Finance and Economics Discussion Series

Federal Reserve Board, Washington, D.C.
ISSN 1936-2854 (Print)
ISSN 2767-3898 (Online)

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2023-043

Please cite this paper as:
Ost, Ben, Weixiang Pan, and Douglas Webber (2023). "College Networks and Re-employment of Displaced Workers," Finance and Economics Discussion Series 2023-043. Washington: Board of Governors of the Federal Reserve System, https://doi.org/10.17016/FEDS.2023.043.

College Networks and Re-employment of Displaced Workers

Ben Ost[*], Weixiang Pan[†] and Douglas Webber[‡]

Abstract

We provide the first evidence on the role of college networks in the re-employment of displaced workers. An extensive literature examines the consequences of layoffs, but the factors which facilitate re-employment are relatively under-studied. Using administrative data and a cross-cohort design, we find that network connections with actively-hiring employers increase the re-employment rate. This result is driven by re-employment at contact's firms suggesting that a stronger network does not improve worker quality more broadly. These results suggest that college has the potential to improve employment outcomes beyond improved human capital and signaling.

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1. Introduction

An extensive literature examines the effect of job displacement on the earnings in both the short and long-run (Jacobson, LaLonde, and Sullivan 1993; Couch and Placzek 2010; Jacobson, LaLonde, and G. Sullivan 2005). These studies show that displaced workers recover most of their earnings within a few quarters but fail to return to their pre-displacement earnings level years after the initial separations. Though the literature on job displacement is large, few studies examine factors that facilitate re-employment following layoff. Studying how displaced workers find their first jobs helps understand the challenges faced by these workers, and can provide guidance on how best to target resources to those recently laid off.

In this study, we provide the first evidence on the effect of networks formed in college on the re-employment of laid-off workers. Specifically, we examine whether displaced workers are more likely to find employment when the firms that employ their contacts from college are actively hiring. Using administrative data from the state of Ohio linking higher education to a database of matched employer-employee Unemployment Insurance (UI) records, we focus on a sample of displaced workers who lost jobs in mass layoff events. We define one's college contacts as the group of individuals who had first enrolled in the same college, with the same major and in the same semester.[1] A displaced worker's *employer network* includes their college contacts' employers at the time of displacement. Adopting the Active Employer Network (AEN) measure from Hellerstein et al. (2019), we examine how the hiring rate of the employer network at the time of displacement predicts re-employment.

[1] Focusing on students who enter college with a declared major helps mitigate concerns that major declarations are caused by cohort peers.

To isolate causal relationships, we compare workers laid off at the same firm who differ in their network quality. However, variation in network quality within a firm is still likely to be endogenous, as workers from different majors and institutions may vary in both their network quality and other factors that determine job search outcomes. To address this issue, we exploit cross-cohort variation in network composition within institution-major combinations. While sorting into institutions and majors is likely endogenous, the variation between cohorts is more likely to be uncorrelated with student characteristics and unobserved determinants of outcomes (Hoxby 2000; Carrell and Hoekstra 2010; Anelli and Peri 2017). Intuitively, college contacts in the same institution-major combination but different cohorts can be seen as "control connections". On average, students build stronger bonds if they are from the same cohort than they do if they are from different cohorts. If the "control connections" also have effects on the outcomes (job seekers can obtain job information from contacts from different cohorts of the same program), the estimates are biased in the direction that underestimates the importance of college networks.

The key identification assumption is that unobserved determinants of re-employment are conditionally orthogonal to variation in network strength within university majors. We provide several pieces of evidence in favor of this assumption. First, similar to an event study, we show that network quality of nearby cohorts have no discernable effects on re-employment outcomes. Second, we show that network quality strongly predicts re-employment at in-network firms, but it does not predict higher re-employment at out-of-network firms. This bolsters internal validity because it suggests that network quality is not correlated with unobservable factors that increase general employability.[2] Third, we show that network quality measures are conditionally uncorrelated with a variety of observable characteristics. Finally, our results are robust to controlling for arbitrary major- and institution-level changes in cohort quality (e.g. including institution-by-year-of-entry FE and major-by-year-of-entry FE).

We find that displaced workers who have more actively-hiring employer networks have a higher average rate of re-employment in the quarter following displacement. Specifically, a 1 standard deviation improvement in our measure of network strength increases the re-employment rate by approximately 1 percentage point. This effect is driven by network members who shared a course section and is stronger among students who appear to be more engaged with college (e.g. students with higher GPAs).

Connection to an actively-hiring employer network is expected to primarily affect workers by reducing the time it takes to find re-employment, but it also has the potential to affect earnings and employer quality. This would occur if workers re-optimize based on the quality of their network as would be the case in a traditional job search model. We find some evidence that earnings increase as a result of a more actively-hiring employer network, but find no evidence that workers find re-employment at higher quality firms as measured by their employer premium.[3] We also investigate whether the quality of firms in one's network also predicts re-employment outcomes. We refer to this measure of network quality as the network firm premium (NFP) hereafter. In contrast to our results

[2] It remains possible that network quality is correlated with unobservables that are only valued by in-network firms. To assess the likelihood of this possibility, we examine whether network quality increases re-employment at employers of students in other cohorts from the same major-institution.
[3] Employer premia are estimated following Abowd, Kramarz and Margolis (1999) (AKM hereafter).

for the hiring activity of one's network (AEN), the NFP has no effect on the re-employment rate, but it increases the firm-premium of the re-employment firm.

The literature on the returns to college has generally focused on human capital and signaling with a much smaller literature investigating networking. Importantly, networking is a distinct channel as it can lead to improved labor market outcomes, holding fixed skill (and firm's perceptions of skill). The existing literature on college networks is almost entirely focused on networks in elite contexts such as elite universities, CEOs and boards of directors (Marmaros and Sacerdote 2002; Shue 2013; Kramarz and Thesmar 2013; Zimmerman 2019; Michelman, Price, and Zimmerman 2021). Our study shows that interactions among college contacts are not limited to the narrow circle of elites but are part of a general labor market phenomenon. In this regard, our study is closely related to Zhu (forthcoming) that finds that sharing a class in community college increases the probability of working at the same firm. We replicate Zhu's finding that employment outcomes are clustered within networks and this motivates our central analysis on the effect of network quality.[4]

Unlike much of the literature on college networks, our data include detailed information on the hiring rate of firms in one's college network, facilitating our emphasis on firm-centered metrics of network strength. Firm hiring is a natural component of network quality, but most of the literature has focused more on the characteristics of one's contacts or the strength of network ties (Granovetter 1977; Calvó-Armengol and Jackson 2007; Kramarz and Skans 2014; Gee, Jones, and Burke 2017). Particularly for

[4] The clustering of outcomes within networks establishes the existence of network effects, but is not informative regarding the effect of network quality.

displaced workers, whether a contact is a strong or weak tie may be less consequential than whether the contact's firm is actively hiring at the time of displacement.

Our paper is also related to the literature on how labor market networks help displaced workers obtain re-employment. A broad literature illustrates that social networks improve job-finding prospects on average (Calvó-Armengol and Jackson 2007; Topa 2001; Bayer, Ross, and Topa 2008; Beaman 2012; Hellerstein, McInerney, and Neumark 2011; Kramarz and Skans 2014). In this literature, the most closely connected paper is Hellerstein, Kutzbach, and Neumark (2019) that also examines the effect of the hiring rate of firms in one's network on re-employment following displacement. Hellerstein et al. (2019), however, study networks based on individuals who live in the same census tract (approximately 4,000 individuals per network), whereas we study the role of college-major-cohort based networks.

A limitation of our analysis is that network connections are measured imperfectly. As in most studies on labor market networks, we do not observe the actual connections through which job opportunities flow and it is likely that many of the "ties" do not yield any bond or information flow. Many papers examine networks formed on other dimensions and it is worth discussing whether the network connections we study are expected to be similar.[5] Compared to studies that use measures of friends, online social networks or family, our networks involve weaker ties and it is less likely that any individual "tie" is a real bond. In contrast, our network measure is likely more connected

[5] Researchers study labor market networks in other forms of social groups such as neighbors (Topa 2001; Bayer, Ross, and Topa 2008; Hellerstein, McInerney, and Neumark 2011; Schmutte 2015), ethnic groups (Munshi 2003; Beaman 2012; Dustmann et al. 2016), friends (Cappellari and Tatsiramos 2015), family members (Magruder 2010; Kramarz and Skans 2014), former coworkers (Bandiera, Barankay, and Rasul 2009; Cingano and Rosolia 2012), and online social networks (Gee, Jones, and Burke 2017).

than networks based on residence in the same Census tract or broad ethnic groups, especially when these groups are defined coarsely. Importantly, students from the same major-institution are likely to have similar professional experiences and this may increase the value of this type of network compared to say, Census tracts or ethnic groups.

2. Conceptual framework

Network effects have the potential to influence re-employment either through direct peer effects or through firm connections. Direct peer effects are those traditionally studied in the education literature.[6] In our context, a direct peer effect could affect re-employment either by directly improving human capital or by improving skills that aid in job search such as resume writing. The firm connection mechanism, on the other hand does not require that workers learn from their peers. Instead, peers simply act as a conduit connecting displaced workers to firms.

For the direct peer effect channel, the determining factor is the characteristics of peers in one's network and this is the focus of much of the peer effects literature in education. In contrast, for the firm connections channel, the quality of one's peers is secondary to what firm they work at and whether that firm is actively hiring at the right time. The richness of our data allows us to assess whether the network effects we document are likely to be driven by the peer-effects channel. Moving forward, unless

[6] There is a rich literature on the importance of peer effects in education, far too broad to fully discuss here. See Sacerdote (2011) for a comprehensive overview of the peer effects literature. In higher education, there is generally mixed evidence on the impact of peer ability on academic outcomes such as GPA (Sacerdote, 2001; Zimmerman, 2003; Steinbrickner and Steinbrickner, 2006; Carrel et al, 2009; Griffith and Rask, 2014).

otherwise specified, when we refer to "network effects" we mean those that operate through firm connections rather than direct peer effects.

Our study also relates to the literature on college quality as network effects can be thought of as part of the returns to an institution's "quality". This literature generally attributes differences across schools to causal differences in the return to some measure of quality (Black and Smith, 2006; Hoekstra, 2009; Griffith and Rask, 2016; Andrews, Li, and Lovenheim, 2016; Goodman, Hurwitz, and Smith, 2017; Canaan and Mouganie, 2018; Mountjoy and Hickman, 2021), though there are notable exceptions to this finding (Dale and Krueger, 2002; Dale and Krueger, 2014). More skilled/connected peers can improve labor market prospects through channels entirely independent of human capital or signaling, the channels most commonly discussed in the college quality literature.

From a search-theoretic perspective, having a stronger job network could impact both the likelihood of employment and wages conditional on employment. This is true even in the absence of peer effects which improve a worker's on-the-job productivity or job search abilities. In this case, the mechanism would be a more active network could lead to more job offers, leading the worker to have a higher reservation wage.

3. Data and measurements

We use data from the Ohio Longitudinal Data Archive[7] (OLDA), which links higher education information (HEI) and the state's UI earnings records. The higher education

[7] The Ohio Longitudinal Data Archive is a project of the Ohio Education Research Center (oerc.osu.edu) and provides researchers with centralized access to administrative data. The OLDA is managed by The Ohio State University's CHRR (chrr.osu.edu) in collaboration with Ohio's state workforce and education agencies (olda.ohio.gov), with those agencies providing oversight and funding. For information on OLDA sponsors, see http://chrr.osu.edu/projects/ohio-longitudinal-data-archive

data cover all students who attended public universities in Ohio from 2000 to 2017, including demographics and detailed college administrative information, such as course enrollment, majors, and academic outcomes. The UI earnings data cover all formal employment in Ohio from 1995 to 2017, excluding federal workers and self-employed workers. For each job, the data consist of quarterly earnings and employer IDs.

Our analysis requires identifying workers who are displaced during mass layoff events. While the data do not provide a direct measure of layoffs, we follow the standard in the literature on displaced workers that uses similar data to identify displaced workers (Jacobson, LaLonde, and Sullivan 1993; Hellerstein, Kutzbach, and Neumark 2019). First, the end of an employment spell is identified as a job separation if three conditions are satisfied: (1) the worker is an attached worker who has worked for an employer for at least four consecutive quarters before separation; (2) the job was the worker's highest paying job in the quarter before separation; and (3) the worker has not returned to the employer in the two years following separation. Then, we retain only separations in a mass layoff event. We deem a firm to have had a mass layoff event if the firm initially had at least 25 workers and experienced a drop in employment of at least 30 percent during a period of one year.

For the main analysis, we restrict the displaced worker sample to workers who enrolled in one of the public universities in Ohio as a fall entrant between 2000 and 2009 and entered college with a declared major.[8] We stop at the 2009 cohort in order to have sufficient labor market data both before and after mass layoff events. To limit the

[8] 91% of students enter college with a major declared. Majors are identified based on one's entering major to minimize the possibility that major choice is determined by peer quality. The sample is further limited to majors that have at least 10 students for all cohorts of the college.

likelihood of endogenous enrollment due to labor market shocks, we restrict the sample to first enrollment at age 20 or younger. The final sample contains 40,180 workers who were displaced as part of a mass-layoff event.

3.1 Measuring employer network strength

We define a contact network as all individuals from the same college-major-year-of-entry combination. A displaced worker's employer network contains the firms that employ their contacts at the time of the layoff. Our principal measure of network strength is the "active employer network", adopted from Hellerstein, Kutzbach, and Neumark (2019) to capture the hiring activities of employers in networks. Importantly, AEN is calculated based on each displaced worker's entire network, not just other displaced workers.

AEN is determined by the contacts' employment status and the hiring rate of their employers:

$$AEN = \frac{1}{N}\sum_{k}^{N} I_k \frac{H_{j(k)}}{L_{j(k)}} \tag{1}$$

N is the number of contacts in one's network, I_k is an indicator of employment in Ohio for contact *k*. $L_{j(k)}$ is the total number of workers for firm *j* at which contact *k* is working, and $H_{j(k)}$ is the number of new hires at that firm. Thus $\frac{H_{j(k)}}{L_{j(k)}}$ is the ratio of new hires to total employment at the contact's firm. This measure captures the idea in network models (Calvó-Armengol and Jackson 2007; Montgomery 1991) that a job searcher's network contacts share information about vacancies at their workplaces. We standardize AEN to have mean zero and standard deviation 1. Our AEN measure can vary for 3 reasons. First, different workers have different contacts, resulting in a different AEN. Second, for a fixed set of contacts, the AEN can change over time as contacts move employers or change employment status. Finally, for a fixed set of contact-employers, AEN can change as the employers alter their hiring rate.

It is worth highlighting two important elements regarding the construction of the AEN measure. First, AEN is constructed as a hiring rate rather than the number of hires to reflect the fact that a contact at a larger firm likely has a smaller influence on any given listed job compared to a contact at a smaller firm. Hellerstein et al. note that larger firms have many employees, each of whom may be referring workers from their own network resulting in more referral competition. Second, AEN does not include a measure of how many contacts an individual has. This is a limitation of the AEN measure, as a worker with more contacts is expected to obtain more referrals. Unfortunately, we have no good measure of network size and even if we did, it would likely be endogenous as workers determine their network size based on their networking skill. The closest we have to a proxy of network size is counting the number of individuals in one's institution-major-cohort cell, but this is a poor proxy for network size because students likely know a smaller share of their cohort as it grows in size.[9]

3.3 Other variables

We obtain student background measures using the data from higher educational records on demographics and students' county of residence and zip codes (at the time of school enrollment). Median household incomes of zip codes and counties come from the

[9] This issue is similar to Hellerstein et al. as they measures networks based on census tracts and living in a larger census tract does not imply having more contacts.

2011 five-year estimate report of the American Community Survey (ACS), the first year in which the zip code measures are available.

One limitation of the UI wage records is that they contain no information about employment outside of Ohio. Zero values of the employment indicator should be interpreted as “not being employed in Ohio” rather than non-employment (Foote and Stange 2022). Employer networks outside the state are also unobserved. Thus, the actual causal question of interest is: Do displaced workers’ employer networks in Ohio affect their job search outcomes in Ohio? For simplicity, we use phrases such as “employer networks” and “job search outcomes” without explicitly stating that the measures capture only the networks and the labor market inside the state of Ohio. Though the focus on Ohio employment introduces the possibility of sample selection for certain outcomes (discussed later), it still allows us to understand the role of networks. In particular, if workers with stronger Ohio networks are more likely to be employed in Ohio, this implies that the network effect is important. Furthermore, in a later section, we provide suggestive evidence that out-of-state employment is unlikely to be substantially affected by AEN.

3.4 Descriptive Statistics

Table 1 provides descriptive statistics for the sample of displaced workers. 74.3% of displaced workers are re-employed in the quarter following displacement. Because networks are defined based on major and institution, we expect that workers share many characteristics with their network and should therefore be disproportionately likely to be re-employed at a network’s employer, even in the absence of network effects. Nevertheless, because the number of in-network employers is small relative to the total number of employers, workers are far less likely to find re-employment at a contact's employer (9.1%) than an out-of-network employer (65.2%). The average institution-major-entry-year cell has 255 individuals and we refer to this group of individuals as one's college network. Earnings in the year prior to displacement are $26,240 with a standard deviation of $17,824 reflecting relatively large earnings dispersion and mean earnings in line with other studies of young workers who attended college (e.g. slightly higher than Zimmerman, 2014). Appendix Table A1 shows a variety of other characteristics for our sample. The sample is majority white and 52% female. The average age of college entry is considerably younger than that in the general population because we focus on those who enter college before age 20. 29% of the sample comes from a 2-year college with the remainder at 4-year schools.

Before turning to our primary analysis, we first investigate whether network effects appear to be at play for college-major-cohort networks by investigating whether the probability of coworking with one's network is higher than the probability of coworking with individuals who were in the same major-institution, but had a different entry year. This analysis is similar to the main analysis of Zhu (forthcoming), but we define networks based on college-major-cohorts. Figure 1 provides descriptive evidence that network effects among college-major cohorts are relevant. The likelihood of working at the same firm is roughly 20% greater for classmates in the same institution-major-cohort relative to classmates with the same institution-major, but in adjacent cohorts. Though this result suggests that network effects of some sort are at play, it does not assess our primary research question regarding whether workers benefit from access to more actively hiring networks.

4. Empirical framework

The major empirical challenge of identifying the effects of labor market networks is that network members share unobserved traits such as ability and motivation, which are themselves determinants of job search outcomes. Our identification strategy exploits both the exogenous nature of mass layoff events as well as variation in college network composition across cohorts. The baseline model is presented by the following equation:

$$Y_{ismyjt} = \beta AEN_{it} + \delta_j + \delta_t + \delta_{sm} + \delta_y + \gamma X_i + \varepsilon_{ismyjt} \tag{2}$$

where Y_{ismyjt} is an indicator for whether worker i who is laid off by firm j is re-employed by the following quarter t. Subscripts s, y and m represent college, entry-year and major. AEN_{it} is the treatment variables of interest, calculated using equation (1). College-by-major fixed effects (δ_{sm}) account for differences in students' unobservable characteristics across college-major combinations. The intuition is that the variation across cohorts is more likely to be random and uncorrelated with student characteristics, while sorting into institutions and majors tends to be endogenous. Entry-year fixed effects, δ_y, are also included to account for common shocks related to the timing of first college enrollment. Firm fixed effects, δ_j, absorb the source of selection that arises because of the nonrandom sorting of workers into firms. Controlling for firm fixed effects also accounts for common unobserved traits related to location, industry, and other firm characteristics. Time-fixed effects, δ_t, and individual time-varying characteristics (prior displacement earnings and past labor market experience) further absorb any potential prior treatment differences.

The key identifying assumption is that unobserved determinants of job search outcomes are orthogonal to the cross-cohort within-college-major variation in network strengths, conditional on other observables included in Equation (2). A violation of this assumption would be if a displaced worker's unobservable characteristics make them both more attractive to employers and more likely to have higher quality networks.[10] Because students make college enrollment decisions without knowing the particular quality of the entering cohort, it is unlikely that a student's characteristics could cause their peers' network quality. That said, it is certainly possible that a third factor can cause both own quality and peer network quality.

There are two main potential types of concern.[11] First, it is possible that the quality of instruction varies across cohorts within an institution-major such that certain cohorts become more employable and also end up with better peer network quality. Second, it is possible that changes in admission standards, or changes in the desirability of certain colleges, cause students who are innately more employable to enroll in the same cohorts as students who provide a stronger network. It is important to emphasize that general variation in quality across cohorts within an institution-major is not a threat to validity and is in fact the variation we hope to use to identify causal effects.[12]

[10] Network quality is defined based on the hiring rate of college peers' employers – not the characteristics of peers. As such, if more employable students have peers with different characteristics, this is not a violation of the identifying assumption.

[11] A third potential concern is that higher AEN may cause students to be laid off or to avoid layoff, therefore causing sample selection. The tests that we discuss below also have power to detect issues caused by sample selection of this type.

[12] If students were randomly assigned to cohorts, we would expect that certain cohorts would have stronger network strength and those same cohorts would have on average higher employability. This would not create bias because for each displaced worker i, there is no expected correlation between her quality and the quality of employers of the other students in her cohort.

Ultimately, the key identifying assumption is untestable, but we can provide several pieces of indirect empirical evidence to assess its plausibility. First, we can assess whether the strength of one's network is correlated with predetermined observable characteristics. If there were substantial sorting into cohorts, we expect that it would generate a correlation between network measures and key observables. Naturally, finding zero selection on observables does not rule out the potential for selection purely on unobservables.

In Table 2 we present the results of the above-described test that relates predetermined individual characteristics to the measures of network strengths. Specifically, we estimate our primary empirical model (equation 2), but instead of predicting outcomes, we predict a variety of observable characteristics. The coefficient of interest is the effect of AEN which captures how the network measure is correlated with observable characteristics, conditional on the baseline set of fixed effects shown in equation 4. In panel A, Columns (1)-(7) show the correlation between the network measures and observed variables, including gender, race, neighborhood income level, and academic performance in the first semester. In panel B, the outcome variables are the predicted outcome of interest. For example, column (1) combines all the covariates into a predicted re-employment variable, capturing a linear combination of individual features with weights that are chosen to best predict earnings potential after displacement. In Table 2, out of twelve coefficients, none are statistically significant at the 95 percent confidence level and the magnitudes are uniformly small. As such, if there is important selection into cohorts not captured by the fixed effects, it must be of a type that is unrelated to the observable characteristics shown in Table 2.

In Columns 3 and 5 of Panel B we assess whether restricting the sample to employed individuals in Ohio creates sample selection that causes the network measures to be correlated with predicted earnings or predicted employer premia. When the outcome of interest is earnings conditional on employment or employer premia, we necessarily have to condition on re-employment and as we show later, re-employment is caused by the AEN network measure. As such, the employer premium analyses are potentially biased by sample selection. The fact that this sample restriction does not create a relationship between the network measures and predicted employer premia is reassuring as it implies that at least on observables, there is no reason to expect that the sample restriction will drive the estimated effect of network quality on employer premia.

Our second piece of indirect evidence on the plausibility of the key assumption is to assess the sensitivity of our estimates to controlling for two-way FE such as major-by-entry-year FE and institution-by-entry-year FE. In our baseline model, if certain institutions or majors become more selective over time, this can generate a correlation between one's own characteristics and the network measures. Interacting major and institution with entry-year FE removes changes in selectivity at the institution or major level as a potential channel for bias. With the two-way FE controlled for, it remains possible that there are changes in selection at the institution-by-major-by-year level, but institutional factors suggest that institution-by-year and major-by-year factors are likely to be first-order. As such, finding little selection caused by institution-by-year and major-by-year factors provides some reason to expect limited selection caused by institution-by-major-by-year factors.

A third empirical test exploits the hiring behavior at out-of-network firms to assess whether individuals with stronger networks appear to be more employable in observable or unobservable ways. Specifically, we estimate the effect of network quality on the probability of gaining employment at a firm out of network. If individuals with stronger networks also have characteristics that make them more attractive to employers, we expect that employment at out-of-network firms would increase as network quality increases.

Studying employment at out-of-network firms can be thought of as a falsification test since the primary mechanism for a network effect is helping provide access to in-network jobs. That said, it is not a pure falsification test for several reasons. First, workers with a stronger network may be drawn away from out-of-network employers rather than from out of the labor force. Second, a stronger network may have spillover benefits. As an example, having a contact working at Google could potentially help with obtaining employment at Microsoft since the Google contact may form contacts at similar firms. Finally, the direct peer effect channel described in Section 2 could lead to stronger labor market outcomes across the board. Nevertheless, if we observe similar employment effects at in- and out-of-network firms, this would suggest that the effects are driven by selection or peer effects rather than a network effect.

In evaluating the results of the out-of-network falsification test, it is important to recall that only 12% of re-employed workers are hired by in-network firms (0.091/0.743). Assuming in- and out-of-network firms value similar characteristics, if a worker has stronger quality such that they have a 1 percentage point higher expected employment probability, this increases expected employment at in-network firms by 0.12 percentage points and out-of-network firms by 0.88 percentage points. In the absence of a true network effect, it would thus be quite surprising for employment effects to be driven primarily by in-network firms. As with the other tests of our identifying assumption, the in-network vs out-of-network contrast is only a suggestive test because there is a knife-edge case where an overall null effect on out-of-network employment reflects a combination of a negative crowd-out effect combined with a positive selection effect.[13] Evidence on this validity check is discussed after presenting the main results.

Finding no effect on out-of-network employment suggests that students with stronger network quality do not possess skills that make them more employable to firms in general. That said, it is possible that students with stronger network quality possess skills that make them more employable only to the types of firms that hire students from their major-institution. In other words, if out-of-network firms value a very different skill set than in-network firms, the falsification exercise described above is less informative. To address this concern, we conduct an additional falsification exercise where we restrict attention to out-of-network firms that employ students from the same major-institution, but from other entry-years. The idea behind this exercise is that firms that hire students from the same major-institution likely value a similar set of attributes as in-network firms, but students are less directly tied to these firms through their immediate network. Naturally, students may be connected to students from other cohorts, so finding a positive effect on this measure of out-of-network employment does not necessarily undermine the

[13] If there is a crowd-out effect, this would still be operating through our proposed network channel, so it could not drive the results of the falsification test in a world where there are no network effects at all. Also, this knife-edge case is unlikely in light of the evidence presented in other specification checks that suggest that there is not a positive selection effect.

main result. To the extent that cross-cohort networks are important, our estimate of the main effect of AEN will be attenuated.

A final test of the validity of our identification strategy is to construct the AEN using variation from the same institution-major pairings, but using placebo cohorts instead of the actual cohort. Though related to the out-of-network falsification test, the placebo cohort analysis is quite distinct. In the placebo cohort analysis, we examine the effect of network quality from other cohorts on the probability of employment for individuals from cohort t. In the out-of-network falsification test, we examine the effect of own network quality on employment at out-of-network firms. Thus, the out-of-network falsification test is more directly assessing whether individuals with higher network quality are more employable, whereas the placebo cohorts analysis is assessing whether there are trends in AEN quality before and after cohort t that correlate with employment outcomes for students in cohort t.

5. Results

5.1 Primary results and specification checks

Table 3 shows our primary employment results and how these estimates change as we add various controls. Column 1 shows the baseline estimate of the effect of AEN on re-employment in the quarter following layoff. We find that a 1 standard deviation higher AEN increases employment by slightly more than 1 percentage point. Comparing across columns, we see that the results are robust to adding observable covariates, institution-by-year-of-entry fixed effects and major-by-year-of-entry fixed effects. Our estimate of the effect of AEN is approximately twice the magnitude of the estimated effect of census-tract residents estimated in Hellerstein et al. (2019).

Panels A and B in Table 4 show the estimated effect of AEN on re-employment in-network and re-employment out-of-network. For these panels, the estimating sample and independent variables are identical to Table 3 and the only change is the outcomes variable. Table 4 shows that the overall employment effect is driven primarily by in-network firms. Strikingly, as a percent of the mean, the in-network effect is approximately a 10% increase, whereas the out-of-network effect is less than a 0.5% increase. This suggests that network quality is correlated with in-network employment due to a fundamentally different mechanism than out-of-network employment. This evidence is in favor of the network effect mechanism and against the peer effect mechanism. Panel C presents the test described in the prior section where the outcome is employment at out-of-cohort (but still within the same institution-major) employers. Again, the sample and independent variables are identical to that of Table 3. As with Panel B, Panel C shows limited evidence of an effect of network quality on out-of-network employment.

We interpret the lack of an effect of AEN on out-of-network re-employment as suggesting that AEN affects re-employment by increasing referrals rather than improving human capital. To further investigate, in Table 5, we examine whether AEN is related to human capital accumulation as measured by total credits earned, degree receipt and GPA. Consistent with the view that human capital improvement is not an important mechanism for the improved employment, we see no evidence that any of the proxies for human capital are higher when AEN is higher. The estimates are all statistically insignificant and small in magnitude.

In addition to the implications discussed above, the absence of an effect of AEN on out-of-network employment is also relevant for understanding the consequences of our lack of data on out-of-state employment. Interpreted most conservatively, our estimates show that there is an effect of AEN on in-state employment, demonstrating that a network effect is at play. Whether the in-state employment effect translates into a total employment effect depends on whether AEN is helping unemployed workers find a job or simply shifting employment from out-of-network to in-network. The fact that there is no evidence of shifting employment from out-of-network to in-network within the state of Ohio is suggestive evidence against the notion that AEN is simply shifting the location of employment. Our expectation is that workers would likely experience an increase in out-of-network, in-state, employment before experiencing a substantial increase in out-of-state employment.

Our final piece of evidence suggesting that our estimates are not driven by unobservable selection is that network quality of nearby cohorts is not predictive of own employment. Figure 3 shows the "effect" of network quality of various cohorts on own employment. Unlike Table 4 where the independent variable is always own AEN and the outcome varies, in Figure 3, the outcome is always overall re-employment and the different estimates correspond to the effect of AEN measures based on different cohorts. The own-cohort network effect is a clear outlier relative to adjacent cohorts, providing evidence against selection mechanisms at the institution-major level.

5.2 Extensions

5.2.1 Shared sections

Students may derive a different benefit from network contacts depending on the strength of the ties. Theoretical predictions of the strength of network ties are ambiguous since weak ties are more likely to provide novel information flows compared to strong ties (Granovetter, 1977). In our context, certainly all students in a particular institution-major-cohort are not equally well-acquainted, or possibly even acquainted at all. Though we have no direct measure of contact strength, we can measure whether two students were in the same section – increasing the likelihood that they were acquainted. Naturally, students from the same major may know each other even if they never overlap in their coursework. One caveat for this analysis is that students select courses endogenously so the network measure based on shared coursework is more susceptible to selection bias than the overall cohort-based network measure. Table 6 presents this analysis, dividing up one's network based on whether contacts have shared a course section together.[14] We find that network effect appears to operate exclusively through contacts in shared sections. This test can be seen as supporting previous empirical work such as Gee, Jones, and Burke (2017), which found that closer network ties lead to improved labor market outcomes.

5.2.2 Heterogeneity across students

Though all students could form connections during college, we expect that students who are more engaged with college are more likely to form these connections. In particular, students who live on campus and regularly attend class have more

[14] The sample is slightly smaller because we do not have course identifiers for all students. In comparing these estimates to our earlier analysis, it is important to keep in mind that the shared-section networks are smaller than the shared-cohort networks so we expect smaller coefficients mechanically.

opportunities to engage with their cohort peers. We lack a direct measure of student engagement, but we examine heterogeneity according to several dimensions that we expect to correlate with engagement. Some covariates are missing for some students so sample sizes do not exactly sum to the total from earlier tables.

Table 7 shows heterogeneity in the effect of AEN according to type of school, whether the student graduates, GPA in the first term, and credit hours attempted in the first term. Though estimates are not generally statistically different across different subsamples, there is a general pattern where students who we expect to be more engaged with college show a larger estimate effect than students who are likely less engaged. The starkest contrast is that student's with a low GPA show no evidence of a benefit from AEN whereas students with above a 2.5 show a large benefit. We speculate that this reflects differences in college engagement, though it can plausibly also reflect differences in networking ability or other mechanisms. One exception to the general pattern is that students who attempt fewer than 12 credit hours show treatment effects that are larger than students who attempt more than 12 credit hours, though the less than 12 credit hours group's estimate is noisy.

5.2.3 Heterogeneity across majors

Though networks can be used for students of any major, we hypothesize that they may be more valuable in some majors than others. For example, network quality may be particularly valuable for business majors both because business schools emphasize the importance of networking while in college and because it may be relatively difficult to assess quality during an interview for business-related jobs. In contrast, we speculate that networking may be less vital for more quantitative fields since it is more feasible to demonstrate quality directly. Table 8 shows the effect of AEN split according to business majors, engineering/math/science majors and other majors. Consistent with our expectations, the effect of AEN on re-employment is twice as large for business majors compared to the overall estimates. Contrary to our expectations, there is no evidence that the benefit of networking is smaller for quantitative majors and the point estimates is larger in magnitude. We cannot statistically differentiate across the estimates for different major groups, so this prevents any strong conclusions regarding across major heterogeneity.

5.2.4 Cohort size and time since displacement

A larger network should lead to better employment outcomes since it translates into a larger number of employer connections. Though we lack data on network size we are able to study the effect of cohort size since within a major-institution, cohort size varies. It is unclear whether cohort size should be expected to increase network size. On the one hand, as cohort size grows, there are more potential contacts and this may result in more realized contacts. On the other hand, a student may form a network of a certain size, regardless of the total number of students available. In that case, cohort size and network size would be uncorrelated.

To assess whether network quality is more important for larger cohorts, we estimate separate analyses for larger cohorts (>100) and smaller cohorts (<100). The estimates are fairly similar across columns 2 and 3 of Table 9, indicating that larger cohorts do not appear to lead to more network contacts. Column 1 shows a continuous version of this test where we interact log cohort size with AEN and this confirms the split-sample analysis from columns 2 and 3.

In columns 3-5 of Table 9 we investigate whether the effect of network quality differs based on the years between the first enrollment and the displacement event. This is motivated by the notion that networks formed in college may fade over time so if an individual is laid off soon after college, they may be more strongly connected to their college network compared to if they are laid off many years after college. Interestingly, we find that the impact of one's college network does not appear to die out over time, at least not at the 6-year threshold we are able to examine. Finding little heterogeneity according to the timing of layoff is consistent with strongly persisting networks, but it could also reflect that much of the lost connection occurs immediately after leaving college and there is limited further decline.

5.2.5 The effect of network quality on earnings and firm quality

Though an active hiring network is primarily expected to affect re-employment, it has the potential to lead to higher wages since workers may increase their reservation wage as a result of the increased flow of jobs. Our data has no information on hourly wages, but we can examine the effect of network quality on earnings in the quarter following layoff. This measure is limited since it reflects a combination of wage changes with changes in quarterly hours. Column 1 of Table 10 shows the effect of AEN on earnings in the quarter following displacement. We find that a 1 standard deviation increase in AEN leads to earnings that are $153 higher, an approximate 3% increase off of the base of $5,061. Column 2 of Table 10 shows the effect of AEN on earnings, conditional on employment. Though conditioning on employment has the potential to create sample selection, we showed earlier in Table 2 that there is no difference in predicted earnings conditional on employment – mitigating this concern to some extent. We find that conditional on employment, a 1 standard deviation increase in AEN leads to quarterly earnings that are $96 higher.

Even conditional on employment, the earnings effect may be driven by a change in quarterly hours caused by finding employment earlier in the quarter. Though we cannot ultimately differentiate hours from wages, one piece of suggestive evidence is to examine the effect of AEN on quarterly earnings in quarter t+2, conditional on employment at the same firm in quarters t+1, t+2 and t+3.[15] Workers who obtain re-employment in quarter t+1 and remain at the same firm for quarter t+1 through t+3 are likely to be employed for the entire duration of quarter t+2. For this sample, it is still possible that AEN affects t+2 earnings through hours, but it is less likely to operate through the timing of re-employment. Column 3 shows that the estimated effect on t+2 earnings for this sample is similar to the estimate for t+1 suggesting that the earnings effect is not entirely driven by the timing of employment.

A recent literature highlights the importance of firm quality and firm premia in wage determination (Abowd, Kramarz, and Margolis 1999; Card, Heining, and Kline 2013; Card 2022) and as a source of earnings losses experienced by displaced workers in particular (Fackler, Mueller, and Stegmaier 2021; Lachowska, Mas, and Woodbury 2020; Schmieder, Heining, and Von Wachter, Till 2019; Moore and Scott-Clayton 2019). Here, we provide the first evidence of the effect of the network hiring rate on the firm premium at the re-employment firm. To do so, we first follow the approach developed by Abowd, Kramarz, and Margolis (AKM) to estimate the firm-specific premium at each firm.

[15] Limiting the sample to continuously employed workers introduces another potential sample selection concern, providing another reason that this analysis is only suggestive.

Details regarding the construction of the firm-specific premia are provided in appendix A. We then estimate the effect of AEN on the firm-specific premium at the re-employing firm.[16] Column 4 of Table 10 shows a small positive, but statistically insignificant effect of AEN on the firm premium.

Though a faster arrival rate of job offers has the potential to increase the firm-specific premium through a change in the reservation wage, a higher AEN by itself does not imply that the quality of employers in one's network is higher. An alternative measure of network quality is the average employer-premium of employed contacts at the time of displacement. We refer to this network quality measure as the "network firm premium" (NFP) and standardize it to have mean zero and standard deviation 1. A higher NFP is not expected to translate into a faster arrival rate of offers, but to the extent that workers obtain jobs at their network's employers, a higher NFP can lead to a higher firm-premium at the re-employing firm.

In Table 11, we estimate our preferred model, but include both the AEN and NFP measures of network quality. In contrast to the effect of AEN, Column 1 shows that the effect of NFP on re-employment, is negative and statistically insignificant.[17] Column 2 of Table 11, however, shows that having a 1 standard deviation increase in NFP increases the firm premium at the re-employing firm by 0.0199. Despite having no effect on

[16] This analysis necessarily restricts the sample to workers who are re-employed. A small number of workers are re-employed at firms that are too small to estimate a firm-specific premium and thus the sample for this analysis is slightly smaller than the employed sample. In Table 2 we verified that AEN is not related to predicted outcomes for this sample.

[17] Because NFP is constructed from estimated firm-premia, the standard errors presented in Table 10 may be too small. The most natural approach to correcting these standard errors would be to bootstrap the entire process, but this is infeasible because estimating the firm-specific premia is time-intensive, even for a single iteration. As a result, we emphasize the magnitude of the NFP estimates and are cautious regarding our conclusions of statistical significance.

employment, NFP thus leads to higher earnings in period t+1 (shown in column 3) an effect driven by higher earnings conditional on employment (shown in column 4).

6. Conclusion

In this paper, we examine the impact of employer networks formed through college contacts on job search outcomes for displaced workers. We find that these networks are an important feature of the labor market, with a stronger network significantly increasing the likelihood of landing a job following displacement. Our identification strategy uses variation across cohorts within an institution-by-major cell and we provide a variety of pieces of evidence in favor of the key identifying assumptions. The results are consistent with a network effect rather than being driven by peer effects, and there is some suggestive evidence that stronger ties within one's network yield better results for displaced workers.

Putting these results in the context of the broader literature on networks, our paper confirms the key role that such networks play in the labor market. In order to isolate variation that is plausibly exogenous, we necessarily focus on one specific network definition. We ignore the many other dimensions on which networks are formed within college (roommates, students in other cohorts, students in other majors, students in common clubs/jobs/athletic teams), not to mention employer networks formed after college. The fact that this one narrowly defined network has economically meaningful effects years after college underscores the importance of studying how networks drive our labor market.

Establishing that college networks are used in job search also has implications for the literature on the returns to college. Most of the literature to date has emphasized human capital and signaling explanations for why college improves labor market outcomes. Importantly, both of these mechanisms involve firms valuing higher education because it is predictive of higher human capital (either through directly increasing human capital or by signaling high human capital). The network benefits of higher education are quite distinct as a firm may give preference to referred applicants, even if they have the same expected productivity.

Appendix A Measurement of firm-specific premia

The firm quality outcome requires measuring the firm-specific premium for each firm. We estimate firm-specific premia, $\psi_{j(i,t)}$, using the AKM model:

$$lny_{it} = \theta_i + \psi_{j(i,t)} + X_{it}\beta + \varepsilon_{it} \qquad (3)$$

The logarithm of real earnings of worker *i* working in firm *j* in period *t* is a function of additive worker and firm fixed effects. The worker fixed effect, θ_i, captures all time-invariant characteristics, such as gender, race, family background, innate ability, and early human capital investment. The firm effects, $\psi_{j(i,t)}$, represent the firm-specific contribution to wages after controlling for individual workers' characteristics. X_{it} is a vector of individual time-varying covariates, which include information on labor market experience and tenure.

For our estimation of the AKM model, we follow the typical implementation in the literature and construct a panel dataset at the worker-by-year level. For each worker in each year, the primary employer is determined based on the job that pays the highest total earnings. Thus, each person-year observation is associated with one single employer. The earnings outcome is calculated by taking the average quarterly earnings of the worker from his or her main employment during the year. Workers in the analysis sample of displaced workers are excluded from the AKM estimation so that the two samples are mutually exclusive. The estimation of the AKM model is limited to observations between 2003 and 2016. The sample for the AKM decomposition contains approximately 24 million worker-by-year observations that cover 3.6 million workers and three hundred thousand firms.

The AKM construction of firm quality mechanically generates an average firm premium for the state is zero, but our sample's average firm premium is 11.1%, consistent with the notion that college attenders are disproportionately represented at high-premium firms (Engbom and Moser, 2017). The standard deviation of the firm effects is 0.364, in the same range as past work (Card, Heining, and Kline 2013; Card, Cardoso, and Kline 2016; Macis and Schivardi 2016).[18]

[18] Bonhomme et al (2022) notes that the standard deviation of the firm effects is likely overstated, despite each estimated firm effect being unbiased. This has important implications for assessing the share of earnings variance explained by firms.

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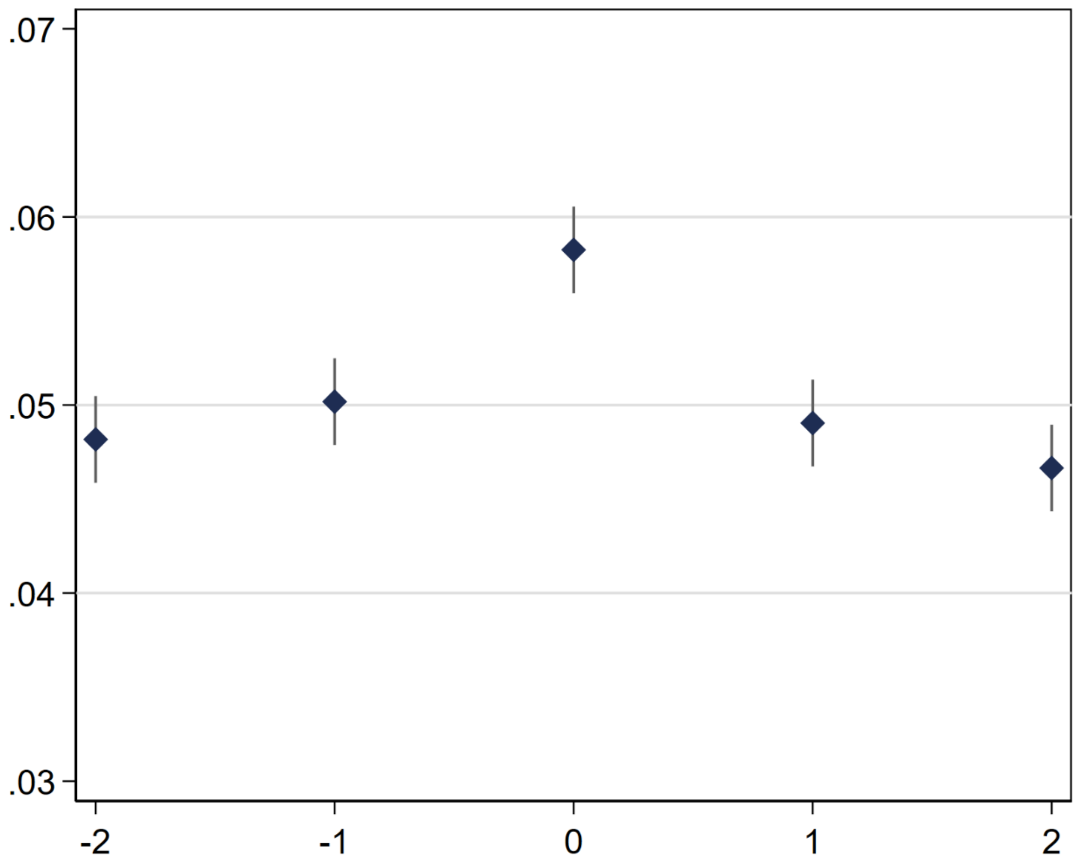
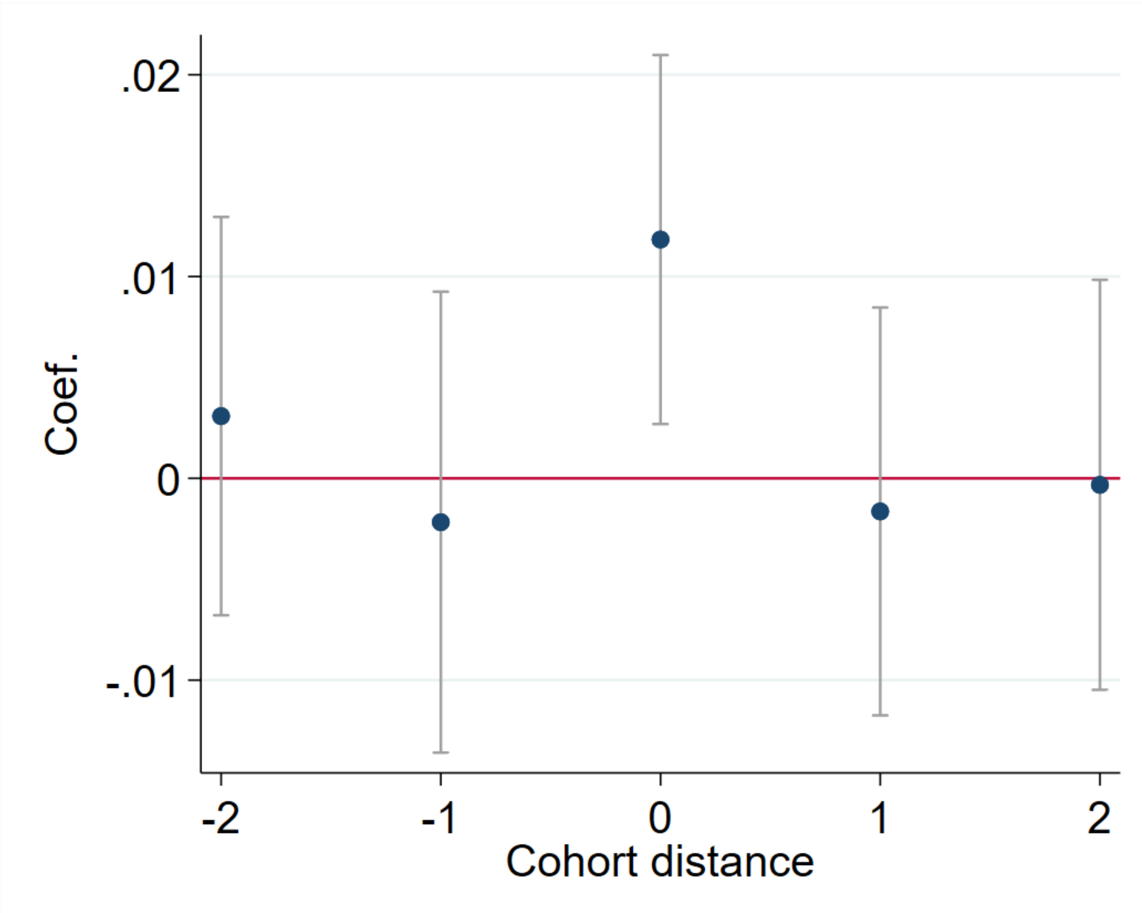
Figure 1: Coworking probability and cohort distance



Notes: Data are from the Ohio Longitudinal Data Archive (OLDA). For x=0, the Y axis is the probability of the displaced workers being employed in the following quarter by an employer of an individual who is in the same institution-major group and in the same cohort. For the other x values, the Y value is the probability of being employed by an employer of a worker who is in the same institution-major but a different cohort (x years away).

Figure 2: The effects of employer networks on reemployment by cohort distance



Notes: Data are from the Ohio Longitudinal Data Archive (OLDA). The sample comprises displaced workers as described in the text and in Table 1. The figure shows the estimated effect of AEN from different cohorts on reemployment probability. For x=0, the Y axis is the estimated effect of AEN calculated using peers from the same institution-major group and in the same cohort. For the other x values, AEN is calculated using peers from the same institution-major but a different cohort (x years away).

Table 1: Summary statistics

Variable	Mean	SD	Obs
Re-employment in the quarter following displacement	0.743	0.437	40,180
Re-employment at a contact's employer	0.091	0.288	40,180
Re-employment at an employer of peers of other cohorts	0.130	0.336	40,180
Firm premium in the quarter following displacement	0.111	0.379	29,656
Earnings in the quarter following displacement	5,061	5,454	40,180
Earnings in the quarter following displacement (without zero)	6,813	5,302	29,850
Earnings in the 2nd quarter following displacement (without zero)	7,064	5,302	30,371
Earnings in previous year (2012 dollar, $1000s)	26,240	17,824	40,180
Active employer network (AEN)	0.000	1.000	40,180
Network firm premium (NFP)	0.000	1.000	40,180
Number of employers in network	125.697	159.260	40,180
Number of employers in college-major combination	1298.115	1863.061	40,180
Number of peers in network	255.143	343.930	40,180
Number of peers in college-major combination	2871.495	4280.231	40,180

Note: The data are from the Ohio Longitudinal Data Archive (OLDA). Median household incomes of the zip code and county of residence come from the 2011 five-year estimate report of the American Community Survey (ACS). Earnings are in 2012 dollars.

Table 2: Balance test

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Female	White	Black	Hours attempted 1st term	1st term GPA	County median income	Zip code median income
AEN	0.00693*	-0.000502	0.00107	-0.00324	0.00995	72.49	-64.16
	(0.00419)	(0.00368)	(0.00277)	(0.0368)	(0.0101)	(69.66)	(155.6)
Observations	40180	40180	40180	40180	40077	39302	38507

Panel B

	(1)	(2)	(3)	(4)	(5)
	Predicted reemployment	Predicted earnings	Predicted conditional earnings	Predicted firm quality	Predicted firm quality (conditional on reemployed)
AEN	0.000135	-0.703	2.650	0.0000203	0.000119
	(0.000126)	(4.887)	(7.775)	(0.000156)	(0.000188)
Observations	37699	37699	28108	37699	27922

Notes: The data are from the Ohio Longitudinal Data Archive (OLDA). We restrict the sample to displaced workers who lost their jobs in mass layoff events and who had enrollment records in the data prior to the event. All regressions include displaced firm fixed effects, time (quarter) fixed effects, cohort fixed effects, and institution-by-major fixed effects. Standard errors, shown in parentheses, are clustered at the firm level. (*, $p<0.10$; **, $p<0.05$; ***, $p<0.01$).

Table 3: Effects of employer networks on re-employment

	(1)	(2)	(3)	(4)	(5)
			Re-employed		
AEN	0.0115***	0.0117***	0.0116***	0.0116***	0.0118**
Standard error	(0.00433)	(0.00433)	(0.00433)	(0.00435)	(0.00466)
Additional Controls Included					
Prior earnings		X	X	X	X
Individual & network covariates			X	X	X
Cohort by Institution Interactions				X	X
Cohort by Major Interactions					X
R-squared	0.417	0.419	0.419	0.426	0.469
Observations	37699	37699	37699	37699	37699

Notes: The data are from the Ohio Longitudinal Data Archive (OLDA). We restrict the sample to displaced workers who lost their jobs in mass layoff events, had enrollment records in the data prior to the event, and had information on basic and additional control variables. All regressions include time (quarter) fixed effects, firm fixed effects, entry-year fixed effects, and institution-by-major fixed effects. Additional individual variables include gender, indicators for white and black races, first semester credit hours attempted and first-term academic measures (credit hours attempted and GPA). Network characteristics include contacts' average values for the abovementioned variables. Standard errors, shown in parentheses, are clustered at the firm level (*, p<0.10; **, p<0.05; ***, p<0.01).

Table 4: Effects of employer networks on re-employment in networked firms

	(1)	(2)	(3)	(4)	(5)
Panel A			*Re-employed by employers in network*		
AEN	0.00885***	0.00883***	0.00880***	0.00853***	0.00851***
Standard error	(0.00257)	(0.00257)	(0.00256)	(0.00257)	(0.00307)
R-squared	0.338	0.338	0.339	0.348	0.380
Panel B			*Re-employed by employers outside network*		
AEN	0.00261	0.00287	0.00284	0.00306	0.00331
Standard error	(0.00465)	(0.00465)	(0.00466)	(0.00474)	(0.00526)
R-squared	0.408	0.409	0.410	0.417	0.459
Panel C			*by employers of students in other cohorts in same major-institution*		
AEN	0.00382	0.00388	0.00380	0.00387	0.00368
Standard error	(0.00340)	(0.00340)	(0.00342)	(0.00341)	(0.00379)
R-squared	0.332	0.332	0.333	0.342	0.383
Additional Controls Included					
Prior earnings		X	X	X	X
Individual & network covariates			X	X	X
Cohort by Institution Interactions				X	X
Cohort by Major Interactions					X
Observations	37699	37699	37699	37699	37699

Notes: Notes to Table 3 apply here as well. The 3 panels differ only in terms of the outcome. In panel A, the outcome is employment at an in-network firm. In panel B, the outcome is employment at an out-of-network firm. In panel C, the outcome is employment at an out-of-network firm that employs students from other cohorts in the same major-institution. Standard errors, shown in parentheses, are clustered at the firm level (*, p<0.10; **, p<0.05; ***, p<0.01

Table 5: Academic outcomes

	(1)	(2)	(3)
	Degree	Final credit hours	Final GPA
AEN	-0.00391	-0.544	-0.00951
SE	(0.00488)	(0.636)	(0.00676)
Observations	37699	37686	37699

Notes: The sample is restricted to displaced workers who lost their jobs in mass layoff events, had enrollment records in the data prior to the event, have outcome information and have information on basic and additional control variables. The outcomes are academic outcomes meant to capture human capital accumulation. The estimating equation is the same as that in Table 3, column 5.

Table 6: Strong vs weak ties

	(1)	(2)	(3)
		Reemployment	
AEN (sections shared)	0.00876**		0.00878**
	(0.00355)		(0.00358)
AEN (no section shared)		0.000262	-0.000287
		(0.00361)	(0.00364)
Observations	34504	34504	34504

Notes: The outcome is re-employment and the treatment variable of interest is AEN, split according to whether the students share a section. Column 3 adds both measures simultaneously. All specifications include the same controls as in Table 3, column 5. Standard errors, shown in parentheses, are clustered at the firm level (*, p<0.10; **, p<0.05; ***, p<0.01).

Table 7 Subsamples by student characteristics

	4-year college	2-year college	Degree completer	Non-completer	1st GPA>=2.5	1st GPA<2.5	1st hours >=12	1st hours<12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
AEN	0.0143**	0.00750	0.0164	0.00841	0.0159	-0.00401	0.0128*	0.0163
	(0.00628)	(0.0111)	(0.0121)	(0.00746)	(0.0102)	(0.00856)	(0.00661)	(0.0165)
Observations	26253	11446	14828	22871	18251	19448	26745	10954

Notes: The notes from Table 3 apply here as well. The outcome is re-employment and the treatment variable of interest is AEN. This table shows heterogeneity in the effect of AEN according to various student characteristic. All specifications include the same controls as in Table 3, column 5. Standard errors, shown in parentheses, are clustered at the firm level (*, p<0.10; **, p<0.05; ***, p<0.01).

Table 8 Subsamples by major categories

	Business & services	Engineering and Math	Others
	(1)	(2)	(3)
AEN	0.0204	0.0179	0.00614
	(0.0164)	(0.0166)	(0.00702)
Observations	7884	7393	22422

Notes: The notes from Table 3 apply here as well. This table shows heterogeneity in the effect of AEN according to student major. All specifications include the same controls as in Table 3, column 5. Standard errors, shown in parentheses, are clustered at the firm level (*, p<0.10; **, p<0.05; ***, p<0.01).

Table 9 Subsamples by cohort size and timing of displacement

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample	cohort size < 100	cohort size>=100	Full sample	within 6 years	after 6 years
AEN	0.0194	0.0107	0.0132	0.0127*	0.0122	0.0131
	(0.0125)	(0.00718)	(0.0121)	(0.00735)	(0.00933)	(0.00911)
AEN x log(cohort size)	-0.00197					
	(0.00315)					
AEN x years after 1st enrollment				-0.000171		
				(0.00112)		
N	37699	18809	18890	37699	16824	20875

Notes: The notes from Table 3 apply here as well. This table shows heterogeneity in the effect of AEN according to cohort size and timing of displacement. Columns 1 and 4 show continuous versions testing for heterogeneity using interaction terms and the other columns show discrete versions. All specifications include the same controls as in Table 3, column 5. Standard errors, shown in parentheses, are clustered at the firm level (*, p<0.10; **, p<0.05; ***, p<0.01).

Table 10: Effects of employer networks on other labor market outcomes

	(1)	(2)	(3)	(4)
	Earnings (with zeros)	Earnings (without zeros)	Earnings at the 2nd quarter conditional on at-least-3-quarter spells	Firm quality
AEN	152.7***	95.71*	97.15	0.00369
SE	(48.49)	(49.07)	(65.24)	(0.00420)
Observations	37699	28108	18399	27922

Notes: The outcomes are various measures of earnings or firm quality and the treatment variable of interest is AEN. In column 3, the outcome is earnings in the second quarter and the sample is restricted to workers continuously employed at the same firm from t+1 to t+3. As discussed in the text, this restriction increases the likelihood that the worker is employed for all of quarter t+2. Firm quality comes from an AKM decomposition described in the text and appendix A. All specifications include the same controls as in Table 3, column 5. Standard errors, shown in parentheses, are clustered at the firm level (*, p<0.10; **, p<0.05; ***, p<0.01).

Table 11: The effect of network firm premia (NFP) on re-employment outcomes

	(1)	(2)	(3)	(4)
	Re-employed	Firm quality (conditional)	Earnings	Earnings (conditional)
NFP	-0.00754	0.0198***	118.3*	244.2***
SE	(0.00675)	(0.00585)	(66.04)	(66.38)
AEN	0.0113**	0.00504	160.5***	112.1**
SE	(0.00467)	(0.00424)	(48.52)	(48.89)
Mean outcome	0.743	0.111	5,061	6,813
Observations	37699	27922	37699	28108

Notes: The data are from the Ohio Longitudinal Data Archive (OLDA). The sample is based on the one described in Table 3 and in columns 2 and 4, it is further restricted to those who obtain a job in the quarter following displacement. NFP and AEN are both standardized to have mean 0 and standard deviation 1. All specifications include the same controls as in Table 3, column 5. Standard errors, shown in parentheses, are clustered at the firm level (*, p<0.10; **, p<0.05; ***, p<0.01).

Table A1: Summary statistics of control variables

Variable	Mean	SD	Obs
Female	0.521	0.500	40,180
White	0.828	0.378	40,180
Black	0.102	0.302	40,180
Age when starting college	18.430	0.495	40,180
Age when displaced	24.835	3.466	40,180
Credit hours attempted in 1st semester	12.782	4.163	40,180
GPA in 1st semester	2.443	1.052	40,180
Two-year college	0.291	0.454	40,180
Zipcode median annual income	53,468	17,028	38,507
County median annual income	48,833	8,205	39,302
Complete degree	0.399	0.490	40,180

The data are from the Ohio Longitudinal Data Archive (OLDA). We restrict the sample to displaced workers who lost their jobs in mass layoff events, had enrollment records in the data prior to the event, and had information on basic control variables.