People I know:
social networks and job search outcomes.

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Abstract

Theoretical models linking social networks and job search outcomes generally conclude that, all else equal, individuals belonging to larger and *better* networks find jobs more quickly and end up earning higher wages. Empirical work on the role of networks for job search has so far been limited to comparing the role of networks to that of other methods of search. In this paper we tackle a so far empirically unexplored question: the impact of network size, composition and quality on unemployment duration and wages. We address these relationships using a comprehensive data-set of matched worker-firm social security records. The data cover the universe of individuals employed in a very integrated local labor market in northern Italy over the period 1975-1997, allowing us to reconstruct individual working histories for over one million workers. By tracking who worked together when, where and for how long we can build several measures of networks individual workers have potential access to. We then relate these network indicators to unemployment duration and entry wages for a subset of over 13,000 exogenously displaced workers. Results are in line with several theoretical predictions and point to statistically significant and robust network effects on unemployment duration. We use our data on individual working histories to argue that these effects are not likely to be driven by individual heterogeneity. The quantitative importance of networks on post-displacement wages seems to be rather limited.

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1 Introduction

Since the early studies by Granovetter (1973) a large body of empirical evidence has documented the widespread use of social contacts in the job search process and their effectiveness in shaping its outcomes. Jobs obtained through informal channels are in excess of 50 percent, independently of the country or the type of occupation under scrutiny. Moreover, social contacts are shown to be a relatively more effective search vehicle in terms of offer and acceptance rates, wages earned and subsequent tenure. Finally, it has been shown that even employers make large use of social contacts in the recruiting process and fill a relevant share of their vacancies with referred applicants (among others, Holzer (1987), Holzer (1988), Blau and Robins (1990)).

The theoretical underpinnings of this evidence are straightforward. In a world of imperfect information belonging to a network of contacts may improve the quantity and quality of the information one has access to: job seekers may be informed about potential employment opportunities by their contacts; employers may be informed about applicants' quality by their social ties. In the last decade theoretical contributions have explored several implications of these simple assumptions. Montgomery (1991) shows that wage dispersion arises if the hiring process is based on referrals and concludes it is positively related to the density and the degree of inbreeding of social networks; Calvo-Armengol and Jackson (2004) explicitly model the structure of social contacts and show that individual employment status is positively correlated across people in the same network and over time. Relaxing the assumption of fixed contacts by assuming that social ties are more likely to be established and maintained between people sharing the same labor market status, Bramoullé and Saint-Paul (2004) generate unemployment duration dependence. Fontaine (2004) embeds the workings of social networks in a formal model of a labor market with frictions to endogenize wages and job offers arrival rates and shows how equilibrium wage dispersion, employment rates and average wages are positively related to network size. In a similar framework, Bentolila, Michelacci and Suàrez (2004) show how the availability of more social contacts in a world with heterogeneous jobs and skills may lead to a mismatch of workers' comparative productive advantage and their occupational choices.

In this paper we provide a quantitative assessment of a common implication of most of these models, namely the fact that workers endowed with larger and *better* networks should exhibit a higher probability of leaving unemployment. We also explore the consequences of network characteristics for wages, although the theoretical predictions seem somewhat more ambiguous. The main contribution of the paper consists in exploiting measures of network extension and quality at the individual level: for a large sample of exogenously displaced Italian workers we obtain individual-specific measures of the number and quality of potential contacts they have access to when unemployed and investigate whether differences in size and quality of networks reflect in different unemployment durations and wages.

Our results show that larger social networks significantly reduce the time spent unemployed. Our most conservative estimate suggests that increasing network size from the first quartile to the median (23 additional contacts) reduces unemployment duration in our sample by 10-15 days. Moreover, we find that the labor market status of one's contacts matter: a 10 percentage points increase in the share of employed contacts reduces unemployment duration by more than one month; the effect nearly doubles if the higher employment share is due to contacts who recently experienced job-turnover. On the contrary, we only find limited effects as concerns entry wages. We exploit the availability of detailed information on the working histories of each individual in our data to argue that these results are not likely to be driven by individual heterogeneity.

These findings are relevant in several respects. For example, the existence of a causal effect of the number of contacts on one's employment probability may contribute to explain the so-called "labor market pooling" effects. Since Marshall (1890) argued that intense labor market interactions in more populated (urban) areas constitute one of the main sources of agglomeration economies a large literature has studied the mechanisms by which proximity improves the probability and quality of matching between firms and workers (Krugman (1991), Duranton and Puga (2004)); the reasonably higher number of ties one can establish in denser areas might constitute one such mechanisms. From the normative point of view, the most immediate policy implication is that interventions to sustain and improve the employment rate of a given population will affect also the employment outlooks of individuals connected to those directly targeted. Therefore concentrating interventions into tighter networks will magnify the initial effect, the more so the tighter the cluster. In turn, if clusters are not totally separated the effect will propagate to other, possibly indirectly, connected individuals (Calvo-Armengol and Jackson (2004)).

Despite their relevance the effects of network extension and quality on unemployment and wages have hardly been empirically investigated. Most studies have typically aimed at assessing the relevance of social contacts as a job search method. These works exploit information on how the current job has been found or on the search method adopted when unemployed to study whether and how relying on social ties rather than on alternative methods affects wages, observed tenure or unemployment duration[1].

Wahba and Zenou (2003) is the only work we are aware of to explore the effects of network characteristics on individual labor market outcomes. Using local population density as proxy for the number of potential contacts a given individual has access to, and local unemployment rate to capture the quality of such contacts, they estimate the effect of network characteristics on the probability that the current job was found through contacts as opposed to alternative methods. They find that employed individuals living in denser and low-unemployment cities are more likely to have found the

[1] Using different samples of US workers, Holzer (1987), Holzer (1988) and Blau and Robins (1990) showed that contacts (friends and relatives) are one of the most frequently used methods of search and it generates the highest rates of job offer and acceptance of job offer conditional on its use. Subsequent works extended those findings to European countries (e.g. Gregg and Wadsworth (1996) for the UK and Addison and Portugal (2002) for Portugal). Simon and Warner (1992) showed that consistently with standard matching models where contacts help reducing the uncertainty about productivity of the unemployed, jobs found through referrals yield higher entry wages (but lower subsequent growth) and longer tenure.

job through contacts, an effect they attribute to the size and quality of the network. Their approach differs from ours in many ways. First, their network variables have no individual variation since they assign the same network extent/quality to all workers in the same location. Second, we explicitly focus on unemployment spells caused by exogenous shocks (firm closures). Hence we can quantify (if any) the expected reduction in involuntary unemployment associated with changes in the extent and quality of networks for the representative displaced worker. Estimating the correlation between network characteristics and the probability of using contacts is uninformative in this sense. For example, in a world where the expected tenure of jobs found through contacts relative to other methods were larger in denser areas one would also find a larger share of jobs found through networks, even if the probability of exiting unemployment was independent of density and all workers chose randomly among the available search methods. Third, by relying on population density as a source of identification their estimates might be picking up other forces that determine choices of residence or place of work (city vs. country selection process) but are unrelated to network-effects. For example, it has been documented that better workers select in cities (Glaeser and Marè (2001)). If for some reason (as self-confidence, etc.) such workers rely more on contacts than, say, on employment offices with respect to other people, then even if there are no network effects, the estimated coefficient on density (capturing the city-country differences) would be positive. By relying on a measure of network that allows us to control for local characteristics we do not have similar problems.

The paper is organised as follows. The next section describes the dataset and illustrates how we recover our basic measure of individual's network extension. We then turn to the empirical analysis: in sections (3) and (4) we investigate the relationship between unemployment duration and network size and the employment rate of the network; in section (5) a similar analysis is developed for entry wages. We then conclude.

2 Data, definition and preliminary evidence.

Our data sources are three National Security Service (INPS) archives providing information on the worker, the firm and the characteristics of the match for any work episode occurred over the period 1975-1997 in two Italian provinces[2]; movers are tracked so that the same information is available even if they worked in other areas of the country. For each worker our data allow to recover several individual-specific as well as firm-specific characteristics. Most relevant to our purposes, for each work episode we know its starting and final dates (month-year) as well as the number of weeks paid during each year and yearly pay. We can therefore recover the precise length (in months) of any given employment or non-employment spell, as well as job tenure at any point in time and weekly wages.

Our goal is to relate network characteristics to the length of an unemployment spell and to the subsequent entry wages. Since we do not know the reasons why a given unemployment episode begun, we restrict our analysis to a sample of about 13,000 workers displaced by a manufacturing firm-closure[3]. The exogenous source of the job loss helps in limiting problems due to selection into unemployment. Figure (1) reports the empirical distribution of the length of the unemployment spell.

The main ingredient of our analysis is a measure of network extension and of its characteristics at the individual level. We exploit the fact that our data allow us to precisely identify all people working in any given month for any given firm to recover the number of potential contacts established on the job: we define one's social network as the pool of individuals she worked with in the five years prior to displacement, an arbitrary but convenient time interval we call the *network building* period (henceforth NB). Therefore, our basic measure of network size is simply the number of co-workers met during the five years prior to displacement. Figure (2) shows the distribution of this measure of social

[2]The two provinces (Treviso and Vicenza) are located in the north-eastern part of the country. The overall population was 1,6 million people as of the 2001 Population Census. As a whole, the north-eastern regions are characterized by small firm size, tight labor markets, high density of economic activity; in year 2000 per capita GDP in this part of the country was 22,400 euro, 20 percent higher than the national average. The two provinces we have data about account for 3,3 percent of the Italian GDP.

[3]In the appendix we provide a full description of the main data sources and we detail how we moved from the universe of workers to the final sample.

network. We exclude from the pool of potential contacts those being displaced by the same firm since they are more likely to be competitors than useful acquaintances. For any of the remaining useful contacts we can recover several individual characteristics that allow us to describe and explore other interesting features of this potential network.

Our basic measure of network extension is certainly a variable measured with error. For example, it may not include a relevant part of one's social network, namely acquaintances made off the job which may also play a role in the job search process. Alternatively, by including all co-workers at firms visited during the NB period we might be overestimating the extension of one's network for those individuals who were employed at larger firms. We will deal with these and other related issues and we will show that this broad measure of network extension conveys information on the underlying number of contacts. Moreover, we will argue that this information content can be improved in several ways.

3 Unemployment duration and network size

In this section we relate unemployment duration of exogenously displaced workers to a measure of the number of contacts they have potentially accumulated on the job. Individuals are therefore not associated to a specific search method. The underlying assumption is that using personal contacts is not an exclusive method and that it is virtually costless.

We start from the following simple linear specification of (the log of) the length of individual i's unemployment spell following her displacement from firm j at time t:

$$u_{ijt} = \alpha + \gamma \log(Size_{ijt}) + \beta_1 X_{ijt} + \epsilon_{ijt} \tag{1}$$

where $Size_{ijt}$ is our indicator of the extent of the network of potential social contacts as of time t, X_{ijt} is a quite exhaustive set of controls for individual and firm characteristics that will be detailed as we go on and ϵ_{ijt} is an error term. Our main concern in the estimation of the strength of network-size effects (γ) is spurious correlation between network size and unemployment duration induced by the omission of relevant variables. To this purpose we condition the estimation of γ in regression (1) to a large set of individual and firm-specific variables recovered from our worker-firm matched data. Such variables are included in X_{ijt} together with standard individual observable characteristics (age, gender, qualification and firm tenure). Let us briefly discuss them.

First, we account closing-firm fixed effects. This implies that the identification of parameter γ stems from comparing workers displaced by the same firm and endowed with different network sizes. Firm-specific fixed effects allow to control for many characteristics of the closing establishment (e.g. year of closure, sector, location, size or any combination of these) whose omission might induce spurious correlation between our measure of size and unemployment. As a simple example, suppose we were to compare two workers displaced in a period of expansion and in the trough of a recession, respectively. Other things equal the first worker is likely to have visited in the past larger expanding firms than the second, likely to have visited downsizing firms. Being in an expansion, it's also likely that the former will on average find a job earlier than the second, but this will be because of the business cycle and not thanks to the larger imputed network size[4]. A further important advantage of estimating (1) with closing-firm fixed effects is that they also allow to account for omitted sources of individual heterogeneity to the extent that these are correlated with firm characteristics. For example, it is often argued that good workers are employed by large firms[5]. In this case closing firm fixed effects would control for the fact that average workers quality is different across closing firms. Note, however, that only if good workers re-enter employment relatively more rapidly not controlling for sorting would

[4]Alternatively, a worker employed in a declining sector but populated by large firms (say, chemicals) will have a larger network than one working in a small expanding one. Yet, upon displacement, the former will reasonably take longer to get a job. Finally, if closures differ significantly as to the number of displaced workers, observed unemployment spells of workers displaced by larger firms might reflect the larger number of people searching for a job at the same time: this congestion effect might imply larger unemployment spells for workers displaced by larger firms.

[5]Abowd, Kramarz and Margolis (1999) for France, Casavola, Cipollone and Sestito (1999), for Italy. Among others, Kremer (1993), Kremer and Maskin (1996), Saint-Paul (2001) develop theoretical models where the technology is such that in equilibrium workers may end up being segregated according to their skills.

lead to biased results.

Second, we exploit the possibility of tracking the working history of any individual in the data to further control for other sources of heterogeneity which might confound our inference. For example, information on the displaced workers' wage profile during the NB period allows to control for the fact that past earnings might have a signaling role for prospective employers, higher wages signaling more productive workers. This source of individual heterogeneity might bias our results if, say, differences in workers' productivity depend on employers' provided training and this is more frequent in large firms[6]. Similarly, we will account for differences in the individual probability of re-entering employment or in the propensity to stay out of employment using the length of workers' past unemployment spells. Omitting this control could bias our results since, in our data, workers who are more often employed are potentially assigned, all else equal, more contacts[7].

Third, since we also observe the whole working history and several individual characteristics of each network member we can control for many features of the network likely to be informative about characteristics of firms visited prior to displacement. For example, it is known that the *quality* of firms one has visited in the past constitutes, everything else constant, valuable information to potential employers during the recruiting process. If better firms are also on average larger in size then network size would be spuriously correlated with observed unemployment duration.

A final concern arises from the fact we include in a given individual i's network of contacts all the persons she has worked with in the previous five years. Clearly, working for Microsoft does not imply that one has personally met all other Microsoft employees although it sounds reasonable that she knows more people than a comparable individual who has been employed in, say, a small car workshop.

[6]On the positive relationship between wages, tenure, training and firm size see Oi and Idson (1999).

[7]Note however that also past wages may constitute a control for heterogeneous turn-over rates. For example, in a frictional labor market firms post more vacancies for more productive workers. In equilibrium this leads to higher wages and exit rates from unemployment for these individuals. Alternatively, if workers are homogeneous as concerns productivity but different in terms of exit rates from unemployment (say, they bear different search costs), again workers with shorter unemployment spells earn more because of the higher outside option.

This feature of our measure would imply an attenuation bias under reasonable assumptions about the structure of the error term. For example, if we assume that the true underlying network (S^*) and our measure ($Size$) are related according to $S^* = min\{Size, \bar{S}\}$, meaning that an individual knows everybody up to $\bar{S}$ people so that working in larger firms at most allows to establish contacts with $\bar{S}$ co-workers, the measurement error will be positively correlated with the underlying true variable biasing an OLS estimate of γ towards zero.

3.1 Main results

The first column of table (1) reports the results of a baseline estimation of unemployment duration on a dummy for sex, a quadratic in age, a set of dummies for the qualification at displacement (blue-collar, white-collar and manager), a quadratic in job tenure at displacement and closing-firm fixed effects. In the second column this basic model is extended allowing two additional controls for individual heterogeneity: the (log of) average weekly wage at displacement and the share of time spent unemployed over the five years prior to displacement (our NB period). Following the previous discussion, we expect to estimate a negative coefficient on pre-displacement wage, a positive one on the time previously spent unemployed and a lower network-size elasticity with respect to the first column.

As can be seen comparing the results in columns (1) and (2) the effect of network size on unemployment duration is negative and significant in both specifications. Moreover, the coefficients on the two additional controls are significant and have the expected sign: more productive and more frequently employed workers experience shorter post-displacement unemployment spells[8]. Adding such controls reduces the estimated elasticity to network size from about 0.1 to nearly 0.07. The elasticity estimated

[8]We have also experimented with a more flexible specification of past wages and employment experience. In particular, wage at displacement and the measure of labor market participation have been replaced by the entire wage profile and employment history over the five years prior to displacement to proxy for potentially dynamic heterogeneity. We have included, for each of the 10 past semesters the weekly wage earned and the share of time spent employed. Results on the parameter of interest turned out to be robust to this more flexible specification of individual controls.

in columns (2) implies that increasing the number of contacts from the 25th to the 50th percentile of its distribution, that is 23 additional potential contacts, reduces unemployment duration by more than 4 percent (table (2)). The average unemployment duration in our sample is about 10 months; the 23 additional contacts thus reduce unemployment duration by about 15 days.

The specification in columns (2) only accounts for heterogeneity potentially correlated with network size that reflects into wages and labor market attachment. In column (3) we extend it and include controls for individual mobility costs and preferences. Since we know where the individual lives and where each firm she has visited in the past is located, we can recover the average distance she has commuted to go to work during the network building period. Given her wage and other characteristics, this should proxy for an individual's propensity to commute. We believe this source of heterogeneity might be relevant since, all else equal, an individual with a lower mobility cost has access to a larger labor market and might pick better opportunities: if these are correlated with firm size again our estimate of γ might be biased. In particular, results in column (3) include a quadratic of the average distance of one's residence from past workplaces and a set of dummies for the residence of the displaced individual (not reported) which control for any geographic difference among individuals (say, different economic density, better transportation infrastructures, etc.). As expected, there is a negative correlation between distance and unemployment duration. The coefficient of interest is basically unaltered; the point estimate is slightly larger in absolute value but still within the confidence interval of the one reported in column (2).

We have argued in the previous section that firm-specific FE may take partially care of individual heterogeneity if workers are sorted across firms according to these characteristics[9]. However, if sorting in the closing firm is not perfect (say, because of a negative shock to the workforce) this strategy is not enough. We try to account for this possibility by recovering from each individual network some

[9]Note however that if the labor market does not sort workers across firms according to some rule (say, better workers into larger firms), the parameter γ is consistently estimated.

measures that capture some relevant characteristics of past firms in a synthetic way. In particular, since we observe the whole working histories and wages of each network member, along with several characteristics (sex, age, qualification, residence) we estimated a wage equation with individual data where the log of weekly wages of each network member is projected on a set of dummies for sex, qualification, year and location along with a quadratic in age and gender- and qualification-specific returns to experience. We then computed for each individual network the average unexplained wage and included it in our main regression along with the age-sex composition of the individual network. The average wage premium in the network should therefore capture past firm as well as co-workers unobserved characteristics that might affect the length of one's unemployment spell. For example, if larger firms pay a premium and a given displaced individual has worked often in large firms because of some individual characteristic that also makes him re-enter more rapidly when displaced, the average wage premium in the network would account for this correlation. Moreover, we include in the control set a range of measures of geographic distance among network members. This is meant to capture the attractiveness or choosiness of a given firm. We expect better firms to be more choosy when hiring for a given position. This, in turn, should imply that its employees proceed from a wider area than those of a worse, less choosy firm. For example, a top department of economics is able to attract professors from all over the world. Results are again unaffected. As expected, the average wage premium in the network is negatively correlated with unemployment duration. This may capture either past firm characteristics that affect one's unemployment duration (say, having worked for a good firm is a good referral) or, for example, the quality of one's contacts, a higher wage premium signaling better contacts, more efficient in the refereeing process. As to the other controls, the age composition of the network seems to be positively correlated with one's unemployment duration, while the gender composition does not seem to play a role.

As a final robustness check, we estimated the model allowing jointly for the above mentioned sets of controls. Results, reported in column (4) are again unchanged with respect to column (2). Accordingly, the implied cuts in unemployment duration associated to raising the number of contacts from the 25th to the 50th percentile of its distribution are also very similar (see table (2)). In what follows we will refer to (2) as to our preferred specification.

All in all, the results are robust to several quite exhaustive specifications of the information set that control for individual heterogeneity stemming from worker's productivity, labor market attachment, mobility costs, local features as well as for past firms' characteristics as measured by firms' wage premia, gender-age composition of the labor force and firms' choosiness and/or attractiveness.

In table (3) we look at the same baseline regression for specific sub-groups. We see that the effect of a 25-to-50 shift is larger for males and is basically the same for people younger or older than 30; there seems to be no network effect for white-collars. Finally, focusing on a sub-sample of male blue-collars the implied effect of an increase in network size is in excess of 10 percent (and higher than 30 percent for a 25th-75th experiment). For them, the average unemployment spell would be shortened by 19 days.

3.2 Measurement and an estimation for true networks size

The evidence presented so far shows that the measure of network extension is negatively and significantly correlated with unemployment duration, a result robust to a number of extensions. Still, the variable we exploit is affected by measurement error since it implicitly assumes that an individual who has worked in a large firm knows all of its employees or, at the very least, that they all can contribute to shorten her unemployment spell[10].

We now try to assess whether the data point to a reasonable value for the *true* size of the network[11].

[10]Our measure may partly captures the idea of extended or indirect networks. Theoretical work has shown how also the structure of a network plays a role (e.g. Calvo-Armengol and Jackson (2004)). In general, the implication is that one need not to know directly her network members to access the information they collect.

[11]Modern anthropological and psychological studies have investigated whether there exists an optimal group size for human beings. Based on evidence drawn from modern hunter-gatherer societies as well as other productive organizations such as autarkic societies or professional military corps they generally conclude that it exists and ranges around a hundred

We do that assuming that social contacts are maintained in a simple but reasonable way. Formally, we hypothesize that an individual cannot maintain a useful relationship with more than $\bar{S}$ individuals at the same time. Therefore the measure of network size we use in our regressions is $S^* = min\left\{Size, \bar{S}\right\}$ where $Size$ is our original measure. The estimated equation then becomes:

$$u_{ijt} = \alpha + \beta X_{ijt} + \gamma \log\left(min\left\{Size_{ijt}, \bar{S}\right\}\right) + \epsilon_{ijt} \tag{2}$$

where we jointly estimate β, γ and the threshold $\bar{S}$. The strategy is to estimate equation (2) over a large grid of possible values for $\bar{S} \in [5, 250]$ and select the set of parameters that minimizes the overall sum of squared residuals. The non-differentiability of the model prevents from a formal calculation of the relative standard errors. We therefore bootstrapped 300 random samples from our data stratifying the sample at the closing-firm level and ran the grid search to estimate a bound for each sample. Figure (3) shows the values of the criterion function at the corresponding value of $\bar{S}$ in the baseline and extended specifications of the set of controls (respectively, columns (2) and (6) in table (1)) along with the empirical distribution deriving from the bootstrap exercise. In both cases the sum of squared residuals is minimized at around $\bar{S} = 40$. The empirical distributions show that this is indeed the value that is more frequently estimated. The corresponding point estimate for the elasticity to network size is $\hat{\gamma}_{40} = -0.197 \quad (SE = 0.0339)$, implying that increasing an individual's network size from 25th of the underlying distribution to $\bar{S}$ (15 additional contacts) reduces her unemployment duration by nearly 9 percent, about one month less out of employment. In about 85 percent of the simulations for the baseline model and 70 percent for the extended one the estimated network bound falls in the range $[10, 70]$.

A way to qualify the above result is to break up the set of potential contacts we recover from our data in order to try to isolate those that are more likely to be true contacts. The data provide individual characteristics of network members and allow us to focus in particular on three subsets of the gross

individuals (see Dunbar (1992) for a detailed survey).

network measure. First, we look at contacts living in the same town as the displaced worker. These are more likely to be truly known since, beyond work, there are other opportunities to meet (children in the same school, theater, etc.). Second, we focus on contacts having the same qualification or of the same gender on the grounds that these are the ones most likely to bear relevant information about job opportunities. For example, an accountant is more likely to collect information about suitable job openings from another clerk than from a janitor. The same thing will happen if occupation is gender segregated. Our estimating equation becomes:

$$u_{ijt} = \alpha + \beta X_{ijt} + \gamma \log(S(x)) + \gamma_C \log(S^C(x)) + \epsilon_{ijt} \tag{3}$$

where $S(x)$ is the number of contacts that comply with condition x (say, live in the same town) and $S^C(x)$ is the complementary set.

To gauge the effects of a plain recomposition of the network in favor of the core group holding network size constant one cannot simply compare the point estimates of the two elasticities: a recomposition of the network towards a specific subgroup may imply different percentage changes in each of the two subsets. Therefore a simple comparison of the two estimated elasticities, which implicitly amounts to assessing the net effect of two percentage variations equal in absolute value but opposite in sign, would also embed a change in network size. Letting $d = \frac{\Delta S(x)}{S(x)}$, the appropriate exercise consists in evaluating the quantity $\Delta u = d\left(\gamma - \gamma_C \frac{S(x)}{S^C(x)}\right)$ where it is clear that, unless $S(x) \approx S^C(x)$, comparing the point estimates may be highly misleading.

Table (4) summarizes the estimated coefficients γ and γ_C along with the effect (in percentage terms) on unemployment duration of a re-composition of the network towards the core group holding size constant[12]. In the first row, for example, we considered a shift from the 25th to the 50th percentile of the distribution of contacts living in the same municipality (calculated in correspondence of the median network size in the sample) and a corresponding reduction in the number of those living outside the

[12]Results are based on the elasticities estimated using the most exhaustive specification of the control set.

town. The implied effect of such re-shuffling (involving 7 contacts) is about 2.5 percent amounting, in correspondence to the length of the average unemployment spells, to a reduction of duration by nearly 7 days. Results in the second and third rows suggest that while redistributing the network towards contacts of the same qualification is not effective in reducing unemployment, increasing the number of contacts of the same gender as i is more effective, shortening the experienced unemployment spell by about 11 days.

4 Unemployment duration and network employment rate

A further implication of theoretical works on the role of social networks concerns the effects of one's contacts labor market status. Theory suggests at least two reasons why having more employed contacts shortens one's unemployment spell. On the one hand, employed contacts pass on more valuable information to unemployed ones since they turn down any job opportunity that pays a wage below the one currently earned. On the other hand, a higher employment rate (holding network size constant) implies a lower number of unemployed contacts so that there is less competition for this information. From an empirical point of view, however, such a correlation might reflect forces other than a genuine causal effect. For example, an individual and her contacts might be more likely to be employed simply because during the NB period they worked in a good firm and, as we mentioned above, this constitutes a positive signal to potential employers. As in the previous section, we control for this and similar issues using detailed information on the composition of each individual network and its average wage premium along with closing firm fixed effects and information on individual mobility choices and location.

Our estimating equation becomes:

$$u_{ijt} = \alpha + \beta X_{ijt} + \gamma \log(Size_{ijt}) + \delta \frac{E_{ijt}}{Size_{ijt}} + \epsilon_{ijt} \tag{4}$$

where E_{ijt} is the number of previously established contacts who are employed at the time i is displaced and δ captures the effect of a better network, that is one with a higher employment rate.

Table (5) reports results for the several specifications of the control set used in table (1). The estimated coefficients on the employment rate are extremely stable across specifications, always statistically very significant and imply sizable reductions in the length of an unemployment spell. Results reported in table (6) show that increasing the employment rate from the 25th percentile to the median reduces unemployment duration by between 8.2 and 9.5 percent. In terms of time, this means exiting from unemployment between 25 and 29 days earlier. As before, all additional controls are statistically significant. In particular, a higher network quality, as measured by the average wage premium, is associated with a higher probability of leaving unemployment. Note also that, consistently with the theoretical prediction that the number of contacts and their employment rate are positively correlated (Calvo-Armengol and Jackson (2002)), the estimated elasticities to network size turn out to be lower; the implied effects of increasing network extension are reduced by about 1 percentage point.

Another potential source of variability in the quality or quantity of information available in a given network is the job-mobility of one's contacts. For example, if some contacts have recently successfully changed job they may convey more useful information than contacts who still stick to their previous occupation: they have probably surveyed several opportunities, they have gathered information and so on. Once they find a job this information is no longer valuable to them and can be spread in the network. On the other hand, network members who meanwhile did not change job are likely to have less information on potential opportunities, either because they are not looking for a job or because they have not yet completed their quest for a new occupation To verify this hypothesis we estimate the following model:

$$u_{ijt} = \alpha + \beta X_{ijt} + \gamma \log(Size_{ijt}) + \delta \frac{E_{ijt}}{Size_{ijt}} + \delta_M \frac{M_{ijt}}{Size_{ijt}} + \epsilon_{ijt} \quad (5)$$

where, as before, E_{ijt} is the total number of employed contacts at the time of displacement and

$M_{ijt} \subseteq E_{ijt}$ is the subset of employed contacts who, by the time i is displaced, have successfully changed job since they first met i. Table (7) reports the results for the various specifications of the control set. Consistently with our story, successful job switchers are more effective in reducing one's unemployment duration: a one percentage point increase in the employment rate of the network, holding size constant, has about twice the effect on unemployment duration if it is due to network members that changed job as compared to the case where it is network members who did not leave their previous job. For the specification of column (2) shifting the employment rate from the 25th to the 50th percentile for a displaced worker with median network size would reduce unemployment duration by about 26 days in one case and by about 50 in the other.

5 Wages and network characteristics

Theory has extensively explored the consequences of the existence and use of social networks for wages and their dispersion. However, the theoretical predictions concerning wages appear to be somewhat ambiguous. On the one hand, a larger network produces more information so that a given unemployed individual may select the best offer out of a larger pool (Arrow and Borzekowski (2004), Calvo-Armengol and Jackson (2004)). Moreover, being endowed with a larger network increases the bargaining power of the employee when sharing the match rents with the employer (Fontaine (2004)). Alternatively, the screening role played by one's contacts leads to higher wages because of the better signal to the employer (Montgomery (1991)). On the other hand, if a larger network gives access to more jobs, but these are unsuited to a given individual, he may still decide to accept them trading off a lower wage with a shorter unemployment spell (Bentolila et al. (2004)).

Here we explore the sign of the relationship between network characteristics and entry wages of displaced individuals regressing the (log of) the entry weekly wage on the same control sets discussed in the previous sections. Results are reported in table (9). As shown in the first column network size appears to be positively correlated with entry wages. However, the relationship becomes insignificant as soon as we move to the next column where we add basic controls for individual heterogeneity (log of weekly wage at displacement and labor market participation during the NB period); the result is unchanged in column (a) where we use the extended set of controls. In columns (b) and (c) we include our measures of the network employment rate and of the job switching rate along with the extended control set. While the extension and the employment rate of the network do not turn out to be correlated with entry wages, the share of contacts who switched job since they where first met turns out to positively affect them in a statistically significant way.

All in all, we conclude that while we found quite convincing evidence that a larger and better pool of social contacts helps reducing unemployment duration, it does not seem to play a relevant role in determining the wage received upon re-employment.

6 Conclusions

Understanding the workings of the labor market contributes to shed light on several distributional and efficiency issues as well as to design policies.

In this paper we have investigated whether and how the network of social contacts one establishes on the job helps him in exiting unemployment and whether it affects his re-employment wage. Differently from previous studies, we focus on measures of social network defined at the individual level, which we recover from a comprehensive Italian matched employer-employee dataset for a sample of about 13,000 exogenously displaced individuals.

Consistently with theoretical predictions, our findings show that the number of contacts one has access to when unemployed and their current labor market status contribute in a statistically significant way to shorten his unemployment spell. In our sample, an increase of the number of contacts from the 25th percentile to the median shortens the average unemployment spell (about 10 months) by 10-15 days. A similar experiment conducted on the number of employed contacts, implying an increase of the employment rate of about 7.5 percentage points, reduces unemployment duration by between 25 and 30 days; we also show that contacts who recently changed job contribute significantly more than contacts who did not to shortening one's unemployment spell, the semi-elasticity of unemployment duration to the share of job switchers being almost twice as large. We do not find much evidence of an effect of the number of contacts or of their current labor market status on wages; this might reflect the fact that also theoretical predictions are ambiguous in this respect, since different and opposite effects may be at play. We extensively draw on our data to argue that these results are unlikely to be driven by omitted variables or other sources of individual heterogeneity. We perform several experiments to show that the set of potential contacts we identify is a reasonable proxy of the social ties one establishes on the workplace.

However, several questions remain open. First, one would like to exploit more comprehensive information concerning the recruiting process of firms to be able to disentangle the information sharing hypothesis from the referral one. Second, we have focused on on-the-job contacts. However, other social connections unrelated to the workplace (friends, relatives, etc.) might play a role.

Data appendix

We combine three National Security Service (INPS) archives providing, for any work episode occurred over the period 1975-1997 in two Italian provinces, information on the worker, the firm and the characteristics of the match, respectively; movers are tracked so that the same information is available even if they worked in other areas of the country over the same period. The workers file contains data on more than one million individuals, including gender, date of birth, place (or foreign country) of birth, place of residence and an identifier for the firm they are employed in. The information is anonymous and workers are identified by a progressive code. No information on education, marital status or family size is available. The contributive file contains information on the worker-firm match, including qualification (available with a breakdown in apprentices, blue-collar workers, white-collar workers and managerial workers) and type of contract (whether full or part-time). Gross nominal wages are recorded with a breakdown in periodic current earnings (*competenze correnti*) and other non-periodic payments (*altre competenze*) and include overtime payments. The wage information always relates to a single firm, and never spans more than one calendar year: it reports the total pay received for the year or fraction of year worked, together with the number of months/weeks paid. If the worker changes jobs, a new record is opened including the total pay for the period from the start of the new spell to the end of the calendar year (or the date of termination of employment at the new firm, whichever is more recent). The third archives (firms file) contains information concerning location (at municipality level), the industry affiliation with a three-digit level breakdown and the average employment size of all firms existing in the two provinces from 1975-1997, whether they are still trading or not. The exact dates at which they started and ceased trading (if occurred within the period spanned) are also provided.

Construction of the dataset

The period covered by our data contains about 20,000 firm closures. Most involve very small businesses: more than 50 percent of closing firms averaged only 1 or 2 workers in the 12 months previous to closure. In what follows we will focus on 2,136 firms that averaged more than 10 employees in the 12 months previous to closure (representing the upper 11 percent of the size distribution of the closing firms). At the date of closure these firms employed 15,683 workers, only 40 percent of the number of employees observed in the firms in the previous 12 months. In particular while the pattern of exit in the year preceding that date is quite regular (QUALIFY "REGULAR": with respect to what? Other firms that do not close?) they peak up in the last three months. Hence we decided to consider as *displaced* all workers who are observed in each firm up to 90 days previous to the date of closure. This rule is likely to leave in the sample only workers worse than average, that is those who either did not realize they were going to lose the job or who did not find any other job. Yet, since the cross-sectional nature of our data will not allow us to explicitly control for individual unobserved heterogeneity we believe this selection rule constitutes a first step in the direction of working with a relatively homogeneous sample of observations. This reduces the number of workers to 22,838. Moreover we only included displaced workers who completed a full NB period (five years) and only considered closures occurred over the period 1980-94 to limit right censoring of unemployment spells. Accordingly, we will restrict to 1,766 closures and 19,021 observations. The number firms in our working-sample is further reduced by dropping 209 *spurious* cases of establishments appearing as closing but whose workers are shortly after observed in a new firm whose name is just a re-labeling of the previous one. This reduces the number of workers by 2,754. Also, nearly 1,000 workers are dropped by the following reasons. First, we want to be sure our measure of unemployment spell is correct; by merging our data with the INPS archives of the self-employed we identified and dropped those workers (261) who moved to self employment during the period we attributed to unemployment. Moreover we dropped (178) workers who are observed having unreasonably long unemployment spell (i.e. larger that 100 months), suggesting they dropped out the workforce. Second, we excluded (92) workers experiencing multiple closures over the period spanned by our data, and limited our analysis to full time blue and white collar, dropping apprentices. Finally we had to drop workers employed in firms we could not localize geographically. The final sample amounts to 13,141 workers displaced by 1,372 firms.

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Figure 1: Distribution of unemployment spells.

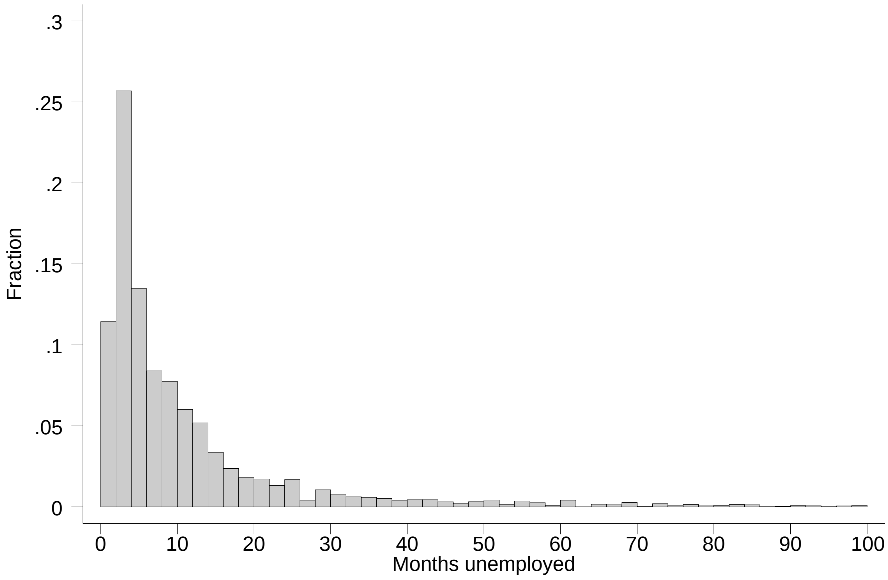

Figure 2: Cumulative distribution function of network size.

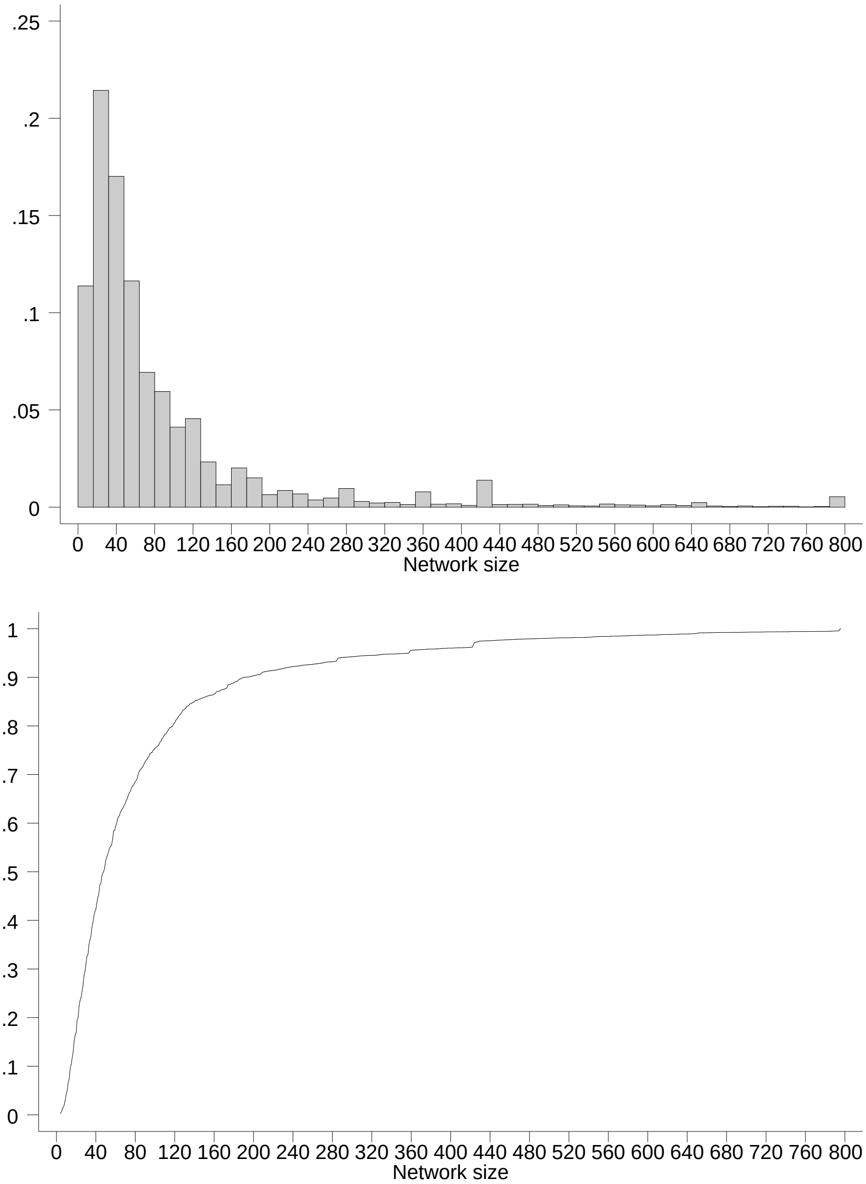

Figure 3: Network upper bounds and bootstrapped densites.

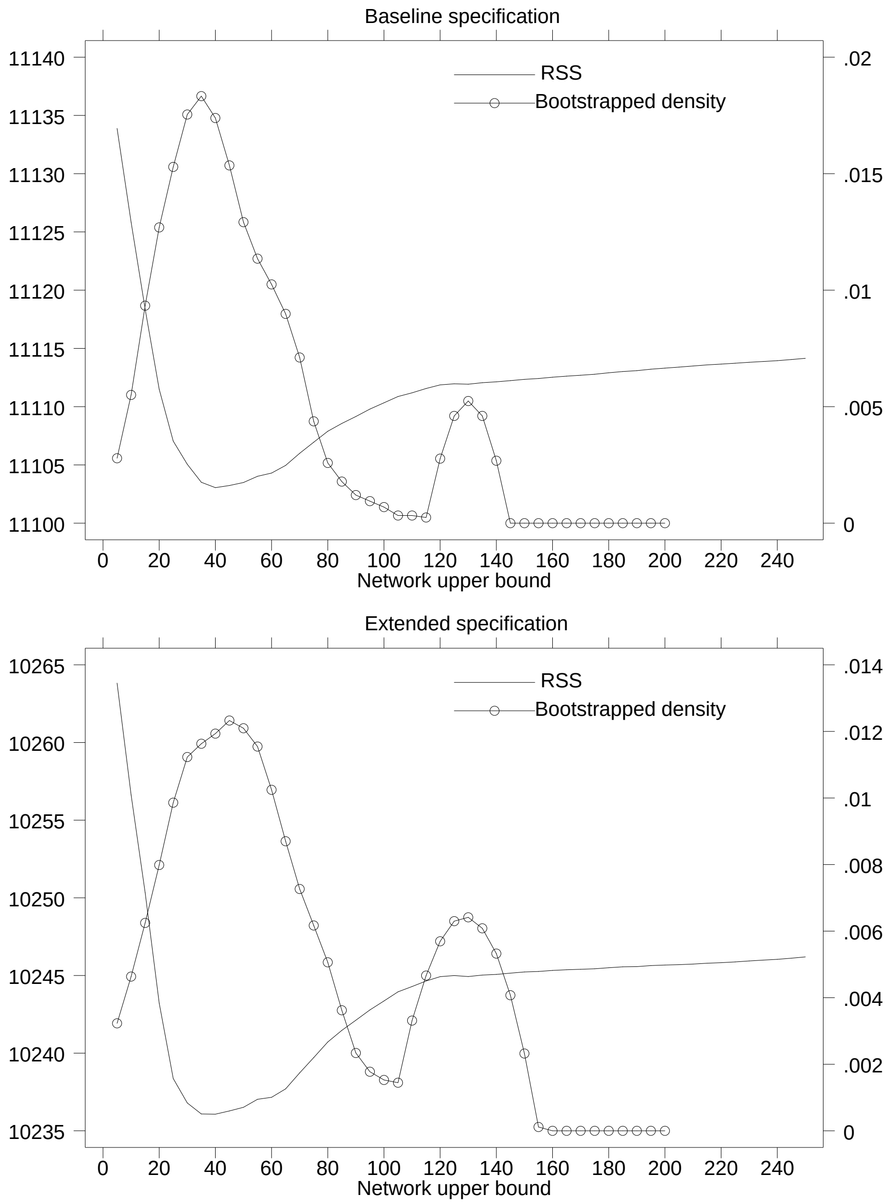

Table 1: Network size and unemployment duration.

	Baseline	Basic Controls	Mobility Controls	Past Firms Controls	Extended Controls
Female	0.3661	0.3577	0.3437	0.3790	0.3630
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age	0.0196	0.0292	0.0274	0.0283	0.0269
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age^2	-0.0002	-0.0003	-0.0003	-0.0003	-0.0003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure	-0.0008	0.0001	0.0000	0.0001	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure^2	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
White-collar	-0.1047	-0.0676	-0.0815	-0.0637	-0.0745
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Manager	0.1133	0.3158	0.2633	0.3369	0.2944
	(0.033)	(0.034)	(0.038)	(0.034)	(0.038)
Network size	-0.1111	-0.0684	-0.0723	-0.0703	-0.0720
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Wage at displ.		-0.1617	-0.1548	-0.1592	-0.1523
		(0.001)	(0.001)	(0.001)	(0.001)
Past Unemployment		0.3771	0.3726	0.3669	0.3657
		(0.003)	(0.003)	(0.003)	(0.004)
Distance:					
-from past firms (mean)			-0.2796		-0.2908
			(0.031)		(0.031)
-from past firms (mean)^2			0.1523		0.1558
			(0.029)		(0.029)
-among contacts (mean)				0.0164	0.1026
				(0.031)	(0.034)
-among contacts (median)				-0.1307	-0.2956
				(0.066)	(0.073)
Average wage premium				-0.5028	-0.4763
				(0.010)	(0.011)
Share of:					
-young females				-0.9735	-1.0348
				(0.411)	(0.124)
-young males				-1.1432	-1.1968
				(0.415)	(0.124)
-middle females				-1.1356	-1.2343
				(0.422)	(0.156)
-middle males				-0.8642	-0.9888
				(0.457)	(0.137)
-old females				0.0723	0.0000
				(0.550)	(0.000)
-old males				0.0000	0.2401
				(0.000)	(0.592)

Dependent variable: log of months spent out of employment. Obs: 13,195; firms: 1,367.

Table 2: Network size: implied percentage change in unemployment duration.

	Baseline	Basic Controls	Mobility Controls	Past Firms Controls	Extended Controls
Elasticity	-0.1111	-0.0684	-0.0723	-0.0703	-0.0720
25-50	-6.8	-4.2	-4.5	-4.4	-4.5
25-75	-13.8	-8.7	-9.2	-9.0	-9.2

25-50 corresponds to an increase of network size of 92% (23 additional contacts); 25-75 corresponds to an increase of network size of 292% (73 additional contacts).

Table 3: Unemployment duration and network extension in specific subsamples

	Subsample of:						
	Males	Females	Young	Old	Blue collar	White collar	Male-BC
Network size	-0.086 (0.023)	-0.066 (0.023)	-0.067 (0.029)	-0.069 (0.029)	-0.076 (0.017)	0.033 (0.057)	-0.105 (0.025)
25-50	-5.6	-4.2	-4.2	-4.3	-4.5	2.3	- 7.2
25-75	-11.2	-8.2	-8.9	-8.8	-9.7	4.7	-13.8

Dependent variable: log of months spent out of employment.

Table 4: Network composition

	γ	γ_C	25-50	25-75
Residence	-0.0229 (0.008)	-0.0526 (0.014)	-2.5	-2.6
Gender	-0.0431 (0.013)	-0.0218 (0.013)	-3.6	-5.6
Qualification	-0.03725 (0.017)	-0.03239 (0.014)	1.3	4.6

Dependent variable: log of months spent out of employment. Obs: 13,195; firms: 1,367.

Table 5: Network extension, employed contacts and unemployment duration.

	Baseline	Basic Controls	Mobility Controls	Past Firms Controls	Extended Controls
Female	0.3467	0.3369	0.3247	0.3553	0.3412
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age	0.0165	0.0278	0.0264	0.0274	0.0264
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age^2	-0.0002	-0.0003	-0.0003	-0.0003	-0.0003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure	-0.0013	-0.0003	-0.0003	-0.0003	-0.0003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure^2	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
White-collar	-0.1014	-0.0668	-0.0812	-0.0649	-0.0771
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Manager	0.1193	0.3199	0.2753	0.3367	0.2973
	(0.033)	(0.034)	(0.038)	(0.034)	(0.038)
Network size	-0.1046	-0.0487	-0.0514	-0.0530	-0.0537
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Employment rate	-0.0115	-0.0134	-0.0129	-0.0122	-0.0116
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Wage at displ.		-0.1603	-0.1546	-0.1592	-0.1533
		(0.001)	(0.001)	(0.001)	(0.001)
Past Unemployment		0.4899	0.4841	0.4725	0.4699
		(0.003)	(0.003)	(0.003)	(0.004)
Distance:					
-from past firms (mean)			-0.2813		-0.2864
			(0.030)		(0.030)
-from past firms (mean)^2			0.1372		0.1409
			(0.028)		(0.028)
-among contacts (mean)				-0.0301	0.0562
				(0.031)	(0.034)
-among contacts (median)				-0.0695	-0.2268
				(0.065)	(0.072)
Average wage premium				-0.2848	-0.2620
				(0.011)	(0.012)
Share of:					
-young females				-0.7528	-0.6473
				(0.408)	(0.125)
-young males				-0.9088	-0.7977
				(0.412)	(0.125)
-middle females				-0.8208	-0.7570
				(0.419)	(0.158)
-middle males				-0.7836	-0.7259
				(0.453)	(0.136)
-old females				-0.0798	0.0000
				(0.545)	(0.000)
-old males				0.0000	0.3542
				(0.000)	(0.588)

Dependent variable: log of months spent out of employment. Obs: 13,195; firms: 1,367.

Table 6: Employed vs. unemployed: implied percentage change in unemployment duration.

	Baseline	Basic Controls	Mobility Controls	Past Firms Controls	Extended Controls
			Network extension		
Elasticity	-0.1046	-0.0487	-0.0514	-0.0530	-0.0537
25-50	-6.4	-3.0	-3.2	-3.3	-3.3
25-75	-13.1	-6.3	-6.6	-6.8	-6.9
			Employment rate		
Semi-elasticity	-0.0115	-0.0134	-0.0129	-0.0122	-0.0116
25-50	-8.3	-9.5	-9.2	-8.7	-8.4
25-75	-16.5	-18.9	-18.2	-17.4	-16.6

Network size: 25-50 corresponds to an increase of network size of 92% (23 additional contacts); 25-75 corresponds to an increase of network size of 292% (73 additional contacts). Employment rate: the implied effect is computed shifting the employment rate from the 25th to the 50th (or 75th) percentile of its distribution conditional on network size having the median size observed in the sample. 25-50 corresponds to an increase of the employment rate of 7.5 percentage points; 25-75 corresponds to an increase of the employment rate of 15.6 percentage points.

Table 7: Network size, employed contacts and job switchers.

	Baseline	Basic Controls	Mobility Controls	Past Firms Controls	Extended Controls
Female	0.3497 (0.025)	0.3388 (0.025)	0.3286 (0.026)	0.3607 (0.026)	0.3484 (0.027)
Age	0.0169 (0.007)	0.0262 (0.007)	0.0241 (0.007)	0.0274 (0.007)	0.0258 (0.007)
Age^2	-0.0002 (0.000)	-0.0003 (0.000)	-0.0003 (0.000)	-0.0003 (0.000)	-0.0003 (0.000)
Tenure	-0.0008 (0.000)	0.0000 (0.000)	0.0000 (0.000)	0.0000 (0.000)	0.0000 (0.000)
Tenure^2	0.0000 (0.000)	0.0000 (0.000)	0.0000 (0.000)	0.0000 (0.000)	0.0000 (0.000)
White-collar	-0.1122 (0.031)	-0.0743 (0.032)	-0.0857 (0.033)	-0.0733 (0.032)	-0.0832 (0.033)
Manager	0.0900 (0.180)	0.2947 (0.182)	0.2618 (0.193)	0.3025 (0.182)	0.2701 (0.193)
Network size	-0.1361 (0.015)	-0.0910 (0.016)	-0.0958 (0.017)	-0.0876 (0.017)	-0.0893 (0.018)
Employmnent rate	-0.0108 (0.001)	-0.0123 (0.001)	-0.0117 (0.001)	-0.0120 (0.001)	-0.0115 (0.001)
Job switching rate	-0.0121 (0.001)	-0.0115 (0.001)	-0.0116 (0.001)	-0.0117 (0.001)	-0.0120 (0.001)
Wage at displ.		-0.1617 (0.027)	-0.1555 (0.029)	-0.1608 (0.027)	-0.1544 (0.029)
Past Unemployment		0.3793 (0.056)	0.3621 (0.059)	0.3947 (0.057)	0.3835 (0.060)
Distance					
-from past firms (mean)			-0.2962 (0.172)		-0.2954 (0.173)
-from past firms (mean)^2			0.1256 (0.167)		0.1253 (0.167)
-among contacts (mean)				0.0616 (0.174)	0.1489 (0.182)
-among contacts (median)				-0.1607 (0.253)	-0.3093 (0.266)
Average wage premium				-0.2107 (0.103)	-0.1847 (0.107)
Share of:					
-young females				0.1929 (0.636)	0.3759 (0.356)
-young males				-0.0785 (0.638)	0.1143 (0.355)
-middle females				-0.0109 (0.643)	0.1280 (0.397)
-middle males				-0.1788 (0.668)	-0.0516 (0.368)
-old females				-0.1186 (0.732)	
-old males					0.3463 (0.759)

Dependent variable: log of months spent out of employment. Obs: 13,195; firms: 1,367.

Table 8: Stayers vs. job switchers: implied percentage change in unemployment duration.

	Baseline	Basic Controls	Mobility Controls	Past Firms Controls	Extended Controls
			Network extension		
Elasticity	-0.1361	-0.0910	-0.0958	-0.0876	-0.0893
25-50	-8.3	-5.6	-5.9	-5.4	-5.5
25-75	-16.6	-11.5	-12.0	-11.1	-11.3
			Employment rate		
Semi-elasticity	-0.0108	-0.0123	-0.0117	-0.0120	-0.0115
25-50	-7.8	-8.8	-8.4	-8.6	-8.2
25-75	-15.6	-17.5	-16.7	-17.2	-16.4
			Job switching rate		
Semi-elasticity	-0.0121	-0.0115	-0.0116	-0.0117	-0.0120
25-50	-15.8	-16.4	-16.1	-16.3	-16.1
25-75	-30.2	-31.1	-30.6	-31.0	-30.7

Network size: 25-50 corresponds to an increase of network size of 92% (23 additional contacts); 25-75 corresponds to an increase of network size of 292% (73 additional contacts). Employment rate: the implied effect is computed shifting the employment rate from the 25th to the 50th (or 75th) percentile of its distribution conditional on network size having the median size observed in the sample. 25-50 corresponds to an increase of the employment rate of 7.5 percentage points; 25-75 corresponds to an increase of the employment rate of 15.6 percentage points. Job switching rate: the implied effect is computed assuming that the 25-50 (25.75) increase in overall employment rate is due to contacts who changed job meanwhile.

Table 9: Network size, employment and job switching rates and entry wages.

	Baseline	Basic Controls	Extended Controls (a)	(b)	(c)
Female	-0.2282	-0.2193	-0.2286	-0.2278	-0.2279
	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
Age	0.0025	-0.0010	-0.0009	-0.0009	-0.0007
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Age^2	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure	0.0001	-0.0002	-0.0002	-0.0002	-0.0002
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure^2	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
White-collar	0.2054	0.1766	0.1765	0.1766	0.1771
	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)
Manager	0.9428	0.8011	0.7448	0.7447	0.7479
	(0.067)	(0.067)	(0.071)	(0.071)	(0.071)
Network size	0.0116	-0.0023	0.0005	-0.0002	0.0039
	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)
Employment rate				0.0004	0.0005
				(0.000)	(0.000)
Job switching rate					0.0015
					(0.000)
Wage at displ.		0.1124	0.1064	0.1064	0.1066
		(0.010)	(0.011)	(0.011)	(0.011)
Past Unemployment		-0.1160	-0.1221	-0.1261	-0.1139
		(0.020)	(0.022)	(0.022)	(0.022)
Distance:					
-from past firm (mean)			0.0980	0.0979	0.1008
			(0.063)	(0.063)	(0.063)
-from past firm (mean)^2			0.0823	0.0829	0.0841
			(0.061)	(0.061)	(0.061)
-among contacts (mean)			-0.0765	-0.0747	-0.0896
			(0.067)	(0.067)	(0.067)
-among contacts (median)			0.1352	0.1326	0.1461
			(0.098)	(0.098)	(0.098)
Average wage premium			0.2932	0.2851	0.2695
			(0.038)	(0.039)	(0.039)
Share of:					
-young female			0.5226	0.5080	0.3797
			(0.127)	(0.128)	(0.130)
-young male			0.4791	0.4640	0.3486
			(0.127)	(0.129)	(0.130)
-middle female			0.5028	0.4847	0.3750
			(0.143)	(0.144)	(0.146)
-middle male			0.4291	0.4191	0.3392
			(0.134)	(0.134)	(0.135)
-old female					
-old male			0.2406	0.2363	0.2288
			(0.278)	(0.278)	(0.278)

Dependent variable: log of weekly entry wage. Obs: 13,195; firms: 1,367.