People I Know: Job Search and Social Networks

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We assess the strength of information spillovers relating unemployment duration of workers displaced by firm closures to their former colleagues' current employment status. Displaced-specific networks are recovered from a 20-year panel of matched employer-employee data. Spillovers are identified by comparing performances of codisplaced workers. A one-standard-deviation increase in the network employment rate reduces unemployment duration by about 8%; the effect is magnified if contacts recently searched for a job and if their current employer is spatially and technologically closer to the displaced worker; stronger ties and lower competition for information favor reemployment. Several indirect tests exclude other interaction mechanisms.

I. Introduction

The aim of this article is to test whether the duration of unemployment of individuals exogenously displaced by firm closures is affected by the current employment status of their contacts and to establish whether this

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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 sociodemographic 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 could thus be higher than private returns, as individual employment improves the prospects of unmatched connected 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 a 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, 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 article, we focus on networks of former fellow workers. This is a relevant set of contacts to focus on because the workplace is a major source of social connections and because former colleagues are a natural

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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 1975–97. The data provide detailed information on individual sociodemographic characteristics, earnings and tenure at any job, employer's characteristics, and employment status at each point in time. Importantly, they allow us to identify each pair of coworkers and the common tenure at any given employer.

We define the network of fellow workers a displaced employee has access to on the displacement date as the pool of individuals he worked with for at least 1 month over a fixed predisplacement time window. This definition and the full coverage of the data allow us to recover the map of direct and indirect social connections of the displaced employee 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 us to assess the response of unemployment duration to contacts' current employment rate overcoming several identification issues commonly encountered in nonexperimental 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 individuals who have previously worked together, the displaced person 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

[1] This problem is especially important when a lack of information on the relevant network leads to approximations based on observed individual traits (e.g., 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), Luttmer (2005), and Bayer, Ross, and Topa (2008). Research on the effects of neighborhood quality on individual outcomes typically overcomes the problem of omitted individual characteristics exploiting programs that randomly give incentives to some households to move to more affluent neighborhoods (Katz, Kling, and Liebman 2001; Kling, Liebman, and Katz 2007) or directly assign individuals to other residential locations (Oreopoulos 2003); alternatively, Weinberg, Reagan, and Yankow (2004) explicitly model the individual residential choice.

together (e.g., they accumulate the same specific skills). We address these sources of bias in a number of complementary ways. 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., codisplaced) fellow workers share the same unobserved traits 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 workers' subsequent labor market outcomes, including predisplacement 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 the network employment rate is assumed to be orthogonal to individual unobserved traits that also affect employment and earnings. Finally, individual-specific networks allow us to control in a detailed way for omitted variable bias related to the specific labor market or residential location of the displaced workers by exploiting network variation within the relevant labor market, industry, and neighborhood.

We find that a larger share of currently employed contacts significantly shortens the 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% (roughly 3 weeks for the average spell). This effect is substantial: as a benchmark, a one-standard-deviation increase in 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 displaced workers, such as an additional randomly employed contact when the search spell exogenously begins, and argue that they are therefore unlikely to be driven by mechanisms of interaction other than the facilitation of job-relevant information.

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 statistically 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 worker's entry wages and the network employment rate. Because the reservation wage includes all the information available to the displaced worker, anticipated differences in contacts' employment status should be reflected in entry wages. However, we again fail to find any statistically significant correlation. Taken together, this evidence allows us 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.[2] The data allow us to explore important dimensions of heterogeneity across contacts, such as ties' intensity, job search activity, sectoral and spatial proximity, and 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 considerably 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 (e.g., Holzer 1988; Blau and Robins 1990; Simon and Warner 1992; Addison and Portugal 2002). 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 preestablished covillagers at the destination, thus increasing the incentives to migrate. Wahba and Zenou

[2] 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; Bramoullé and Saint-Paul (2010) have emphasized the role of social inbreeding, whereby ties are more easily maintained among employed individuals than between people in different labor market conditions, for the patterns of unemployment.

(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 a few attempt to trace such effects to local information exchange. The approach of Bayer et al. (2008) builds on the neighborhood literature; they use detailed residential and working location information 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 article 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 article proceeds as follows. In Section II, we outline the empirical model and discuss the main identification issues. Next, in Section III, we describe the data and the underlying labor market. We present the main results in Section IV and several extensions in Sections V and VI. A set of robustness checks is discussed in Section VII. Section VIII presents conclusions.

II. 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 worker i (u_i) to the share of employed contacts as of the starting date of the unemployment spell, t_0, the network employment rate ER_{it_0}:

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

where N_{it_0} is the overall size of the network, and X_{it_0} and e_{it_0} are, respectively, observed and unobserved determinants of unemployment duration.[3] 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 the lower competition for the opportunities circulated in the network. Interpretation of least-squares 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.

A. Identification

A causal interpretation of least-squares estimates of γ from (1) requires that network characteristics are uncorrelated with the residual. In nonexperimental 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 unobserved characteristics if the labor market sorts workers across firms along such a 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.[4] In

[3] Such a statistical representation implicitly assumes that the duration of unemployment spells is distributed exponentially, thus with a constant hazard rate. This would result, e.g., from a standard stationary search model in which the hazard of leaving unemployment is $\lambda[1 - F(w^R)]$, with λ the Poisson arrival rate of job offers, $F(w)$ their cumulative distribution, and w^R the optimally set reservation wage. We discuss this interpretation further in the following section.

[4] Studies of network effects are typically hindered by another, perhaps more relevant, difficulty. Manski (1993) shows that if individual outcomes reflect both contemporaneous and reciprocal influences of peers' outcomes (endogenous effect) and those of peers' characteristics unaffected by current behavior (contextual effect) and if individual outcomes result from a social equilibrium, it is impossible to separately identify the endogenous and the contextual effects in linear models of individual behaviors (the *reflection* problem). Several ways of overcoming such a fundamental difficulty have been put forth that rely on the specific structure of

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 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 1975–97 in a small area in northern Italy. The data provide information on employment status and employer identity at a monthly frequency, allowing us 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 at any of the firms he was employed in during the 5 years prior to displacement. 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 us to focus on exogenous unemployment spells and to overcome the potential selection bias arising if individuals with better networks

the network (see, e.g., Lee 2007; Bramoullé, Djebbari, and Fortin 2009) or of the decision problem (Brock and Durlauf 2001). However, our framework is unaffected by such a difficulty because we are not interested in the causal effect of group achievements on the same contemporaneous individual outcomes (as, e.g., in Bertrand et al. 2000; Duflo and Saez 2003; Calvo-Armengol, Patacchini, and Zenou 2009; De Giorgi, Pellizzari, and Redaelli 2010). Rather, in our setting we relate the duration of the subsequent unemployment spell of a displaced worker exogenously entering unemployment at t_0 to contacts' employment status at t_0. Therefore, contacts' outcomes are predetermined with respect to the subsequent outcome of the exogenously displaced worker instead of being jointly determined through a social equilibrium relationship. The combination of predetermined network characteristics and exogenous initiation of unemployment breaks the equilibrium relationship that hinders identification in the typical social effects empirical paper.

[5] For example, Bayer et al. (2008) 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 same ethnicity at different residential locations do not differ in unobserved traits correlated with welfare use.

[6] Most administrative data sets 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, breakups, etc.) are identified and fixed.

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 hand, comparisons of the outcomes of codisplaced workers ensure that all shocks common to the codisplaced workers 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 (e.g., business cycle conditions).

Even within closing firms the correlation between individual outcomes and network characteristics may be driven by omitted factors not accounted for by comparisons of codisplaced workers. This may happen if a displaced worker and his contacts are exposed to different shocks than other codisplaced workers 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 codisplaced workers; similarly, codisplaced 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 us to control for a number of such factors by means of time-varying effects for residential location and skill type. Alternatively, network members may share unobserved fixed characteristics that differ among codisplaced workers. For example, a displaced worker and his contacts may be of higher ability than another codisplaced worker 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 wages and employment propensity of the displaced worker.[7] 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 always took place according to the same rule, then comparisons of codisplaced workers would account for the correlation between unobserved traits 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 observed firms' characteristics.

In summary, our main identifying assumption is that the conditional

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

cross-sectional variation in the network employment rate at the 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 predisplacement firms (i.e., by one's contacts)—are not shared by the codisplaced worker and—although not affecting predisplacement wages and employment—do affect them after displacement.

B. 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 reemployed. However, such an effect is also consistent with other mechanisms of interaction.[8] 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's 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 worker 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 worker who, on the basis of his information on the network, sets his reservation wage and begins searching. If a larger than expected share of contacts is employed and if this generates additional information, then he will be more easily reemployed than a comparable displaced worker 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

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

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, $\lambda(ER) = \exp(\beta ER)$, 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 worker 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 worker i can be written as $u_i = -\beta ER_i + \theta w^R(I_i) + \epsilon_i$ (Kiefer 1998), where we have assumed for notational simplicity that the distribution of wage offers faced by the displaced worker has the exponential form $F(w) = 1 - \exp(-\theta w)$, $\theta > 0$, $w \geq 0$. A regression of observed durations on ER_i would thus yield an estimate

$$\hat{\gamma} = \gamma + \mathrm{Cov}[ER_i, w^R(I_i)]/V(ER_i).$$

Since

$$\mathrm{Cov}[ER_i, w^R(I_i)] = \mathrm{Cov}[E(ER_i|I_i), w^R(I_i)] \neq 0,$$

failing to control appropriately for the determinants of the reservation wage confounds the evidence, both because the displaced worker 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 the network employment rate at the displacement date unanticipated by the displaced worker and therefore is unlikely to be included in the information set underlying the reservation wage policy. This is achieved by conditioning on, among other factors, an unusually large set of predictors of the displaced worker's 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 the 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 our baseline estimates should not be affected by the additional information provided by these indicators in a significant way. 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 association.

III. The Data and the Environment

The data cover over 13 million employment relationships and 1.2 million employment histories over the period 1975–97 in two Italian provinces.[9] 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 (three-digit industry, location, date of birth, and closure if it occurred). We retain only workers who enter unemployment because of firm closures, that 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 1 month over the 5 years prior to firm closure, excluding codisplaced workers.[10] We thus consider only closures that occurred over the subperiod 1980–94. This provides a 5-year predisplacement window over which the network is recovered for all sampled individuals and a minimum 3-year postdisplacement window to track reemployment.[11] We focus only on completed unemployment spells. The final sample includes 9,121 working-age individuals displaced by 1,195 manufacturing firm closures whom we observe in another job after displacement. Importantly, geographic mobility induced by job displacement

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

[10] Notice that we recover the full network of contacts only for displaced workers. This implies that we cannot describe the full map of social connections in the area but only those of displaced workers. While the lack of a complete network map is inessential to the main purpose of the following empirical analysis, it prevents us from describing interesting features of the overall social environment as, e.g., in Goyal, van der Leij, and Moraga-González (2006).

[11] Although these conditions are necessary for an operational definition of the network, they are nonetheless arbitrary. However, we experimented with alternative lengths of the predisplacement window, finding largely unaffected results. As to the length of the joint employment spell required for being network members, we report results that explicitly relax the assumption in Sec. V.

Table 1
Closing Firms and Codisplaced Workers: Descriptive Statistics

	Percentile				
	10th (1)	50th (2)	90th (3)	Mean	Standard Deviation
Number of codisplaced workers	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

Note.—Table entries are the relevant statistics 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.

does not lead to sample selection as workers are tracked if they move to other areas of the country. However, only about 8% of displaced workers are reemployed at firms outside the area, and over three-quarters of them are still within daily commuting distance.[12]

Table 1 reports some descriptive statistics of codisplaced 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. Codisplaced workers are relatively young: the median closing firm has an average age of about 27 and includes 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.[13]

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% of employees have good friends in the workplace; similar shares are found in all other European countries. In addition, several features of the labor market we focus on suggest that fellow workers are likely to meet daily, to stay in

[12] In principle, geographic mobility might affect the network measures for those workers who spent a significant fraction of the 5 years prior to displacement at firms outside of the area, whose employees we cannot track. In practice, however, this is a concern for a very limited share of workers: reflecting the low degree of spatial mobility, nearly 92% of the displaced workers were always employed in the area during the relevant period and an additional 5% were employed there for at least 80% of the time. Restricting the analysis to workers who were always employed within the area does not affect the results of the article.

[13] An LLM 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 LLMs in the two provinces under analysis.

touch, and to have access to valuable job-related information. It is concentrated in a small geographic area (about 1,900 square miles) and is highly self-contained (over 80% of manufacturing workers in the area are also residents; 70% were born there). It is a tight and dynamic labor market (the employment rate of people aged 25–50 is 80%, and their unemployment rate is about 2%; in the rest of the country the corresponding figures are 67% and 8%, respectively), 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 and 345 manufacturing employees per square kilometer, and is dominated by two big industries (textiles and machinery) that account for more than half of local employment.[14]

Figure 1 reports the distribution of network size (top) and of its employment rate (bottom). 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% of displaced workers have fewer than 150 links.[15] Contacts are typically employed on the displacement date. On average, the network employment rate is about 67%, with a standard deviation of about 20 percentage points. Network size and employment rate are only weakly correlated: a linear projection of the former on the latter and a constant shows that 10 additional contacts are associated with a 0.1-percentage-point higher employment rate. A more detailed inspection of the relationship between network size and employment rate is displayed in figure 2. There we plot the mean and median employment rate by 5-percentage-point bins of network size corresponding to ventiles of its marginal distribution, together with the 20th and 80th centiles of the employment rate in the corresponding size bin. Again, there appears to be no systematic relationship but for the slightly higher dispersion of employment rates among smaller networks, a consequence of their limited size. In conclusion, this evidence suggests that recovering individual networks from previous working histories, thereby assigning larger networks to individuals employed at larger firms or with higher job turnover, does not introduce any systematic pattern in network employment rates.

[14] As a benchmark, in Santa Clara County, California (1,300 square miles)—apparently the heart of Silicon Valley—the 2000 US Census reports about 13 private nonfarm establishments and 250 private nonfarm employees per square kilometer, with an average size of private nonfarm establishments of about 20 employees. The employment rate of people 16 years and over was 64.5% and the unemployment rate 3.7%, against a 62% employment rate and a 3.1% unemployment rate for the same population in the labor market we study at the end of the 1990s (calculations are based on data from the US Census 2000 Gateway, http://quickfacts.census.gov/qfd, and Istat's Labor Force Survey).

[15] Such contacts are often related to other displaced workers (on average, to about four). We will exploit this fact in Sec. VI to measure the degree of competition for the information available in the network.

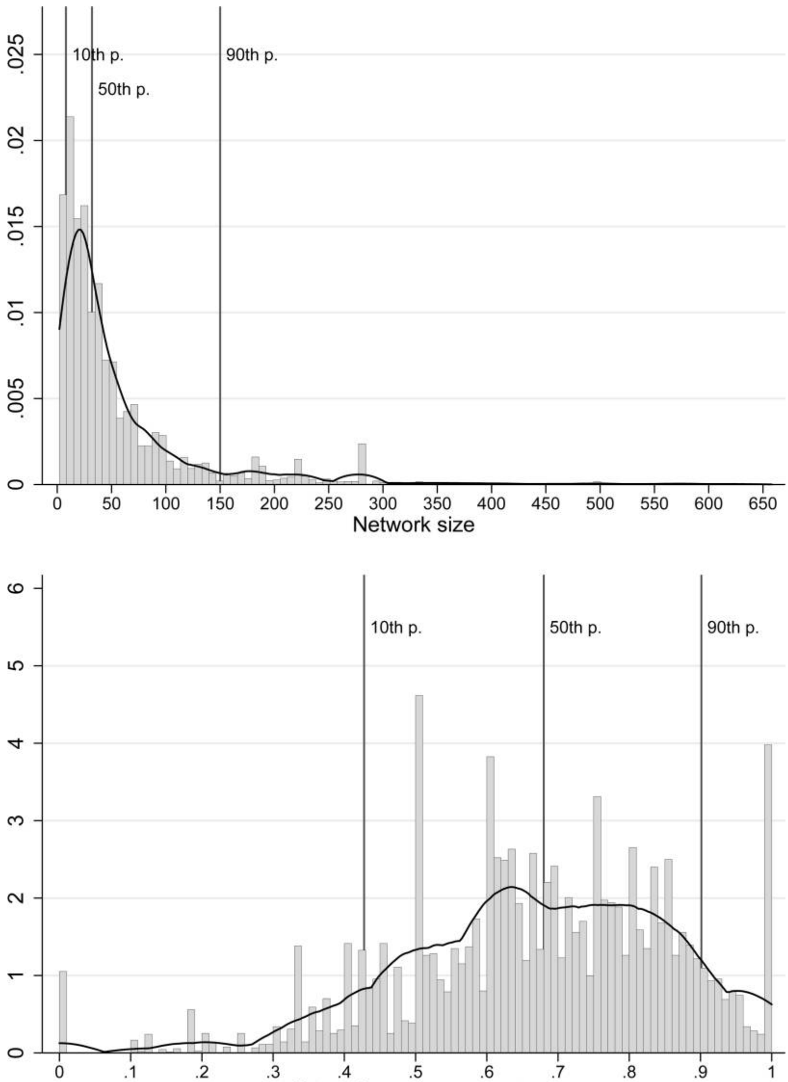

FIG. 1.—Network characteristics: size and labor market status. The figure reports the sample distribution of network size (top) and the network employment rate (bottom); the associated estimated Gaussian kernel density using the Stata default value for bandwidth is set as $b = 0.9 \min\{\mathrm{SD}(x), \mathrm{IQ}(x)/1.349\}/N^{(1/5)}$.

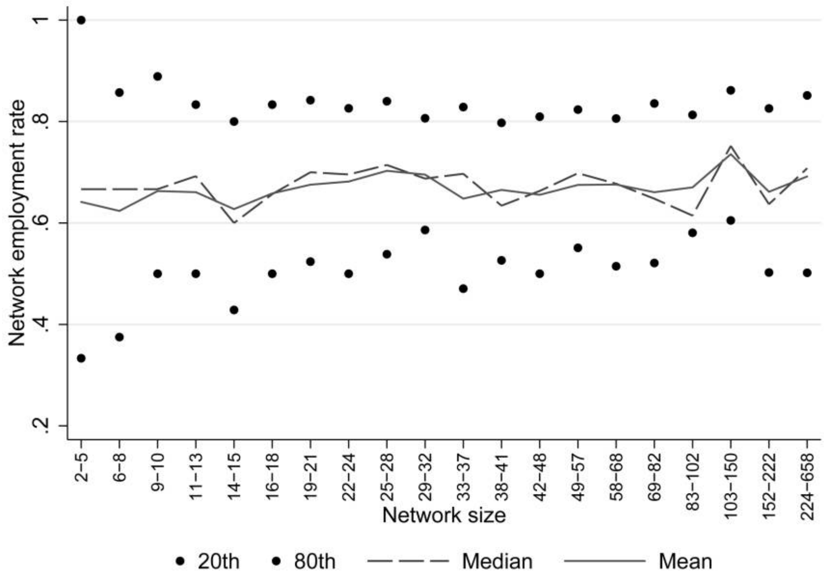

FIG. 2.—Network employment rate and size. The figure reports the 20th and 80th centiles, the median, and the mean of the network employment rate (vertical axis) for networks of the size within the bin reported on the horizontal axis. Bins correspond to ventiles of the overall distribution of network size.

In figure 3, we describe several demographic characteristics of the networks. Contacts generally live nearby the displaced workers, the median network displaying an average distance of contacts from the displaced workers of about 3.5 miles and generally in the same LLM. However, as for codisplaced 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% of the networks have an average age of about 36; networks do not appear to be clustered by age, the median average age difference being just below 10 years. Overall, individual networks appear to be rather heterogeneous, allowing us 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 4. Completed unemployment spells are rather short by European standards: the median at 5 months and the average at about 10; only 5% last longer than 3 years. However, the fact that we retain only completed spells may raise concerns about the mean-

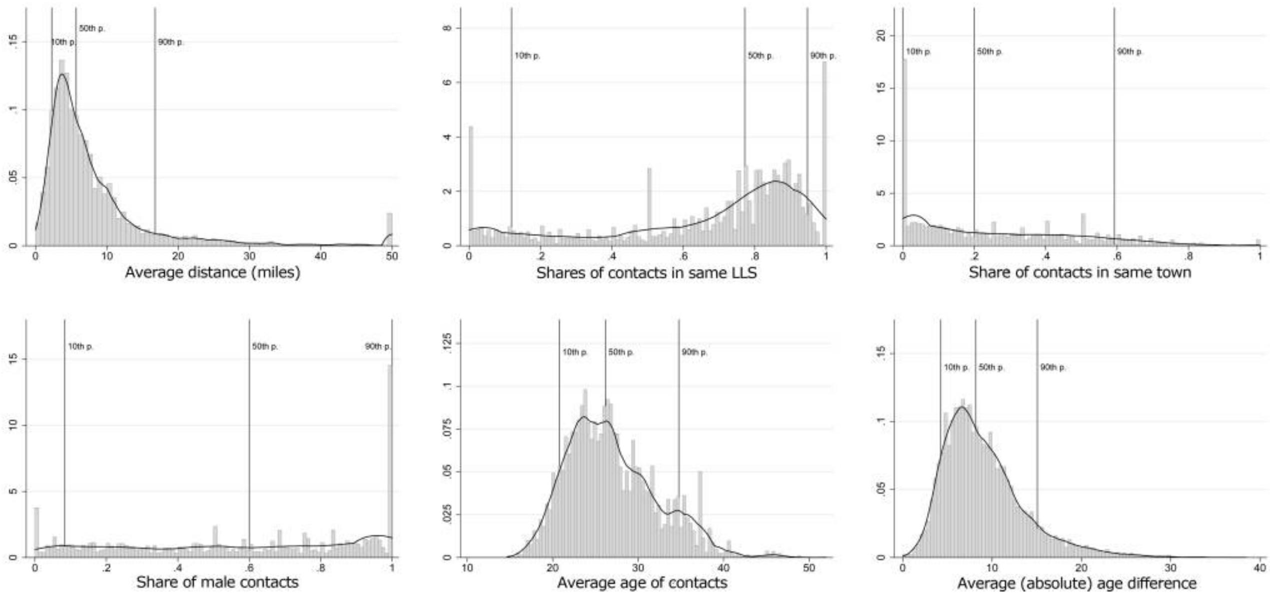

FIG. 3.—Network characteristics: geography and demography. The figure reports the sample distribution of the average distance between a displaced worker and his contacts, the proportion of contacts in the network who live in the same local labor market and town, the proportion of male contacts, the average age of contacts, and the average age difference between the displaced worker and his contacts. In all figures the observational unit is the displaced worker, and the distribution refers to characteristics of his network. Bandwidth for the Gaussian kernel is set using the Stata default value, $b = 0.9 \min\{\mathrm{SD}(x), \mathrm{IQ}(x)/1.349\}/N^{(1/5)}$.

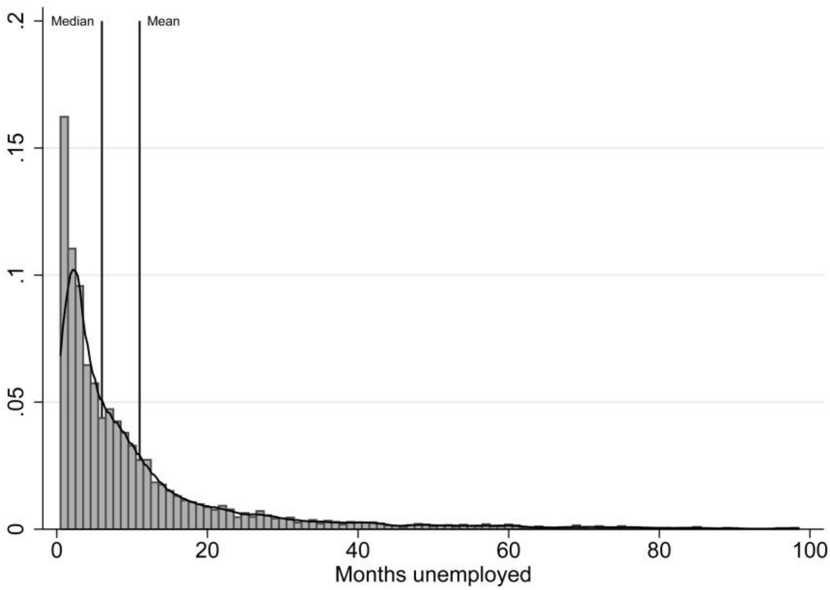

FIG. 4.—Unemployment duration. The figure reports the sample distribution of completed unemployment spell durations. Bandwidth for the Gaussian kernel is set using the Stata default value, $b = 0.9 \min\{\mathrm{SD}(x), \mathrm{IQ}(x)/1.349\}/N^{(1/5)}$.

ingfulness of the estimates either because of the mechanical truncation at time-varying thresholds for unemployment duration or because labor market participation, and thus selection into the sample, occurs on the basis of network characteristics. In Section VII, we argue that neither issue appears to be empirically relevant.

IV. Results

A. Baseline Results

Table 2 reports results for several specifications of a regression of (log) unemployment duration on the employment rate of the network at the displacement date and on (the log of) network size.[16] Column 1 of the table accounts for only 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. The correlation between unemployment duration and the

[16] A detailed description of the variables used in the regressions is available in the online data appendix.

Table 2
Unemployment Duration

	(1)	(2)	(3)	(4)	(5)
(Log) network size	−.027	.022	−.029	−.057	−.066
	(.017)	(.020)	(.038)	(.044)	(.047)
Network employment rate	−.294*	−.314**	−.385**	−.336*	−.348*
	(.120)	(.120)	(.126)	(.146)	(.125)
Wage at displacement		−.231**	−.228**	−.235**	−.290**
		(.060)	(.059)	(.066)	(.073)
Predisplacement wage growth		.133	.130	.0673	.197
		(.108)	(.110)	(.120)	(.132)
Predisplacement unemployment		.398**	.521**	.446**	.433**
		(.083)	(.105)	(.119)	(.129)
No. employers prior to displacement					
−1			−.274**	−.347**	−.390**
			(.092)	(.105)	(.115)
−2			−.188**	−.244**	−.287**
			(.066)	(.078)	(.086)
−3			−.073	−.110	−.145^{+}
			(.062)	(.074)	(.082)
Average size of firms prior to displacement			.039	.064	.069
			(.049)	(.056)	(.062)
Average commuted distance prior to displacement			−.005	.060	−.031
			(.037)	(.121)	(.133)
Closing firm fixed effects	Y	Y	Y	Y	Y
Year × LLM	N	N	N	Y	N
Year × three-digit sector experience	N	N	N	Y	N
Year × two-digit sector experience × LLM	N	N	N	N	Y
Town and three-digit sector fixed effects	N	N	N	N	Y
Observations	9,121	9,121	9,121	9,121	9,121

NOTE.—Huber-White robust standard errors are in parentheses. The 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. Predisplacement variables are computed over the 5 years prior to firm closure. Four or more predisplacement employers is the excluded category. See the data appendix for detailed variable definitions.
$^{+}$ Significant at 10%.
* Significant at 5%.
** Significant at 1%.

network employment rate is negative and statistically significant, but no statistically significant 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 workers 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 codisplaced workers. First, in column 2, we augment the basic specification with the displaced worker's earnings profile (captured combining average wage at closure and average wage growth) and the average length of his unemployment spells over the five predisplacement years.[17] Intuitively, if sorting occurs along unobserved characteristics that are reflected in wages or the 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 with 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 unobserved traits, while not reflected in individual predisplacement 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 nonwage 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, and 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.[18] In column 3, we thus account for the average size, the number of firms the unemployed worked at in the 5 years prior to displacement, and a measure of propensity to commute.[19] Inclusion of such controls yields a somewhat larger estimate of the effect of the 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

[17] Results are unchanged if we allow for a considerably more flexible specification that considers the whole predisplacement wage and employment history in the estimating equation.

[18] Our data do not allow us to distinguish the causes of job separations. The number of past employers could therefore capture either voluntary job switching, plausibly associated with improved working conditions (including the quality of coworkers), or involuntary separations due to firing, plausibly signaling poor worker quality.

[19] Notice that controlling for the number and the average size of past employers implies, in particular, that variation in the measure of network size is induced by coworker turnover at each past firm.

skills—but codisplaced 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 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 worker LLM of residence and year-specific three-digit industry effects corresponding to the sector in which the displaced worker accumulated the longest tenure in the 5 years prior to displacement.[20] Identification thus hinges on variation in contacts' labor market status within the closing firm, within the LLM, and within the 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 differently workers endowed with that skill and living in the LLM than coresidents with different skills or individuals with similar skills from other LLMs.[21] This would be a concern if codisplaced workers (and their networks) were different in terms of LLM-skills combinations. Ideally we would include a full set of interaction effects of year, three-digit industry, 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 two-digit industry-LLM-year interactions together with town and three-digit industry fixed effects to absorb permanent differences among towns in the same LLM (e.g., distances) and among subindustries belonging to the same two-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 columns 4 and 5 imply that a one-standard-deviation increase in the network employment rate (corresponding to about 20 percentage points) reduces unemployment duration by about 7%, around 3 weeks for the average unemployment spell. As a benchmark, increasing individual wage at displacement by one standard deviation

[20] Specifically, we compute the sector tenure cumulating the worker's firm-specific tenures by his three-digit industry affiliation. We have experimented with other plausible definitions of sector experience, and results were unaffected. For example, we have used dummies for the most recent sector excluding the closing firm, which is captured by the CFFE.

[21] 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.

would imply a reduction in unemployment duration of about 10%, 4 weeks at the average duration. On the other hand, we do not find evidence of a significant network size effect. This remains true using more flexible specifications, for example, using indicator variables for different classes of network size. In Section VII, we discuss to what extent this might be explained in terms of measurement error.

B. Alternative Interpretations

Under the identifying assumption that the (conditional) variation in contacts' employment rate at the 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 II, these empirical findings are also consistent with other interaction mechanisms. For example, they may reflect peer pressure or social concerns 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 worker 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 II 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, where column 1 displays the relevant estimates from our baseline specification.

In columns 2 and 3, we relate the displaced worker's unemployment duration to the employment rate of his network measured in periods prior to 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 similar results using network employment rates computed at past but close points in time. The two columns report our findings for the employment rate measured 2 and 3 years prior to displacement.[22] In neither case do we find a statistically significant correlation: the point estimates are quite different, but both fall well within the range of the (same) associated standard error.

In columns 4–6, we augment the baseline specification with several

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

Table 3
Alternative Mechanisms

		Past Employment		Contacts' Ability			Expected Employment		
	Baseline (1)	−2 Years (2)	−3 Years (3)	Wage (4)	Wage Fixed Effects (5)	Employer Fixed Effects (6)	(7)	(8)	Entry Wage (9)
(Log) network size	−.057	−.068	−.066	−.050	−.059	−.058	−.068	−.054	.013
	(.044)	(.044)	(.044)	(.044)	(.044)	(.044)	(.044)	(.044)	(.014)
Network employment rate	−.336*			−.382**	−.319*	−.356*		−.365*	−.051
	(.146)			(.148)	(.150)	(.174)		(.165)	(.049)
Past employment rate		.112	.011						
		(.170)	(.170)						
Contacts' ability				.278+	−.112	.042			
				(.150)	(.218)	(.188)			
Expected employment rate							−.253	.151	
							(.340)	(.381)	
Wage at displacement	−.235**	−.236**	−.235**	−.242**	−.234**	−.235**	−.237**	−.237**	.144**
	(.066)	(.066)	(.066)	(.066)	(.066)	(.066)	(.066)	(.066)	(.024)
Predisplacement unemployment	.446**	.411**	.402**	.455**	.445**	.447**	.424**	.439**	−.099*
	(.119)	(.119)	(.119)	(.119)	(.119)	(.119)	(.119)	(.122)	(.041)
Observations	9,121	9,121	9,121	9,121	9,121	9,121	9,119	9,119	9,121

Note.—Huber-White robust standard errors are in parentheses. The dependent variable is the log of months unemployed (in cols. 1–8) and the log real weekly entry wage (in 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 employers prior to displacement, average commuted distance prior to displacement, year-specific sector experience, and local labor market effects. In cols. 2 and 3, past network employment rate is defined as the share of contacts employed at least 6 months 2 and 3 years prior to the displacement date. In cols. 4–6, contacts' ability is defined as the network average of the residual from a linear regression of the contact's weekly wage at the firm-year when he met the displaced worker on sex-specific and qualification-specific quadratics in age and full sets of three-digit sector and town dummies (col. 4); estimated contact fixed effects from a linear regression of contact wages over their employment history (col. 5); and estimated contact fixed effects from a linear regression of the yearly share of weeks spent in employment over the 5 predisplacement years (col. 6). In cols. 7 and 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 the displacement date on a sex-specific quadratic in age, contact past (log) wage, and town and year effects. Standard errors in cols. 4–8 have been bootstrapped on the basis of 200 replicas of the generated regressor. See the data appendix for detailed definitions of variables and methods.

+ Significant at 10%.

* Significant at 5%.

** Significant at 1%.

measures of contacts' ability. If estimates of the effects of the 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 in which they met the displaced workers. Specifically, we augment the baseline specification with the network average of residuals from an auxiliary cross-sectional regression of wages on a set of observed individual and job characteristics so as to account for observed differences among contacts that are reflected in wages but are not necessarily correlated with their innate ability.[23] 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. 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.[24] Inclusion of these proxies leaves the estimated coefficient on the current employment rate largely unaffected.[25]

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 worker 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 concerns may lead to a more intensive search. We augment the baseline specification with a measure 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 the displacement date from an auxiliary probit regression of the current (at the displacement date) employment status

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

[24] More specifically, individual log yearly real wages over the period 1980–95 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 gender dummy. As to the employment propensity, contact fixed effects are estimated from gender-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.

[25] Incidentally, note that this result also provides further support for 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.

on a gender-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 worker to account for unobserved (to us) heterogeneity among contacts that may affect their expected (by the displaced) employment status. Results show that such a proxy for the expected employment rate attracts a negative but not statistically significant coefficient (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 worker) innovations to the 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 worker would be willing to accept lower wages because he attaches a higher value to employment than an otherwise identical individual with fewer employed connections. On the contrary, unexpected innovations to network status could not be embedded in the reservation wage policy and thus would not be reflected in subsequent observed wages. Following this line of reasoning, in column 9, we project the displaced worker's observed entry weekly wage on the same set of covariates included in the baseline specification for unemployment duration. The point estimate of the network employment rate effect is much lower in absolute value, and although still relevant in magnitude, it is not statistically significant. 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 worker.

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.

V. 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 sharing it or willingness to share it with the job seeker.

Our first exercise looks at contacts' recent job turnover. Intuitively, contacts that have recently changed jobs have plausibly engaged in some search activity and possess information (at least regarding their new employer) that 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 worker (stayers, S_i) and those who meanwhile changed employers (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 that this distinction is highly relevant since among currently employed contacts it is mostly recent job switchers who contribute to reemployment. On the basis of these estimates, a one-standard-deviation increase in the network employment rate (about 20%) achieved by bringing into new jobs currently unemployed contacts (thus increasing the share of job switchers) would shorten unemployment duration by around 12% (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 to the jobs where they met the displaced workers. 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 unobserved determinants of the displaced workers' unemployment duration.

These results say that contacts more up to date with the current stance of the labor market are more helpful in reemployment.[26] 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. On the basis of this intuition, Bentolila,

[26] In principle, this effect may also reflect the fact that contacts who changed jobs can provide referrals to their current employer. However, because referrals require that the employer knows the worker and trusts her advice (e.g., Montgomery 1991), they should be more effective the longer the tenure of the mover at the new firm. We experimented with augmenting the specification with movers' average tenure and found it to be ineffective in shaping the displaced worker's unemployment duration.

Michelacci, and Suárez (2010) show that information networks may lead to worse employment outcomes if contacts are employed in industries whose technology the displaced worker is unfamiliar with or whose required skills he is not endowed with. We define a metric of skill distance between the displaced worker and the contact matching the current industry affiliation of each contact to that in which the displaced worker accumulated the longest tenure. According to our definition, close contacts are those employed in the sector more relevant to the displaced worker. We exploit both two- and three-digit sector definitions, with the intuition that contacts outside the broader two-digit sector are farther away than contacts outside the narrow three-digit but still within the two-digit aggregation. Results in columns 3 and 4 of table 4 show that technological distance is a relevant factor for the effectiveness of information networks. Contacts outside the broad two-digit classification seem to play no role in helping reemployment, whereas within the two-digit industry, those closest to the displaced worker's skills (employed in the relevant three-digit sector) appear to be more helpful.

A second aspect we consider is contacts' working location and its proximity to displaced workers. If job seekers have a preference for working close to home, the information that contacts working closer to their residence are exposed to is more likely to be relevant. We recover measures of contacts' current workplace distance from the displaced worker's residence and define close (CE_i) and far (FE_i) contacts as those working at a distance below and above the sample median, respectively. In column 5 of table 4, we report results obtained replacing displaced worker *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 be relevant. With the overall employment rate held constant, an increase in the share of close contacts by one standard deviation of the overall employment rate reduces unemployment duration by a week.[27]

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 us 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

[27] 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.

Table 4
Information Availability and Circulation

			Distance from Contacts':					
	Baseline (1)	Contacts' Turnover (2)	2-Digit (3)	3-Digit (4)	Workplace (5)	Residence (6)	Ties' Intensity (7)	Propensity to Share (8)
(Log) network size	−.057	$-.082^{+}$	−.058	−.057	−.057	−.057	−.060	−.057
	(.044)	(.045)	(.044)	(.044)	(.044)	(.044)	(.044)	(.044)
Network employment rate	−.336*							−.336*
	(.146)							(.146)
Movers (M_i/N_i)		−.581**						
		(.170)						
Stayers (S_i/N_i)		$-.252^{+}$						
		(.150)						
Close (CE_i/N_i)			−.426**	−.373*	−.371*	−.328*		
			(.150)	(.150)	(.150)	(.150)		
Far (FE_i/N_i)			−.124	$-.298^{+}$	$-.267^{+}$	−.345*		
			(.170)	(.160)	(.150)	(.150)		
Strong (SE_i/N_i)							−.445**	
							(.170)	

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weak (WE_i/N_i)							−.307*	
							(.150)	
Match quality								−.061
								(.194)
Wage at displacement	−.235**	−.232**	−.231**	−.234**	−.236**	−.235**	−.231**	−.235**
	(.066)	(.066)	(.066)	(.066)	(.066)	(.066)	(.066)	(.066)
Predisplacement unemployment	.446**	.436**	.436**	.441**	.444**	.447**	.372**	.447**
	(.119)	(.120)	(.120)	(.120)	(.120)	(.120)	(.130)	(.119)
Observations	9,121	9,121	9,121	9,121	9,121	9,121	9,121	9,121

NOTE.—Huber-White robust standard errors are in parentheses. The dependent variable is (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 employers prior to displacement, average commuted distance prior to displacement, year-specific sector experience, and local labor market effects. In col. 2, movers are contacts that at the displacement date are employed at a different workplace than the one where they met the displaced worker; stayers still keep the job where they met the displaced worker. In cols. 3 and 4, technologically close contacts are those currently employed in the two-digit (col. 3) or three-digit (col. 4) sector where the displaced worker accumulated the longest tenure in the 5 years prior to displacement. Columns 5 and 6 are geographic distance from employed contacts' current workplace (col. 5) and residence (col. 6); close contacts are those (working or living) at a distance below the median distance across all networks. In 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 workers and their employed contacts. In col. 8, contacts' wage premium is measured by the network average of the residuals of an individual-level regression of contacts' current (log) wage on their past wage, sex, a quadratic in age, sector, LLM, and year dummies. Standard errors in col. 8 have been bootstrapped on the basis of 200 replicas of the generated regressor. See the data appendix for detailed definitions of variables and methods.

$^{+}$ Significant at 10%.

* Significant at 5%.

** Significant at 1%.

networks, contacts differ in terms of residential location (see fig. 3). 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 kilometers (4.5 miles). Not surprisingly, the two definitions are significantly correlated: living nearby the displaced worker increases the probability of working nearby the displaced worker by almost a half; still, about one-fourth of residential neighbors work farther away.[28] In column 6 of table 4 we replace the shares of close and far contacts based on working location with those based on residential location. Results do not show any relevant 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 either because stronger ties are more likely to interact or because they are more willing to transfer information.[29] Specifically, our data allow us to develop a measure of ties' intensity based on common tenure at the workplace where the displaced worker 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, for example, on common residential location. As above, we define weak (strong) ties as those contacts with whom the displaced individual worked less (longer) than the sample median joint

[28] 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

$$G(W = 0,\ R = 1) \approx 0.13 \approx G(W = 1,\ R = 0).$$

[29] Economists are increasingly paying attention to how the type of relationship entertained by two individuals shapes 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 coworkers (2010) and on incentives provided to the workforce in the presence of such ties (2009); Calvo-Armengol et al. (2009) show that pupils' school outcomes are more affected by those of peers the more central they are to their network of friends. In a setting closer to ours, Datcher Loury (2006) shows that obtaining a job through a prime-age male relative leads to higher wages.

tenure, a year in our data.[30] In column 7 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% (nearly a month at the average spell); the effect is lower, below 3 weeks, if the higher employment rate stems from a larger share of weaker ties.

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, e.g., Calvo-Armengol 2004; 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 observed characteristics, μ_j contact fixed unobserved 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 the contact's (log) wage on contact and firm observed characteristics and on the contact's past wage to proxy for individual unobserved characteristics.[31] This provides an estimate for ϕ_{jf}, which is then averaged at the network level. Intuitively, networks with higher contacts' average wage premium should be networks in which more information is circulated. Results obtained augmenting the baseline speci-

[30] 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 to information precisely because they are exposed to different environments.

[31] Specifically, the control set includes, together with the contact's past wage, gender- and qualification-specific quadratics in age and dummies for gender, qualification, three-digit sector, contact's residence, and time.

Table 5
Network Composition: Qualification, Age, and Sex

	(1)	(2)	(3)
Network size	.020	.023	.019
	(.020)	(.020)	(.020)
Past unemployment	.406**	.408**	.396**
	(.083)	(.084)	(.083)
Wage at displacement	−.230**	−.232**	−.228**
	(.060)	(.060)	(.060)
Employment rate:			
Same qualification	−.336**		
	(.125)		
Different qualification	−.110		
	(.188)		
Same cohort		−.329*	
		(.135)	
Different cohort		−.276*	
		(.140)	
Same sex			−.024
			(.142)
Different sex			−.379**
			(.125)
Observations	9,121	9,121	9,121

NOTE.—Huber-White robust standard errors are in parentheses. The dependent variable is (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 employers prior to displacement, average commuted distance prior to displacement, year-specific sector experience, and local labor market effects. Employment rate is computed separately for the complementary groups defined as follows: in col. 1, the qualification is blue/white collar; in col. 2, the cohort is a [−4, +4]–year window around the displaced worker's age.
+ Significant at 10%.
* Significant at 5%.
** Significant at 1%.

fication with our index of propensity to share information, column 8 of table 4, show that it has no effect on unemployment duration.[32]

We also explore heterogeneity in networks' effectiveness along three major sociodemographic dimensions, the underlying idea being that a displaced worker may be more likely to benefit from relevant information or stay in touch with contacts with similar traits. Specifically, we consider breakdowns of the network employment rate based on contacts' qualification (blue/white collar), age, and gender. Results are reported in table 5. Column 1 shows that network effects are entirely driven by employed contacts with the same qualification. Thus, displaced blue collars will benefit only from employed blue collars, consistently with the idea that the information accessed by the latter is relevant for the former. On the other hand, age does not appear to represent a major obstacle to information flows: the share of employed contacts in the same [−4, +4]–year cohort has a slightly higher, though not statistically different, impact on

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

unemployment duration (col. 2). Interestingly, the breakdown by sex suggests that displaced workers benefit from a higher employment rate among contacts of the opposite sex (col. 3).

The findings in this section confirm that employed connections are an important channel through 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 displaced workers. These findings are based on the implicit assumption that contacts are exclusive in that they are linked only 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; 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.

VI. 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 structure of social links determined by our definition of the relevant network, we can 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 a 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 codisplaced workers is induced by differences in the number of contemporaneously displaced individuals by a different firm closure their contacts are linked to. Such a 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 CFFE. Re-

Table 6
Indirect Connections

		Indirect Networks		
	Baseline (1)	Competitors (2)	Providers (3)	Networked Firms (4)
(Log) network size	−.057	−.014	−.058	.044
	(.044)	(.047)	(.044)	(.064)
Network employment rate	−.336*	−.276+	−.337*	−.334*
	(.146)	(.150)	(.150)	(.150)
No. competitors		.007*		
		(.003)		
No. indirect links			.003	
			(.027)	
Networked firms				−.146*
				(.067)
Wage at displacement	−.235**	−.235**	−.235**	−.233**
	(.066)	(.066)	(.066)	(.066)
Predisplacement unemployment	.446**	.475**	.447**	.421**
	(.119)	(.120)	(.120)	(.120)
Observations	9,121	9,121	9,121	9,121

NOTE.—Huber-White robust standard errors are in parentheses. The dependent variable is (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 employers prior to displacement, average commuted distance prior to displacement, year-specific sector experience, and local labor market effects. In col. 2, competition is measured by the average number of indirect connections to other contemporaneously displaced individuals. In col. 3, providers is measured by the number of employees at contacts' current employers. In col. 4, networked firms is measured by (the log of) the number of different contacts' current employers. See the data appendix for detailed variable definitions.
+ Significant at 10%.
* Significant at 5%.
** Significant at 1%.

sults reported in column 2 of table 6 show that a higher degree of competition significantly slows down reemployment. Increasing the number of competitors by 10 units (roughly corresponding to a shift from the first to the third quartile in our sample) raises unemployment duration by 7%, roughly equivalent to the effects of a 20% 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 worker; 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 the contact's current coworkers.[33] In column 3 of table 6, we augment the baseline specification

[33] To be fully consistent, we should have recovered for each of the contacts all individuals he worked with over the prior 5 years. The exercise turned out to be computationally burdensome.

with the (log) average size of indirect networks. Results do not show any statistically 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 worker is connected to through his contacts. Intuitively, if contacts gather information on the workplace by word of mouth, 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 unemployment duration by about 15%.

VII. Discussion and Further Robustness Checks

Throughout the article, 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% of sampled displacements. Thus, truncation may affect a nonnegligible 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% last 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. Second, the observed characteristics of nonreentrants suggest that most of them might not be actively participating because of either fertility (about half of nonreentrants are women aged 20–34) or retirement (about one-fifth are aged 50 or more) decisions.[34] This intuition is further supported by the fact that the share of nonreentrants 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.

Table 7 reports two exercises to address the truncation issue empirically. First, we considerably extended the minimum number of follow-up years by restricting the sample to closures that occurred up to 1990. Hence, each displaced worker is allowed at least 7 years for reentry. Results reported in column 1 broadly confirm our previous findings. Second, we estimated a set of linear models for the probability of still being 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

[34] Labor force survey data show that in the area we study, more than 20% of unemployed young women are back in employment after 1 year whereas about 75% exit the labor force; similarly, more than 90% of unemployed older people exit the labor force after 1 year whereas about 5% are in employment.

Table 7
Robustness Checks

	Dependent Variable			
		Still Unemployed After		
	Unemployment Duration (Log) (1)	9 Months (2)	12 Months (3)	15 Months (4)
(Log) network size	−.048	−.065**	−.085**	−.073**
	(.052)	(.017)	(.016)	(.015)
Network employment rate	−.362*	−.182**	−.126*	−.101+
	(.177)	(.055)	(.053)	(.052)
Wage at displacement	−.300**	−.118**	−.114**	−.109**
	(.091)	(.021)	(.020)	(.020)
Predisplacement unemployment	.414**	.090*	.067	.071+
	(.154)	(.046)	(.045)	(.043)
Observations	5,961	11,057	11,057	11,057

NOTE.—Huber-White robust standard errors are in parentheses. The dependent variable in col. 1 is the log of months unemployed; the baseline specification is estimated on the subsample of workers displaced from closures that occurred in 1980–90. Cols. 2–4 report estimates from linear probability models; the dependent variable is a dummy equal to one if still unemployed after the number of months specified. 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 employers prior to displacement, average commuted distance prior to displacement, year-specific sector experience, and local labor market effects. See the data appendix for detailed variable definitions.
+ Significant at 10%.
* Significant at 5%.
** Significant at 1%.

in the previous sections, we still find that a higher network employment rate reduces the probability of unemployment at the various horizons.[35]

A final puzzling feature of our results is the absence of any effect of the size of the network (table 2). We detect a statistically significant and negative effect only on unemployment in table 7, where the underlying sample also includes displaced workers who are never observed to reenter employment. This would suggest that network size may play an important role concerning the participation decision rather than in shaping unemployment durations at reasonable horizons. However, another potential explanation for the general absence of an effect is that it may be a consequence of the measurement error induced by defining network size as

[35] A related concern is that our estimates are inconsistent because of a sample selection bias if postdisplacement participation decisions are affected by the employment rate in the network; i.e., displaced workers endowed with better networks are more likely to participate because of the more favorable odds of receiving a job offer. Such a 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 the network employment rate and individual unobserved unemployment determinants would arise. Unfortunately, a formal analysis of this issue is not possible because of data limitations and the lack of credible exclusion restrictions for the participation equation.

the simple count of predisplacement coworkers. In particular, we may be assigning too many contacts to some individuals. For example, if an individual cannot maintain more than 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 that we can rank coworkers in a firm of size $N > Z$ with some distance metric from the displaced worker (say because they work in different units) and that the probability of meeting farther individuals decays with distance at rate γ. Let $P^n = e^{-\gamma \max\{0, n-Z\}}$ be the probability of meeting a coworker who is in position $n = \{1, \dots, N\}$. Because the true ranking within a firm is unknown, the probability that coworker i is in position n of the ranking is $P(n_i = n) = 1/N$.[36] Therefore, the probability that the displaced worker actually meets coworker i is given by

$$P_i = \sum_{n=1}^{N} P(n_i = n) \times P^n = \sum_{n=1}^{N} \frac{P^n}{N}.$$

Making use of the definition of P^n, after some algebra, we obtain

$$P_i = \frac{Z + [e^{-\gamma}/(1 - e^{-\gamma})][1 - e^{-\gamma(N-Z)}]}{N}.$$

Knowing Z and γ, we can thus weight each assigned coworker and redefine network measures accordingly.

In table 8, 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 the results reported in table 2, those on the effects of the network employment rate are largely unaffected by the correction.

[36] This probability is obtained noticing that in a 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.

Table 8
Measurement Error Corrections

	Z			
	5 (1)	10 (2)	15 (3)	20 (4)
γ = .25:				
(Log) network size	−.153*	−.111+	−.097+	−.088+
	(.071)	(.058)	(.052)	(.050)
Network employment rate	−.403*	−.395*	−.384*	−.374*
	(.160)	(.156)	(.154)	(.152)
γ = .75:				
(Log) network size	−.177*	−.117+	−.100+	−.091+
	(.079)	(.060)	(.053)	(.050)
Network employment rate	−.403*	−.397*	−.387*	−.377*
	(.162)	(.157)	(.154)	(.152)
γ = 1.25:				
(Log) network size	−.184*	−.118+	−.101+	−.091+
	(.081)	(.060)	(.054)	(.050)
Network employment rate	−.403*	−.398*	−.387*	−.377*
	(.162)	(.157)	(.154)	(.153)
Observations	9,121	9,121	9,121	9,121

NOTE.—Huber-White robust standard errors are in parentheses. The dependent variable is (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, wage at displacement, predisplacement time in unemployment, average size, and dummies for the number of employers prior to displacement, average commuted distance prior to displacement, year-specific sector experience, and local labor market effects. Network characteristics are computed weighting each contact acquired in a firm of size N by $P_i = \{Z + [e^{-\gamma}/(1 - e^{-\gamma})][1 - e^{-\gamma(N-Z)}]\}/N$ if $N > Z$ and $P_i = 1$ otherwise. See the data appendix for detailed variable definitions.

+ Significant at 10%.
* Significant at 5%.
** Significant at 1%.

VIII. Conclusions

Local and nonmarket 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, and information sharing. In this article, 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 opportunities. Unemployment spells are considerably 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 worker. 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 the 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 a contact's current employment status. Consistently with this argument, we find no statistically significant effect of contacts' employment on the displaced worker's 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 relevant spillover effects on job-finding rates of socially connected unemployed individuals. We argue that 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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