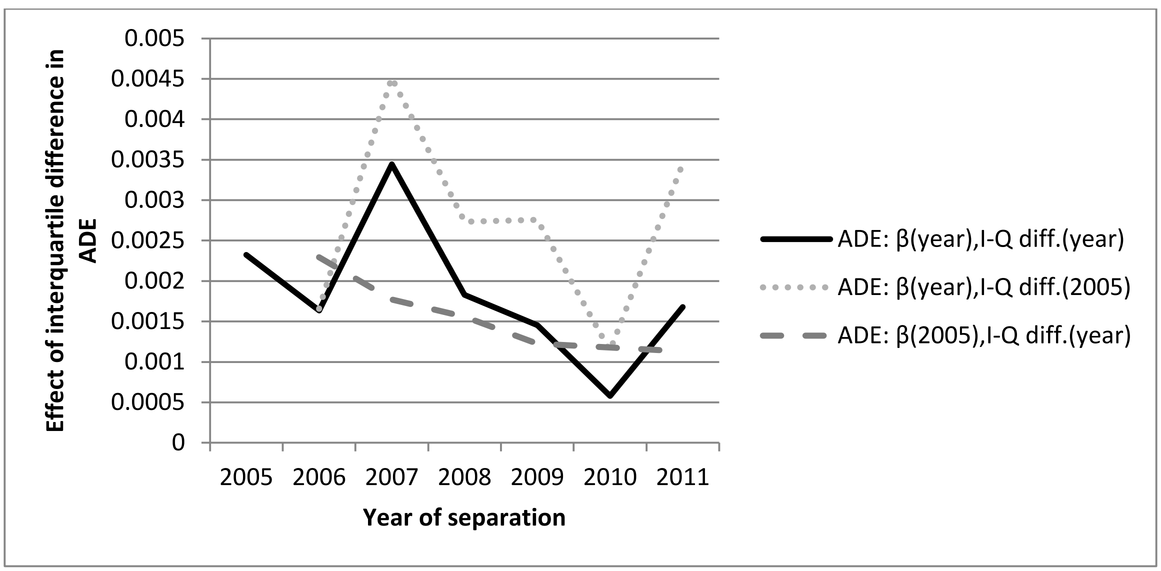
Notes: See notes to Table 4b. "Estimated interquartile effects" are reported in the second panel of the table. "Network effects" are the estimated coefficients in the first panel of the table. In the two sets of hypothetical estimates, the network measures or network effects are held at their 2005 values.

Figure 5b: Employment at a neighbor's employer in quarter following displacement, full sample, estimated interquartile effects and effects with network measures held fixed or network effects held fixed

ABE



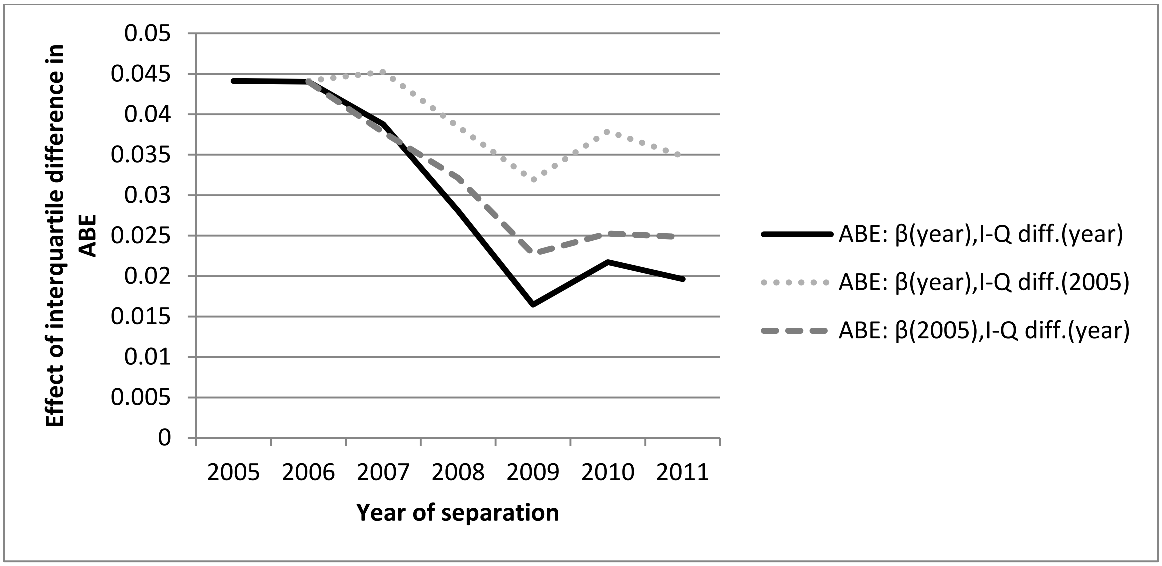
ADE



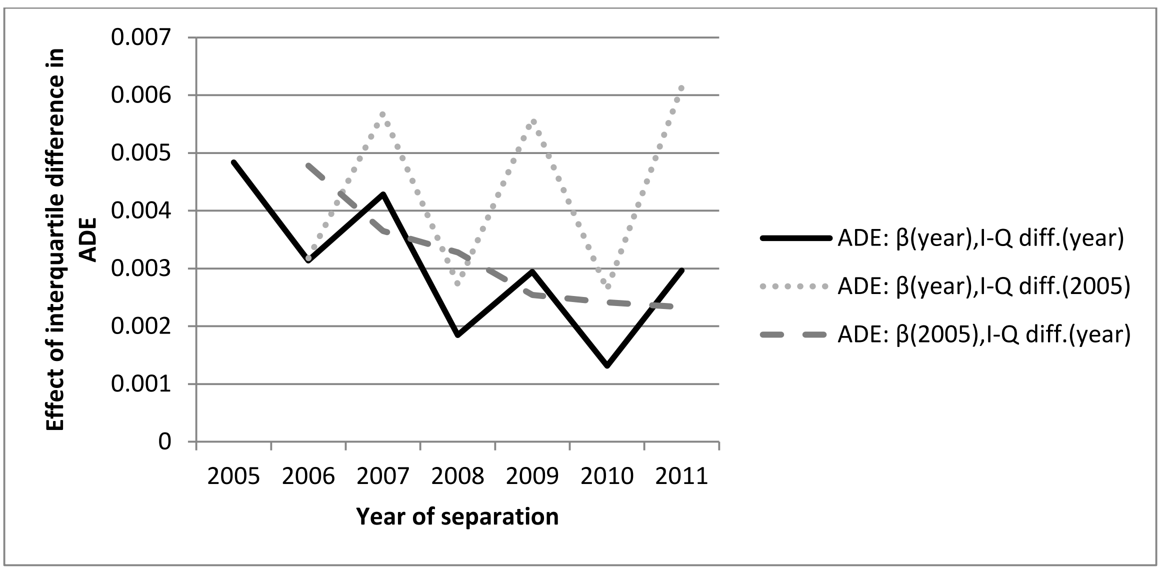
Notes: See notes to Figure 4a and Table 5a.

Figure 5c: Employment at a neighbor's employer in quarter following displacement, conditional on re-employment, estimated interquartile effects and effects with network measures held fixed or network effects held fixed

ABE



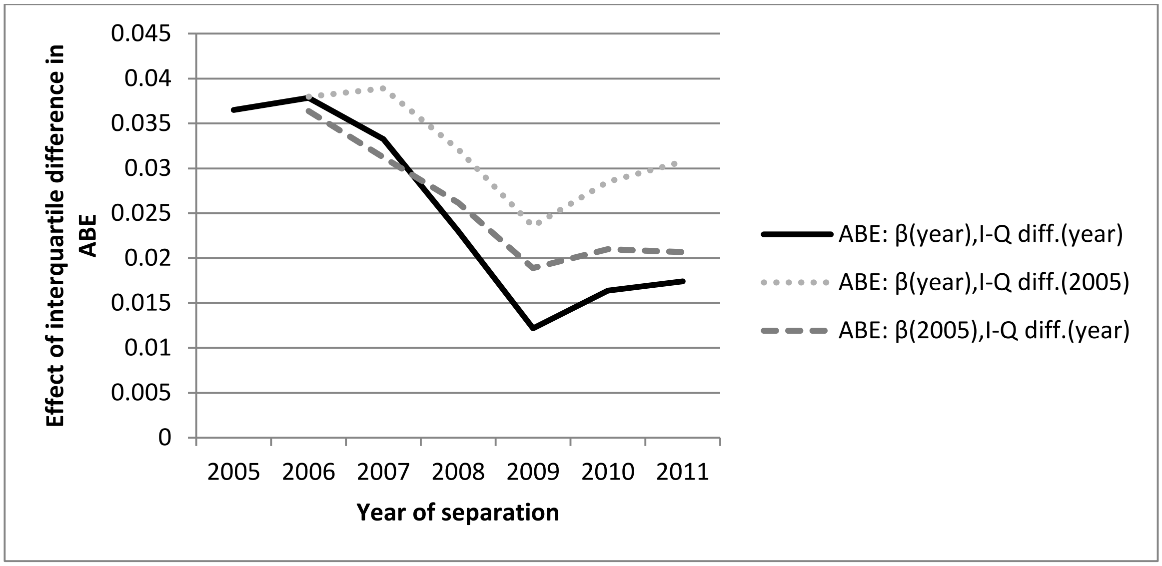
ADE



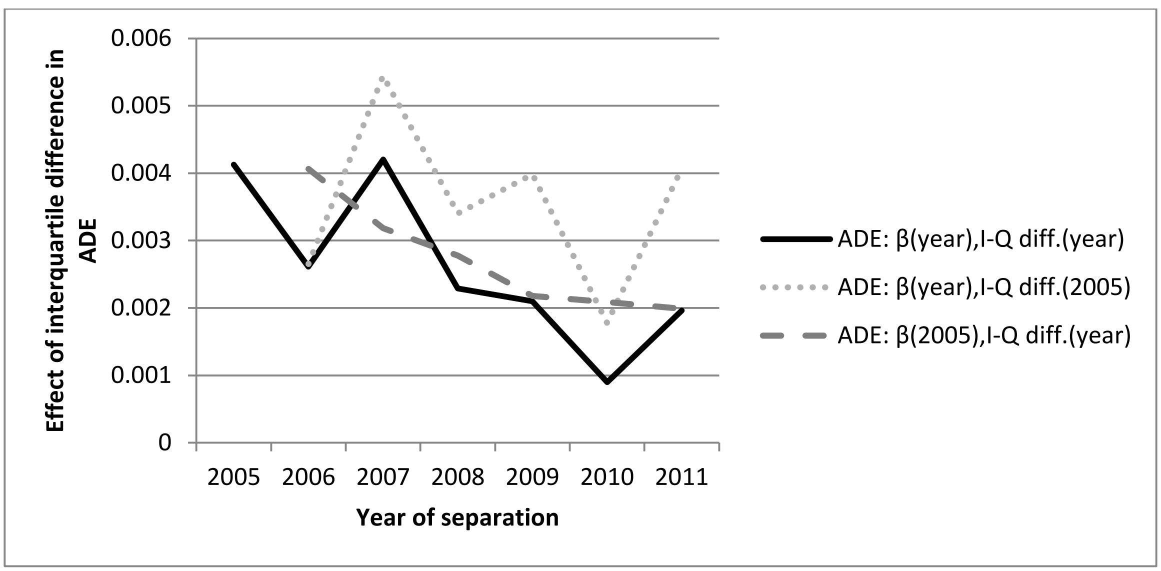
Notes: See notes to Figure 4a and Table 5b.

Figure 5d: Employment at a neighbor's employer in quarter following displacement, low-earnings sample (pre-displacement earnings < $50,000), estimated interquartile effects and effects with network measures held fixed or network effects held fixed

ABE



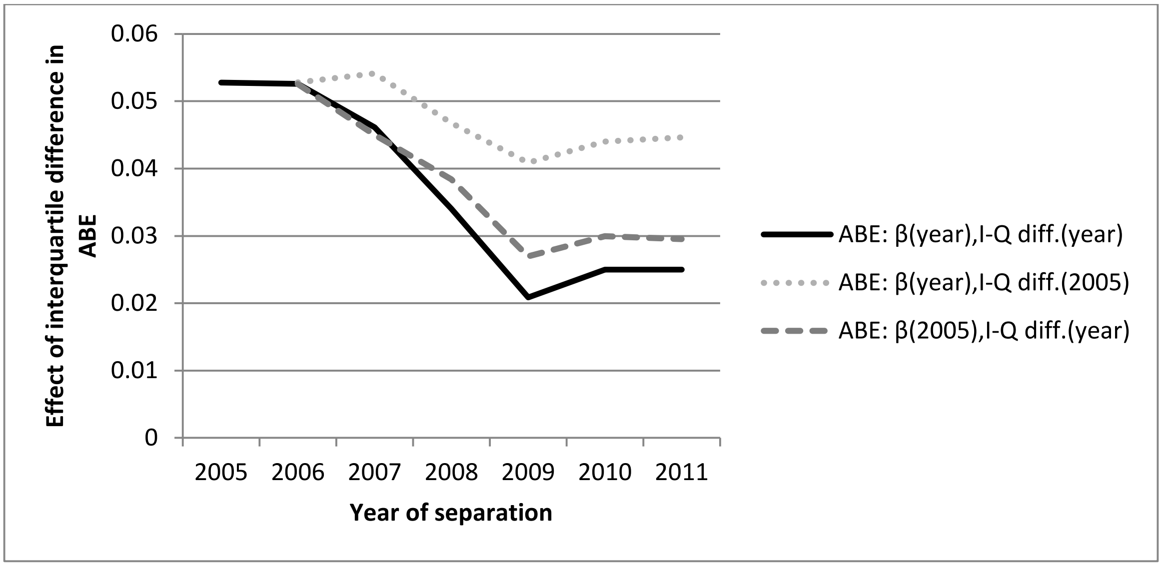
ADE



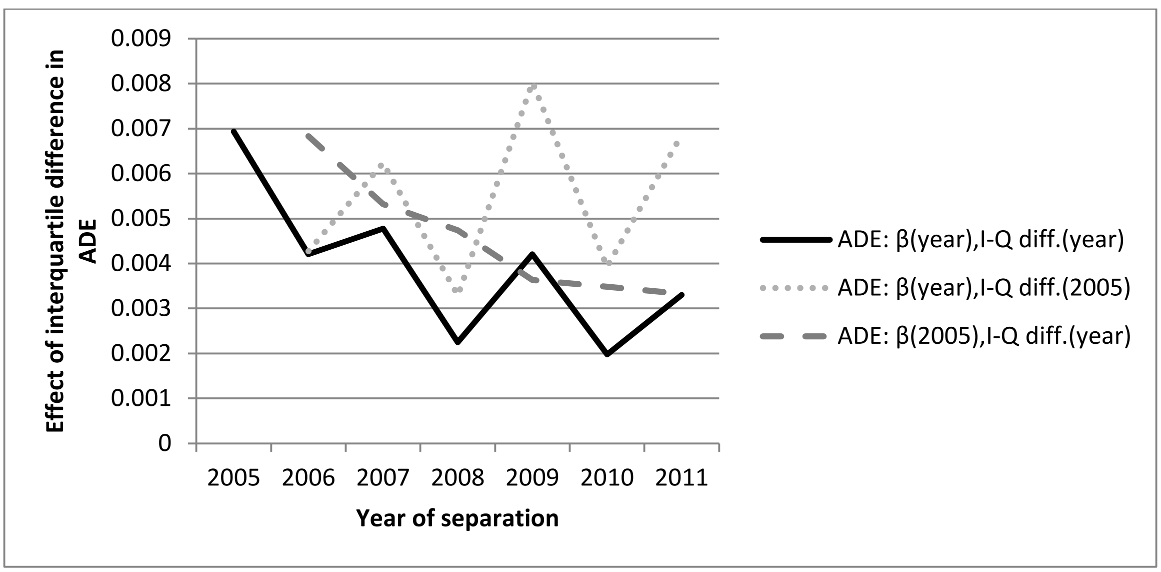
Notes: See notes to Figure 4a and Table 5c.

Figure 5e: Employment at a neighbor's employer in quarter following displacement, conditional on re-employment, low-earnings sample (pre-displacement earnings < $50,000), estimated interquartile effects and effects with network measures held fixed or network effects held fixed

ABE



ADE



Notes: See notes to Figure 4a and Table 5d.

Table 1: Sample means

Variable	Mean
Employment indicator in quarter after displacement	0.572
Earnings in quarter after displacement (1,000s 2010:Q1$)	4.651
Earnings at employer in previous year (1,000s 2010:Q1$)	34.486
Earnings from other jobs in previous year (1,000s 2010:Q1$)	1.462
Census tract poverty rate	0.131
Age 19 to 24	0.149
Age 25 to 34	0.296
Age 35 to 44	0.230
Age 45 to 54	0.202
Age 55 to 64	0.123
Female	0.460
Male	0.540
White non-Hispanic	0.537
Black non-Hispanic	0.186
Other race non-Hispanic	0.017
Asian non-Hispanic	0.058
Hispanic	0.203
Agriculture and mining (11,21)	0.009
Utility, wholesale, transportation (22,42,48-49)	0.082
Construction (23)	0.096
Manufacturing (31-33)	0.121
Retail, administrative, other services (44-45,56,81)	0.268
Professional services (51-55)	0.193
Education, health, public (61,62,92)	0.121
Local services (71,72)	0.111
Displaced in 2005	0.130
Displaced in 2006	0.136
Displaced in 2007	0.145
Displaced in 2008	0.188
Displaced in 2009	0.179
Displaced in 2010	0.114
Displaced in 2011	0.108
Displaced in quarter 1	0.226
Displaced in quarter 2	0.245
Displaced in quarter 3	0.254
Displaced in quarter 4	0.275
Observations (1,000s)	8,383

Notes: Calculations from LEHD data. NAICS industry sector code ranges are listed.

Table 2: Longitudinal variation in sample

Displacement (year) (1)	Observations (1,000s) (2)	Percent sample observations (3)	Layoff events (1,000s) (4)	Percent layoff events (5)	Average displaced workers per layoff event (6)	Average earnings at displaced job in previous year (7)	Average earnings at other jobs in previous year (8)	Employment rate in quarter after job loss (9)	Average earnings in quarter after job loss (10)
2005	1,087	13.0	268	12.7	107.9	33,796	1,516	0.624	5,117
2006	1,144	13.6	283	13.4	95.3	34,112	1,613	0.636	5,256
2007	1,212	14.5	307	14.5	73.9	34,916	1,582	0.624	5,110
2008	1,575	18.8	396	18.7	65.5	34,413	1,523	0.560	4,436
2009	1,502	17.9	377	17.8	56.2	35,498	1,378	0.470	3,677
2010	955	11.4	254	12.0	80.7	34,415	1,270	0.542	4,499
2011	909	10.8	231	10.9	91.3	33,741	1,287	0.583	4,861
All years	8,383	100.0	2,115	100.0	79.1	34,486	1,462	0.572	4,651

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

Table 3: Estimated effect of control variables on employment outcomes

Variable	Employment in quarter following displacement (1)	(2)
Active broad employer (*ABE*) network	2.453***	0.780***
Active broad tract (*ABT*) network	-0.980***	-0.353***
Active deep employer (*ADE*) network	2.739***	0.598**
Active deep tract (*ADT*) network	14.975***	4.574***
Poverty rate	-0.137***	-0.096***
Earnings ($1,000s) at employer in previous year	0.002***	0.003***
Earnings ($1,000s) from other jobs in previous year	0.014***	0.014***
Age 19 to 24	0.094***	0.089***
Age 25 to 34	0.045***	0.041***
Age 45 to 54	-0.049***	-0.042***
Age 55 to 64	-0.167***	-0.145***
Female	-0.001	-0.008***
Black non-Hispanic	-0.019***	-0.016***
Other race non-Hispanic	-0.019***	-0.010***
Asian non-Hispanic	-0.034***	-0.020***
Hispanic	-0.016***	-0.006***
Agriculture and mining (11,21)	-0.060***	
Utility, wholesale, transportation (22,42,48-49)	-0.043***	
Construction (23)	-0.076***	
Manufacturing (31-33)	-0.128***	
Retail, administrative, other services (44-45,56,81)	0.001	
Professional services (51-55)	-0.022***	
Education, health, public (61,62,92)	-0.061***	
Displaced in 2006	0.006	
Displaced in 2007	0.002	
Displaced in 2008	-0.032***	
Displaced in 2009	-0.082***	
Displaced in 2010	-0.022***	
Displaced in 2011	0.016***	
Quarter 2	0.001	
Quarter 3	-0.001	
Quarter 4	-0.008***	
Constant	0.416***	
Employer/year/quarter/county fixed effects	No	Yes
Number of fixed effects included (1,000s)		2,056
R-squared (within for fixed effects)	0.072	0.043
Observations (1,000s)	8,383	8,383
Mean of dependent variable	0.572	0.572

Notes: Employment estimates are from linear probability model for an indicator of employment. The establishment-level network measures are top-coded at the 99th percentile. The omitted indicators are for age 35 to 44, male, white non-Hispanic, local services (NAICS Sectors 71 and 72), and displacements occurring in 2005. Dummy variables for the quarter of displacement are included in the regressions but not reported. Robust standard errors, clustered by employer/year/quarter/county, are computed. Standard errors 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 4a: Effects of networks on employment status in quarter following displacement, full sample

Displacement years	2005-2011	2005	2006	2007	2008	2009	2010	2011
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Active broad employer (*ABE*) network	0.78***	0.85***	0.80***	0.76***	0.79***	0.65***	0.61***	0.73***
	(0.02)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.07)	(0.07)
Active broad tract (*ABT*) network	-0.35***	-0.20**	-0.41***	-0.39***	-0.49***	-0.49***	-0.42***	-0.43***
	(0.04)	(0.08)	(0.08)	(0.09)	(0.10)	(0.13)	(0.15)	(0.15)
Active deep employer (*ADE*) network	0.60**	0.63	-0.47	1.42**	1.63***	-0.22	-0.18	0.39
	(0.25)	(0.60)	(0.60)	(0.60)	(0.58)	(0.65)	(0.81)	(0.86)
Active deep tract (*ADT*) network	4.57***	5.43***	6.00***	1.01	3.79***	9.61***	3.52**	7.30***
	(0.47)	(1.07)	(1.06)	(1.20)	(1.19)	(1.37)	(1.67)	(1.75)
Interquartile effects								
Active broad employer (*ABE*) network	0.0194	0.0217	0.0205	0.0167	0.0145	0.0086	0.0091	0.0107
Active broad tract (*ABT*) network	-0.0044	-0.0032	-0.0067	-0.0048	-0.0049	-0.0038	-0.0037	-0.0038
Active deep employer (*ADE*) network	0.0007	0.0011	-0.0008	0.0018	0.0019	-0.0002	-0.0002	0.0003
Active deep tract (*ADT*) network	0.0023	0.0041	0.0046	0.0006	0.0018	0.0033	0.0012	0.0024
Worker control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employer/year/quarter/county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of fixed effects included (1,000s)	2,115	268	283	307	396	377	254	231
R-squared (within)	0.04	0.04	0.04	0.04	0.04	0.05	0.05	0.04
Observations (1,000s)	8,383	1,087	1,144	1,212	1,575	1,501	955	909

Notes: Robust standard errors in parentheses, clustered by employer/year/quarter/county. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Employment estimates are from linear probability model for an indicator of employment. The establishment-level network measures are top-coded at the 99^{th} percentile. The worker control variables are all the variables listed in Table 3, column (2).

Table 4b: Effects of networks on employment status in quarter following displacement, low-earnings sample (pre-displacement earnings < $50,000)

Displacement years	2005-2011	2005	2006	2007	2008	2009	2010	2011
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Active broad employer (*ABE*) network	0.74***	0.83***	0.77***	0.69***	0.72***	0.65***	0.64***	0.68***
	(0.02)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.08)	(0.08)
Active broad tract (*ABT*) network	-0.38***	-0.20**	-0.47***	-0.34***	-0.49***	-0.56***	-0.49***	-0.54***
	(0.04)	(0.09)	(0.09)	(0.11)	(0.12)	(0.15)	(0.18)	(0.18)
Active deep employer (*ADE*) network	1.17***	1.23*	0.24	2.45***	2.35***	-0.32	-0.01	0.94
	(0.29)	(0.69)	(0.69)	(0.70)	(0.67)	(0.77)	(0.96)	(1.01)
Active deep tract (*ADT*) network	3.51***	4.67***	4.51***	-0.19	2.45*	8.66***	3.53*	6.23***
	(0.55)	(1.24)	(1.25)	(1.41)	(1.39)	(1.60)	(1.93)	(2.05)
Interquartile effects								
Active broad employer (*ABE*) network	0.0189	0.0217	0.0201	0.0154	0.0135	0.0088	0.0096	0.0101
Active broad tract (*ABT*) network	-0.0048	-0.0033	-0.0078	-0.0042	-0.0050	-0.0045	-0.0043	-0.0048
Active deep employer (*ADE*) network	0.0015	0.0021	0.0004	0.0033	0.0028	-0.0003	0.0000	0.0008
Active deep tract (*ADT*) network	0.0018	0.0036	0.0035	-0.0001	0.0012	0.0031	0.0012	0.0021
Worker control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employer/year/quarter/county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of fixed effects included (1,000s)	1,852	235	249	270	349	328	220	201
R-squared (within)	0.04	0.04	0.04	0.04	0.04	0.05	0.04	0.04
Observations (1,000s)	6,447	847	885	926	1,216	1,136	733	705

Notes: See notes to Table 4a. *** p<0.01, ** p<0.05, * p<0.1.

Table 4c: Effects of networks on employment status in quarter following displacement, high-earnings sample (pre-displacement earnings ≥ $50,000)

Displacement years	2005-2011	2005	2006	2007	2008	2009	2010	2011
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Active broad employer (*ABE*) network	0.50***	0.48***	0.53***	0.53***	0.62***	0.34***	0.26*	0.58***
	(0.04)	(0.11)	(0.11)	(0.10)	(0.11)	(0.12)	(0.15)	(0.14)
Active broad tract (*ABT*) network	-0.12	-0.08	-0.10	-0.25	-0.31	-0.13	-0.14	0.10
	(0.08)	(0.17)	(0.15)	(0.19)	(0.20)	(0.25)	(0.31)	(0.30)
Active deep employer (*ADE*) network	-0.86	-1.01	-2.47*	-1.51	-1.16	1.12	0.73	-1.47
	(0.53)	(1.37)	(1.33)	(1.28)	(1.25)	(1.38)	(1.67)	(1.75)
Active deep tract (*ADT*) network	6.39***	6.36***	8.39***	3.30	9.32***	6.64**	4.52	10.43***
	(1.00)	(2.43)	(2.25)	(2.55)	(2.54)	(2.83)	(3.70)	(3.62)
Interquartile effects								
Active broad employer (*ABE*) network	0.0113	0.0110	0.0120	0.0107	0.0104	0.0043	0.0037	0.0080
Active broad tract (*ABT*) network	-0.0015	-0.0013	-0.0017	-0.0029	-0.0029	-0.0010	-0.0011	0.0008
Active deep employer (*ADE*) network	-0.0009	-0.0016	-0.0038	-0.0017	-0.0012	0.0009	0.0006	-0.0011
Active deep tract (*ADT*) network	0.0028	0.0044	0.0059	0.0017	0.0040	0.0021	0.0013	0.0031
Worker control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employer/year/quarter/county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of fixed effects included (1,000s)	722	90	96	105	136	134	85	76
R-squared (within)	0.04	0.04	0.04	0.04	0.04	0.05	0.04	0.04
Observations (1,000s)	1,935	240	259	286	358	365	223	204

Notes: See notes to Table 4a. *** p<0.01, ** p<0.05, * p<0.1.

Table 5a: Effects of networks on employment at a neighbor's employer in quarter following displacement, full sample

Displacement years	2005-2011	2005	2006	2007	2008	2009	2010	2011
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Active broad employer (*ABE*) network	1.16***	1.26***	1.30***	1.34***	1.11***	0.78***	1.00***	1.03***
	(0.02)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.06)	(0.06)
Active broad tract (*ABT*) network	-4.98***	-4.90***	-5.06***	-4.76***	-4.88***	-4.81***	-5.01***	-5.27***
	(0.04)	(0.09)	(0.08)	(0.09)	(0.10)	(0.12)	(0.13)	(0.14)
Active deep employer (*ADE*) network	1.62***	1.37**	0.98*	2.66***	1.61***	1.63***	0.67	2.04***
	(0.22)	(0.55)	(0.55)	(0.54)	(0.51)	(0.51)	(0.68)	(0.73)
Active deep tract (*ADT*) network	15.88***	20.28***	21.08***	10.63***	13.44***	11.43***	11.97***	13.03***
	(0.48)	(1.14)	(1.00)	(1.10)	(1.50)	(1.07)	(1.59)	(1.52)
Interquartile effects								
Active broad employer (*ABE*) network	0.0288	0.0322	0.0332	0.0296	0.0204	0.0104	0.0150	0.0151
Active broad tract (*ABT*) network	-0.0621	-0.0789	-0.0830	-0.0580	-0.0489	-0.0378	-0.0441	-0.0462
Active deep employer (*ADE*) network	0.0019	0.0023	0.0016	0.0034	0.0018	0.0015	0.0006	0.0017
Active deep tract (*ADT*) network	0.0079	0.0153	0.0160	0.0060	0.0064	0.0040	0.0040	0.0043
Worker control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employer/year/quarter/county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of fixed effects included (1,000s)	2,115	268	283	307	396	377	254	231
R-squared (within)	0.01	0.02	0.02	0.01	0.01	0.01	0.01	0.01
Observations (1,000s)	8,383	1,087	1,144	1,212	1,575	1,501	955	909

Notes: Robust standard errors in parentheses, clustered by employer/year/quarter/county. *** p<0.01, ** p<0.05, * p<0.1. Employment estimates are from linear probability model for an indicator of employment. The establishment-level network measures are top-coded at the 99th percentile. The worker control variables are all the variables listed in Table 3, column (2), with the exception of the year controls (which here are subsumed into the employer/year/quarter/county fixed effects).

Table 5b: Effects of networks on employment at a neighbor's employer in quarter following displacement, conditional on re-employment

Displacement years	2005-2011	2005	2006	2007	2008	2009	2010	2011
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Active broad employer (*ABE*) network	1.60***	1.71***	1.71***	1.76***	1.49***	1.24***	1.47***	1.35***
	(0.03)	(0.07)	(0.07)	(0.07)	(0.08)	(0.09)	(0.10)	(0.11)
Active broad tract (*ABT*) network	-8.07***	-7.79***	-7.76***	-7.46***	-8.35***	-9.82***	-8.85***	-8.73***
	(0.06)	(0.11)	(0.12)	(0.14)	(0.17)	(0.24)	(0.23)	(0.24)
Active deep employer (*ADE*) network	2.63***	2.81***	1.85**	3.30***	1.58*	3.25***	1.53	3.58***
	(0.35)	(0.82)	(0.83)	(0.83)	(0.84)	(1.03)	(1.19)	(1.18)
Active deep tract (*ADT*) network	21.51***	27.21***	27.72***	16.38***	19.27***	15.30***	18.29***	16.14***
	(0.77)	(1.64)	(1.45)	(1.67)	(2.90)	(2.11)	(2.61)	(2.46)
Interquartile effects								
Active broad employer (*ABE*) network	0.0406	0.0441	0.0441	0.0388	0.0280	0.0165	0.0217	0.0196
Active broad tract (*ABT*) network	-0.1033	-0.1285	-0.1298	-0.0908	-0.0844	-0.0776	-0.0778	-0.0765
Active deep employer (*ADE*) network	0.0033	0.0048	0.0031	0.0043	0.0018	0.0029	0.0013	0.0030
Active deep tract (*ADT*) network	0.0112	0.0211	0.0216	0.0094	0.0095	0.0055	0.0061	0.0054
Worker control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employer/year/quarter/county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of fixed effects included (1,000s)	1,597	215	229	246	298	252	182	174
R-squared (within)	0.02	0.04	0.04	0.02	0.02	0.02	0.01	0.01
Observations (1,000s)	4,798	678	727	756	883	706	518	530

Notes: See notes to Table 5a. *** p<0.01, ** p<0.05, * p<0.1.

Table 5c: Effects of networks on employment at a neighbor's employer in quarter following displacement, low-earnings sample (pre-displacement earnings < $50,000)

Displacement years	2005-2011	2005	2006	2007	2008	2009	2010	2011
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Active broad employer (*ABE*) network	1.29***	1.40***	1.46***	1.49***	1.23***	0.90***	1.09***	1.18***
	(0.02)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.07)	(0.07)
Active broad tract (*ABT*) network	-4.98***	-4.90***	-5.19***	-4.69***	-4.83***	-4.79***	-4.89***	-5.28***
	(0.04)	(0.09)	(0.09)	(0.11)	(0.11)	(0.12)	(0.15)	(0.16)
Active deep employer (*ADE*) network	2.14***	2.36***	1.52**	3.12***	1.95***	2.28***	1.02	2.33***
	(0.25)	(0.64)	(0.64)	(0.64)	(0.59)	(0.60)	(0.80)	(0.86)
Active deep tract (*ADT*) network	16.47***	19.56***	20.63***	12.75***	14.40***	11.98***	13.85***	15.23***
	(0.55)	(1.28)	(1.18)	(1.30)	(1.72)	(1.28)	(1.81)	(1.79)
Interquartile effects								
Active broad employer (*ABE*) network	0.0329	0.0365	0.0379	0.0333	0.0230	0.0122	0.0164	0.0174
Active broad tract (*ABT*) network	-0.0628	-0.0793	-0.0859	-0.0577	-0.0492	-0.0381	-0.0436	-0.0469
Active deep employer (*ADE*) network	0.0027	0.0041	0.0026	0.0042	0.0023	0.0021	0.0009	0.0020
Active deep tract (*ADT*) network	0.0329	0.0365	0.0379	0.0333	0.0230	0.0122	0.0164	0.0174
Worker control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employer/year/quarter/county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of fixed effects included (1,000s)	1,852	235	249	270	349	328	220	201
R-squared (within)	0.01	0.02	0.02	0.01	0.01	0.01	0.01	0.01
Observations (1,000s)	6,447	847	885	926	1,216	1,136	733	705

Notes: See notes to Table 5a. *** p<0.01, ** p<0.05, * p<0.1.

Table 5d: Effects of networks on employment at a neighbor's employer in quarter following displacement, low-earnings sample (pre-displacement earnings < $50,000), conditional on re-employment

Displacement years	2005-2011	2005	2006	2007	2008	2009	2010	2011
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Active broad employer (*ABE*) network	1.89***	2.01***	2.01***	2.06***	1.78***	1.55***	1.67***	1.70***
	(0.04)	(0.09)	(0.08)	(0.09)	(0.09)	(0.11)	(0.12)	(0.13)
Active broad tract (*ABT*) network	-8.32***	-8.08***	-8.15***	-7.58***	-8.54***	-10.14***	-8.93***	-9.18***
	(0.07)	(0.13)	(0.13)	(0.16)	(0.19)	(0.26)	(0.28)	(0.28)
Active deep employer (*ADE*) network	3.18***	3.90***	2.40**	3.51***	1.85*	4.52***	2.21	3.87***
	(0.42)	(0.97)	(0.97)	(0.98)	(0.99)	(1.25)	(1.45)	(1.43)
Active deep tract (*ADT*) network	23.33***	27.09***	27.67***	20.72***	21.37***	17.69***	22.76***	20.45***
	(0.92)	(1.89)	(1.74)	(1.99)	(3.44)	(2.58)	(3.08)	(2.98)
Interquartile effects								
Active broad employer (*ABE*) network	0.0494	0.0528	0.0526	0.0461	0.0340	0.0209	0.0250	0.0250
Active broad tract (*ABT*) network	-0.1080	-0.1337	-0.1376	-0.0934	-0.0878	-0.0813	-0.0796	-0.0816
Active deep employer (*ADE*) network	0.0041	0.0069	0.0042	0.0048	0.0022	0.0042	0.0020	0.0033
Active deep tract (*ADT*) network	0.0127	0.0216	0.0223	0.0124	0.0110	0.0066	0.0081	0.0071
Worker control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employer/year/quarter/county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of fixed effects included (1,000s)	1,349	184	196	210	253	207	151	147
R-squared (within)	0.02	0.04	0.04	0.02	0.02	0.02	0.02	0.02
Observations (1,000s)	3,584	516	551	566	660	512	383	397

Notes: See notes to Table 5a. *** p<0.01, ** p<0.05, * p<0.1.

Table A1: Sample composition

Displacement year	2005	2006	2007	2008	2009	2010	2011	All
Variable								
Sex								
Male	50.8	52.6	53.2	56.1	57.5	53.3	52.2	54.0
Female	49.2	47.4	46.8	43.9	42.5	46.7	47.8	46.0
Age								
19 to 24	16.3	16.2	15.5	14.8	13.5	14.1	14.0	14.9
25 to 34	29.5	29.4	30.0	29.6	29.0	30.0	30.2	29.6
35 to 45	23.9	23.7	23.3	23.0	22.9	22.2	21.9	23.0
45 to 54	19.4	19.6	19.8	20.4	21.2	20.4	20.2	20.2
55 to 64	10.9	11.2	11.5	12.2	13.4	13.3	13.6	12.3
Race/ethnicity								
White	53.5	53.6	54.1	53.2	53.6	54.4	53.6	53.7
Black	20.6	18.8	18.2	18.4	17.5	18.2	18.9	18.6
Other	1.6	1.6	1.6	1.7	1.7	1.7	1.7	1.7
Asian	5.8	5.6	5.6	5.8	6.2	5.7	5.5	5.8
Hispanic	18.6	20.4	20.6	20.8	21.0	20.1	20.3	20.3
Industry (NAICS Sector)								
Agriculture and mining (11,21)	0.9	0.9	0.8	0.8	1.1	1.1	1.1	1.0
Utility, wholesale, transportation (22,42,48-49)	8.2	8.0	7.3	8.3	9.1	8.2	7.8	8.2
Construction (23)	6.8	8.6	10.4	10.9	11.2	9.7	8.1	9.6
Manufacturing (31-33)	11.5	11.5	11.9	13.9	15.2	9.4	8.0	12.1
Retail, administrative, other services (44-45,56,81)	26.9	28.0	26.1	29.3	26.0	23.9	25.7	26.8
Professional services (51-55)	18.1	19.1	20.7	18.3	19.4	20.2	19.3	19.3
Education, health, public (61,62,92)	14.9	12.4	12.2	9.0	8.8	14.3	16.9	12.1
Local services (71,72)	12.7	11.6	10.6	9.5	9.2	13.1	13.2	11.1
Previous year earnings (in 2010:Q1 $)								
< $25,000	38.7	37.7	35.8	37.1	35.4	39.1	40.7	37.5
$25,000 to $50,000	39.3	39.6	40.6	40.2	40.3	37.6	36.8	39.4
$50,000 to $75,000	15.4	16.0	16.5	15.9	16.8	15.8	15.3	16.0
> $75,000	6.6	6.7	7.1	6.8	7.6	7.5	7.2	7.1
Sample (1,000s)	1,087	1,144	1,212	1,575	1,502	955	909	8,383
Sample share	12.967	13.647	14.458	18.788	17.917	11.392	10.843	100.000

Notes: Calculations from LEHD data.

Table A2: Distribution of network measures, by percentiles

Network measure	p10	p25	p50	p75	p90
Active broad employer (ABE) network	0.04189	0.05000	0.06078	0.07481	0.08980
Active broad tract (ABT) network	0.01437	0.01854	0.02407	0.03102	0.03917
Active deep employer (ADE) network	0.00031	0.00055	0.00098	0.00175	0.00306
Active deep tract (ADT) network	0.00010	0.00018	0.00033	0.00067	0.00139

Notes: Calculations from LEHD data.