DISCUSSION PAPER SERIES

No. 6818

PEOPLE I KNOW: JOB SEARCH AND SOCIAL NETWORKS

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LABOUR ECONOMICS

Centre for Economic Policy Research

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Available online at: **www.cepr.org/pubs/dps/DP6818.asp**

ISSN 0265-8003

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Discussion Paper No. 6818
August 2008

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CEPR Discussion Paper No. 6818

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ABSTRACT

People I Know: Job Search and Social Networks*

We assess the information spillovers generated by the exchange of job-related information within networks of fellow workers exploiting administrative records covering all employment relationships established in a specific local labor market over 20 years. We recover individual-specific networks of former colleagues for a sample of workers exogenously displaced by firm closures and relate their subsequent unemployment duration to the share of employed contacts at displacement date. Individual-specific networks and the longitudinal dimension of the data allow to account for most plausible sources of omitted variable bias. In particular, identification rests on within-closure within-neighborhood and within-skill comparisons conditional of a wide range of predictors for the displaced and his contacts' employment status, such as lagged wages and labor market attachment. We find that contacts' current employment rate has statistically significant effects on unemployment duration: a one standard deviation increase in the network employment rate reduces unemployment duration by about 8 percent; as a benchmark, a one standard deviation increase in own wage at displacement is associated with a 10 percent lower unemployment duration. These effects are magnified if contacts recently searched for a job and if their current employer is closer, both in space and in skills requirements, to the displaced. We find that stronger ties and lower competition for the available information also speed up re-employment. A number of specification checks and indirect tests suggests the estimated spillover effect of contacts' current employment status is driven by information exchange rather than by other interaction mechanisms.

JEL Classification: D0 and J0
Keywords: job search, social networks and spillover effects

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*We are indebted to Antonio Ciccone for comments, discussions and support. We thank for their comments Josh Angrist, Toni Calvo, David Card, Ken Chay, Piero Cipollone, Juan Dolado, Maia Guell, Andrea Ichino, Juan F. Jimeno, Matthias Messner, Enrico Moretti, Federica Origo, Laura Pagani, Sevi Rodriguez-Mora, Gilles Saint-Paul, Paolo Sestito, Roberto Torrini and seminar participants at AIEL 2004, Brucchi-Luchino Workshop 2004, EALE/SOLE 2005, NBER Summer Institute 2007, Bank of Italy, Bocconi University, University of Padova, University of California – Berkeley. We thank the Center for Labor Economics at UC Berkeley for hospitality. We are grateful to Giuseppe Tattara and Marco Valentini for supplying and helping us with the data. We are responsible for any mistakes. The views expressed here are our own and do not necessarily reflect those of the Bank of Italy. This is an extensively revised version of Bank of Italy's Discussion Paper No. 600.

Submitted 23 April 2008

People I Know: Job Search and Social Networks

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April 23, 2008

Abstract

We assess the information spillovers generated by the exchange of job-related information within networks of fellow workers exploiting administrative records covering all employment relationships established in a specific local labor market over 20 years. We recover individual-specific networks of former colleagues for a sample of workers exogenously displaced by firm closures and relate their subsequent unemployment duration to the share of employed contacts at displacement date. Individual-specific networks and the longitudinal dimension of the data allow to account for most plausible sources of omitted variable bias. In particular, identification rests on within-closure within-neighborhood and within-skill comparisons conditional of a wide range of predictors for the displaced and his contacts' employment status, such as lagged wages and labor market attachment. We find that contacts' current employment rate has statistically significant effects on unemployment duration: a one standard deviation increase in the network employment rate reduces unemployment duration by about 8 percent; as a benchmark, a one standard deviation increase in own wage at displacement is associated with a 10 percent lower unemployment duration. These effects are magnified if contacts recently searched for a job and if their current employer is closer, both in space and in skills requirements, to the displaced. We find that stronger ties and lower competition for the available information also speed up re-employment. A number of specification checks and indirect tests suggests the estimated spillover effect of contacts' current employment status is driven by information exchange rather than by other interaction mechanisms.

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1 Introduction

The aim of this paper is to test whether the duration into unemployment of individuals exogenously displaced by firm closures is affected by the current employment status of their contacts and to establish whether this effect depends on the transmission of job-related information from employed contacts to job seekers. The circulation of job-related information is often claimed to be a major factor underlying the large variability of employment outcomes across otherwise similar socio-demographic groups. The basic intuition is that if employed individuals have privileged access to information on available employment opportunities, the degree to which job seekers become aware of such opportunities depends on their connections to the former group. In such a framework the social returns to finding a job are thus higher than private returns, as individual employment improves the prospects of unmatched agents. In addition, such spillover effects have the potential to turn small labor market shocks into sustained differences across groups in terms of labor market participation, employment, and earnings (Calvo-Armengol and Jackson (2004)).

Despite its positive and normative relevance, an empirical assessment of such mechanism is difficult to implement (see Ioannides and Datcher Loury (2004) for a review). First, information on actual contacts is generally unavailable. Researchers usually proxy the relevant group on the basis of some arbitrary metric of distance, thus making it difficult to reconcile the evidence obtained with specific channels of interaction. Second, even having characterized a relevant group for the exchange of job-related information, one has to deal with the possibility that common factors affect the employment status of an individual and of his contacts (Manski (1993), Manski (2000), Moffitt (2001)). Third, even a causal estimate has to be contrasted with alternative sources of spillovers with similar empirical predictions and yet unrelated to the transmission of information on available employment opportunities. For example, if utility while unemployed depends negatively on the employment rate of one's contacts, perhaps because of social norms, a higher network employment rate would also lead to shorter unemployment durations (e.g. Akerlof (1980), Akerlof and Kranton (2000)).

In this paper we focus on networks of former fellow workers. This is a relevant set of contacts to focus upon because the workplace is a major source of social connections and because former colleagues are a natural reference when searching for a job. Granovetter (1995) finds that acquaintances from previous jobs account for a remarkable proportion of jobs found through personal contacts, plausibly because of their direct knowledge of the job seeker's skills and motivations and because of their being exposed to relevant information. We draw on a long panel of administrative records that cover *all* employment relationships established in a small and densely populated area in northern Italy over the period 1974-1997. The data provide detailed information on individual socio-demographic characteristics, earnings and tenure at any job, employer's characteristics, and employment status at each point in time. Importantly, they allow to identify each pair of co-workers and the common tenure at any given employer. We define the network of fellow workers a displaced has access to at displacement date as the pool of individuals he worked with for at least one month over a fixed pre-displacement time window. This definition and the full coverage of the data allow to recover the complete map of direct and indirect social connections and to describe it along a variety of dimensions correlated with the likelihood, the intensity and the relevance of the information flows between any two network members.

Individual-specific networks and the longitudinal dimension of the data allow to assess the response of unemployment duration to contacts' current employment rate overcoming several identification issues commonly encountered in non-experimental studies of network effects. These arise because group members may share some unobserved trait or be exposed to common factors affecting both individual outcomes and the network characteristics of interest[1]. Because in our setting networks are formed by

[1]This problem is especially important when lack of information on the relevant network leads to approximations based on observable individual traits (for example, residential location, age, sex or race) whereby all individuals sharing it belong to the same reference group. Examples in various environments are Glaeser, Sacerdote and Scheinkman (1996), Bertrand, Luttmer and Mullainathan (2000), Aizer and Currie (2004), Bayer, Ross and Topa (2005), Luttmer (2005). Research on the effects of neighborhood quality on individual outcomes typically overcomes the problem of omitted individual characteristics exploiting programs that randomly incentivate some households to move to more affluent neighborhoods (Katz, Kling and Liebman (2001), Kling, Liebman and Katz (forthcoming)) or directly assign individuals to other residential locations (Oreopoulos (2003)); alternatively, Weinberg, Reagan and Yankow (2004) explicitly model

individuals who have previously worked together, the displaced and his contacts will systematically share relevant latent determinants of their employment status, if (i) the labor market sorts individuals across firms along that dimension, or (ii) workers become similar in ways that will affect their subsequent employment performance by working together (e.g. they accumulate the same specific skills). We address these sources of bias in a number of complementary ways[2]. First, we control for the presence of common latent determinants induced by sorting comparing individuals contemporaneously displaced by the same closing firm. If workers are sorted with the same rule over time, then former and current (i.e. co-displaced) fellow workers share the same unobservables and within-firm comparisons absorb differences across networks correlated with its employment rate. Second, we control for potential within closing firm unobserved heterogeneity with a large set of predictors for the displaced subsequent labor market outcomes, including pre-displacement realizations of job-seekers' unemployment and earnings as well as indicators of the specific human capital accumulated on the job. Conditional on these controls, the identifying variation in network employment rate is orthogonal to individual unobservables that also affect employment and earnings. Finally, individual-specific networks allow to control in a detailed way for omitted variable bias related to the specific labor market or residential location of the displaced by exploiting network variation within the relevant labor market, industry and neighborhood.

We find that a larger share of currently employed contacts significantly shortens unemployment duration of comparable displaced workers. A one standard deviation increase in the network employment rate leads to a reduction in unemployment duration of about 8 percent (roughly 3 weeks for the average spell). This effect is substantial: as a benchmark, a one standard deviation increase in

the individual residential choice.

[2]Another relevant issue in studies of network effect is the "reflection" problem. This arises when networks are defined so that the subjects of analysis contemporaneously belong to the relevant group whose characteristics are being investigated, leading to a fundamental identification problem (Manski (1993), Moffitt (2001)). This is usually the case when networks are backed out from sampled data on the basis of observable traits. In our setting, we look at the response of the displaced to the status of their non-displaced former fellow workers. Therefore the research design does not suffer from the reflection problem as the subjects of analysis do not contemporaneously act as network members.

own weekly wage at displacement is associated with a reduction of about 4 weeks for the average unemployment spell. Under the assumption that the conditional variation is orthogonal to unobserved determinants of unemployment duration, the result provides a causal estimate consistent with the diffusion of job-related information by one's employed contacts. We provide further evidence that our estimates represent the effect of innovations to the current employment status of contacts unanticipated by the displaced, such as an additional randomly employed contact when the search spell exogenously begins, and argue they are therefore unlikely to be driven by alternative mechanisms of interaction. We proceed as follows. First, we show the results to be unaffected by the inclusion of direct predictors of the current employment status of contacts obtained from their specific characteristics, earnings and employment histories. Second, we do not detect any significant relationship between unemployment duration and the share of employed contacts at close but prior-to-displacement points in time, suggesting that our estimates do not reflect persistent behavioral differences across networks. Finally, we estimate the relationship between the displaced entry wages and the network employment rate. Because the reservation wage includes all the information available to the displaced, anticipated differences in contacts' employment status should reflect into entry wages. However, we again fail to find any statistically significant correlation. Taken together, this evidence allows to credibly rule out alternative interaction mechanisms that reflect the optimal response of the job seeker to the *perceived* status of his contacts, such as those arising from peer pressure.

Having established the presence of a statistically significant effect of the network employment rate on unemployment duration, we explore the role of contacts' labor market characteristics and that of social structure for the transmission of information. The likelihood and the content of information exchanges within a network are shaped by the features of the links individuals entertain with each other and by the structure of connections within and across networks[3]. The data allow to explore important

[3]For example, Calvo-Armengol and Jackson (2004) have stressed the role of the structure of direct and indirect connections in determining information flows, individual outcomes and the aggregate effects of labor market shocks; Bramoullè and Saint-Paul (2004) have emphasized the role of social inbreeding, whereby ties are more easily maintained

dimensions of heterogeneity across contacts, such as ties' intensity, job search activity, sectoral and spatial proximity as well as the role of indirect networks as competitors or information generators. We find that stronger ties tend to reinforce the baseline network effect; this is also magnified by contacts' physical and technological proximity and by contacts' recent job turnover, an indicator of job search activity. Finally, we show that the presence of competing job seekers from outside the displaced network but linked to an employed network member significantly dampens the effect of contacts' current employment status. Overall, we read this evidence as supportive of the fact that a relevant portion of job-related information acquisition takes place through informal networks, even in a small and concentrated labor market such as the one we study.

Research on the role of informal hiring channels has a long tradition. Many studies have documented differences between labor market outcomes of individuals reporting to have searched through personal contacts and through other methods[4]. However, lack of information on contacts availability and on their characteristics makes it hard to properly account for the selection determined by the choice of the search method. This is likely to play an important role: Munshi (2003) shows that labor outcomes of Mexican migrants improve when they are endowed with a larger network of pre-established co-villagers at destination, thus increasing the incentives to migrate; Wahba and Zenou (2005) find that in Egypt jobs are more likely to have been found through personal contacts in more densely populated areas; finally, Datcher Loury (2006) shows that jobs obtained through contacts are better than those found through formal methods only when the contact is a prior generation male relative, presumably more likely to have "useful characteristics" for the job seeker. Among the studies that relate individual outcomes to characteristics of a reference group such as the residential neighborhood, only few attempt to trace such effects to local information exchange. Bayer et al. (2005)'s approach builds on the neighborhood literature; they use detailed residential and working location information

among employed individuals than between people in different labor market conditions, for the patterns of unemployment.

[4]For example, Holzer (1988), Blau and Robins (1990), Simon and Warner (1992), Addison and Portugal (2002).

to show that people living on the same block in Boston are more likely to work at the same location than pairs living in neighboring blocks within the same block group and that this likelihood increases when the individuals share certain demographic characteristics. A different approach is that of Topa (2001) and Conley and Topa (2002) who show that the spatial patterns of unemployment rates across Chicago census tracts are consistent with the exchange of information along plausible metrics of social distance. Against this background, our paper contributes to the understanding of network effects in the labor market by developing a meaningful definition of job information network based on having shared the workplace and by studying its relationship with the outcomes of workers displaced by the same firm closure and active in the same local labor market.

The paper proceeds as follows. In the next section we outline the empirical model and discuss the main identification issues. Next, we describe the data and the underlying labor market. We present the main results in Section 4 and robustness checks and a discussion in Section 7. We then conclude.

2 The empirical model

To assess to what extent social networks generate information relevant to job seekers and contribute to matching workers to jobs we relate (the log of) unemployment duration of displaced i (u_i) to the share of employed contacts - the network employment rate ER_i - as of the starting date of the unemployment spell, t_0:

$$u_i = \alpha + \gamma ER_{it_0} + \theta \log(N_{it_0}) + X_{it_0}\beta + e_{it_0} \tag{1}$$

where N_{it0} is the overall size of the network, and X_{it_0} and e_{it_0} are, respectively, observed and unobserved determinants of unemployment duration. The specification captures the basic notion that, all else equal, a larger share of employed contacts raises the odds of leaving unemployment because of the better access to job relevant information and of the lower competition for the opportunities circulated in the network. Interpretation of least square estimates of γ from (1) as the effects of information generated in the network, however, faces two major obstacles. First, the empirical correlation between network characteristics and unemployment duration may simply reflect an omitted variable bias due to determinants correlated with the network employment rate. Second, even a convincing causal estimate may reflect mechanisms other than the facilitation of job-related information. Let us address these issues in turn.

Identification

A causal interpretation of least square estimates of γ from (1) requires that network characteristics are uncorrelated with the residual. In non-experimental settings this may fail because an agent and his contacts share unobserved characteristics proxied by the network employment rate or are exposed to common exogenous unobserved factors (Manski (1993), Moffitt (2001)). In our setting individuals are assumed to be socially related because they have worked in the same firms. Hence, a job seeker and his contacts might share some relevant unobservable characteristics if the labor market sorts workers across firms along such dimension. Thus, a negative correlation between individual unemployment duration and contacts' employment rate might reflect the fact that more able individuals tend to work together and, because of their higher ability, are also more likely to be employed at any point in time. On the other hand, a job seeker and his contacts may be exposed to specific common unobserved factors. For example, because they have accumulated the same expertise on the common past job former coworkers might be exposed to the same skill-specific labor market shocks. Finally, a selection bias may arise if individuals with better networks are more likely to search for a job. In general, most of these sources of correlation have to be assumed away because, lacking detailed information on contacts' identity and on the process of network formation, reference groups are usually proxied on the basis of some cross-sectional measure of spatial, cultural or social proximity[5]. This implies

[5]For example, Bayer et al. (2005) study job referrals among residential neighbors under the assumption that, within census block groups, individuals are randomly distributed across blocks; Bertrand et al. (2000) explore social effects in welfare participation within ethnic groups at a given residential location under the assumption that individuals of the

that network characteristics exhibit no variation within these groups, preventing controls for omitted variables at those levels of aggregation.

We recover individual-specific networks drawing on longitudinal matched employer-employee social security records that cover any work episode over the period 1975-1997 in a small area in northern Italy. The data provide information on employment status and employer identity at monthly frequency allowing to establish for any pair of individuals whether, when and for how long they worked together at a specific firm. We assign to each job seeker a specific network by tracking his previous employment history and identifying all his former fellow workers. In this setting two individuals will be endowed with the same network only if their employment histories fully overlap. This generates narrow sources of identifying variation, for example within residential and working locations, industry, demographic groups and, importantly, firms.

We consider workers entering unemployment because of firm closures[6]. This allows to focus on exogenous unemployment spells and to overcome the potential selection bias arising if individuals with better networks are more likely to start searching. More importantly, it allows estimating network effects by comparing individuals who are employed at the same firm when they simultaneously start searching. This has two main advantages. On the one hand, if workers are sorted across firms along some unobserved dimension correlated with relevant network characteristics (say, ability), comparing individuals displaced by the same firm absorbs this source of correlation. On the other, comparisons of the outcomes of co-displaced workers ensure that all shocks common to co-displaced are taken into account. For example, those related to the specific location, sector of activity and other characteristics of the firm as well as to the closure date.

Even within closing firms the correlation between individual outcomes and network characteristics

same ethnicity at different residential locations do not differ in unobservables correlated with welfare use.

[6]Most administrative datasets do not record the reasons why a given employment relationship ended. Focusing on firm closures thus isolates a subset of exogenous separations. The data we use are checked so that false firm closures (e.g. change of name, break-ups, etc.) are identified and fixed.

may be driven by omitted factors not accounted for by comparisons of co-displaced workers. This may happen if a displaced and his contacts are exposed to different shocks than other co-displaced and their contacts, for example because an individual and his network have accumulated similar skills while working together in the past, and these differ from those of other co-displaced; similarly, co-displaced workers may reside at different locations and so may their contacts so that relevant local labor market conditions differ within closing firms. Individual-specific networks allow to control for a number of such factors by means of time-varying effects for residential location and skill type[7]. Alternatively, network members may share unobserved fixed characteristics which differ among co-displaced. For example, a displaced and his contacts may be of higher ability than another co-displaced and his contacts. Because we observe the entire employment and earnings history, we can control for such potential sources of bias with lagged values of the the displaced wages and employment propensity[8]. Notice however that these additional controls are needed only if sorting along the relevant dimension fails *exclusively* in the closing firm. In fact, if sorting took always place according to the same rule then comparisons of co-displaced workers would account for the correlation between unobservables and network characteristics; on the other hand, if workers were always randomly assigned to firms there could be no omitted variable bias induced by sorting. Finally, we control for a variety of former employers' characteristics to address the possibility that prior to displacement the individual strategically selected firms on the basis of observable firms' characteristics.

In summary, our main identifying assumption is that the conditional cross-sectional variation in network employment rate at displacement date is orthogonal to individual unobserved heterogeneity within closing firms, residential location and skill type. The assumption would fail if the controls missed individual fixed characteristics that - although shared by past coworkers in pre-displacement

[7]More specifically, we define skill type dummies on the basis of the sector where the displaced spent most of his tenure in the pre-displacement period.

[8]We cannot estimate our model allowing for individual fixed effects because only very few individuals experience more than one closure within the time window we consider.

firms (i.e. by one's contacts) - are not shared by the co-displaced and - although not affecting pre-displacement wages and employment - do affect them *after* displacement.

Interpretation

A spillover effect of contacts' current employment status is consistent with information sharing, whereby better connected individuals collect more job-related information and are more easily re-employed. However, such an effect is also consistent with other mechanisms of interaction[9]. For example, a larger share of employed contacts may increase the opportunity cost of unemployment in the presence of certain social norms or because of peer pressure (Akerlof (1980)); it may also improve the possibilities of financing job search, in ways similar to the mechanisms underlying households' labor supply choices (Swaim and Podgursky (1994), van der Klaauw (1996), Manacorda (2006)). While still of interest, the presence of such mechanisms would lead to different positive and normative conclusions.

Tracing the empirical evidence to specific channels of interaction is a difficult task. In general, all interaction mechanisms will affect a job seeker behavior through his optimal search strategy, which is based on his information on the current status of the network. For example, peer pressure induces the displaced to modify his behavior depending on his assessment of his contacts' status. In other words, he will lower his reservation wage if he knows, suspects or expects more of his contacts to be employed. Similarly, expectations of a higher arrival rate, perhaps because of the larger share of contacts, will lead him to raise his acceptance threshold. However, if the current network status affects search outcomes also through the information channel then even unexpected innovations may have an effect. Consider a displaced that, based on his information on the network, sets his reservation wage and begins search. If a larger than expected share of contacts is employed and if this generates

[9] More generally, Manski (2000) groups the social effects into those working through an agent's constraints, through her expectations and through her preferences.

additional information, then he will be more easily re-employed than a comparable displaced with the same expectations and a lower than expected share of employed contacts. These differences are however unlikely to affect behaviors through other channels because they were not in the relevant information set when setting the optimal search strategy. The argument can be formalized within a simple search model. Let us assume that both the utility flow when unemployed, $v(ER)$, and the arrival rate of job offers, $\lambda(ER) = \exp(\beta ER + \gamma X)$, depend on the network employment rate: $v(ER)$ represents channels that affect the cost of unemployment, such as peer pressure; the information channel is instead represented by $\lambda(ER)$. Consider now a displaced who only imperfectly observes the employment rate of his network, perhaps because a full survey of his contacts' current employment status is too costly. His subjective assessment will be based on his information set I which may include information on contacts' characteristics, on the current stance of the labor market and so on. Such an agent will therefore set a reservation wage based on his expectations of the arrival rate $E(\lambda(ER)|I)$ and utility while unemployed $E(v(ER)|I)$, $w^R(E(v(ER)|I), E(\lambda(ER)|I)) = w^R(I)$. Under these assumptions, the log of observed unemployment duration of displaced i can be written as $u_i = -\gamma ER_i - \beta X_i + \theta w^R(I_i) + \epsilon_i$ (Kiefer (1998)), where we have assumed for notational simplicity that the distribution of wage offers faced by the displaced is of the exponential form $F(w) = 1 - \exp(-\theta w)\,, \quad \theta > 0, w \geq 0$. A regression of observed durations on ER_i and X_i would thus yield an estimate $\hat{\gamma} = \gamma + cov(ER_i, w^R(I_i))/V(ER_i)$. Since $cov(ER_i, w^R(I_i)) = cov(E(ER_i|I_i), w^R(I_i)) \neq 0$, failing to control appropriately for the determinants of the reservation wage confounds the evidence, both because the displaced may be subject to peer pressure, thus determining a relationship between the reservation wage and the perceived employment rate, and because his optimal search strategy reflects the expectations about the arrival rate.

Our reading of the results relies on this intuition. The empirical strategy laid out in the previous section aims at isolating idiosyncratic innovations in network employment rate at displacement date unanticipated by the displaced and therefore unlikely to be included in the information set underlying the reservation wage policy. This is achieved by conditioning, among other factors, on an unusually large set of predictors of the displaced labor market status and earnings as well as on detailed common factors, such as local labor market conditions. Further evidence that our estimates do not reflect mechanisms that affect the relative utility of unemployment is obtained as follows. First, we develop a number of contact-specific predictors of employment status at displacement date and include them in the baseline specification. These predictors are obtained from auxiliary fixed-effect and probit regressions that exploit all the available longitudinal information on contacts' characteristics and employment and earnings histories. If the identifying variation is due to unexpected innovations in the network employment rate then baseline estimates should not be significantly affected by the additional information provided by these indicators. Second, we look at the effect of the network employment rate on entry wages. Because the optimal reservation policy includes all the information available to the job seeker, if identification relies on unanticipated innovations to the share of employed contacts we should expect to find no significant association.

3 The data and the environment

The data cover over 13 millions employment relationships and 1.2 million employment histories over the period 1975-1997 in two Italian provinces[10]. Individuals are tracked if they move to other areas of the country. Each record describes an employment relationship, providing information on the months covered in the position, individual demographics (including age, gender and places of birth and of residence), weekly earnings, and employer information (3-digit industry, location, date of birth and closure if occurred). We only retain workers who enter unemployment because of firm closures, that

[10] A province is an administrative unit composed of smaller towns. The two provinces we focus upon are Treviso and Vicenza, located in the northern region of Veneto, and contain, respectively, 121 and 95 towns, each with an average working-age population of about 5,000.

is those who were still employed by the firm in its last month of activity.

An individual's social network is defined as all fellow workers he worked with for at least one month over the 5 years prior to firm closure, excluding co-displaced workers. We thus consider only closures occurred over the sub-period 1980-1994. This provides a 5-year pre-displacement window over which the network is recovered for all sampled individuals and a minimum 3-year post-displacement window to track re-employment. We focus only on completed unemployment spells. The final sample includes approximately 9,000 working-age individuals displaced by about 1,000 manufacturing firm closures whom we observe in another job after displacement.

Table (1) reports some descriptive statistics of co-displaced workers and closing firms. Rows represent variables for which we have computed means at the closing firm level; columns report statistics on the sample distribution of these means. Co-displaced workers are relatively young, the median closing firm with an average age of about 27, and typically blue collar workers. They tend to live in the same local labor market (LLM) where their employer is located, although not in the same smaller town[11].

Survey evidence supports the presumption that the workplace is an important place for developing social connections. The 2001 Special Eurobarometer survey reports that in Italy over 70 percent of employees have good friends on the workplace; similar shares are found in all other European countries. In addition, several features of the labor market we focus upon suggest that fellow workers are likely to meet daily, to stay in touch, and to have access to valuable job-related information. It is concentrated in a small geographic area (about 1,900 mi^2), and is highly self-contained (over 80 percent of manufacturing workers in the area are also residents; 70 percent were born there). It is a tight and dynamic labor market (the employment rate of people aged 25-50 is 80 percent and their unemployment rate at about 2 percent), characterized by small one-plant firms, three quarters of them employing at most 13 workers. Finally, economic activity is very dense, with 23 manufacturing firms

[11]A local labor market is defined as a cluster of smaller towns characterized by a self-contained labor market, as determined by the Italian national statistical institute (Istat) on the basis of the degree of workday commuting by the resident population. The 1991 population census identified 19 local labor markets in the two provinces under analysis.

and 345 manufacturing employees per square km, and dominated by two big industries (textiles and machinery) that account for more than half of local employment[12].

Figure (1) reports the distribution of network employment rate and size. Workplace networks are of limited size, a consequence of the small firm size in the underlying labor market. The median number of contacts is 32 and 90 percent of displaced have less that 150 links. Contacts are typically employed at displacement date. On average, the network employment rate is about 67 percent, with a standard deviation of about 20 percentage points. In figure (2) we describe several demographic characteristics of the networks. Contacts generally live nearby the displaced, the median network displaying an average distance of contacts from the displaced of about 3.5 miles, and generally in the same LLM. However, as for co-displaced workers, within LLMs contacts do not appear to be clustered in the same towns. Contacts are slightly more likely to be males, reflecting the higher participation rates of men; on average they are young, 90 percent of the networks with an average age of about 36; networks do not appear to be clustered by age, the median average age difference just below 10 years. Overall, individual networks appear to be rather heterogeneous allowing to absorb a number of potential sources of spurious correlation between their characteristics and individual outcomes.

Finally, we will focus on completed unemployment spells. The empirical distribution is depicted in figure (3). Completed unemployment spells are rather short, the median at 5 months and the average at about 10; only 5 percent lasts longer than 3 years. However, the fact that we only retain completed spells may raise concerns on the meaningfulness of the estimates either because of the mechanical truncation at time-varying thresholds for unemployment duration or because of the more relevant possibility that labor market participation, and thus selection into the sample, occurs on the basis of

[12]As a benchmark, in Santa Clara county (1,300 mi^2) - apparently the heart of Silicon Valley - the 2000 US Census reports about 13 private non-farm establishments and 250 private non-farm employees per square km, with an average size of private non-farm establishments of about 20 employees. The employment rate of people 16 years and over was 64.5 percent and the unemployment rate 3.7 percent, against a 62 percent employment rate and a 3.1 percent unemployment rate for the same population in the labor market we study at the end of the 90s (calculations based on data from the US Census 2000 Gateway, http://quickfacts.census.gov/qfd, and Istat's Labor Force Survey).

network characteristics. In section (7) we show neither issue to be empirically relevant.

4 Results

4.1 Baseline results.

Table (2) reports results for several specifications of a regression of (log) unemployment duration on the employment rate of the network at displacement date and on (the log of) network size. The first column of the table only accounts for a limited set of individual characteristics (age, sex, tenure and qualification at closure) and for the closing firm fixed effect (CFFE). The identifying variation in the network employment rate thus stems from differences between workers contemporaneously displaced by the same firm. Unemployment duration turns out to be significantly and negatively correlated with the network employment rate while no effect of (log) network size is detected. A causal interpretation of such estimates relies on the assumption that within closing firm contacts' characteristics do not proxy for unobserved determinants of individual unemployment duration. The assumption would be satisfied even if the displaced have not been randomly assigned to fellow workers prior to displacement, as long as the assignment rule is stable over time so that it holds also in the closing firm. Under this hypothesis, the within-firm variation of network characteristics is orthogonal to unobserved determinants of unemployment duration.

Knowledge of each individual's employment history allows us to weaken this assumption and to account for the possibility that, while correlated with the network employment rate, individual unobserved characteristics differ among co-displaced workers. First, in column (2) we augment the basic specification with the displaced earnings profile (captured combining average wage at closure and average wage growth) and the average length of his unemployment spells over the 5 pre-displacement years[13]. Intuitively, if sorting occurs along unobservable characteristics that reflect into wages or

[13]Results are unchanged if we allow for a considerably more flexible specification that considers the whole pre-

employment likelihood over time (e.g. ability), accounting for past individual realizations of these outcomes absorbs the within closing firm residual correlation between unemployment duration and network characteristics. In fact, while both indicators are significantly correlated to unemployment duration, attracting the expected signs, the coefficient on the network employment rate is largely unaffected.

Second, we account for the possibility that the relevant unobservables, while not reflected into individual pre-displacement outcomes such as wages and unemployment, are correlated with the characteristics or the number of past firms. Compensating wage theory suggests that workers might sort across firms on the basis of their preferences for the combination of wage and non-wage benefits offered by the firm (Rosen (1986)). Thus, for example, large firms may be able to attract better workers by offering fringe benefits such as day care, health insurance, meals (Woodbury (1983), Oyer (2005)). Similarly, they are shown to be more likely to provide training opportunities to their employees (Oi and Idson (1999)). As to the number of job switches, it may be associated with changes in the working environment[14]. In column (3) we thus account for the average size, the number of firms the unemployed visited in the pre-displacement period and a measure of propensity to commute[15]. Inclusion of such controls yields a somewhat larger and more precise estimate of the effect of network employment rate.

Finally, we address the possibility that our results are driven by shocks common to network members and not captured by the CFFE. This would be the case if, for example, contacts have accumulated the same specific skills - but co-displaced workers differ in the skills they accumulated in the past - so that different networks could be subject to different industry-specific shocks. Similarly, if individuals

displacement wage and employment history in the estimating equation.

[14]Our data do not allow to distinguish the causes of job separations. The number of visited firms could therefore either capture voluntary job-switching, plausibly associated with improved working conditions (including the quality of co-workers), or involuntary separations due to firing, plausibly signaling poor worker quality.

[15]Notice that controlling for average firm size and the number firms visited prior to displacement imply, in particular, that variation in the measure of network size is induced by coworkers turnover at each past firm.

mostly work locally - but not while in the closing firm - they would be largely subject to the same local shocks as their contacts. In column (4) we augment the specification with a full set of year-specific local labor market effects for the displaced LLM of residence and year-specific 3-digit industry effects corresponding to the sector where the displaced accumulated the longest pre-displacement tenure[16]. Identification thus hinges on variation in contacts' labor market status within closing firm, within LLM and within industry. This specification may however fail to capture industry-LLM specific shocks. For example, a new plant requiring a specific skill in a given LLM would plausibly affect workers endowed with that skill and living in the LLM differently from co-residents with different skills or individuals with similar skills from other LLMs[17]. This would be a concern if co-displaced workers (and their networks) were different in terms of LLM-skills combinations. Ideally we would include a full set of interaction effects of year, 3-digit and LLM to account for this possibility. However this would saturate the model. In column (5) we thus experiment with a modified set of dummies and allow for a full set of 2-digit industry-LLM-year interactions together with town and 3-digit industry fixed effects to absorb permanent differences among towns in the same LLM (e.g. distances) and among sub-industries belonging to the same 2-digit sector (e.g. skills). In both specifications we still find a statistically significant negative effect of the network employment rate on unemployment duration. Note also that time-varying residential location effects account for the potential presence of residential neighborhood effects.

The estimated coefficients in the last two columns imply that a one standard deviation increase in network employment rate (corresponding to about 20 percentage points) reduces unemployment duration by about 7 percent, around 3 weeks for the average unemployment spell. As a benchmark, increasing individual wage at displacement by one standard deviation would imply a reduction in

[16] We have experimented with other plausible definitions of sector experience and results were unaffected. For example, we have used dummies for the most recent visited sector excluding the closing firm, which is captured by the CFFE.

[17] LLM-industry shocks may of course also be events taking place in other industries or LLMs that affect in the same way people with the same skills and in a given LLM. For example, a plant closing in a given LLM-industry would possibly have effects on neighboring LLMs and sectors through general equilibrium effects.

unemployment duration of about 10 percent, 4 weeks at the average duration.

4.2 Alternative interpretations

Under the identifying assumption that the (conditional) variation in contacts' employment rate at displacement date is orthogonal to unobserved determinants of unemployment duration, the estimates presented above represent a causal effect that is consistent with the working of informal job search channels, whereby better connected job seekers have an advantage in collecting job-related information. However, as discussed in section (2), these empirical findings are also consistent with other interaction mechanisms. For example, they may reflect peer pressure or social concern whereby the perception that one's social ties will be employed (either because they are more able or because they comply with the norm) leads the displaced to put more effort into search. While still of interest, the presence of such mechanisms would lead to different positive and normative conclusions.

The exercises presented below implement the falsification strategy outlined in section (2) by augmenting the baseline specification with direct measures of contacts' ability based on longitudinal observations on their employment and earnings performance and testing additional implications of the presence of alternative sources of spillover. Results are reported in table (3), whose column 1 displays the relevant estimates from our baseline specification.

In columns (2) and (3) we relate the displaced unemployment duration to the employment rate of his network measured in periods prior but close to displacement. If the coefficients estimated in the baseline specification (col. (1)) reflected persistent behavioral differences across networks (e.g. social norms), we should expect to find similarly significant correlations with the network employment rates at past but close points in time. The two columns report results for the employment rate measured 2 and 3 years prior to displacement. In neither case we find a statistically significant correlation[18].

[18] In both cases a contact is considered employed if he was working more than 6 months. Alternative definitions of the pre-displacement network employment rate yield substantially equivalent results

In columns (4) to (6) we augment the baseline specification with several measures of contacts' ability. If estimates of the effects of network employment rate were traceable to variation in average ability across networks rather than to information circulation we would expect the baseline estimate to be weakened by directly controlling for ability. In column (4) we consider a proxy based only on contacts' wages at firms-years where they met the displaced. Specifically, we augment the baseline specification with the network average of residuals from an auxiliary cross-sectional regression of wages on a set of observable individual and job characteristics so as to account for observable differences among contacts that reflect into wages but are not necessarily correlated with their innate ability[19]. While the average ability of the network turns out to be weakly and positively correlated with unemployment duration, the estimated effect of the network employment rate is unaffected and turns out to be even larger and more precisely estimated. In columns (5) and (6) we exploit the longitudinal information on each contact to proxy for contacts' ability. We recover individual-specific effects from panel regressions of contacts' wages (col. 5) and fraction of year spent in employment (col. 6) on a set of individual controls and augment the baseline specification with the average ability of contacts[20]. Inclusion of these proxies leaves the estimated coefficient on the current employment rate largely unaffected[21].

Columns (7) and (8) directly address the possibility that the estimated effect reflects the *expected* component of the current employment status of the network. A displaced may respond to a higher expected employment rate of his contacts because he embeds the privileged access to information in his search strategy; alternatively, and along the same lines discussed above, peer pressure and social concern may lead to search more intensively. We augment the baseline specification with a measure

[19] Specifically, we consider a quadratic in age interacted with sex and qualification, sex, age, qualification, time, residential location and sector dummies.

[20] More specifically, individual log yearly real wages over the period 1980-1995 were projected on (log) weeks worked, a quadratic in age, its interaction with a qualification dummy, year and sector effects; the resulting individual fixed effects were further regressed on a sex dummy. As to the employment propensity, contact fixed effects are estimated from sex-specific linear regressions of the fraction of the year spent in employment over the 5 years prior to displacement on a quadratic in age and year-LLM interactions.

[21] Incidentally note that this result also provides further support to the claim that estimates do not reflect an omitted variable bias traceable to sorting of workers across networks on the basis of their unobserved ability.

of the predicted employment rate of the network. While an important determinant of the expected employment status of a contact is his ability, current local labor market conditions and other contacts' characteristics also do play a role. We thus obtain the predicted probability of employment for each contact at displacement date from an auxiliary probit regression of their current (at displacement date) employment status on a sex-specific quadratic in age, a full set of time and town of residence effects and the wage of the contact in the firm-year he met the displaced to account for unobservable (to us) heterogeneity among contacts which may affect their expected (by the displaced) employment status. Results show that such proxy for the expected employment rate has no statistically significant effect in itself (col. 7) and that it does not affect the estimated effect of the current network employment rate (col. 8).

A final indirect check that the source of identifying variation is unexpected (to the displaced) innovations to network employment rate is based on entry wages. As previously discussed, a job seeker sets his reservation wage on the basis of his information on, among other things, network status. For example, if he perceives a higher arrival rate because of his better connections he would raise the threshold for accepting an offer; alternatively, if peer effects are such that utility while unemployed is lower the more contacts are employed the displaced would be willing to accept lower wages, because he attaches a higher value to employment than an otherwise identical individual with less employed connections. On the contrary, unexpected innovations to network status could not be embedded in the reservation wage policy and thus would not reflect into subsequent observed wages. Following this line of reasoning, in column (9) we project the displaced observed entry weekly wage on the same set of covariates included in the baseline specification for unemployment duration. We find no significant effect of the current network employment rate. Complementary regressions that include the several proxies introduced above for contacts' quality and predicted employment status along with the employment rate confirm this finding, consistently with the initial claim that the identifying variation in the employment rate is unexpected by the displaced.

Taken together, the results in table (3) suggest that unpredictable innovations to the current employment rate of the network have a statistically significant and economically relevant negative effect on unemployment duration. We interpret this evidence as the effects of information sharing among related individuals, whereby job seekers with better connections fare better in the labor market.

5 Information availability and diffusion

The findings of the previous section are consistent with the main assumption of network models of the labor market that employed contacts have privileged access to job relevant information and circulate it in the network. Here we further refine those findings exploiting the heterogeneity among contacts along dimensions plausibly correlated with the usefulness of the information they can convey and with the likelihood of or willingness to share it with the job seeker.

Our first exercise looks at contacts' recent job turnover. Intuitively, contacts that have recently changed job have plausibly engaged in some search activity and collected information which can be spread in the network. Recent job switchers should therefore be more conducive to the transmission of relevant information than contacts who did not experience job changes since they met the job seeker. To verify this hypothesis we distinguish between currently employed contacts who still maintain the job where they met the displaced (stayers, S_i) and those who meanwhile changed employer[22](movers, M_i). We split the overall network employment rate in the share of movers (M_i/N_i) and of stayers (S_i/N_i), where $E_i = M_i + S_i$. Results in column 1 of table (4) show this distinction is highly relevant since among currently employed contacts it is mostly recent job switchers who contribute to re-employment. Based on these estimates, a one standard deviation increase in the network employment rate (about 20 percent) achieved by bringing into new jobs currently unemployed contacts (thus increasing the

[22]Of course we observe actual job changes. A stayer may thus have been searching not finding anything attractive. Still he could have collected valuable information.

share of job switchers) would shorten unemployment duration by around 12 percent (about 5 weeks at the average spell); the effect would be less than a half if the higher employment rate was achieved by keeping currently unemployed contacts into the jobs where they met the displaced. We see this result as strongly supportive of our identification strategy. Conditional on the set of covariates, contacts' mobility choices are most likely orthogonal to unobservable determinants of the displaced unemployment duration. These results say that contacts more up to date with the current stance of the labor market are more helpful in re-employment. However, other characteristics of contacts' current employment are also likely to determine the usefulness of the information exchanged. Intuitively, if contacts circulate information they collect locally then the environment to which they are exposed is most likely a determinant of the employment opportunities they can inform about. Below, we focus on contacts' sectoral affiliation and working location.

Contacts' sectoral affiliation is a realistic proxy for the skill content of the jobs they can inform about. Based on this intuition, Bentolila, Michelacci and Suàrez (2004) show that information networks may lead to worse employment outcomes if contacts are employed in industries whose technology the displaced is unfamiliar with or whose required skills he is not endowed with. We define a metric of skill distance between the displaced and the contact matching the current industry affiliation of each contact to that in which the displaced accumulated the longest tenure. According to our definition, close contacts are those employed in the sector more relevant to the displaced We exploit both 2- and 3-digit sector definitions, with the intuition that contacts outside the broader 2-digit sector are farther away than contacts outside the narrow 3-digit but still within the 2-digit aggregation. Results in columns 2 and 3 of table (4) show that technological distance is a relevant factor for the effectiveness of information networks. Contacts outside the broad 2-digit classification seem to play no role in helping re-employment while, within the 2-digit industry, those closest to the displaced skills (employed in the relevant 3-digit) appear to be more helpful.

A second aspect we consider is contacts' working location and its proximity to the displaced. If job seekers have a preference for working close to home, the information that contacts working closer to the displaced residence are exposed to is more likely to be relevant. We recover measures of contacts' current workplace distance from the displaced residence and define close (CE_i) and far (FE_i) contacts those working at a distance below and above the sample median, respectively. In column 4 of table (4) we report results obtained replacing displaced i's overall network employment rate E_i/N_i with the shares of close and far contacts, $CE_i/N_i + FE_i/N_i = E_i/N_i$. Spatial proximity of contacts' current working location turns out to play a significant role. Holding the overall employment rate constant, an increase in the share of close contacts by a one standard deviation of the overall employment rate reduces unemployment duration by a week[23]. Because proximity also increases the likelihood of interaction, this result could be seen as evidence that close contacts matter because they are the ones interaction occurs with rather than because they convey more relevant information. Our definition of network allows to address this question in a clean way. While most existing studies define a network on the basis of residential proximity, precisely because it is a plausible proxy for the likelihood of interaction, we define the relevant pool of contacts on the basis of their common working experience. This implies that within networks contacts differ in terms of residential location (see fig. (2)). We thus reclassify contacts on the basis of the distance of their residential location from the job seeker's following the same strategy used for working location: close (far) residential contacts are the ones living at a distance below (above) the sample median distance between residential locations. It turns out that both the median distance from work and the median distance from residence are about 7.5 km (4.5 miles). Not surprisingly, the two definitions are significantly correlated: living nearby the displaced increases the probability of working nearby the displaced by almost a half; still, about one

[23]Interestingly, results not reported here show that the findings on technological and spatial proximity are enhanced if we further consider the job switcher status of (technologically or spatially) close and far contacts: close job switchers turn out to be the most relevant source of information.

fourth of residential neighbors works farther away[24]. In column 5 of table (4) we replace the shares of close and far contacts based on working location with that based on residential location. Results do not show any significant difference between the two types of contacts: both are equally effective in reducing unemployment, supporting the interpretation that the findings in the previous column are indeed driven by the higher relevance of information conveyed by working neighbors rather than by the higher likelihood of interaction with them.

Next, we address the question how ties' strength helps job finding. This may happen both because stronger ties are more likely to interact with or because they are more willing to transfer information[25]. Specifically, our data allow to develop a measure of ties' intensity based on common tenure at the workplace where the displaced and a given contact met. Since this is based on an actual interaction, it plausibly measures the likelihood with which two individuals will interact in a finer way than standard measures based on common residential location and sociodemographic traits. As above, we define weak (strong) ties as those contacts with whom the displaced worked less (longer) than the sample median tenure, a year in our data[26]. In column 6 of table (4) we replace the overall network employment rate with the shares of strong (SE_i/N_i) and of weak (WE_i/N_i) employed contacts ($E_i = SE_i + WE_i$) in the network. Ties' intensity with employed contacts turns out to be a relevant determinant of job search success: an increase of one standard deviation in the overall employment rate of the network obtained by raising the number of strong ties reduces unemployment duration by 9 percent (nearly a month at the average spell); the effect is lower, below 3 weeks, if the higher employment rate stems

[24]Specifically, the joint distribution of working (W) and residential (R) neighbors is such that: $G(W = 1, R = 1) \approx 0.37 \approx G(W = 0, R = 0)$ and $G(W = 0, R = 1) \approx 0.13 \approx G(W = 1, R = 0)$.

[25]Economists are increasingly paying attention to how the type of relationship entertained by two individuals shape their behaviors and economic choices. For example, in a series of recent papers Bandiera, Barankay and Rasul, have addressed the role of friendship ties on work effort of co-workers (Bandiera, Barankay and Rasul (2007b)) and on incentives provided to the workforce in the presence of such ties (Bandiera, Barankay and Rasul (2007a)); in a different setting Datcher Loury (2006) shows that obtaining a job through a prime age male relative leads to higher wages.

[26]Note that our operational definition is different from the standard concept of weak and strong ties adopted in the sociology literature. There a tie between two individuals is stronger the more their sets of contacts overlap. Granovetter (1995) argues that weak ties are more conducive of information precisely because they are exposed to different environments.

from a larger share of weaker ties[27].

Finally, we ask whether contacts' current match quality affects their propensity to transfer information. Models of job information networks typically assume that contacts transfer information they become aware of and are not interested in (see, for example, Calvo-Armengol (2004) and Calvo-Armengol and Jackson (2004)). An important element of such interest is certainly the wage being offered relative to the one currently earned by the contact. To quantify this incentive we need to know the position of a contact in the relevant distribution of wages. The intuition is that the higher the rank the less likely he is to retain information for himself. If there was no heterogeneity across individuals, current wages would be the natural index to look at. However, because individuals are different simply comparing wages across contacts would be incorrect. To overcome this problem, we develop a wage-based index of how well contacts are currently matched factoring out the effect of individual characteristics. Formally, let $w_{jf} = bZ_{jf} + \mu_j + \phi_{jf}$ be the (log) wage of contact j at firm f, with Z_{jt} contact and firm observable characteristics, μ_j contact fixed unobservable characteristics and ϕ_{jf} match-specific characteristics; we are interested in measuring the latter. We implement this definition using the residual of a regression of observed contacts' (log) wage on contact and firm observable characteristics and on contact past wage to proxy for individual unobservables[28]. This provides an estimate for ϕ_{jf} that is then averaged at the network level. Intuitively, networks with higher contacts' average wage premium should be networks where more information is circulated. Results obtained augmenting the baseline specification with our index of propensity to share information, column 7 of table (4), show it has no significant effect on unemployment duration[29].

The findings in this section confirm that employed connections are an important channel through

[27] Results not reported here show that the result is robust to alternative plausible thresholds, for example computed on the overall distribution of the duration of joint employment spells.

[28] Specifically, the control set includes, together with contact past wage, sex- and qualification-specific quadratics in age, and dummies for sex, qualification, 3-digit sector, contact residence and time.

[29] Experiments with slightly different specifications of the conditioning set in the auxiliary regression yield the same results.

which information on employment opportunities is circulated. We find a stronger role for contacts whose characteristics make them likely to be exposed to more relevant information and more likely to interact with the displaced. These findings are based on the implicit assumption that contacts are exclusive in that they are only linked to the job seeker. However, the role of indirect connections both as additional information generators and as potential competitors has been well emphasized in the theoretical literature (Calvo-Armengol (2004) and Calvo-Armengol and Jackson (2004)). In the next section we provide a first empirical assessment of the effects of indirect connections and network structure on the duration of job search.

6 Indirect connections: competitors and information providers

We explore the role of two types of indirect ties, direct competitors and indirect information providers. Both issues are typically hard to address because, lacking information on the structure of the networks, it is impossible to recover indirect links. Moreover, in studies in which networks are proxied by some metric of proximity the implicit assumption is that groups are fully isolated from each other. This is not the case in our setting. Since we observe the whole structure of social links determined by our definition of the relevant network we can easily recover indirect links among individuals.

We begin with the role of competition for the information generated in the network. The advantages of a good connection may be reduced by stronger competition for information because, ceteris paribus, it makes it less likely to actually learn about a given job opportunity. In our setting a natural measure of such kind of competition is the contemporaneous presence of other displaced job seekers. Specifically, we proxy the degree of competition for the information held by a given contact j with the number of displaced individuals he is *contemporaneously* connected to, D_j. Therefore a displaced individual i connected to contact j will have to compete with $D_j - 1$ other displaced job seekers. We thus augment the baseline specification with the network average number of such competitors, that is

$(\sum_{j\in C(i)} D_j)/N_i$. Notice that variation across co-displaced workers is induced by differences in the number of contemporaneously displaced individuals by a different firm closure their contacts are linked to. Such measure provides an exogenous shift in the degree of competition for a given information source as long as common sources of displacement across firms (e.g. business cycle shocks) are absorbed by the closing firm fixed effect. Results reported in column 2 of table (5) show that a higher degree of competition significantly slows down re-employment. Increasing the number of competitors by 10 units (roughly corresponding to a shift from the 1st to the 3rd quartile in our sample) raises unemployment duration by 7 percent, roughly equivalent to the effects of a 20 percent reduction in the employment rate.

Indirect connections are also a channel to improve the information content of a given tie. As Granovetter (1973) noticed, a contact whose network does not overlap with that of the job seeker is more likely to provide novel information than one who shares most of his contacts with the unemployed; the latter would most likely be a duplicated information source. To explore the relevance of this argument we implement two exercises. First, we assign to each contact a specific network of employees. Consistently with the specific network we have looked at, we proxy a contact-specific network with his *current* co-workers[30]. In column 3 of table (5) we augment the baseline specification with the (log) average size of indirect networks. Results do not show any significant effect of indirect ties. A second exercise, aiming at assessing the role of duplicated information sources, consists in augmenting the baseline specification with the (log) number of *firms* a displaced is connected to through his contacts. Intuitively, if contacts gather information by word-of-mouth on the workplace, having one's contacts more concentrated in a given firm would imply more duplication of relevant information and, ceteris paribus, less effective connections. Results in column 4 are indeed consistent with this intuition. Doubling the number of firms holding constant the number of employed contacts reduces

[30]To be fully consistent we should have recovered for each of the contacts all individuals he worked with over the prior five years. The exercise turned out to be computationally burdensome.

unemployment duration by about 15 percent.

7 Discussion and Further Robustness Checks.

Throughout the paper we have focused on a sample of individuals observed in employment after exogenous displacement due to firm closures. Completed unemployment spells account for over 80 percent of sampled displacements. Thus, truncation may affect a non negligible fraction of spells that would have been completed had the observation window been larger. Several considerations suggest that calendar-date truncation is not likely to be a major determinant of our findings, however. First, uncensored spells are relatively short: the median length is 5 months, the average is 10 and only about 5 percent lasts longer than 36 months. This suggests that the fraction of right censored spells at the end of 1997 should be limited even for 1994 closures, the last wave we retain in the sample. In fact, our results are robust to restricting the sample to closures up to 1990, for whom truncation should be much less relevant due to the wider window allowed for reentry[31]. Second, while the administrative nature of our data does not allow to fully describe labor market participation decisions, the observable characteristics of non re-entrants suggest that most of them might not be actively participating, because of either fertility (about half of non re-entrants are women aged 20-34) or retirement (about one fifth are aged 50 or more) decisions[32]. This intuition is further supported by the fact that the share of non re-entrants is rather constant across displacement years whereas we would expect it to increase as we approach the end of the sample if it was related to sample censoring. A related and potentially more relevant concern is that our estimates are inconsistent because of a sample selection bias if post-displacement participation decisions are affected by the employment rate in the network. Displaced endowed with better networks are more likely to participate because of the

[31] We also experimented with imposing common truncation rules at 36, 48 and 60 months to all unemployment spells originating from closures occurred between 1980 and 1990 to obtain a balanced sample. Results were largely unaffected.

[32] Labor force survey data show that in the area we study more than 20 percent of unemployed young women is back in employment after one year, while about 75 exits the labor force; similarly, more than 90 percent of unemployed older people exits the labor force after one year while about 5 percent are in employment.

more favorable odds of receiving a job offer. However, if this was the case, the selection process would generate an attenuation bias: people with otherwise longer expected durations tend to search because of more effective connections, and thus a positive correlation between network employment rate and individual unobserved unemployment determinants would arise.

Table (6) reports several experiments that address the truncation issue. First, we run a standard two-step estimate. Ideally, we should exploit a set of plausible exclusion restrictions to properly identify network effects on unemployment duration when formally accounting for the participation decision. Unfortunately, such instruments are unavailable and we have to rely on functional form identification and thus on tail behavior of the inverse Mills ratio. With these caveats in mind, the estimated effect of network employment rate and its precision are in line with our previous results (col. 1). Second, we estimate a set of linear models for the probability of being still jobless after 9, 12 and 15 months from displacement on all spells originating from sampled firm closures (cols. 2-4). Consistently with the main results in the previous sections, we still find that a higher network employment rate reduces the probability of unemployment at the various horizons.

A final puzzling feature of our results is the absence of any effect of the size of the network (table (2)). However, this may be a consequence of the measurement error induced by defining network size as the simple count of pre-displacement coworkers. In particular, we may be assigning too many contacts to some individuals. For example, if an individual cannot maintain more that Z contacts the measurement error would be zero whenever the number of contacts does not exceed the threshold and $\epsilon_i = C_i - Z$ otherwise, where C_i is the measured extension. Under these assumptions the measurement error would display a mechanical and positive correlation with the underlying true network, C_i^*, generating the standard attenuation bias. We attempt to shed light on this issue and develop a way to correct the size measure assuming that, above a certain threshold Z, the individual meets a coworker only with some probability. Let us assume we can rank coworkers in a firm of size $N > Z$ with some distance metric from the displaced (say, because they work in different units), and that the probability of meeting farther individuals decays with distance at rate γ. Let the $P^n = e^{-\gamma \max\{0,n-Z\}}$ the probability of meeting coworker who is in position $n = \{1, ..., N\}$. Because the true ranking within a firm is unknown the probability that coworker i is in position n of the ranking is $P(n_i = n) = 1/N$ [33]. Therefore, the probability that the displaced actually meets coworker i is given by $P_i = \sum_{n=1}^{N} P(n_i = n) * P^n = \sum_{n=1}^{N} P^n/N$. Making use of the definition of P^n, after some algebra, we obtain $P_i = \left(Z + (e^{-\gamma}/(1-e^{-\gamma}))(1-e^{-\gamma(N-Z)})\right)/N$. Knowing Z and γ we can thus weight each assigned coworker and redefine network measures accordingly. In table (7) we use the corrected network size measures and present results under alternative assumptions on Z and γ. Results suggest that measurement issues may explain the absence of scale effects in previous specifications. Even assuming a slow decay of the probability of meeting additional workers we detect some negative effect of scale consistently with theoretical predictions. The effect loses significance as we increase the threshold or lower the decay rate, thereby going back to the original error-ridden measure. Reassuringly, in comparison with those reported in table (2) the results on the effects of the network employment rate are largely unaffected by the correction.

8 Conclusions

Local and non-market interactions have received a lot of attention as potential causes of persistent segregation and differential behaviors along a number of dimensions. The sources of these effects can be manifold: social norms, peer pressure, conformism, information sharing. In this paper we have shown that job search outcomes of exogenously displaced workers are significantly affected by the employment rate of their contacts when entering unemployment and by a number of other features of their network related to the relevance and likelihood of information exchanges on available employment

[33] This probability is obtained noticing that in firm of size N there are $N!$ possible rankings of the workers and $(N-1)!$ rankings such that a given position is occupied by a specific coworker.

opportunities. Unemployment spells are significantly shorter when a larger share of contacts are currently employed; the effect is magnified by contacts' recent job search activity and when their current employer is closer, both in space and in skills requirements, to the displaced. We find that stronger ties enhance network effectiveness. By recovering the entire map of direct and indirect connections, we show that sharing an employed contact with unconnected individuals simultaneously searching for a job weakens its effect on job finding rates; also, contacts' effectiveness is weakened when they are exposed to the same working environment. Results are robust to inclusion of direct measures of contacts' ability and of contact-specific predictors of current employment based on their employment and earnings histories up to displacement. We view this finding as supportive of the interpretation that the estimates reflect the effect of unexpected innovations to contact's current employment status. Consistently with this argument, we find no effect of contacts' employment on the displaced subsequent earnings, suggesting that the identifying variation is not embedded in the optimal reservation wage set by displaced job seekers.

Overall, the results show that individual employment has significant spillover effects on job finding rates of socially connected unemployed individuals. We argue these spillover effects reflect the increased availability of job-related information to job seekers generated by their employed connections. As such, the findings show that information networks and informal hiring channels are an important means to overcome information shortages even in a small and dense local labor market populated by largely homogeneous individuals as the one we study.

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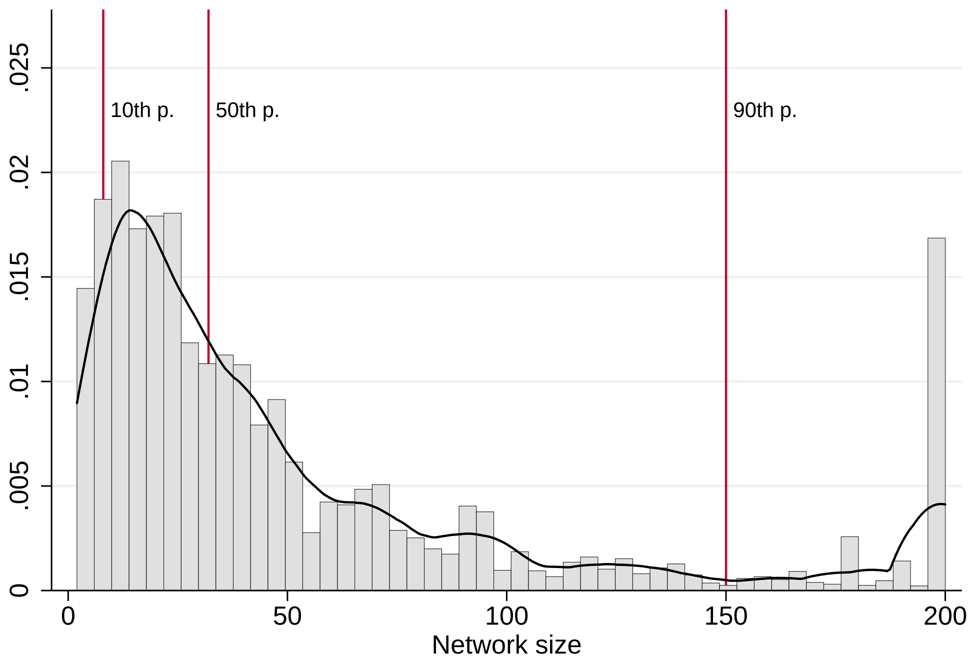
Figure 1: Network characteristics: size and labor market status.



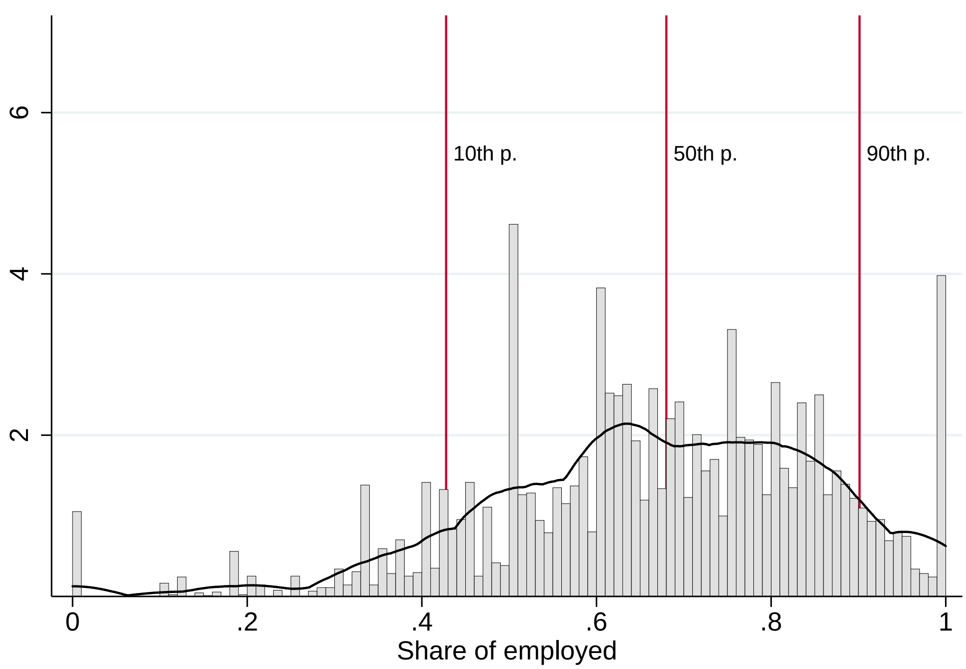


Figure 2: Network characteristics: geography and demography.

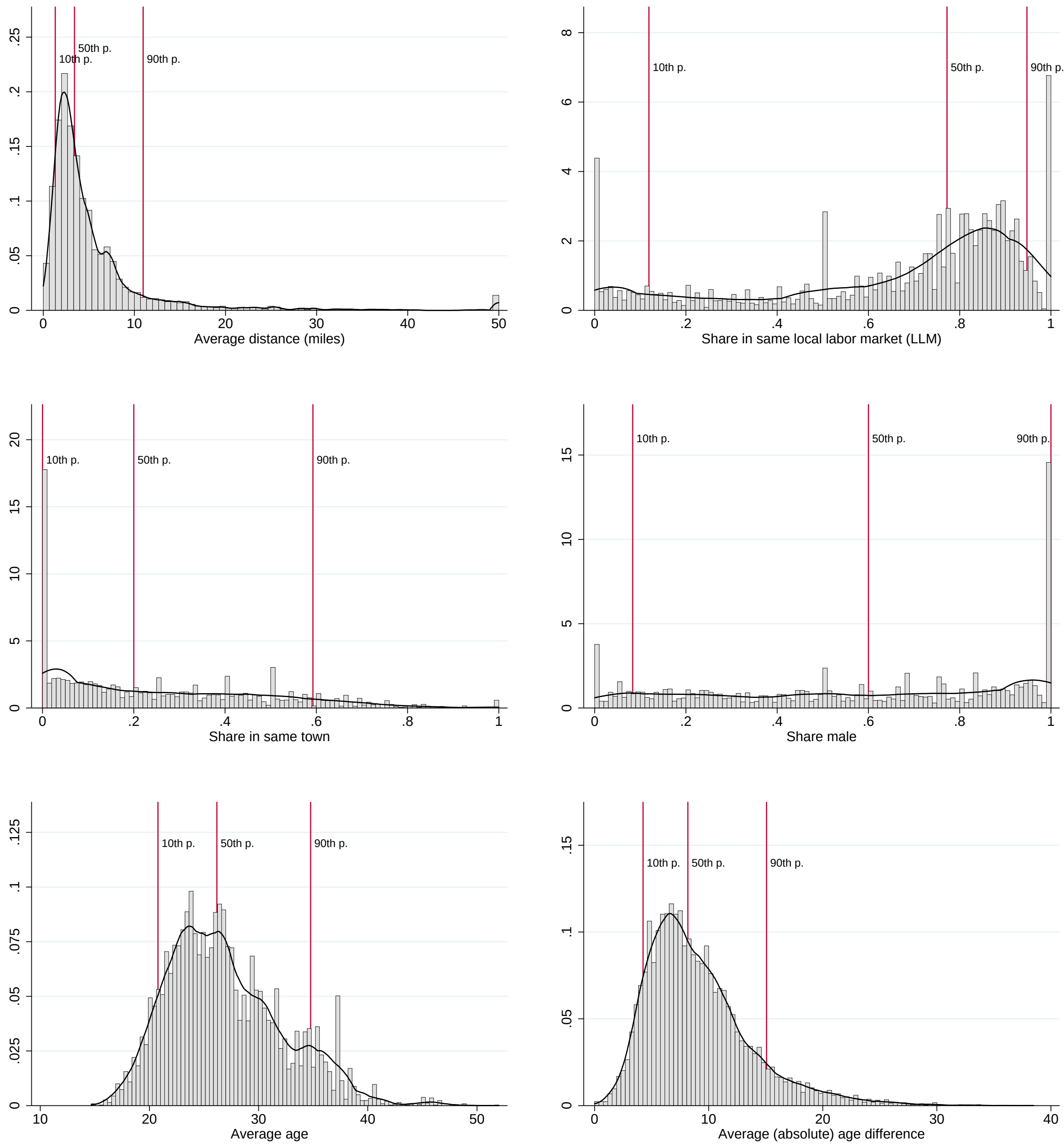


Figure 3: Unemployment duration.

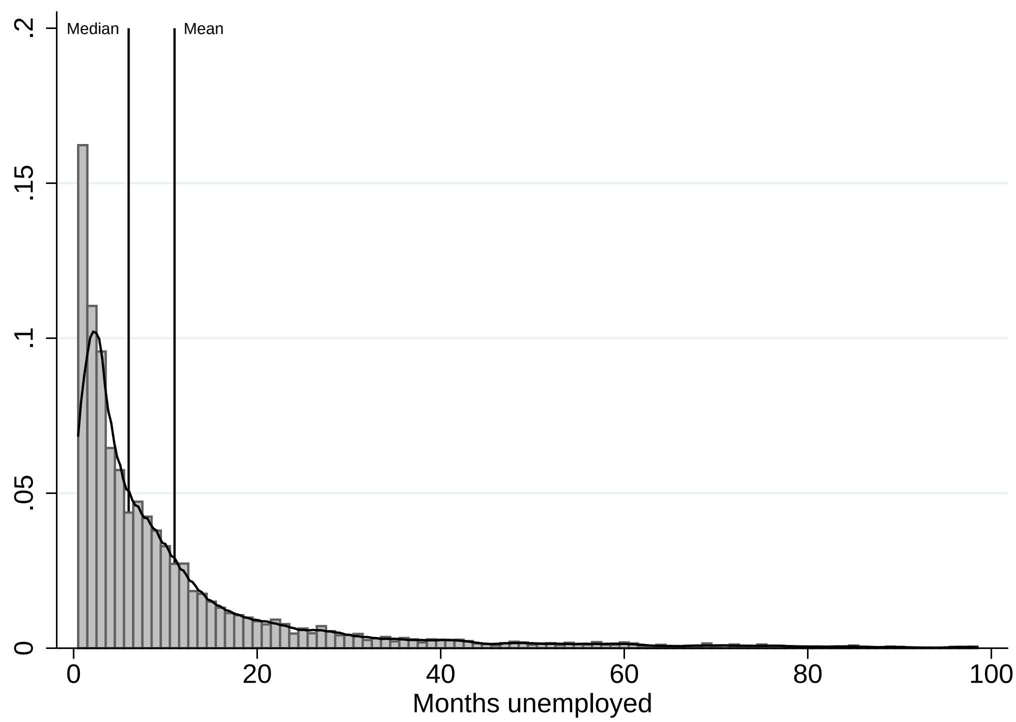


Table 1: Closing firms and codisplaced workers: descriptive statistics.

	(1)	(2)	(3)	(4)	(5)
	Percentile			Mean	Standard Deviation
	10th	50th	90th		
Number of codisplaced	1	5	15	7.6	10.2
Average Age	20	27	38	28	7
% Male	0	66.7	100	57.1	39.8
% Blue Collar	0	100	100	82.0	32.8
% live in:					
- same LLM as closing firm	14.3	88.9	100	76.0	31.8
- same town as closing firm	0	33.3	100	38.2	33.2

Table entries are the corresponding column statistic computed on the sample distribution of the closing-firm level row variable. Codisplaced workers are defined as those working in the closing firm in the last month of activity.

Table 2: Unemployment duration.

	(1)	(2)	(3)	(4)	(5)
(log) Network size	-0.0274	0.0224	-0.0294	-0.0571	-0.066
	(0.017)	(0.020)	(0.038)	(0.044)	(0.047)
Network employment rate	-0.294*	-0.314**	-0.385**	-0.336*	-0.348*
	(0.12)	(0.12)	(0.13)	(0.15)	(0.125)
Wage at displacement		-0.231**	-0.228**	-0.235**	-0.290**
		(0.060)	(0.059)	(0.066)	(0.073)
Pre-displacement wage growth		0.133	0.130	0.0673	0.197
		(0.11)	(0.11)	(0.12)	(0.132)
Pre-displacement unemployment		0.398**	0.521**	0.446**	0.433**
		(0.083)	(0.11)	(0.12)	(0.129)
N. of firms visited prior to displacement					
– 1			-0.274**	-0.347**	-0.390**
			(0.092)	(0.11)	(0.115)
– 2			-0.188**	-0.244**	-0.287**
			(0.066)	(0.078)	(0.086)
– 3			-0.073	-0.110	-0.145†
			(0.062)	(0.074)	(0.082)
Average size of firms prior to displacement			0.039	0.064	0.069
			(0.049)	(0.056)	(0.062)
Average commuted distance prior to displacement			-0.005	0.060	-0.031
			(0.037)	(0.12)	(0.133)
Closing firm FE	Y	Y	Y	Y	Y
Year X Local labor market	N	N	N	Y	N
Year X 3-dig sector experience	N	N	N	Y	N
Year X 2-dig sector exp. X LLM	N	N	N	N	Y
Town and 3-dig sector FE	N	N	N	N	Y
Obs.	9121	9121	9121	9121	9121

Robust standard errors in parentheses.
(†) significant at 10%, (*) significant at 5%, (**) significant at 1%.
Dependent variable is the (log of) months spent unemployed after displacement. All regressions also include controls for gender, a quadratic in age and tenure in the closing firm and four qualification dummies. Sector experience dummies are defined on the basis of the longest pre-displacement sector tenure of the displaced.

Table 3: Alternative mechanisms.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline	Past employment		Contacts' ability			Expected employment		Entry wage
		-2 yr	-3 yr	Wage	Wage FE	Empl. FE			
(log) Network size	-0.057	-0.068	-0.066	-0.051	-0.059	-0.057	-0.068	-0.054	0.013
	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.014)
Network employment rate	-0.336*			-0.381**	-0.319*	-0.342*		-0.367*	-0.051
	(0.15)			(0.15)	(0.15)	(0.15)		(0.16)	(.049)
Past employment rate		0.112	0.011						
		(0.17)	(0.17)						
Contacts' ability				0.265†	-0.112	0.069			
				(0.15)	(0.22)	(0.28)			
Expected employment rate							-0.253	0.160	
							(0.34)	(0.38)	
Wage at displacement	-0.235**	-0.236**	-0.235**	-0.242**	-0.234**	-0.235**	-0.237**	-0.237**	0.144**
	(0.066)	(0.066)	(0.066)	(0.066)	(0.066)	(0.066)	(0.066)	(0.066)	(0.024)
Pre-displacement unemployment	0.446**	0.411**	0.402**	0.454**	0.445**	0.448**	0.424**	0.438**	-0.099*
	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.041)
Observations	9121	9121	9121	9121	9121	9121	9119	9119	9121

Robust standard errors in parentheses. (†) significant at 10%; (*) significant at 5%; (**) significant at 1%.

Cols. (2)-(3) - past network employment rate is defined as the share of contacts employed at least 6 months 2 and 3 years prior to displacement date. Cols. (4)-(6) - contacts' ability is defined as the network average of: the residual from a linear regression of contact's weekly wage at the firm-year when met the displaced on sex-specific and qualification-specific quadratics in age, and full sets of 3-digit sector, town and effects (col. 4); estimated contact FE from a linear regression of contact wages over their employment history (col. 5); estimated contact FE from a linear regression of the yearly share of weeks spent in employment over the 5 pre-displacement years (col. 6). Cols. (7)-(8) - contacts' expected employment is defined as the network average of the predicted probability of employment estimated from a probit model of the employment status at displacement date on a sex-specific quadratic in age, contact past (log) wage and town and year effect.

Dependent variable is log of months unemployed (cols. 1-8) and log real weekly entry wage (col. 9). All regressions also include a closing firm fixed effect, controls for gender, a quadratic in age and tenure in the closing firm, four qualification dummies, wage growth prior to displacement, average size and dummies for the number of firms visited prior to displacement, average commuted distance prior to displacement, year-specific sector experience and local labor market effects. Sector dummies are defined on the basis of the longest pre-displacement sector tenure of the displaced.

Table 4: Information availability and circulation.

	(1) Baseline	(2) Contacts' turnover	(3) Distance from contacts': 2-dig	(4) 3-dig	(5) workplace	(6) residence	(7) Ties' intensity	(8) Propensity to share
(log) Network size	-0.057 (0.044)	-0.082† (0.045)	-0.058 (0.044)	-0.057 (0.044)	-0.057 (0.044)	-0.057 (0.044)	-0.060 (0.044)	-0.056 (0.044)
Wage at displacement	-0.235** (0.066)	-0.232** (0.066)	-0.231** (0.066)	-0.234** (0.066)	-0.236** (0.066)	-0.235** (0.066)	-0.231** (0.066)	-0.236** (0.066)
Pre-displacement unemployment	0.446** (0.12)	0.436** (0.12)	0.436** (0.12)	0.441** (0.12)	0.444** (0.12)	0.447** (0.12)	0.372** (0.13)	0.447** (0.12)
Network employment rate	-0.336* (0.15)							-0.334* (0.15)
Movers (M_i/N_i)		-0.581** (0.17)						
Stayers (S_i/N_i)		-0.252† (0.15)						
Close (CE_i/N_i)			-0.426** (0.15)	-0.373* (0.15)	-0.371* (0.15)	-0.328* (0.15)		
Far (FE_i/N_i)			-0.124 (0.17)	-0.298† (0.16)	-0.267† (0.15)	-0.345* (0.15)		
Strong (SE_i/N_i)							-0.445** (0.17)	
Weak (WE_i/N_i)							-0.307* (0.15)	
Match quality								-0.061 (0.18)
Obs.	9121	9121	9121	9121	9121	9121	9121	9121

Robust standard errors in parentheses. (†) significant at 10%, (*) significant at 5%, (**) significant at 1%.
Col. (2): movers are contacts that at displacement date are employed at a different workplace than the one where they met the displaced; stayers still keep the job where they met the displaced. Cols. (3)-(4): technologically close contacts are those currently employed in 2-digit (3) or 3-digit (4) sector where displaced accumulated the longest tenure in 5 years prior to displacement. Cols. (5)-(6): geographic distance from employed contacts' current workplace (5) and residence (6); close contacts are those (working or living) at a distance below the median distance across all networks. Col. (7): ties' intensity is measured by the number of months worked at the same workplace; strong contacts are those with common tenure above the sample median common tenure between displaced and their employed contacts. Col. (8): contacts' wage premium is measured by the network average residual of a regression of contacts' current (log) wage on their past wage, sex, a quadratic in age, sector, LLM and year dummies.
Dependent variable: (log) months unemployed. All regressions also include a closing firm fixed effecet, controls for gender, a quadratic in age and tenure in the closing firm, four qualification dummies, wage growth prior to displacement, average size and dummies for the number of firms visited prior to displacement, average commuted distance prior to displacement, year-specific sector experience and local labor market effects. Sector dummies are defined on the basis of the longest pre-displacement sector tenure of the displaced.

Table 5: Indirect connections.

	(1)	(2)	(3)	(4)
		Indirect networks		Networked
	Baseline	Competitors	Providers	firms
(log) Network size	-0.057	-0.014	-0.058	0.044
	(0.044)	(0.047)	(0.044)	(0.064)
Wage at displacement	-0.235**	-0.235**	-0.235**	-0.233**
	(0.066)	(0.066)	(0.066)	(0.066)
Pre-displacement unemployment	0.446**	0.475**	0.447**	0.421**
	(0.12)	(0.12)	(0.12)	(0.12)
Network employment rate	-0.336*	-0.276†	-0.337*	-0.334*
	(0.15)	(0.15)	(0.15)	(0.15)
N. of competitors		0.007*		
		(0.0030)		
N. of indirect links			0.003	
			(0.027)	
Networked firms				-0.146*
				(0.067)
Obs.	9121	9121	9121	9121

Robust standard errors in parentheses. (†) significant at 10%, (*) significant at 5%, (**) significant at 1%.
Col. (2): competition is measured by the average number of indirect connections to other contemporaneously displaced individuals. Col. (3): number of employees at contacts' current employers. Col. (4): log of the number of different contacts' current employers. Dependent variable: (log) months unemployed. All regressions also include a closing firm fixed effect, controls for gender, a quadratic in age and tenure in the closing firm, four qualification dummies, wage growth prior to displacement, average size and dummies for the number of firms visited prior to displacement, average commuted distance prior to displacement, year-specific sector experience and local labor market effects. Sector experience dummies are defined on the basis of the longest pre-displacement sector tenure of the displaced.

Table 6: Robustness checks.

Dependent variable:	(1) Unemployment duration (log)	(2) Still unemployed after: 9 months	(3) 12 months	(4) 15 months
(log) Network size	-0.050	-0.065**	-0.085**	-0.073**
	(0.037)	(0.017)	(0.016)	(0.015)
Network employment rate	-0.348**	-0.182**	-0.126*	-0.101†
	(0.125)	(0.055)	(0.053)	(0.052)
Wage at displacement	-0.218**	-0.118**	-0.114**	-0.109**
	(0.047)	(0.021)	(0.020)	(0.020)
Pre-displacement unemployment	0.442**	0.090*	0.067	0.071†
	(0.098)	(0.046)	(0.045)	(0.043)
Observations	11057	11057	11057	11057

(†) significant at 10%, (*) significant at 5%, (**) significant at 1%.
Col. (1): control set includes Heckman correction term from auxiliary selection model (see text for details). Cols. (2)-(4): dependent variable is dummy equal to 1 if still unemployed after (column head) number of months; control set is that of baseline specification. All regressions also include a closing firm fixed effect, controls for gender, a quadratic in age and tenure in the closing firm, four qualification dummies, wage growth prior to displacement, average size and dummies for the number of firms visited prior to displacement, average commuted distance prior to displacement, year-specific sector experience and local labor market effects. Sector dummies are defined on the basis of the longest pre-displacement sector tenure of the displaced.

Table 7: Measurement error corrections.

	(1)	(2)	(3)	(4)
Z:	5	10	15	20
		$\gamma = 0.25$		
(log) Network size	-0.153*	-0.111†	-0.097†	-0.088†
	(0.071)	(0.058)	(0.052)	(0.050)
Network employment rate	-0.403*	-0.395*	-0.384*	-0.374*
	(0.160)	(0.156)	(0.154)	(0.152)
		$\gamma = 0.75$		
(log) Network size	-0.177*	-0.117†	-0.100†	-0.091†
	(0.079)	(0.060)	(0.053)	(0.050)
Network employment rate	-0.403*	-0.397*	-0.387*	-0.377*
	(0.162)	(0.157)	(0.154)	(0.152)
		$\gamma = 1.25$		
(log) Network size	-0.184*	-0.118†	-0.101†	-0.091†
	(0.081)	(0.060)	(0.054)	(0.050)
Network employment rate	-0.403*	-0.398*	-0.387*	-0.377*
	(0.162)	(0.157)	(0.154)	(0.153)
Observations	9121	9121	9121	9121

Robust standard errors in parentheses.
(†) significant at 10%, (*) significant at 5%, (**) significant at 1%.
All regressions also include a closing firm fixed effect, controls for gender, a quadratic in age and tenure in the closing firm, four qualification dummies, wage growth prior to displacement, wage at displacement, pre-displacement time in unemployment, average size and dummies for the number of firms visited prior to displacement, average commuted distance prior to displacement, year-specific sector experience and local labor market effects; sector experience dummies are defined on the basis of the longest pre-displacement sector tenure of the displaced. Network characteristics are computed weighting each contact acquired in a firm of size N by $P_i = \left(Z + (e^{-\gamma}/(1-e^{-\gamma}))(1-e^{-\gamma(N-Z)})\right)/N$ if $N > Z$ and $P_i = 1$ otherwise.