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Tommaso Colussi
Livio Romano

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IZA

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ABSTRACT

Is There a Preferential Treatment for Locals in the Labor Market? Evidence from Takeovers*

This work analyzes the extent to which local social networks affect workers' labor market outcomes and firms' economic performance. By exploiting variations in firms' ownership generated by takeovers, we find that belonging to the same community of origin as the new employer significantly increases an employee's job retention probability. Finally, we show that the share of local employees retained after the takeover is negatively associated with the probability of closure of the acquiring firm.

JEL Classification: D22, J24, J63, J7
Keywords: social capital, local social networks, takeovers, firm behavior

Corresponding author:
Tommaso Colussi
IZA
Schaumburg-Lippe-Str. 5-9
53113 Bonn
Germany
E-mail: colussi@iza.org

1 Introduction

Social connections at work largely influence workers' and firms' behavior (Ioannides and Datcher Loury 2004). In the labor economics literature several studies have focused on the role played by co-worker interactions, showing that they generate spillover effects on workers' productivity (Bandiera et al. 2005; Mas and Moretti 2009).[1] Another strand of the literature has analyzed social relations across different layers of the firm hierarchy, such as between managers and their subordinates. Bandiera et al. (2009) provide evidence that managers tend to favor socially connected workers independently of their skills: the overall effect of managers' favoritism on firms' economic performance is negative. Giuliano et al. (2011) instead focus on the role of race and ethnicity in shaping manager-employee relationships; racial matches between managers and their subordinates increase subordinates' chances of promotion and reduces their quits and dismissals.

This paper extends the literature on the superior-subordinate relationships by focusing on the the role of *local social networks* in the labor market. We define an employer and an employee to belong to the same social network if they come from the same municipality.[2] We then investigate whether employers show a preferential treatment for workers of their network by looking at their employment outcomes, ultimately analyzing how this behavior affects firms' economic performance.

To this end, we employ a matched employer-employee dataset coming from the social security records of the Italian region of Veneto. Information on the city

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[1] Empirical studies have tested that social pressure, arising when employees work alongside individuals with whom they frequently interact, can partially internalize free-riding externalities built in teamwork environments (Mas and Moretti 2009). Similarly, Bandiera et al. (2005) show that monitoring of friends working in the same team, helps workers internalizing the negative externality their effort imposes on teammates when the pay scheme switches from individual to relative incentives.

[2] The use of the municipality as a proxy for the community of origin is consistent with Wellman (1996), which characterizes a community network as having "*a densely knit core of confidants, many of whom are immediate kin. This core is surrounded by more sparsely knit ties with other kin, friends, neighbors and coworkers*" (p.348). Several studies have also found that people tend to interact mainly with individuals sharing similar characteristics (Marmaros and Sacerdote 2006; Bandiera et al. 2008)

of origin of workers and firms, along with their working histories, allows us to observe whether the two parties involved in the employment relationship share the same community of origin. In particular, a worker is considered *local* (i.e *matched* with the current employer) whenever his/her city of birth is the same as the one in which the firm has its headquarters. We further define as *localism* the practice under which employers favor workers from their community of origin (the inside group) over those coming from other communities (the outside group)

Evidence for localism is recovered by looking at the effect of workers matching with the current employer on their employment probabilities. To deal with potential endogeneity issues arising from non-random sorting of workers into firms, we exploit a quasi-experimental setting in which firms' takeovers generate a change in ownership; whenever the acquiring firm comes from a different city than the acquired firm, a change in the matching between the origin of the incumbent workers and that of the employer is observed.

The identification strategy relies on the assumption that, within the acquired firm, the probability of a worker being matched with the new employer is as good as random, conditional on being unmatched with the previous employer (i.e. the acquired firm). Within-acquired firm comparisons of ex-ante unmatched employees allow us to control for acquisitions and worker heterogeneity respectively, and thus to recover a consistent estimate of localism. More specifically, we estimate the extent to which being matched with the new employer affects workers' labor market outcomes, and in particular, the probability of staying employed in the acquired firm after the takeover.

Regression results show a positive and significant effect from employee-employer connections: being from the same city of origin as the new employer increases the probability of preserving the job by about 2.7 percentage points, which accounts for about 4% of the average probability of staying at the firm. To control for the possibility that takeover decisions are affected by the presence of ex-post matched workers in the target firm, we check whether the probability of an acquisition is correlated with the existence of potentially connected workers in the firm. Reassuringly, our results reject this hypothesis.

We also check whether the average effect varies with both workers' and firms' characteristics. We find that the size of the community negatively affects the intensity of localism. Moreover, the effect is relevant only for relatively large ac-

quired firms, it holds for all occupational levels, but it is particularly strong when managers are involved; this last category of workers also experience a pay rise of about 7.7 percentage points with respect than comparable non-local workers. No significant differences instead are observed for blue and white collars, a result consistent with the idea that collective agreements are more binding for these two classes of workers.

One plausible interpretation of these results has to do with the imperfect information faced by the employer when making hiring and firing decisions: local social networks, i.e. the strong and weak ties among the individuals of the same local community (Granovetter 1983), ease the dissemination of information and reduce the noise around worker' skills and reliability. In order to check whether this explanation is plausible we run a test in a similar vein to Weber and Zulehner (2014), estimating whether the increase in the relative share of locals, as a result of the acquisition, is correlated with the probability of closure for the acquiring firm. If localism acts a screening device reducing information uncertainties about worker's characteristics (Montgomery 1991), we should observe that the survival probability does not decrease and may even increase as a result of this preferential treatment for locals. Consistently with our hypothesis, we find that a higher share of local employees retained after a takeover implies a lower probability of closure for the firm.

This paper contributes to the literature on social connections in several ways. First, it provides first empirical evidence on the role of employer-employee connections based on the community of origin on employees' outcomes. Second, it investigates the heterogeneity of the local social network effect across different firms' and workers' characteristics. Finally, it estimates whether *localism* has positive or detrimental consequences on firms' performance.

The remainder of the paper is organized as follows: Section 2 discusses the empirical setting and the identification; the data are presented in Section 3. Section 4 tests the existence of a preferential treatment for locals, showing its effect for both workers and firms and discussing the results. Finally, Section 5 concludes.

2 Empirical strategy

2.1 Setting

To test if a bias for employees based on their community of origin exists in the labor market, we look at whether workers' employment outcomes change when they are exposed to an exogenous shock in the matching between their city of origin and that of their employer. Specifically, we define workers as being matched (local) if their city of birth is the same as the city in which the employer has its headquarters, and un-matched (non-local) otherwise.[3]

The main empirical challenge is therefore to find exogenous variation in the matching status of workers such that, whenever a difference in employment status across workers is observed, it can be directly interpreted as the causal effect of the change in the connection status. We thus exploit changes in firms' ownership induced by an acquisition, as this can modify the matching between incumbent workers and the new employer. Figure 1 shows that, when an acquisition takes place, four distinct groups of workers can be identified, differing not only in their ex-post matching with the acquiring firm's origin but also in their ex-ante matching with the origin of the acquired firm.

Figure 1: Types of incumbent workers affected by firm acquisition

		Matched After	
		Yes	No
Matched Before	Yes	**MM**	**MU**
	No	**UM**	**UU**

Workers belonging to groups **MM** and **MU** are both characterized by being matched with the municipality of the acquired firm, but they differ with respect to the acquiring firm; the first group is still connected after the acquisition, while

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[3]In this paper (un-)matched and (non-)local workers are used interchangeably and they are synonym for workers (non-)connected with the employer.

the latter is not. On the other hand, workers belonging to groups **UM** and **UU** are both characterized by being unmatched with the municipality of the acquired firm but, while the first group becomes matched after the acquisition, the latter remains non-connected.

In order to identify the effect of a connection with the employer on the employment status of workers, we could, in principle, exploit the loss of the connection status for those workers previously matched, i.e. by comparing groups **MM** and **MU**. Alternatively, we could also compare the change in the matching for those workers who were previously non-connected, i.e. groups **UM** and **UU**; the latter strategy has the advantage to allow us comparing outcomes of workers within the same acquired firm, which would be unfeasible with groups **MM** and **MU**.[4]

For the reasons discussed above, our empirical analysis is centered around workers who were unmatched prior to the acquisition. Our treatment and control groups are, respectively, those who were matched and unmatched after the acquisition. Figure 2 illustrates the experimental design we will exploit through-

Figure 2: Acquisition and matching status
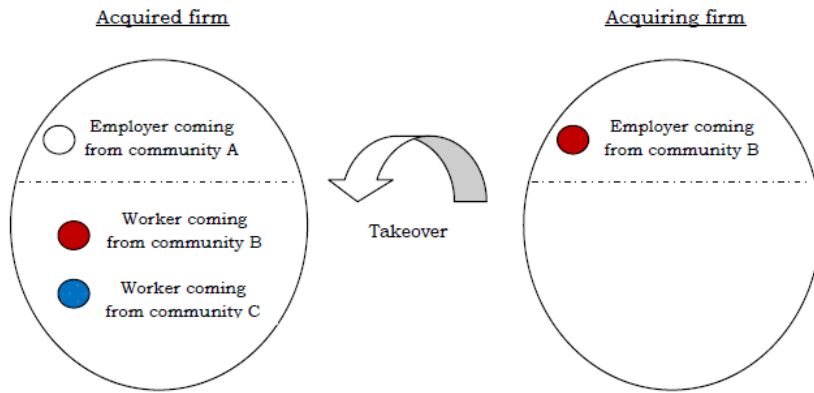
out the analysis. In particular, imagine a new employer belonging to community B, choosing between two potential incumbent workers, identical in all respects except for the fact that one belongs to the employer's community B, and the other to community C, where B and C are both different from the community of the former employer. The question we then try to address is whether there is a systematic higher probability for the worker belonging to community B of

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[4]Within the same acquired firm, either all ex-ante matched workers are still matched after the acquisition, or they are all unmatched after the event.

retaining the job than for the worker coming from C. If this is the case, then it can be argued that a preferential treatment for "locals" in the labor market exists.

2.2 Identification

When comparing the employment status after the acquisition, we are not only capturing the effect of the decision undertaken by the employer, but also the optimal reaction of the workers to the takeover. It is reasonable to assume that workers, when deciding whether to leave their current job, are internalizing their expectations about their future career. Nevertheless, for the purpose of our study, disentangling demand and supply-side effects is not essential as, in both cases, an observed difference between the treatment and control group will signal that a change in matching between a worker and a firm has implications for the worker's opportunities in the firm.

However, the distinction between demand and supply of labor still plays a role in our analysis when we come to choose the time window to be used prior to the acquisition. Since it takes time for the announcement of an acquisition to be put into practice, by restricting our attention only to those individuals still working in the firm at the time of the acquisition, we may totally disregard the supply-side effect of the announcement. It may be that some workers decide to leave their job and find another one as a result of the announcement; if this is the case, when the acquisition is realized, only a fraction of the initial incumbent workers are observed. Moreover, if the choice to voluntarily quit is systematically related to the matching status of future workers, then we could no longer consistently estimate the effect of localism on the probability of a worker retaining his job, as there would be a problem of self-selection. Figure 3 represents the time window of our analysis.

Figure 3: Time window considered in the analysis

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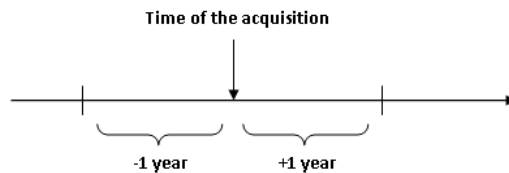
To circumvent this potential selection issue, we choose to include in our analysis all individuals that were employed in the acquired firms one year before the acquisition, and then look at their employment status one year after the acquisition takes place. In practice we estimate the following linear probability model:

$$[Employment \quad Status_{ijb}|C_i \neq C_j] = \beta_0 + \beta_1 Matched_{ijb} + f_j + \epsilon_{ijb} \qquad (1)$$

where $Employment \quad Status_{ijb}$ is a dummy variable equal to one whenever individual i, originally employed in firm j, is still working for the acquiring firm b one year after the acquisition; $Matched_{ijb}$ is a dummy equal to one whenever the incumbent worker is born in the same municipality in which the acquiring firm's headquarters are established, and zero otherwise; f_j are acquired firm fixed-effects (i.e. takeover episode fixed effects). Our analysis is conditional on the worker's place of birth C_i being different from the municipality in which the acquired firm's headquarters are established, C_j. Our parameter of interest is represented by β_1, which captures any systematic difference in the employment status of matched and unmatched workers. The identification strategy relies on the assumption that, controlling for unobserved characteristics at the acquired-firm level, and comparing groups of workers that were ex-ante unmatched, the matching status of the worker is as good as randomly assigned. In Sections 3 and 4 we provide several tests to corroborate the validity of this assumption.

3   Data and descriptives

In this paper we use matched employer-employee data from the Italian Social Security Administration (INPS).[5] This dataset covers the universe of private sector employment relationships between January 1975 and December 2001 for the Italian region of Veneto.[6]

The primary unit of observation in the data is a firm-worker match for each calendar year. Both workers and firms in the data are individually identifiable and can be followed over time. The data provide information about different

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[5]For an extensive description of Veneto economy see Tattara and Anastasia (2003).

[6]Veneto is one of twenty Italian regions, located in the north-east of Italy. According to the last Census (2010) Veneto had a population of about 4.9 million, corresponding to 8% of the total Italian population.

aspects of the employment relationship, such as the total compensation paid in each year and the occupation, along with workers' demographic characteristics, i.e. age, gender, and city of birth. For each firm appearing in the data, we have information on the date of opening and closure, the sector of activity (at the 3 digit level) and the municipality where they have their headquarters.[7]

As explained in the previous section, we exploit changes in the matching status of workers given by firm takeovers. The dataset provides information on firm closures: a specific variable indicates the exact date (monthly) at which a firm stopped operating, distinguishing between real closures and takeovers. Acquisitions reported in the dataset only consist of incorporations in which the acquired firms disappear from the dataset as an independent economic entity, and become part of the acquiring firm. When the acquiring and acquired firms come from different cities, takeovers generate variation in the matching status between employers' and employees' cities of origin. For instance, a change in the matching status of workers occurs when a firm based in Venice is taken over by a firm coming from any other Italian city but Venice. Since the INPS dataset provides information on the municipality in which the worker was born, and the municipality in which the firm has its headquarters, it is possible to check whether the worker and his employer belong to the local social network.[8]

The dataset follows every worker from the moment they first started working in Veneto, even if they subsequently find a job in an Italian municipality outside Veneto. The absorbing state includes non-employment, death or retirement, and movements abroad. For this reason, our sample only includes acquired Veneto firms because it is only for these firms that we are able to look at post-acquisition outcomes for all workers. Table 1 displays the main characteristics of takeovers observed in Veneto.

Insert Table 1 here

Overall we observe more than 10,500 acquired firms and 9,673 acquiring firms, implying that some "buyers" acquired more than one firm in the whole period. In

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[7]In this paper, the word municipality and city are interchangeable, and refer to the Italian administrative division "comune".

[8]For a similar reason, we conduct our analysis on incumbent workers, and not on new hires. Since we do not observe the whole workforce of an acquiring firm based outside the Veneto region, we cannot tell whether a new worker appearing after the takeover was recently hired or whether he was an incumbent who had been relocated to the new branch in Veneto.

order to account for acquisition heterogeneity in our analysis, we include acquired firm fixed effects. Therefore, we retain only those acquired firms that employed at least two unmatched workers, i.e. workers that do not come from the same city as the acquired firm, and where at least one of those workers has to be matched with the buyer. Moreover, in order to generate a change in the matching status only acquisitions in which the buyer and the target firms come from different cities can be considered. The sample analyzed is thus reduced to 954 acquired firms and 911 buyers.

Looking at the number of months since the firms started business and at the share of blue collars employed, there appear to be no significant differences between the sample analyzed and the entire sample of takeovers. Similarly, the distribution across industries of the selected firms largely mimics that of the overall sample, with the greater majority being employed in the clothing industry (10.5%), shoes manufacturing (6.5%) and machinery (5%). The firms analyzed are larger on average, but mainly because the overall dataset also includes one-worker firms. It is interesting to notice that the share of buyers from a different province is relatively small: most of the acquisitions occur within the same county. Finally, about 60% of the acquired firms operate in the same industry as the firm buyer, and this share is the same in the two samples. It is interesting to notice that the share of buyers from a different province is relatively small: most of the acquisitions occur within the same county. Finally, about 60% of the acquired firms operate in the same industry as the firm buyer, and this share is the same in the two samples.

Figure 4 plots the share of acquired firms considered in the analysis over the total number of active firms for every month in the period 1976-2000; dotted vertical lines indicate the end of every year.

By looking at figure 4 it seems that most of the firms were acquired at the end of the calendar year; this is clearly due to fiscal and bureaucratic reasons. Since the deal for a takeover is usually reached months before the acquisition becomes official, it is likely that most employees voluntarily quit or get dismissed as a result of the ongoing acquisition before the event is formally realized. As discussed in the previous section, in order to avoid self-selection issues, we only keep workers who were employed in the acquired firm for at least 12 months before the acquisition, as changes in the employment relationship that occurred

Figure 4: Timing of the acquisitions (% of acquired over total active firms)
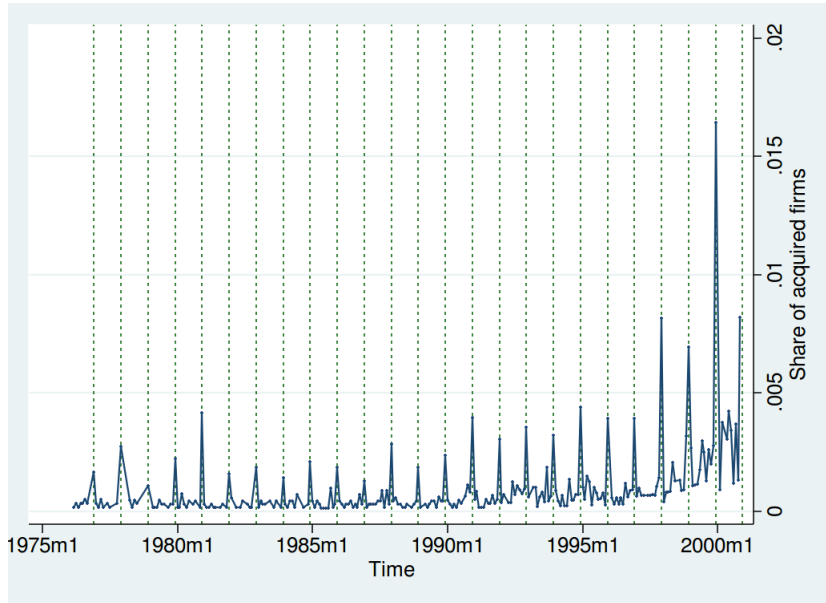
just before the takeover event could be a consequence of the acquisition itself. For the same reason, we do not include workers that joined the firm in the 12 months before the acquisition.

Finally, results from a balancing test on the randomness of the matching status is performed are reported in Table 2. The table summarizes the main observable characteristics of the workers in the firms analyzed by matching status: those unmatched with both the old and the new employer (70%), those who were ex-post matched (11%) and those who used to be matched with the initial employer (18%). The final sample includes about 48,000 workers, of which more than 39,000 are ex-ante unmatched workers.

Insert Table 2 here

Ex-post matched (**UM**) and never matched workers (**UU**) do not have any significant differences in the main characteristics, such as occupation, age and weekly wage. This is not true for ex-ante matched workers: they are on average older and earn lower wages, probably because they are employed in lower skilled occupations. These statistics are computed at firm level in order to control for self-selection into firms. Matching status thus appears to be random only among workers that are ex-ante unmatched, justifying our identification strategy.

4 Empirical findings

4.1 Main estimates

This section presents regression results of equation (1). The finale sample is composed of 39,239 incumbent workers employed in the acquired firm 12 months before takeover. Only ex-ante un-matched workers are included in the analysis, i.e. groups **UU** and **UM**. All specifications include acquired firm fixed effects. Finally, standard errors are clustered at the municipality of birth level to allow for arbitrary correlation of errors across workers born in the same municipality.

In order to corroborate the validity of the identification assumption we also include, a set of controls for age, gender, five occupational dummies (corresponding to trainees, blue collar workers, white collar workers, managers, and others), the migrant status of the worker, and the relative wage within the firm, i.e. the ratio between the individual weekly wage and the average firm weekly wage. Results are shown in columns (1) and (2) of Table 3.

Insert Table 3 here

It emerges from Table 3 that ex-post matched workers, relative to ex-post un-matched workers, have a statistically higher probability of retaining their job as following the takeover. The difference is about 3 percentage points, and accounts for roughly 4% of the average outcome variable. The coefficient capturing the magnitude of localism is not affected when we include the set of controls previously specified, reassuring us about the validity of our identification strategy. It is also interesting to notice that employees' gender does not significantly affect the probability of staying in the job, while the migrant status enters with a negative sign.[9] Furthermore, relative to blue collar workers (the baseline category), trainees have a much lower probability of staying. This is not surprising as traineeships are generally the most precarious positions in a company. Managers, the category of workers that is directly appointed by the shareholders and that

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[9]The inclusion of this variable is important for the validity of our specification as this characteristic is not random between the treatment and the control group. In fact, there are no immigrants that are matched with the new employer, thus, controlling for it allows us to consistently estimate our parameter of interest. Excluding migrants from the sample does not change the significance and magnitude of the estimated coefficients.

generally changes with a change in firm ownership, also have a low probability of staying relative to the blue color workers.

4.2 Heterogeneity and intensity of localism

This section shows how the average effect documented in the subsection 4.1 varies depending on the different characteristics of both workers and firms. In particular, the following variables are analyzed:

- Size of the new employer's community of origin

- Occupation of incumbent workers

- Size of the acquired firm

The size of the new employer's community of origin is equal to the number of inhabitants in the municipality in which the acquiring firm's headquarters are established in the year of the acquisition.[10] The intuitive idea is that the larger the community the lower the probability that the employer and the employee belong to the same local social network, and thus the more difficult it is for the employer to infer the intangible qualities of local workers, even if both parties share the same origin. Thus, we expect that the preferential treatment for locals should disappear as the municipality gets larger.

The occupation of incumbent workers is used to test if the bias for locals is stronger for high-skilled occupations, where tasks are not standardized and personal ability matters more. Finally, the size of the acquired firm, in terms of employees, is a dummy equal to one whenever the firm's size is above the median of the distribution and zero otherwise. Since the smaller the pool of incumbent workers is, the easier (and less costly) an accurate evaluation of potential employees will be, we can expect this variable to capture the degree of asymmetric information suffered by the new employer.[11] In particular, we would expect the preferential treatment to be stronger the larger is the pool of incumbent workers.

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[10]We construct four dummies corresponding to: number of inhabitants $< 5,000$; between 5,000 and 49,999; between 50,000 and 249,000; $\geq 250,000$.

[11]The idea is that, when facing few potential applicants, the employer can reduce the uncertainty at a relative low cost, for instance by checking their abilities personally, after a short trial period. The same strategy becomes too costly when the pool of applicants is large, and here is where the local networks become relevant.

In columns (3) to (5) of Table 3, the interactions between the $Matched_{ijb}$ dummy and each of the three variables specified above are added to the econometric specification presented in equation (1).

Looking at the size of the municipality, the effect of localism tends to decrease as the size of the community increases, although the effect is statistically significant only for very large municipalities. With respect to the different occupations, compared to blue-collars (the baseline group), the only interaction term that is statistically significant and economically relevant is the one relating to managers. This implies that when managers are local, the probability of staying in the job is around 17 percentage points higher than the benchmark. For the other occupations, the effect of the local network is statistically similar, and is equal to an additional 2 percentage points, relative to unmatched workers. By looking at the effect of the size of the acquired firm, it appears that the observed preferential treatment for locals only matters when the pool of potential applicants is relatively large, and is non-existent when the number of applicants is relatively small.

4.3 Robustness

A potential threat to identification is that the determinants of the acquisition event may be correlated with the outcome variables; in particular, an acquisition may take place depending on the existence of workers that will be matched with the origin of the new employer. This circumstance would pose serious doubts on the validity of our results; we could not then claim that the parameter β_1 estimates the average effect of localism on employment outcomes, rather than the effect of the presence of local workers on the probability of takeover. In order to address this issue, we check whether the existence of potentially matched incumbent workers alters the probability that an acquisition will take place. Given that our variable of interest cannot be estimated without knowing the identity of the acquiring firm (and in particular, the location of its headquarters, which we use as a proxy for the employer's community of origin), we proceed as follows: first, for each acquired firm,[12] we cluster all firms, acquired or not, that were established

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[12]In order to increase the size of the sample, and because the existence of potentially matched workers is not limited to those acquired firms where both **UM** and **UU** classes of workers are present, we consider all acquired firms in the period under scrutiny.

in the same municipality at the year of the acquisition, were in the same sector of activity, and of the same class size.[13] Then, for each firm within the cluster, we assign a value equal to one if at least one worker is matched with the potential buyer, and zero otherwise.[14] Finally, we estimate a linear probability model of the following form:

$$Acquired \ \ Status_{ic} = \beta_0 + \beta_1 Matched_{ic} + X'_{ic}\beta_2 + f_c + \epsilon_{ic} \qquad (2)$$

where $Acquired \ \ Status_{ic}$ is a dummy equal to one if firm i, belonging to cluster c, is acquired and zero otherwise; $Matched_{ic}$ is a dummy capturing the existence of potentially matched workers within the acquired firm within cluster c; X'_{ic} is a set of firm controls, including the average age of the workforce, the average firm age, the average firm wage (as a proxy for firm productivity), the fractions of blue collar workers, white collar workers, and managers over the total workforce, and the share of male workers; finally, f_c is the cluster fixed-effect. If the existence of potentially matched workers does not alter the probability of an acquisition taking place, β_1 should be zero. Results are shown in Table 4.

Insert Table 4 here

Table 4 indicates that the probability of an acquisition is not determined by the existence of potentially matched workers within the firm, while it is positively correlated with other firm characteristics, such as age of the firm, the average wage (which may be capturing the productivity level of the workforce) and the share of managers. Thus, we can confidently argue that the presence of ex-post matched workers is relevant for the new employer only after the acquisition has occurred, and does not drive the acquisition event.

4.4 Localism and firms' economic performance

As mentioned in the introduction, the effects of localism on firm performance should be positive if local social networks help the employer reducing the information asymmetry between the two sides of the hiring process. Localism is thus an efficient screening device that should positively affects the firm performance.

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[13]We split the sample into 5 classes, depending on the number of employees: less than 6, between 6 and 15, between 16 and 99, between 100 and 499, and above 499.

[14]We also tried an alternative specification, where the dummy is replaced with the share of potentially matched workers, but results were analogous.

To proxy for the employers' bias towards local employees, we use a dummy variable that is equal to one whenever the ratio between the number of local workers and the number of non-local workers retained twelve months after the takeover is higher than the corresponding ratio twelve months before the takeover.[15]

Because we do not have direct information on firms' revenues or profitability, we measure the effect of localism on firm performance by looking at the probability that the acquiring firm shuts down following the takeover.[16] As already specified in Section 3, INPS data cover years from 1975 to 2001, but information on firm closures is available up to December 2003.[17]

To estimate the effect of localism on firms' survival probability we employ continuous-time hazard models. The survival time is defined as the months between the takeover took place and the disappearance of the firm from the sample. If a firm does not shut down before December 2003, we indicate the survival time as censored: in the time window considered we observe 296 firm closures, accounting for about 31% of the sampled firms, i.e. 69% of survival times are right censored. We use the Cox proportional hazard model and we estimate the firm's probability of closure at any month t following the takeover, conditioning on still being active up to month $t-1$:

$$h_{jb}(t) = \lambda(t) * exp(\beta_1 Localism_{jb} + X'_b \beta_2) \tag{3}$$

where $\lambda(t)$ is the baseline hazard ratio for firm b following the acquisition of firm j, and t is the number of months that the firm has survived. The model includes a vector of time-invariant characteristics of the acquiring firm, such as the headquarters' county, the industry of activity and dummies for the size of the acquired firm.[18] Dummies for the year of takeover are also added in order to compare firms whose survival times started in the same year. $Localism_{jb}$ is the main explanatory variable; the parameter of interest is thus represented by β_1, and it captures any systematic correlation existing between the preference

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[15]We define the variable that proxies for the intensity of localism as $Localism_{jb}=1$ if $\frac{(Sizelocals)_{j,b,t+1}}{(Sizenon-locals)_{j,b,t+1}} > \frac{(Sizelocals)_{j,b,t-1}}{Sizenon-locals)_{j,b,t-1}}$. where b is the firm buyer, j is the acquired firm and t is the year of the acquisition.

[16]Since there are "buyers" that acquired more than one firm, we consider every pair of acquired and acquiring firms as an independent observation.

[17]We are then able to follow firms for at least 3 years since the takeover.

[18]We split the size in to 5 classes, depending on the number of employees: less than 6, between 6 and 15, between 16 and 99, between 100 and 499, and above 499.

for locals and the firm's economic performance. Table 5 reports the estimated hazard ratios.

Insert Table 5 here

In the first column of Table 5 we only control for the year of the takeover and for the acquired firm's size: the estimated coefficients indicate that whenever the share of local workers retained is higher than the one for non-locals the closure hazard for the acquiring firm is reduced by 32 percent. In order to take into account unobserved shocks potentially affecting the survival probabilities of firms operating in the same industry or local labor market, we include in column (2) controls for the sector of activity (2-digit level) and for the county where the firm is located. The estimated coefficient of the *Localism* variable only slightly changes, still implying a 35 percent significant lower probability of closure for the indicated group of employers. Results in column (3) further include the year in which the firm started its business. The firm's age is supposed to have a high impact on its probability of closure; however, the inclusion of this control does not seem to significantly affect our coefficient of interest: the effect is still significant at 1% level and it predicts a probability of closure for discriminatory employers 37 percent lower than for non-discriminatory ones. To further control for possible confounding factors affecting both the success of the acquiring firm and the likelihood of retaining local workers, in column (4) we also include the interaction between the city of origin, the industry of activity and the year in which the firm was established. In other words, we restrict our attention to firms that operated in the same local market, and for the same number of years. Results in column (4) show that the coefficient of interest is still significant at 5% level and it has increased in magnitude: the probability of closure following a takeover is 63% lower for a discriminatory employer with respect to a non-discriminatory one.

All in all, results from Table 5 indicate that the decision to keep local workers more than non-local workers positively affects firms' economic performance. In particular, these results are consistent with the idea that the existence of a community network in the labor market has a positive effect on firms' outcomes, potentially reducing the costs for the employers of the imperfect information.

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4.5 Alternative explanations

There may be alternative stories that explain the observed preferential treatment for local workers. However, as we will show in this section, while these are consistent with single findings from Tables 3 or 5 they cannot account for the entire set. One plausible explanation has to do with different complementarity of local and non-local workers in the production function of the acquiring firm. In other words, if workers from Venice are better than those from Verona in performing the task required by a Venetian company, then a preference for Venetians would follow, even with perfect information on workers' characteristics. However, this interpretation is inconsistent with a bias that diminishes as the size of the community enlarges. Moreover, it would be difficult to justify why this complementarity decreases as the size of the acquired firm increases.

For similar reasons, we can exclude the possibility that the observed patterns are due to a higher willingness on the part of matched workers to move from the acquired firm's location to get closer to their community of origin. Moreover, we also checked that the local network effect does not depend on the (spherical) distance between the acquired firm and the acquiring firm's headquarters.[19]

A final explanation is related to the fact that, by preferring locals to "foreigners", the entrepreneur can gain visibility within the local social network (D'Aurizio and Romano 2013) or gain political influence, because these workers belong to the same community as the employer. Again, this explanation, while theoretically plausible, would not explain, for instance, why managers should be preferred to white and blue-collar workers. In fact, it is reasonable to assume that visibility and political influence increase with the number of individuals benefiting from then entrepreneur's decisions. As shown by Table 2, blue and white-collar workers account for more than 94% of the overall workforce, while managers account for around 1%.

4.6 Localism and wage differentials

So far, we have documented how the shared community of origin between the employer and the employee had a positive effect on the likelihood of job retention after a takeover. Yet, the effect of localism in the labor market may not be

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[19]Results are available upon request.

entirely captured by the employment dynamics of local and non-local workers, but it can also affect the wage dynamics following the takeover.

We thus look at the wage difference for those incumbent workers still employed in the acquired firm one year after the takeover. Specifically, we estimate the following equation:

$$(\Delta_t wage_{ijb} | E_{ijb} = 1) = \beta_0 + \beta_1 Matched_{ijb} + X'_{ijb}\beta_2 + f_j + \epsilon_{ijb} \qquad (4)$$

where $\Delta_t wage_{ijb}$ is the *log* change in wage occurred between t-1 and t+1 (as defined in Figure 3), for worker i employed in firm j and still working in the acquiring firm b; X' includes sex, age, occupation and relative wage within the acquired firm.[20] Their inclusion is meant to control for possible ex-post differences in the composition of the two groups of workers, which would bias our estimate of β_1 in equation 4. This composition effect is the direct consequence of the different probabilities of retaining a job after the takeover previously documented. Furthermore, to account for the possible heterogeneity across workers, we also check how the wage discrimination varies with their occupation. Results are reported in Table 6. Columns (1) and (2) look at the entire sample of retained workers, without and with control variables respectively; columns (3) to (6) look at the subsamples of trainees, blue-collars, white-collars and managers respectively.

Insert Table 6 here

Results from Table 6, columns (1) and (2), show that, on average, localism is not systematically affecting the wage dynamics of workers that retained their job after the takeover. However, when looking at the different occupations, columns (3) to (6), there is evidence of a positive wage gap for matched trainees and managers, of 7.7 and 6.7 percentage points respectively, after controlling for the set of observable characteristics previously specified. These effects, despite being large in absolute terms, have a negligible impact on the average wage differential because they refer to only 2% of the total number of retained workers in our sample.

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[20]We drop the first and last percentile of the distribution of $\Delta_t wage_{ijb}$ to get rid of outliers

5   Conclusion

In this paper we use a matched employer-employee dataset for the Italian region of Veneto to investigate how local social networks affect economic outcomes of both firms and workers. We define an employee and an employer to belong to the same local social network when they come from the same city of origin, i.e. whenever the city of birth of the employee is the same as the one in which the firm has its headquarters.

By exploiting variations in firm ownership generated by takeovers, we first test whether being connected to the new employer increases or decreases a worker's probability of retaining his job. Within acquired-firm comparisons tell us that coming from the same city of origin as the new employer increases an employee's job retention probability by around 4% of the average probability.

We also find that, conditioning on being employed after the takeover, no significant differences in terms of wage differentials emerge between local and non-local workers. However, the average effect hides substantial heterogeneity across occupations: local managers and trainees experience a change in salary which is respectively around 7 and 8 percentage points higher than the wage dynamics of comparable non-local workers with the same occupation.

We argue that this evidence of a positive effect played by local social networks in the labor market could be explained by the imperfect information faced by employers when making hiring/firing decisions: belonging to the same local community reduces uncertainty about workers' skills and reliability, thanks to direct and indirect connections between the employer and the employee. Consistently with this story, we find that a higher share of local employees retained after a takeover reduces the probability of closure for the acquiring firm. Taken together, our findings confirm that local social networks in the labor market have a sizable effect on both workers' employment outcomes and firms' economic performance.

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Tables

Table 1: *Takeovers in Veneto 1976-2000*

	Overall	Sample analyzed
Number of acquired firms	10,574	954
Number of acquiring firms	9,673	911
Characteristics of takeovers:		
Different city (%)	25.38	100
Different county (%)	9.54	22.75
Same industry (%)	61.80	59.43
Characteristics of acquired firms:		
Average acquired firm size	30.45	50.34
	[95.58]	[147.18]
Average acquired firm age (months)	130.57	117.32
	[80.79]	[76.59]
Share of blue collars	0.71	0.72
	[0.29]	[0.28]
Top Industries (3 digits):		
Clothing (%)	12.25	10.8
Machinery (%)	5.59	5.14
Shoes (%)	5.31	6.5
Construction (%)	4.51	3.88
Woodwork (%)	3.78	4.19

Authors' calculations on INPS data; standard deviations in square brackets.

Table 2: *Characteristics of the sample workers at acquired firm level*

	Matched After (UM)	Never Matched (UU)	Difference UM-UU	Matched Before (MU)
Share over the total workforce	0.11	0.71		0.18
Average age (years)	32.75 [9.50]	32.73 [7.27]	0.02 [8.96]	34.15 [8.92]
Share of male workers	0.57 [0.42]	0.58 [0.37]	-0.00 [0.29]	0.56 [0.40]
Average weekly wage[a]	702.77 [553.56]	683.43 [428.56]	19.34 [545.29]	667.96 [291.34]
Occupation:				
Share of blue collars	0.71 [0.39]	0.72 [0.29]	-0.01 [0.34]	0.73 [0.35]
Share of white collars	0.21 [0.35]	0.21 [0.32]	0.00 [0.32]	0.19 [0.27]
Share of managers	0.01 [0.10]	0.01 [0.05]	0.00 [0.10]	0.01 [0.05]
Observations	5,251	33,988		8,785

[a]: gross, euro 2003. Authors' calculations on INPS data; standard deviations in square brackets.

Table 3: *Localism and employment outcomes*

	Dependent variable: $Employment\ Status_{ijb}$				
	(1)	(2)	(3)	(4)	(5)
Matched After (UM)	0.027***	0.025***	0.053**	0.023***	0.007
	(0.007)	(0.007)	(0.025)	(0.008)	(0.008)
Foreign born		-0.052***	-0.052***	-0.053***	-0.053***
		(0.004)	(0.005)	(0.004)	(0.004)
Trainee		-0.211***	-0.211***	-0.223***	-0.210***
		(0.054)	(0.054)	(0.051)	(0.053)
White Collar		-0.003	-0.003	-0.003	-0.003
		(0.008)	(0.008)	(0.008)	(0.008)
Manager		-0.082***	-0.082***	-0.091***	-0.083***
		(0.021)	(0.021)	(0.021)	(0.021)
$(5,000\text{-}49,999)^{a}$ × **UM**			-0.028		
			(0.027)		
$(50,000\text{-}249,999)^{a}$ × **UM**			-0.032		
			(0.027)		
$(\text{above } 249,999)^{a}$ × **UM**			-0.049*		
			(0.027)		
Trainee × **UM**				0.130	
				(0.090)	
White collar × **UM**				-0.001	
				(0.015)	
Manager× **UM**				0.148**	
				(0.058)	
Acquired Firm Size × **UM**					0.043***
					(0.015)
Constant	0.655***	0.451***	0.451***	0.451***	0.450***
	(0.003)	(0.016)	(0.016)	(0.016)	(0.016)
Controls:					
Individual Characteristics	No	Yes	Yes	Yes	Yes
Acquired Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	39,239	39,239	39,239	39,239	39,239

[a]: refers to number of inhabitants in the municipality where the acquiring firm's headquarters are located. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at city of birth level, in parentheses. Individual characteristics include a gender dummy, age dummies and worker's relative wage within the acquired firm.

Table 4: *Probability of takeovers and local employees*

	Dependent variable: $Acquired\ Status_{ic}$	
	(1)	(2)
Matched After (UM)	0.0016	0.0015
	(0.0016)	(0.0016)
Average Workforce age (years)		0.0000
		(0.0002)
log Firm age (months)		0.0013***
		(0.0002)
Average Wage		0.0001***
		(0.0000)
% Blue collars		-0.0001
		(0.0011)
% White collars		0.0008
		(0.0015)
% Managers		0.0573**
		(0.0221)
% Males		-0.0011
		(0.0008)
Constant	0.0101***	-0.0002
	(0.0002)	(0.0020)
Observations	189,103	189,103

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered for the city of the acquiring firm headquarters, in parenthesis.

Table 5: *Localism and Firm Performance*

	(1)	(2)	(3)	(4)
Localism	0.682***	0.658***	0.634***	0.362**
	(0.084)	(0.085)	(0.086)	(0.156)
Controls:				
Acquired firm size Dummies	Yes	Yes	Yes	Yes
Year of takeover	Yes	Yes	Yes	Yes
Municipality	No	Yes	Yes	Yes
Industry (3-digit)	No	Yes	Yes	Yes
Year of constitution	No	No	No	Yes
Municipality*Industry*Year of constitution	No	No	No	Yes
Observations	952	952	952	952

*: p-value<0.10, **: p-value<0.05, ***: p-value<0.01. Hazard ratios from Cox proportional hazard models. The dependent variable is the firm's closure hazard function, measured in months. Robust standard errors in parenthesis.

Table 6: *Localism and Wage differentials*

	(1)	(2)	(3)	(4)	(5)	(6)
	\multicolumn{2}{c}{Overall}	Trainees	Blue collars	White collars	Managers

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall		Trainees	Blue collars	White collars	Managers
Matched After (UM)	0.002	0.001	0.077*	-0.002	-0.005	.067**
	(0.003)	(0.003)	(0.041)	(0.003)	(0.005)	(0.030)
Trainee	-0.076***	-0.080***				
	(0.023)	(0.023)				
White collars	0.011***	0.041***				
	(0.003)	(0.004)				
Managers	0.021**	0.190***				
	(0.008)	(0.013)				
Constant	0.048***	.082***	0.225***	0.181**	0.113***	-0.028
	(0.001)	(0.007)	(0.083)	(0.092)	(0.007)	(0.115)
<u>Controls:</u>						
Individual Characteristics	No	Yes	Yes	Yes	Yes	Yes
Acquired Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24,459	24,459	139	15,980	7,970	370

Dependent variable: $\Delta_t wage_{ijb}$"

*: p-value$<$0.10, **: p-value$<$0.05, ***: p-value$<$0.01. Robust standard errors, clustered at city of birth level, in parentheses. Individual characteristics include a gender dummy, age dummies and worker's relative real wage (euro) within the acquired firm. We cut the 1th e 99th percentiles of the dependent variable to control for outliers.