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People I Know: Workplace Networks and Job Search Outcomes

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Abstract

We examine the role of information networks for job search outcomes of exogenously displaced individuals. We focus on networks of fellow workers established over time and up to displacement, whose labor market attributes we are able to describe extensively drawing on longitudinal Social Security records that cover all worker-firm matches in a tight labor market in Northern Italy over 20 years. Estimates of network effects are affected by omitted variable bias if the labor market sorts workers across firms along relevant determinants of search outcomes and network characteristics or if past coworkers are exposed to the same shocks. The empirical strategy accounts for these possibilities by comparing subsequent outcomes of workers displaced by the same firm; in addition, we control for potential residual within-firm heterogeneity conditioning on pre-displacement wages and employment history as well as on descriptions of pre-displacement employers and their workforce. Contacts' labor market attributes have a significant effect on a variety of job search outcomes. Employed contacts significantly increase the probability of re-employment. They are more effective if they experienced a recent job change and when geographically and technologically closer to the displaced. Stronger ties and lower competition for the available information also speed up re-employment. While largely irrelevant for unemployment duration, contacts' quality is a significant determinant of entry wages and subsequent job stability.

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1 Introduction

How individuals locate job opportunities, how firms screen applicants and, more in general, how the matching process of workers to jobs takes place are crucial elements for the understanding of the workings of labor markets. An often emphasized aspect is how information on available employment opportunities and on jobs and workers characteristics is transmitted. A large body of research documents the use of personal contacts as a major job search method (Rees (1966), Granovetter (1995), Blau and Robins (1990), Holzer (1988), Topa (2001)) and stresses the role of job information networks as a means that facilitates the diffusion of information on available opportunities (Calvo-Armengol and Jackson (2004), Calvo-Armengol (2004)), or gives access to better paying and more stable jobs, possibly mitigating the informational asymmetries between prospective employers and employees (Jovanovic (1979), Montgomery (1991), Simon and Warner (1992)). Well-documented evidence on the correlation between individual socio-economic outcomes and those of surrounding people sharing certain socio-demographic traits is suggestive that information spillovers may be important, but it is generally consistent with other explanations such as the presence of omitted common characteristics.

In this paper, we investigate how unemployment duration after an exogenous job loss and the quality of the subsequent job - the outcomes of job search - are affected by the current employment status, job turnover and earnings of the network of former fellow workers and, more generally, by the structure and charactersitics of the ties entertained with them. The analysis draws on longitudinal administrative records covering all employment relationships in a dense, tight and spatially concentrated local labor market in northern Italy over more than 20 years. We focus on unemployed workers displaced by firm closures. Their social network is recovered by tracking each agent's employment history and identifying any former fellow worker up to 5 years prior to displacement.

This setting provides a unique opportunity to study the role of social connections in the transmission of job-related information. First, fellow workers are a natural set of contacts to refer to when searching for a job. Social connections are frequently established on the job and fellow workers represent useful sources of information and referrals. Indeed individuals known from previous work situations account for a remarkable proportion of jobs found through personal contacts. They are aware of the job seeker skills, plausibly exposed to information relevant to the displaced and possibly able and willing to share it (Granovetter (1995), Granovetter (2005)).

Second, the data allow to characterize a number of network features potentially relevant to the information transmission mechanism. We can recover the total number of contacts and their specific employment status. More importantly, we can describe in detail each social link. For each contact we measure the intensity of the tie established with the job seeker, based on how long they worked together, and its relevance, based on whether the contact has recently experienced a job switch thus acquiring novel information, whether she lives nearby and works in the same sector as the displaced, and on how many other displaced she is simultaneously connected to.

Third, the research design allows to address some of the identification issues arising in studies of network effect when these are proxied on the basis of geographical proximity or by other observable shared traits[1]. In those cases, a positive correlation between, say, the employment likelihood of an individual and the share of employed neighbors may reflect the presence of omitted neighborhood factors (e.g. the availability of public transport) or the fact that individuals are sorted across neighborhoods on the basis of some omitted trait that also affects their employment status[2]. Even if the individuals were randomly assigned to the neighborhood and no common factor was omitted, the correlation might still reflect a causal effect of neighbors' omitted exogenous characteristics rather

[1]Recent examples in various environments are Glaeser, Sacerdote and Scheinkman (1996), Bertrand, Luttmer and Mullainathan (2000), Aizer and Currie (2004), Bayer, Ross and Topa (2005), Luttmer (2005), Bandiera, Barankay and Rasul (2005)

[2]Research on the effects of neighborhood quality on individual outcomes typically overcomes the problem of omitted individual characteristics exploiting programs that randomly incentivate some households to move to more affluent neighborhoods (Katz, Kling and Liebman (2001), Kling, Liebman and Katz (forthcoming)) or directly assign individuals to other residential locations (Oreopoulos (2003)); alternatively, Weinberg, Reagan and Yankow (2004) explicitly model the individual residential choice.

than their current employment status[3]. Unlike most existing research on social effects, our inference is based on individual-specific networks built up over time as a consequence of own and co-workers job mobility. Network characteristics thus exhibit enough variation to account for an unusually wide set of potential omitted variable biases[4]. On one hand, inference relies on comparisons of individuals displaced by the same firm. This accounts for all sources of heterogeneity across closing firms, such as average individual unobserved characteristics, firm characteristics or cyclical conditions. Importantly, relying on within-closing firm variation in network characteristics implies that our estimates are unbiased even if workers are sorted across firms along some time-invariant unobserved trait correlated with their networks' attributes. Additionally, in comparing co-displaced workers the longitudinal dimenison of the data allows to account for individual pre-displacement realizations of the main dependent variables (earnings and labor market attachment) to absorb residual within-firm individual heterogeneity potentially correlated with networks' characteristics. This may arise, for example, from sorting across pre-displacement firms. Finally, we proxy contacts' omitted fixed characteristics (such as ability) with their past wages and employment history. This implies that variation in network characteristics is unlikely to be driven by variation in contacts' ability, allowing to interpret the results as the genuine effect of information availability generated by idiosyncratic variation in network characteristics, such as an additional randomly employed contact when the search spell exogenously starts.

Our findings show that workplace networks are an important channel of information diffusion and significantly contribute to improving the matching process of workers to jobs. In our preferred specification a one standard deviation increase in the share of employed contacts shortens unemployment

[3]This is typically the case when group outcomes are instrumented with group composition. This has important consequences for policy design since, differently from behaviors or outcomes, exogenous characteristics of group members cannot be manipulated by policy makers. See Hoxby (2000) and Cipollone and Rosolia (2007) for a discussion of the issue.

[4]Few recent studies of social effects in other environemt also rely on networks exhibiting considerably more variation than usually found in similar research. Among them, Mas and Moretti (2006), who study external effects of workplace productivity using changes in the composition of cashiers in a supermarket, Bayer, Pintoff and Pozen (2004), who study social interactions in criminal behavior using changes in the composition of peers in juvenile corrections, Bandiera and Rasul (2002) and Conley and Udry (2005), who study technology adoption in rural Africa using self-reported information on contacts' number and identity.

duration by 7 per cent, about 3 weeks at the average spell. The effect is considerably stronger - 5 weeks - if the increase stems from contacts who recently changed job. As a benchmark, a one standard deviation increase in own wage at displacement reduces unemployment duration by 4 weeks. We also find that competition for the available information matters: a higher number of displaced individuals connected to a given contact significantly delays re-employment. On the other hand, stronger ties with employed contacts increase the probability of finding a job. Finally, our estimates show that contacts' quality, at best only a weak determinant of unemployment duration, significantly contributes to the quality of the new job. In particular a one standard deviation increase in contacts' current wage premium raises average weekly wages in the entry year by about 2 percent and the probability of holding the same job after 12 months by 2.5 percentage points.

We are certainly not the first to address similar questions. Much of the evidence found in the broad literature on neighborhood effects may also be reconciled with information transmission effects. To our knowledge, however, only two recent studies provide evidence on job information networks showing that an individual employment status is affected by the characteristics of his network. Bayer et al. (2005) study the role of informal contacts in the labor market building on the neighborhood literature. They exploit detailed residential Census data to show that pairs of block neighbors in Boston have a higher propensity to work at the same location than pairs of neighbors living in nearby blocks belonging to the same census block group. Insofar as blocks of residence within a block group are randomly chosen their design circumvents the problem of unobserved neighborhood-level determinants of performance. Munshi (2003) shows that recent Mexican migrants to the US are more likely to be employed when a larger proportion of previous migrants from the same origin community is already established at destination. He overcomes omitted variable problems exploiting variation in the stock of past migrants induced by weather shocks in the origin community.

The contributions of our paper lie in the research design and in the possibility of extensively characterise relevant network features. More importantly, we focus specifically on network effects on labor market flows by looking at individual job finding and job separation rates. As the stock-flow approach to the labor market has long made clear, a given employment rate may hide very different labor market dynamics (Blanchard and Diamond (1992), Blanchard and Portugal (2001)). In this respect, a major limitation of studies that relate an agent's employment status at a given point in time to characteristics of her social network is that they are generally silent as to whether such effects work through job finding or separation rates. Directly addressing flows is important since network effects could be such that both inflows and outflows are affected although in the aggregate no correlation with employment rates is detected. And yet the labor market prospects of different populations would be very different precisely because of their social environments. Consider for example two groups exposed to different networks causing hiring and separation rates to be lower in one group than in the other. The population in the first group will thus be more polarised between individuals employed with long tenure and little probability of losing the job and those with long unemployment durations and little chances of getting a job; in the other, individual employment histories will be more similar, with higher turnover, less polarisation and possibly less overall permanent inequality. Alternatively, consider network characteristics that affect only hiring rates or only separation rates. These will generate observable differences across populations also in employment rates. Yet, depending on the specific channel a policy maker might want to consider different policies to compensate the more disadvantaged group.

The paper proceeds as follows. In the next section we outline the empirical model, discuss the main identification issues and our reading of the results. Next, we describe the data and the underlying labor market. In Section 4 we present the main results; these are discussed in Section 5 along with several robustness checks. We then conclude.

2 The empirical model

Economic theory suggests several mechanisms through which contacts' labor market status and other network characteristics may affect job-search outcomes [5]. Social networks may shape information flows on employment opportunities and on the potential employee' characteristics (e.g. Montgomery (1991), Calvo-Armengol and Jackson (2004)), for example because employed contacts learn more quickly about new opportunities or are more likely to be requested to refer some potential employee[6]. Contacts' characteristics may represent a social norm or behavior that group members conform to or a reference point for social status (Akerlof (1997)), so that individuals in high employment groups exert a higher search effort or accept lower wages not to incur in a social punishment. Also, the characteristics of the social network may shape the unemployed 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)).

Our baseline empirical model summarizes these relationships relating unemployment duration and the quality of the subsequent job - measured by the entry wage and its stability - to network size, employment rate and measures of contact's quality and ties' intensity and relevance in a linear fashion:

$$y_i = \alpha + X_{it_0}\beta + N_{C(i)t_0}\gamma + e_{it_0} \tag{1}$$

where t_0 is the starting date of the search spell; y_i the outcome of interest; X_{it_0} and e_{it_0} are, respectively, observed and unobserved individual determinants of the relevant outcome; $N_{C(i)t_0}$ captures the characteristics of i's network of contacts, $C(i)$, at the beginning of the spell.

Interpretation of least square estimates of γ from (1) as causal effects of specific network characteristics requires that these are uncorrelated with the residual. In non-experimental settings this may fail because an agent and his contacts are exposed to common exogenous unobserved factors or share

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

[6] See Ioannides and Datcher Loury (2004) for a detailed survey of the job information networks literature.

unobserved characteristics proxied by network attributes (Manski (1993), Moffitt (2001)). Specifically, in our setting individuals are assumed to be socially related because they have worked in the same firms. Thus, a job seeker and his contacts might share some relevant unobservable characteristic if the labor market sorts workers across firms along such dimension. For example, 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 the higher ability, are also more likely to be employed at any point in time. Alternatively, 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 start search. In general, most of these sources of correlation have to be assumed away because, lacking 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[7].

Our data allow us to overcome some of these difficulties and relax a number of identifying assumptions. We draw on longitudinal matched employer-employee social security records covering any work episode over the period 1975-1997 in a small area in northern Italy. In particular, the data provide information on employment status and employer identity at monthly frequency allowing to establish for any pair of individuals whether, when and for how long they worked together at a specific firm. We can thus assign to each job-seeker an individual network by tracking his previous employment history and identifying all his former fellow workers. In this setting two individuals will be endowed with the same network only if their employment histories fully overlap. This generates narrow sources of identifying variation for example within residential and working locations, industry, demographic

[7]For example, Bayer et al. (2005) study job referrals among residential neighbors under the assumption that, within census block groups, individuals are randomly distributed across blocks; Bertrand et al. (2000) explore social effects in welfare participation within ethnic groups at a given residential location under the assumption that individuals of the same ethnicity at different residential locations do not differ in unobservables correlated with welfare use.

groups and, importantly, firms.

Specifically, in our exercises we focus on workers entering unemployment because of firm closures[8]. This allows us to overcome the potential selection bias arising if individuals with better networks are more likely to start search. More importantly, it allows estimating network effects by comparing individuals who were employed at the same firm when they start searching. This has two main advantages. On the one hand, if workers are sorted across firms along some unobserved dimension correlated with relevant network characteristics (say, ability), comparing individuals displaced by the same firm absorbs this source of correlation. On the other, comparisons of the outcomes of co-displaced workers ensure that all shocks common to codisplaced are taken into account. For example, those related to the specific location, sector, activity and other characteristics of the firm.

Even within closing firms a displaced and his contacts may be exposed to shocks different from other codisplaced and their contacts. For example, an individual and his network are likely to have accumulated similar skills while working together in the past, which may differ from those of other codisplaced; similarly, codisplaced workers may reside at different locations and so may their contacts so that relevant local labor market conditions may differ within firms. Because networks are individual-specific we can account for these potential omitted variables by means of detailed location-specific and pre-displacement skill-specific time dummies[9]. Alternatively, within closing firm a displaced may share unobserved fixed characteristics with his contacts that are different from those of other codisplaced and their networks. To be a source of concern in our exercise, however, this trait must reflect into specific individual labor market outcomes (as wages or employment status) and contacts characteristics (as their employment rate or average compensation). Lagged values of the relevant outcome variables

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

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

proxy for potentially unobserved dimensions of sorting[10]. Also, the econometric specification will include pre-displacement contacts' wages and a variety of former employers' characteristics. Notice however that these additional controls are needed only if sorting along the relevant dimension fails *exclusively* in the closing firm. In fact, if sorting took always place according to the same rule then comparisons of codisplaced workers would account for the correlation between unobservables and network characteristics; on the other hand, if sorting never took place than there would be no source of omitted variable bias.

In summary, our main identifying assumption is that, conditional on this set of controls, cross-sectional variation in network characteristics at displacement date is orthogonal to individual unobserved heterogeneity within closing firms. The assumption would fail if our controls missed individual fixed characteristics that, while shared by past coworkers in pre-displacement firms, are not shared by the co-displaced and, while not affecting a number of pre-displacement outcomes and firm characteristics (wages, employment, location, etc.), do affect them after displacement; also, it would fail due to labor market shocks common to the displaced and his contacts not captured by the closing firm fixed effect, by time-varying residential location and sectoral experience effects. While conclusive evidence on the causal effect of workplace network characteristics can only be obtained in a pseudo-experimental framework, we think the most plausible sources of omitted variable bias are accounted for in our setting. In section (5) we will present a set of exercises in support of the main identifying assumptions.

As said above, many mechanisms could generate such causal relationships. Strictly speaking, the empirical strategy outlined above does not allow to tell them apart. However, the identifying variation underlying our estimates is unlikely to reflect differences in fundamental social determinants of individual choices such as culture, conformism to the group, status seeking. These determinants are likely

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

to be rather persistent and also shared by all the individuals we study because of the very limited size of the underlying social environment (Guiso, Sapienza and Zingales (2006), Glaeser, Laibson and Sacerdote (2002)). On the contrary, our identification is based on plausibly idiosynctratic deviations from conditional average network characteristics such as an additional randomly employed contact when the search spell begins. Thus, we rather believe it more plausible that variation in contacts' current labor market characteristics generates exogenous changes in the information available in the network and potentially transmitted to the displaced. Incidentally, note that the conditioning set in our regressions absorbs all variation across, in particular, cities and experience profiles (city- and experience-year effects) and closing firms. This implies that the displaced learns *directly* about new opportunities arising at his residential location, for his specific experience profile, as well as about those common to codisplaced fellows (say, a neighboring firm hires workers from the closing one), considerably limiting the scope of the job information networks in our empirical exercise.

A final question is how networks shape the available information. A simple search model may clarify the point[11]. In a standard search model with on-the-job search, the rate at which the unemployed finds a suitable job is given by $P_{UE} = \lambda\,[1 - F(b)]$ and the expected entry wage is given by $E(w|w \geq b) = \frac{\int_b^\infty w dF(w)}{(1-F(b))}$, where λ is the job offer arrival rate, b is the unemployment benefit, and $F(.)$ the distribution of wage offers[12]. In this simple setting job information networks may affect the arrival rate λ, by facilitating the information flow, as well as the distribution of job offers $F(.)$, by helping a worker to find a suitable match or determining the kind of job offers passed on (Montgomery (1991), Calvo-Armengol (2004)). Therefore, it is in general hard to disentangle the two channels studying unemployment duration and entry wages. However, the Italian institutional setting lacks a proper unemployment insurance scheme[13]. Under the assumptions of on-the-job search and in the absence of

[11]See Rogerson, Shimer and Wright (2005) for a review of search-theoretic models of the labor market.

[12]This obtains if the arrival rate of job offers when unemployed and when employed are the same, so that all offers above b are accepted.

[13]Throughout the 1980s unemployment benefits were fixed in nominal terms at a trivially small level, implying a replacement rate of less than 2 percent; they were raised to a replacement rate of 7.5 percent in 1988 and only by mid

a significant unemployment insurance scheme, the probability of finding a job would simply equal the job offer arrival rate $P_{UE} = \lambda$ and the expected entry wage would equal the unconditional expected wage implied by the distribution faced by the agent, $E(w|w \geq 0) = E(w)$. This implies that we can interpret the results on unemployment duration as the network effects on the speed at which information is disseminated, λ, and those on entry wages as network effects on the quality of job offers contacts inform the unemployed about, as summarised by the distribution function $F(.)$.

3 The data and the environment

The data cover over 13 millions employment relationships and 1.2 million employment histories over the period 1975-1997 in two Italian provinces[14]. Individuals are also tracked if they move to other areas of the country. Each record describes an employment relationship, providing information on the months covered in the position, individual demographics (including age, gender and places of birth and of residence), weekly earnings, and employer information (3-digit industry, location, date of birth and closure if occurred). We only retain workers who enter unemployment because of firm closures, that is those who were still employed by the firm in its last month of activity.

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

1990s reached a still low 30 percent. There are special arrangements for larger firms that go bankrupt but these are unlikely to play a role in our sample due to the small firm size.

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

Survey evidence supports the presumption that the workplace is an important place for developing social connections. The 2001 Special Eurobarometer survey reports that in Italy over 70 percent of employees have good friends on the workplace; similar shares are found in all other European countries. In addition, several features of the labor market we focus upon suggest that fellow workers are likely to meet daily, to stay in touch, and to have access to valuable job-related information. It is concentrated in a small geographic area (about 5,000 square km), and is highly self-contained (over 80 percent of manufacturing workers in the area are also residents; 70 percent were born there). It is a tight and dynamic labor market (the employment rate of people aged 25-50 is 80 percent and their unemployment rate at about 2 percent), characterized by small one-plant firms, three quarters of them employing at most 13 workers. Finally, economic activity is very dense, with 23 manufacturing firms and 345 manufacturing employees per square km, and dominated by two big industries (textiles and machinery) that account for more than half of local employment[15].

Table (1) reports some descriptive statistics of the closing firms and of the individual networks. Rows represent variables for which we have computed means at the closing firm (panel A) or at the network (panel B) level; columns report statistics on the sample distribution of these means. Co-displaced workers are relatively young, the median closing firm with an average age of about 27, and typically blue collar workers. They tend to live in the same local labor market (LLM) where their employer is located, although not in the same smaller town[16].

As concerns networks, their size appears to be reasonable, a consequence of the limited firm size in the underlying labor market. The number of contacts ranges from 8 persons (10th percentile)

[15]As a benchmark, in Santa Clara county (3,300 km^2) - apparently the heart of Silicon Valley - the 2000 US Census reports about 13 private non-farm establishments and 250 private non-farm employees per square km, with an average size of private non-farm establishments of about 20 employees. The employment rate of people 16 years and over was 64.5 percent and the unemployemnt rate 3.7 percent, against a 62 percent employment rate and a 3.1 percent unemployment rate for the same population in the labor market we study at the end of the 90s (calculations based on data from the US Census 2000 Gateway, http://quickfacts.census.gov/qfd, and Istat's Labor Force Survey).

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

to 150 (90th percentile), with a median of 32. On average, 67 percent of contacts are employed at displacement, with a standard deviation of about 20 percentage points. Contacts live nearby the displaced, the median network displaying an average distance of 5.5 km, and generally in the same LLM. However, as for co-displaced workers, within LLMs contacts do not appear to be clustered in the same towns. Contacts are slightly more likely to be males, reflecting the higher participation rates of men; average age differences range from 4 to 15 years, with a grand mean of about 9 years. Overall, individual networks appear to be rather heterogeneous allowing to absorb a number of potential sources of spurious correlation between their characteristics and individual outcomes.

Before turning to the results, a last remark on the sample. We only retain completed unemployment spells. These account for over 80 percent of the sample of displaced workers. Observed completed spells are relatively short: the median length is 7 months, the average is 12 and less than 7 percent 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. In fact, our results are robust to restricting the sample to closures up to 1990, for whom right censoring should be much less relevant[17]. Yet, the share of displaced workers not observed again in employment is non-negligible. While the administrative nature of our data does not allow to establish to what extent displacement is associated to non-participation, the observable characteristics of non re-entrants suggest that most of them might not be actively participating, because of either fertility (about half of non re-entrants are women aged 20-34) or retirement (about one fifth are aged 50 or more) decisions[18]. This intuition is further supported by the fact that the share of non re-entrants is rather constant across displacement years whereas we would expect it to increase as we approach the end of the sample if it was related

[17]We also experimented with imposing common censoring rules at 36, 48 and 60 months to all unemployment spells originating from closures occurred between 1980 and 1990 to obtain a balanced sample. If censoring was driving our results we would expect that increasing the number of censored observation in the sample would inflate our coefficent estimates. If anything, the evidence (available on request) points in the opposite direction.

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

to sample censoring. A potentially more relevant concern is related to the possibility that post-displacement participation decisions may be affected by network characteristics, thus raising a sample selection issue. For example, if displaced endowed with a good network are more likely to participate, estimates would reflect a standard Heckman-type attentuation bias. In section (5) we provide evidence that our findings are robust to explicit consideration of the censoring and sample selection issues.

4 Results

We begin by focussing on network effects on unemployment duration starting from a basic set of contacts' attributes. We then qualify the workings of the network considering more detailed descriptions of the underlying social links, such as their intensity and relevance, as well as the degree of competion for the available information. Finally, we turn to network effects on the quality of the subsequent job.

4.1 Networks and Unemployment Duration.

Table (2) reports results for several specifications of a regression of (log) unemployment duration on the (log) number of workplace contacts, the share of those employed at displacement and a measure of their current wage premium[19]. Contacts' wage premium captures either the fact that high wage acquaintances share more information or the fact that, a higher wage premium signaling better quality, they are able to provide more credible referrals (e.g. Calvo-Armengol (2004), Mortensen and Vishwanath (1994), Montgomery (1991)). Since individual employment status and earnings are plausibly correlated, availability of both measures allows to disentangle a "quantity" effect, related to the proportion of employed contacts, from a "quality" effect, related to their quality or that of their current match. Results in the first column of the table only account for a limited set of individual characteristics (age, sex, tenure and qualification at closure), and the closing firm fixed effect (CFFE).

[19]Contacts' average wage premium is obtained as the network-level average of the residuals from a wage equation estimated on all individuals belonging to some network and employed at displacement date, controlling for a quadratic in age, sex, qualification and time dummies.

Unemployment duration turns out to be significantly and negatively correlated with the network employment rate and, to a lesser extent, with contacts' wage premium. A causal interpretation of such estimates relies on the assumption that within closing firm contacts' characteristics do not proxy for unobserved determinants of individual unemployment duration. The assumption would be satisfied even if the displaced have not been randomly assigned to fellow workers prior to displacement, as long as the assignment rule is stable over time so that it holds also in the closing firm. Under this hypothesis, 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 network attributes, individual unobserved characteristics differ among co-displaced workers. First, in column 2 we augment the basic specification with the displaced earnings profile (captured combining average wage at closure and average wage growth) and the average length of his unemployment spells over the 5 pre-displacement years[20]. Intuitively, if sorting occurs along characteristics that, though not directly observable, reflect into wages or employment likelihood over time (e.g. ability), accounting for past individual realizations of these outcomes absorbs the within closing firm residual correlation between unemployment duration and network characteristics. In fact, while both indicators are significantly correlated to unemployment duration, attracting the expected signs, the coefficient on the network employment rate is largely unaffected.

Second, we account for the possibility that the relevant unobservables, while not reflected into individual pre-displacement outcomes such as wages and unemployment, are correlated with the characteristics or the number of past firms. Compensating wage theory suggests that workers might sort across firms on the basis of their preferences for the combination of wage and non-wage benefits offered

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

by the firm (Rosen (1986)). Thus, for example, large firms may be able to attract better workers by offering fringe benefits such as day care, health insurance, meals (Woodbury (1983), Oyer (2005)). Similarly, they are shown to be more likely to provide training opportunities to their employees (Oi and Idson (1999)). Alternatively, workers may be attracted to certain firms by the quality of its workforce, for example because this generates learning opportunities, a better working environment or other amenities the individual values positively. As to the number of job switches, it may be associated with changes in the working environment[21]. In column 3 we thus account for a measure of peer quality at past firms - the average wage paid to coworkers - along with the average size and the number of firms the unemployed visited in the pre-displacement period[22]. Inclusion of such controls yields a larger and more precisely estimated 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 skills - but co-displaced workers differ in the skills they accumulated in the past - so that different networks could be subject to different industry-specific shocks. Similarly, if individuals 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 LLM-year fixed effects for the displaced LLM of residence and a full set of 3-digit industry-year fixed effects corresponding to the sector where the displaced accumulated the longest pre-displacement tenure[23]. Inclusion of these controls amounts to assuming that the displaced is directly exposed to LLM- and industry-specific shocks, that is he learns about the randomly arising opportunities in his local labor market and in the industry where he accumulated most of his experience independently from his

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

[22]Notice that these two last controls imply, in particular, that variation in the measure of network size is induced by coworkers turnover at each past firm.

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

contacts. Allowing for these additional controls does not change the basic result that a larger share of employed contacts leads to a shorter unemployment spell; the weak effect of contacts' quality detected in some of the previous specifications disappears altogether.

The estimated coefficient in column 4 implies that a one standard deviation increase in network employment rate (corresponding to about 20 percentage points) reduces unemployment duration by about 7 percent, almost 3 weeks for the average unemployment spell. As a benchmark, increasing individual wage by one standard deviation would imply a reduction in unemployment duration of about 10 percent, 4 weeks at the average duration.

4.2 Information flows and contacts' characteristics

A contacts' propensity and ability to disseminate information depends on characteristics of the tie he has with the displaced and on his access to valuable information. Intuitively, a stronger tie is more likely to share information; similarly, a succesful job seeker may be more informed on current employment opportunities. Differences in these characteristics yield important qualifications on the role of employed contacts and shed further light on the workings of job information networks.

Ties' intensity and congestion

A natural definition of ties' intensity in our setting is the time the displaced spent with a specific fellow worker on the same workplace. Average exposure to contacts thus provides a synthetic measure of ties' strength. Conditional on the time the displaced spent on average in each firm, variation in intensity is induced by the timing of contacts' turnover at past firms. In column 2 of table (3) we augment the main specification (whose results are reported in column 1) with our intensity measures. Specifically, letting m_j the number of months spent with contact j at the same firm, we define average exposure of displaced i to currently employed contacts as $EE_i = \frac{\sum_{j \in E_i} m_j}{E_i}$; similarly, $EU_i = \frac{\sum_{j \in U_i} m_j}{U_i}$ measures average exposure to currently unemployed contacts, where $(E_i + U_i)$ is network size. Results show that longer exposure to currently employed contacts shortens unemployment duration. A one standard deviation increase in average exposure (corresponding to slightly less than 10 months) reduces unemployment duration by 9 percent. Interestingly, lower exposure to future unemployed coworkers turns out to have a similar effect, suggesting the presence of congestion effects at the firm level. A longer presence in the firm of coworkers who will not be useful sources of information at the future displacement date relative to those who will may in fact reduce the net exposure to each contact, thus weakening the strength of the tie with a currently employed contact (Bramoullè and Saint-Paul (2004)).

Competition for information

The advantages of being connected to an employed individual decrease with the number of other job seekers he is in contact with (Calvo-Armengol (2004) and Wahba and Zenou (2005)). The above evidence on the positive effects of network employment rate could therefore reflect the fact that more information is generated in networks with more employed contacts as well as the lower degree of competition for the available information determined by a lower unemployment rate. To single out these two effects we develop an intuitive measure of the degree of competition for the information held by a given contact, namely the number of contemporaneously displaced individuals he is connected to. 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[24]. It thus provides an exogenous shift in the degree of competition for a given information source as long as common sources of displacement across firms (e.g. business cycle shocks) are absorbed by the closing firm fixed effect. Augmenting the basic specification with the average number of competitors shows that a higher degree of competition significantly slows down re-employment (col. 3). Specifically, increasing the number of

[24]Note that if a contact is connected only to workers displaced by the same firm the degree of competition does not vary across codisplaced workers.

competitors by 8 units (corresponding to a shift from the 1st to the 3rd quartile in our sample) raises unemployment duration by about 6 percent.

Networks' information content

We now turn to an analysis of factors that determine the amount of valuable information in the network. We draw on variation in contacts' recent employment decisions and status. The first distinction we make is between currently employed contacts who still maintain the job where they met the displaced (stayers, S_i) and those who meanwhile changed employer (movers, M_i). By the same fact they are no longer employed in a firm the displaced already visited, movers are relatively more likely to be endowed with relevant information than stayers, the more so if switching job required engaging in search activity and collecting information which can then be spread through the network. In column 5 we thus split the network employment rate $\frac{E_i}{N_i}$ in the share of movers, $\frac{M_i}{N_i}$, and of stayers, $\frac{S_i}{N_i}$. The distinction turns out to be highly relevant. The effect of an increase in the overall network employment rate on duration more than doubles when stemming from a higher share of movers as opposed to a higher share of stayers. According to our estimates a one standard deviation increase in employment rate due to a higher share of movers reduces unemployment duration by around 11 percent, corresponding to about 5 weeks for the average unemployment spell; the reduction would be around 2 weeks if the increase in the employment rate was due to a larger share of stayers[25].

The same distinction is carried through to columns 6 and 7 where we address two additional aspects that plausibly signal access to more relevant information, namely technological and geographical proximity. In either case, the basic intuition is simple. Contacts employed in sectors the displaced is more familiar with are likely to play a more relevant role when locating attractive job opportunities; the same holds true for contacts employed in the local market if workers have a preference for working

[25]We also find that the more recent the job switch the stronger the effect of a given share of movers, in line with the intuition that they carry more up-to-date and thus valuable information. Results are available upon request.

closer to their own residence[26]. We augment the previous specification with the shares of movers and of stayers employed in the displacing sector (col. 6) and of those employed at firms relatively close to the displaced residence (col. 7). Results confirm the intuition, further stressing the importance of recent job switchers. For example, a mover employed in the displacing sector is twice more effective, in terms of the unemployed chances of getting a job, than one employed in a different industry; the same qualitative result is true when we look at the geographic location of movers, although the additional effect is lower. On the other hand, only the sectoral distribution of stayers seems to matter[27].

Finally, results in columns 8 and 9, obtained using specifications that include all network characteristics, largely confirm the above findings.

4.3 Entry Wages and Job Stability.

Our findings so far largely confirm the theoretical predictions in that being connected to a larger share of employed contacts significantly contributes to speeding up reentry into employment, the more so the more recently the contact changed job. The role of job information networks for job characteristics is less clearcut, their attributes potentially affecting the features of the new job in several ways. For example, high earning contacts may generate a higher expected wage because they pass on offers they do not find profitable (among others, Calvo-Armengol (2004), Mortensen and Vishwanath (1994)) or because better contacts provide a prospective employer with superior information on the applicant (Montgomery (1991). Additionally, by reducing the uncertainty on the new hire, contacts' referrals may also lead to a longer expected tenure in the new job (Jovanovic (1979), Simon and Warner (1992)).

[26] For example, if offers from farther locations involve a commuting cost, the worker will set a higher reservation wage for jobs at those locations. Additionally, if the arrival rate of offers also depends of search effort (say, acquaintances must be contacted) the lower expected wage of an offer from those locations would lead to put less effort into search for these jobs. Note this would be the case even if the support of the wage distributions was the same at both locations and always above the commuting cost.

[27] As a natural consequence of these findings, increasing network proximity should induce the displaced to re-enter in geographically or technologically closer firms. Results not reported here (available upon request) show that this is the case: a higher share of contacts in the displacing sector increases the chances of being re-employed in that sector and the new job is also closer to the displaced hometown the more movers are employed around it.

On the other hand, certain contacts' characteristics may also lead to lower wages. For example, if the provided information concerns jobs somehow unsuited to the unemployed (for example, by involving new tasks), he may nonetheless decide to accept trading off the lower quality with a shorter unemployment spell (Bentolila, Michelacci and Suàrez (2004)); this in turn may also lead to a shorter tenure on the new job[28].

Table (4) reports estimates from our preferred specification for a number of post-entry outcomes. Results in column 1 show a positive effect of contacts' wage premium on post-displacement wages, compatibly with either high wage contacts passing on high wage offers or their providing more valuable referrals. Increasing contacts' current wage premium by one standard deviation increases entry wages by 1.7 percent, corresponding to 4.2 percent of their standard deviation. As a benchmark, a one standard deviation increase in own wage at displacement raises entry wages by 5.7 percent (13.6 percent of their standard deviation). While relevant in terms of unemployment duration the share of employed contacts does not affect entry wages. These findings are consistent with the idea that, whatever the arrival rate of offers, the option value of turning them down is close to zero. In this case, although they will re-enter at a considerably faster pace, displaced individuals endowed with high employment rate networks would still earn a starting wage which is a random draw from a common distribution[29].

These results largely carry through to job stability. Column 2 looks at the probability of still holding the entry job 12 months after re-entry. Contacts' wage premium has a positive and significant effect on subsequent job tenure. Again, increased stability might follow from better contacts' referrals substantially reducing uncertainty on the quality of the new match, or their sharing information on better jobs, those below their reservation wage. According to our estimates, a one standard deviation

[28]The relationship between search methods and subsequent wages is empirically unclear. Jobs found through contacts are not necessarily associated to higher earnings and some types of contacts are found to lead to lower wages (see Ioannides and Datcher Loury (2004)).

[29]Note however that their lifetime earnings are still higher because of the shorter unemployment spell.

increase in contacts' wage premium increases the probability of holding the same job after one year by 2.7 percentage points, corresponding to 4.5 percent of the average probability. Somewhat strikingly, individuals endowed with a larger share of employed among their contacts are also less likely to keep the entry job, a result entirely driven by the share of stayers (col. 3). Possibly, by being more dated, information conveyed by stayers involves less desirable non-wage job attributes, which are however traded off against a significantly shorter unemployment duration. In fact, consistently with the idea that all job offers are accepted and job search continues on the job, the last column of the table shows that the probability of being employed at the same horizon (12 months) after reentry, regardless of the employer identity, is unaffected by network characteristics (col. 4). This suggests that those who initially ended up in less favourable matches because of their contacts' attributes, look for better opportunities and eventually switch job. Note, however, that this does not imply there are no differences between displaced one year after reentry. For one thing, those with higher quality contacts are more likely to be in a better paying job.

The intensity of the ties with employed contacts, while irrelevant for entry wages, turns out to increase job stability. Plausibly, stronger ties are more successful in supplying a given employer with reliable information, thus leading to less separations. We also find evidence that a larger number of potential competitors reduces expected tenure in the current job.

5 Discussion and Robustness Checks.

The findings of the previous section confirm that a thorough characterization of contacts' labor market attributes is crucial to unveil the mechanisms underlying the workings of job information networks. On the one hand, contacts' current employment status and recent turnover is crucial in channelling information on available opportunities to job seekers. This implies that even temporary shocks to the network employment rate may have long lasting effects because they modify the overall availability of information, the more so the more segregated the network. On the other hand, contacts' quality turns out to be the only relevant network characteristic for the wage and the stability of the subsequent job. This suggests that employers may face serious screening problems when hiring and additional information provided by high quality referrals may significantly reduce the uncertainty involved in a new match (Simon and Warner (1992), Datcher Loury (2006)). Alternatively, it may reflect the fact that contacts in better matches are willing to share more information, possibly because a larger share of the opportunities they learn about is relatively unattractive. In both cases, policies favouring an efficient allocation of workers to jobs may have important spillovers either by increasing the amount of information passed on to untargeted job seekers by their contacts or by complementing, via the referral effect, the information on new hires[30].

5.1 Reverse causality and unobserved heterogeneity

The main identifying assumption required to interpret the results as effects of the information conveyed by a given network is that, conditional on the set of controls, variation in contacts' attributes at displacement date is orthogonal to individual unobserved characteristics. As we discussed above, it may fail if the labor market brings together workers with similar unobserved characteristics or if, by having shared the workplace, contacts are subject to the same shocks (for example, they acquire the same skills). In this section we provide indirect evidence in favor of our identifying assumption addressing three major concerns.

The first is that our controls are not able to pick up unobserved fixed characteristics shared by the displaced and his contacts. If this was the case, however, we should detect significant empirical correlations when relating contacts' attributes to individual outcomes *prior* to displacement. In columns 1 to 4 of table (5) we relate network characteristics to the weekly wage earned and time spent in

[30] Of course, there are no policy implications if our measure of contacts' quality only reflects their innate ability, rather than match specific quality by which the employer could trust a referral. However, since results are conditional also on contacts' past wage premia, this possibility is plausibly ruled out.

employment by the displaced 3 to 5 years before displacement[31]. Network characteristics have no predictive power for these two outcomes, suggesting that results in the previous sections are unlikely to be driven by omitted individual fixed characteristics.

Second, we address the possibility that our results reflect a causal effect of contacts' fixed unobserved characteristics rather than of their current labor market attributes. While still of interest, our estimates would quantify the effects of exposure to contacts' exogenous characteristics rather than their disseminating information obtained on the basis of their current labor market status. However, if this was the case we should be able to detect some significant correlation between individual post-displacement outcomes and his contacts' labor market conditions at some point prior to displacement. In columns 5 to 10 we report results for unemployment duration, entry wages and the probability of holding the same job 12 months from re-entry using network characteristics as measured 4 years before the individual was displaced[32]. Results show that past labor market contacts' attributes are unable to predict any post-displacement outcomes. We read this as supportive of the interpretation that the source of identification is random variation in contacts' current conditions. This leads us to a third concern: we must be sure that random variation in contacts' current labor market attributes is not due to shocks that also affect other displaced. Our empirical strategy accounts for shocks that affect equally co-displaced workers and their contacts through the CFFE; also, the LLM-year and the 3-digit industry-year dummies absorb all LLM-wide and industry-wide labor market shocks[33]. Identification thus hinges on variation in contacts' labor market status among co-displaced workers within LLM and within industry. We may however fail to capture industry-LLM specific shocks. For example, a new

[31] The only difference with respect to the main specification is that, having pooled wage observations for different years, we also include year dummies to capture common cyclical variation in individual wages and employment status.

[32] Specifically, we track each contact and recover his employment status and wage 4 years before displacement. Since we cannot precisely pin down a month when to measure contacts' attributes we proceed as follows. As concerns employment, we weight every contact for the time he spent employed in the first semester of the relevant year; a mover is defined as a contact who in the first semester was in a job other than the one he held the previous year. Results are robust to alternative assumptions of employment status as well as to alternative choices of the relevant pre-displacement year.

[33] Industry dummies are defined on the basis of the 3-digit industry where the displaced accumulated the longest tenure over the pre-displacement period.

plant requiring a specific skill in a given LLM would plausibly affect workers endowed with that skill and living in the LLM differently from co-residents with different skills or individuals with similar skills from other LLMs[34]. This would be a concern if co-displaced workers (and their networks) were different in terms of LLM-skills combinations. We deal with this possibility in table (6). We re-ran the main regressions allowing for a full set of 2-digit industry-LLM-year dummies; we also include town and 3-digit industry fixed effects to absorb permanent differences among towns in the same LLM (e.g. distances) and among sub-industries belonging to the same 2-digit sector (e.g. skills). Results are unaffected by this extension: we still find that contacts' employment status and tenure are the main factors affecting unemployment duration while contacts' wage premium is a significant determinant of the entry wage and the degree of stability of the subsequent job.

5.2 Measurement errors

A puzzling feature of our results is the absence of any effect of the size of the network (table (2)). However, this may be a consequence of the measurement error induced by defining network size as the simple count of pre-displacement coworkers. In particular, we may be assigning too many contacts to some individuals. For example, if an individual cannot maintain more that Z contacts the measurement error would be zero whenever the number of contacts does not exceed the threshold and $\epsilon_i = C_i - Z$ otherwise, where C_i is the measured extension. Under these assumptions the measurement error would display a mechanical and positive correlation with the underlying true network, C_i^*, generating the standard attenuation bias. We attempt to shed light on this issue and develop a way to correct the size measure assuming that, above a certain threshold Z, the individual meets a coworker only with some probability. Let us assume we can rank coworkers in a given firm of size $N > Z$ with some distance metric from the displaced (say, because they work in different units),

[34] 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.

and that the probability of meeting farther individuals decays with distance at rate γ. Let the $P^n = e^{-\gamma \max\{0,n-Z\}}$ the probability of meeting coworker who is in position $n = \{1, ..., N\}$. Because the true ranking within a firm is unknown the probability that coworker i is in position n of the ranking is $P(n_i = n) = 1/N$ [35]. Therefore, the probability that the displaced actually meets coworker i is given by $P_i = \sum_{n=1}^{N} P(n_i = n) * P^n = \sum_{n=1}^{N} P^n/N$. Making use of the definition of P^n, after some algebra, we obtain $P_i = \left(Z + (e^{-\gamma}/(1 - e^{-\gamma}))(1 - e^{-\gamma(N-Z)})\right)/N$. Knowing Z and γ we can thus weight each assigned coworker and redefine network measures accordingly. In table (7) we use the corrected network size measures and present results under alternative assumptions on Z and γ. Results suggest that measurement issues may explain the absence of scale effects in previous specifications. Even assuming a slow decay of the probability of meeting additional workers we detect some negative effect of scale consistently with theoretical predictions. The effect loses significance as we increase the threshold or lower the decay rate, thereby going back to the original error-ridden measure. Reassuringly, in comparison with those reported in table (2) the results on the effects of the network employment rate are largely unaffected by the correction.

5.3 Censoring and sample selection

The results so far are based on a sample of displaced individuals observed again in employment. This may pose two problems. First, results can be affected by right-censoring. By pooling displacements occurred over many years, the censoring rule shrinks over time and is at 36 months for 1994 closures. To check that our results on duration are not affected by censoring we estimated a set of linear probability models of the likelihood of being unemployed after 9, 12, and 18 months from displacement on a sample including all displaced workers (table 8). Consistently with the main results in the previous sections, we find that the probability of unemployment is lower the higher the employment rate at all three

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

horizons.

Second, individuals in the sample of completed spells may represent a selected sub-sample of the displaced. If post-displacement participation was driven by network characteristics our estimates might be affected by selection bias. We check for this possibility with a standard Heckman two-step estimator. Ideally, we should exploit a set of plausible exclusion restrictions to properly identify network effects on unemployment duration. Unfortunately, such instruments are unavailable and we have to rely on functional form identification and thus on tail behavior of the inverse Mills ratio. With these caveats in mind, the estimated network effects for the four outcomes of interest and their standard errors are in line with our previous results (cols. 4-7, table 8).

6 Conclusions

Local and non-market interactions have received a lot of attention as potential causes of persistent segregation and differential behaviors along a number of dimensions. The sources of these effects can be manifold: social norms, peer pressure, conformism, information sharing. In this paper we have shown that job search outcomes of exogenously displaced manufacturing workers are shaped by a number of features of their network of former fellow workers, plausibly capturing the availability and relevance of information on existing employment opportunities. Unemployment spells are significantly shorter when a larger share of contacts are currently employed, and the effect is much stronger when contacts have recently changed job and are employed in markets more relevant to the displaced. Stronger ties enhance network effectiveness, while a higher degree of competition for the information held by a given contact significantly delays re-employment. Contacts' wage premium turns out to be an important determinant of subsequent wages and job stability, consistently with the idea that contacts in better jobs disseminate superior information. Overall, our results confirm that job information networks and informal hiring channels are an important means to overcome information shortages thus improving individual and aggregate labor market performance.

We have specifically focussed on individual job finding and separation rates. On the one hand, this sheds light on the type of information shortage faced by the market, namely whether it is about match characteristics or about the availability of employment opportunities. On the other, knowledge of the specific channels obviously allows to design more appropriate interventions to compensate disadvantaged groups.

By not relying on specific policy interventions or on experimental evidence our strategy can be easily extended to other contexts. In particular, given the increased availability of administrative worker-firm matched records, this approach makes it easy to perform cross-country comparisons to assess the relative importance of informal hiring channels and, possibly, their impact on the workings of aggregate labor markets. Additionally, one could address the pervasiveness of workplace networks extending the analysis to alternative, possibly non-labor, outcomes by linking administrative records as ours to other data sources.

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Table 1: Descriptive statistics.

	(1)	(2)	(3)	(4)	(5)
	Percentile			Mean	Standard Deviation
	10th	50th	90th		
A. Codisplaced Workers.					
Number of codisplaced	1	5	15	7.6	10.2
Average Age	20	27	38	28	7
% Male	0	66.7	100	57.1	39.8
% Blue Collar	0	100	100	82.0	32.8
% live in:					
- same city as CF	14.3	88.9	100	76.0	31.8
- same town as CF	0	33.3	100	38.2	33.2
B. Workplace Networks.					
Total Contacts	8	32	150	60.3	81.1
% Employed Contacts	42.8	68.0	90.1	66.8	19.3
Average Distance (km)	2.1	5.5	17.6	10.2	28.4
% live in:					
- same city as Displaced	11.9	77.2	94.7	66.0	29.6
- same town as Displaced	0	20.0	59.3	25.4	23.3
Average Age Difference	4	8	15	9	5
% Male	8.3	60.0	1	57.6	33.3

Table entries are the corresponding column statistic computed on the sample distribution of the closing-firm level (panel A) and workplace network level (panel B) row variable.

Table 2: Unemployment duration and network characteristics.

	(1)	(2)	(3)	(4)
Network Characteristics:				
–Size	-0.022 (0.018)	0.026 (0.020)	-0.020 (0.038)	-0.047 (0.044)
–Employment rate	-0.284* (0.120)	-0.306* (0.120)	-0.402** (0.126)	-0.365* (0.148)
–Wage premium	-0.242† (0.127)	-0.191 (0.128)	-0.251† (0.146)	-0.220 (0.174)
Wage at displacement		-0.227** (0.060)	-0.232** (0.060)	-0.242** (0.067)
Wage growth in NB		0.122 (0.109)	0.140 (0.111)	0.088 (0.123)
Average unemployment spell in NB		0.393** (0.083)	0.513** (0.105)	0.444** (0.119)
Number of firms visited in NB:				
–1			-0.270** (0.092)	-0.344** (0.105)
–2			-0.186** (0.066)	-0.242** (0.078)
–3			-0.073 (0.062)	-0.107 (0.074)
Average firm size in NB			0.032 (0.049)	0.054 (0.057)
Average wage of coworkers in NB			0.167 (0.144)	0.242 (0.171)
Closing firm FE	YES	YES	YES	YES
Year*City of residence	NO	NO	NO	YES
Year*Sector experience	NO	NO	NO	YES
Obs.	9121	9121	9121	9121
Adj. R^2	0.23	0.24	0.24	0.25

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

Table 3: Unemployment duration and employed contacts.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Information diffusion			Information relevance				
Size	-0.047	-0.054	-0.008	-0.014	-0.072	-0.076†	-0.075†	-0.038	-0.046
	(0.044)	(0.045)	(0.047)	(0.048)	(0.045)	(0.045)	(0.045)	(0.048)	(0.048)
Wage premium	-0.220	-0.219	-0.205	-0.205	-0.198	-0.186	-0.216	-0.180	-0.186
	(0.174)	(0.175)	(0.174)	(0.174)	(0.174)	(0.173)	(0.173)	(0.174)	(0.172)
Employment rate	-0.365*	-0.373*	-0.299*	-0.305*					
	(0.148)	(0.148)	(0.150)	(0.151)					
Exposure to:									
- employed contacts		-0.009*		-0.010*				-0.009*	-0.009*
		(0.004)		(0.004)				(0.004)	(0.004)
- unemployed contacts		0.010*		0.010**				0.010**	0.010**
		(0.004)		(0.004)				(0.004)	(0.004)
Competition			0.007*	0.007*				0.008*	0.007*
			(0.003)	(0.003)				(0.003)	(0.003)
- share of stayers					-0.281†	-0.064	-0.426*	-0.210	-0.129
					(0.149)	(0.176)	(0.178)	(0.152)	(0.201)
- share of movers					-0.602**	-0.335†	-0.395*	-0.549**	-0.142
					(0.173)	(0.203)	(0.191)	(0.175)	(0.216)
Technological distance:									
- share of stayers in displacing sector						-0.300*			-0.324*
						(0.139)			(0.140)
- share of movers in displacing sector						-0.454*			-0.374*
						(0.177)			(0.179)
Geographic distance:									
- share of nearby stayers							0.214		0.216
							(0.141)		(0.142)
- share of nearby movers							-0.277*		-0.253*
							(0.111)		(0.111)
Obs.	9121	9121	9121	9121	9121	9121	9121	9121	9121
Adj. R^2	0.25	0.25	0.25	0.25	0.25	0.25	0.25	0.25	0.25

Robust standard errors in parentheses. (†) significant at 10%; (*) significant at 5%; (**) significant at 1%.
All regressions include controls for gender, a quadratic in age and tenure in the closing firm, four qualification dummies, wage at displacement, wage growth and average unemployment over the NB period, dummies for the number of firms visited over the NB period, their average size, commuted distance, a closing firm FE, year-city of residence and year-3-digit sectoral experience fixed effects. Nearby contacts are defined as those living in towns whose distance from the displaced residence is less than the median distance between displaced and contacts in the sample.

Table 4: Network characteristics, entry wages and job stability.

	(1)	(2)	(3)	(4)
		1 year after entry:		
	Entry Wage	Same Job		Employed
Size	0.006	-0.039†	-0.015	0.003
	(0.018)	(0.022)	(0.022)	(0.017)
Wage premium	0.122*	0.193*	0.167*	-0.078
	(0.059)	(0.086)	(0.085)	(0.063)
Employment rate	-0.052	-0.324**		-0.009
	(0.051)	(0.071)		(0.055)
Competition	-0.000	-0.003*	-0.004*	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Exposure to:				
- to employed contacts	0.000	0.008**	0.008**	0.001
	(0.001)	(0.002)	(0.002)	(0.001)
- to unemployed contacts	-0.000	-0.010**	-0.010**	0.000
	(0.001)	(0.002)	(0.002)	(0.001)
Share of stayers			-0.421**	
			(0.073)	
Share of movers			-0.075	
			(0.081)	
Obs.	9121	8531	8531	8531

Robust standard errors in parentheses. (*) significant at 5%; (**) significant at 1%.
Columns (2-4): linear probability models. Dependent variable: $Y = 1$ if still in entry job after 1 year (cols. 2 and 3); $Y = 1$ if employed after 1 year from re-entry, irrespective of employer's identity (col. 4).
All regressions include controls for gender, a quadratic in age and tenure in the closing firm, four qualification dummies, wage at displacement, wage growth and average unemployment over the NB period, dummies for the number of firms visited over the NB period, their average size, commuted distance, a closing firm FE, year-city of residence and year-3-digit sectoral experience fixed effects.

Table 5: Robustness checks.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Individual Pre-Displacement:				Past Contacts' Characteristics					
	Current Weekly Wage		% Sem. Unemployed		Unemployment Duration		Entry Weekly Wage		Same Job after 12m	
Size	0.005 (0.008)	0.006 (0.008)	0.011 (0.012)	0.014 (0.012)	-0.061 (0.045)	-0.061 (0.045)	0.013 (0.015)	0.014 (0.015)	-0.038† (0.021)	-0.037† (0.021)
Employment rate	-0.024 (0.028)		0.027 (0.037)		-0.035 (0.158)	-0.035 (0.158)	-0.010 (0.050)	-0.011 (0.050)	0.105 (0.074)	0.103 (0.074)
Share of stayers		-0.026 (0.029)		0.024 (0.037)						
Share of movers		-0.016 (0.030)		0.047 (0.045)		0.003 (0.239)		0.095 (0.074)		0.136 (0.110)
Wage premium	0.048 (0.049)	0.048 (0.049)	-0.063 (0.046)	-0.064 (0.045)	0.072 (0.180)	0.072 (0.180)	0.000 (0.058)	0.002 (0.058)	0.083 (0.084)	0.085 (0.084)

Robust standard errors in parentheses. (†) significant at 10%; (*) significant at 5%; (**) significant at 1%.
Columns (1)-(2): dependent variable is weekly wage 3 to 5 years before displacement. Columns (3)-(4): dependent variable is share of 1st semester spent unemployed 3 to 5 years before displacement. Columns (5)-(10): contacs characteristics are determned 4 years before displacement: employment rate is computed weighting each contact for the share of the 1st semester he spent employed; a mover is a contact who in the first semester of the year was in a job other than the one held the previous year.
All regressions include controls for gender, a quadratic in age and tenure in the closing firm, four qualification dummies, wage at displacement, wage growth and average unemployment over the NB period, dummies for the number of firms visited over the NB period, their average size, commuted distance, a closing firm FE, year-city of residence and year-3-digit sectoral experience interactions.

Table 6: Robustness to city-industry shocks.

	(1)	(2)	(3)	(4)	(5)	(6)
	Unemployment Duration		Entry Weekly Wage		1 Year Job Stability	
Size	-0.058	-0.086†	-0.001	-0.001	-0.025	0.006
	(0.048)	(0.049)	(0.017)	(0.017)	(0.023)	(0.023)
Employment rate	-0.372*		-0.063		-0.323**	
	(0.163)		(0.055)		(0.078)	
Share of stayers		-0.282†		-0.063		-0.427**
		(0.164)		(0.057)		(0.079)
Share of movers		-0.627**		-0.064		-0.034
		(0.191)		(0.060)		(0.089)
Wage premium	-0.147	-0.122	0.156*	0.156*	0.212*	0.183*
Obs.	9121	9121	8528	8528	8531	8531
Adj. R^2	0.25	0.25	0.91	0.91	0.09	0.10

Robust standard errors in parentheses.
(†) significant at 10%: (*) significant at 5%; (**) significant at 1%.
All regressions include controls for gender, a quadratic in age and tenure in the closing firm, four qualification dummies, wage at displacement, wage growth and average unemployment over the NB period, dummies for the number of firms visited over the NB period, their average size, commuted distance, a closing firm FE, dummies for town of residence and 3-digit sectoral experience and interactions 2-digit industry-city-year.

Table 7: Measurement error corrections.

	(1)	(2)	(3)	(4)
Z:	5	10	15	20
		$\gamma = 0.25$		
Size	-0.139†	-0.100†	-0.087†	-0.079
Employment rate	-0.430**	-0.423**	-0.413**	-0.403**
Wage premium	-0.212	-0.211	-0.211	-0.212
		$\gamma = 0.75$		
Size	-0.162*	-0.105†	-0.090†	-0.081
Employment rate	-0.431**	-0.425**	-0.416**	-0.406**
Wage premium	-0.213	-0.212	-0.211	-0.212
		$\gamma = 1.25$		
Size	-0.169*	-0.106†	-0.090†	-0.081
Employment rate	-0.431**	-0.426**	-0.416**	-0.406**
Wage premium	-0.213	-0.212	-0.211	-0.212

Robust standard errors in parentheses.
(†) significant at 10%; (*) significant at 5%; (**) significant at 1%.
Econometric model is as in col. 4 of table (2). Network characteristics are computed weighting each contact acquired in a firm of size N by $P_i = \left(Z + (e^{-\gamma}/(1-e^{-\gamma}))(1-e^{-\gamma(N-Z)})\right)/N$ if $N > Z$ and $P_i = 1$ otherwise.

Table 8: Censoring and participation.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Unemployed after:			Duration	Entry wage	Employed after 1 year with:	
	9 m.	12 m.	18 m.			same employer	any employer
Size	-0.062^{**}	-0.084^{**}	-0.072^{**}	-0.040	0.009	-0.044^{**}	0.002
	(0.017)	(0.016)	(0.016)	(0.037)	(0.012)	(0.017)	(0.013)
Employment rate	-0.209^{**}	-0.147^{**}	$-0.093^{\dagger}$	-0.376^{**}	-0.050	-0.282^{**}	0.014
	(0.056)	(0.054)	(0.053)	(0.126)	(0.043)	(0.058)	(0.044)
Wage premium	-0.011	-0.008	-0.014	$-0.260^{\dagger}$	$0.095^{\dagger}$	0.170^{*}	0.039
	(0.065)	(0.062)	(0.060)	(0.146)	(0.050)	(0.067)	(0.051)
Obs.	11091	11091	11091	11091	11091	11091	11091

Robust standard errors in parentheses.
(†) significant at 10%; (*) significant at 5%; (**) significant at 1%.
Cols. 1-3: dependent variable is $d_i = 1$ if unemployed after x months from displacement, $d_i = 0$ otherwise. Cols. 4-7: estimates of main equation in Heckman-type selection model. Selection equation estimated on same information set as main equation. Conditioning set as in column 2 of table 2.