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The Economic Value of Local Social Networks

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Abstract

The idea that local social capital yields economic benefits is fundamental to theories of agglomeration, and central to claims about the virtues of cities. However, this claim has not been evaluated using methods that permit more confident statements about causality. This paper examines what happens to firms that become affiliated with a highly-connected local individual or "dealmaker." We adopt a quasi-experimental approach, combining difference-in-differences and propensity score matching to address selection and identification challenges. The results indicate that firms who link to highly-connected local dealmakers are rewarded with substantial gains in employment and sales when compared to a control group.

Keywords: Cities, economic development, social networks, social capital
JEL Classifications: R11; O12; O18; L14

I. Introduction

Since Alfred Marshall's (1890) observations about the circulation and propagation of ideas in English industrial districts, economists have been motivated to understand if local social networks augment economic performance (Glaeser et al., 1992; Jaffe et al., 1993; Powell et al., 1996; Saxenian, 1996; Feldman and Audretsch 1996a; Casper, 2007; Breschi and Lissoni, 2009). This inquiry intersects with an interest throughout the social sciences in what is known as social capital, a concept that suggests that a higher degree of network centrality increases pecuniary value (Coleman, 1988; Putnam, 1995). While social networks certainly reach beyond individual geographic agglomerations (Kenney and Patton, 2005), the myriad virtues of proximity suggest that cities are the relevant spatial unit for considering how interactions within social networks affect economic outcomes (Feldman and Audretsch, 1996ab; Storper and Venables, 2004; Duranton and Puga, 2004; Rosenthal and Strange, 2004; Ellison et al, 2010). The literature suggests that economic actors earn higher returns in cities with better social capital as defined by more dense social networks, by fostering trust and information sharing, and by lowering transaction costs.

Still the precise mechanisms by which local social capital augments economic performance remain mysterious (Jones 2006; Malecki, 2012). Existing econometric studies represent regional networks in aggregate, with social capital typically captured by measuring the overall size or density of a particular agglomeration's network (e.g. Lobo and Strumsky, 2008). This practice contrasts with the demonstrated relevance of the behavior of individual actors (Hargadon and Sutton, 1997; Burt, 1995, 2004; Stam, 2010). Individuals who bridge distinct strands of a network facilitate connections between firms, and enable the dissemination of new and economically valuable ideas. Moreover, social capital is often embodied in individuals with high human capital (Bourdieu, 1986). These micro-dynamics are lost when networks are considered as aggregate entities. Perhaps most importantly, we have little evidence that links either aggregate or micro-social dynamics to improved economic outcomes in a framework that can generate more confident statements about causality. This is a considerable issue. Practically, we have little clarity on whether the famously dense networks linking Silicon Valley information technology actors have a causal impact on the superior performance of firms in that region, or if instead the networks are an outcome of the region's culture, dynamism, or some other factor?

This paper seeks to address these gaps. Rather than defining local social capital in aggregate, we focus on a particular set of highly connected agents within regional networks, which we define as dealmakers. The term dealmaker is colloquial in entrepreneurship practice, and describes an accomplished actor, who is deeply enmeshed in local social networks, and who leverages these networks to make things happen (Senor and Singer, 2009); in short, these are network brokers with an observably local orientation, living and investing in a place. Feldman and Zoller (2012) identify dealmakers as high connected individuals in terms of their fiduciary roles as founders, executives and board members, and demonstrate that their presence – not the aggregate size or density of social capital networks – is strongly positively correlated with new firm births. This relationship could mean a few different things. One interpretation is that the presence of dealmakers spurs entrepreneurship. Another possibility is that this correlation reflects the reverse causal sequence: vibrant urban economies simply produce more dealmakers, without the latter having a strong independent effect. A third scenario is that some as-yet unmeasured force determines both regional economic dynamism and the existence of dealmakers.

This paper shifts focus to firms within local networks. The primary hypothesis is that, by lowering the costs of making connections and sharing ideas, highly connected individuals augment the economic performance of the firms to which they become connected. We use the term dealmakers to refer to individuals who are highly connected to the network of entrepreneurial firms in a city. Thus, we measure the interlock between local firms with which dealmakers are affiliated. We explore whether dealmakers leverage regional connections to influence firm performance, measured in terms of sales, employment and sales per worker. We also consider whether dealmakers' nodal positions in regional social networks could affect the trajectory of a firm by stimulating a liquidity event, thereby providing original entrepreneurs and investors with a means of converting their ownership equity into cash.

The primary obstacle to identification is that dealmaker links to firms are endogenous. Simply, dealmakers are likely to be drawn to firms that promise success. To address this challenge, this study adopts a quasi-experimental research design. Propensity score matching is used to model the selection process of dealmakers to firms, with propensity scores used to build a counterfactual group of firms that do not link to dealmakers (the control group), but who otherwise resemble those that do (the treatment group). This information is used in a difference-in-differences model that accounts for differences in the evolution of the two groups before and after treatment. Combining these approaches yields benefits: we control for both observable firm characteristics that ought to influence the likelihood of getting a dealmaker, as well as stationary but unobserved properties of those firms. Economists have used one of these approaches to answer a wide variety of questions **(**see for instance, Ashenfelter, 1978**;** Card, 1994**;** Heckman et al, 1997***;*** Grogger and Willis, 2000; Groen and Povlika; 2008**)**, sometimes using them in combination (Arnold and Javorcik, 2009; Görg, and Strobl, 2007)**;** together or separately, they have not yet been used to estimate the effects of urban interpersonal networks on firm performance.

To carry out this research design, a set of 325 firms in life sciences and information technology sectors, located in 12 U.S. high- technology regions, are observed in two time periods: December 2009 and December 2012. Each of these 325 firms added exactly one new individual to their board or management team: 80 firms added an individual who was a regional dealmaker (the treatment group) while 265 firms added an individual without connections to the network of firms. Capital IQ, one of the more comprehensive data sources on entrepreneurial firms available in the United States, provides the sampling frame of firms and dealmakers. We link these data to Dun & Bradstreet (D&B), which provides a wealth of establishment-specific characteristics, such as international trade activities; creditworthiness; ownership structure; as well as employment and sales.

We find *ex post* that firms that get dealmakers have considerably higher growth in sales and employee compared with similar firms that do not get dealmakers. We uncover no significant relationship in our analysis between dealmaker affiliations and acquisitions or sales per employee. In light of the motivating theory, our results suggest that dealmakers' attempts to leverage local social networks actually enhance the performances of firms to which they are connected.

The remainder of the paper is organized as follows. Section II lays out our conceptual framework. Section III describes the empirical approach taken, and Section IV describes our data. Section V presents diagnostics of the analytical procedure. Section VI presents results. Section VI concludes.

II. Conceptual Framework

Consider a universe of firms in a location, where each firm's performance is a function of the quality of its workers*,* firm-specific attributes such as capital, as well as some industry- and region-specific factors. Among the salient drivers of worker quality is the ability to leverage interpersonal connections, or social capital, for the potential gain of the organization (Giuri and Mariani, 2013). Through connections to the regional social network, workers can gain new ideas and human capital that might raise productivity, open new markets, help develop new products, or stimulate mergers, acquisitions or other types of liquidity events. Through these channels, the social network can affect firm performance. By extension, regional economic outcomes will be a function of the performance of individual firms (Saxenian, 1993; Jaffe et al, 1993; Uzzi, 1995).

Workers vary in terms of their position in local social networks. For simplicity, we assume there are two kinds of workers: those that have standard access to the network, and those with a greater quality of social capital, occupying privileged network positions. For simplicity, we call the more highly connected workers *dealmakers*, while we call workers with average social capital *non-dealmakers*. There is a need to consider effects arising not just from dealmakers but also from association with non-dealmakers. Concretely, the combined network connections of non-dealmakers could equal or exceed the reach of a typical dealmaker. Given this potential confounding issue, we must account for the social capital of both kinds of network actors.

Given this framework, we describe firm performance as follows:

$$f(Y_p) = \{l_{dm}, l_{ndm}, K_p, I, R\} \tag{1}$$

where y measures firm performance of firm p in region r; l_{dm} measures the number of dealmakers affiliated with the firm, while l_{ndm} captures the presence of non-dealmakers; K captures firm-specific characteristics; and I and R describe industry- and region-specific factors. Our aim in this paper is estimate the independent causal effects of l_{dm} on y, holding constant other drivers of performance. A description of our empirical approach follows.

III. Empirical Approach

We expect that dealmakers will elicit positive changes in the performance of firms with which they become affiliated. There are at least three empirical approaches to assess the potential effect of associating a dealmaker to a firm. First, the performance of firms after they get a dealmaker could be compared to their pre-dealmaker performance. But, irrespective of any causal dealmaker effects, with this approach any results could reflect unobserved time trends in the performance outcome or some economy-wide shock. Second, the performance of firms that receive the treatment of working with a dealmaker may be compared to a control group of similar firms that lack an affiliated dealmaker. This method, however, risks assigning explanatory value to dealmakers that reflects pre-existing inter-group differences. This poses a particular problem for the proposed research, because there is good reason to believe that: (a) firms that become linked to dealmakers differ from those that do not, and (b) these differences bear upon their performance. Put simply, there could be a selection effect as dealmakers ought to be drawn to firms that have demonstrated success, or show great promise to succeed (Jaffe 2002). This selection process between dealmakers and firms would bias conventional regression approaches and overestimate the impact of adding a dealmaker.

To address these issues, this study adopts a third approach that combines beneficial aspects of the previous two. Specifically, this study considers firm performance before and after adding an executive or board member, while also comparing firms that become affiliated with a dealmaker (the treatment group) to others that receive a non-dealmaker (the control group). For precision, the sample of firms is initially limited to those that have zero dealmakers in the pre-treatment period.

The treatment group is treated by the addition of exactly one dealmaker, with zero non-dealmakers added. The control group does not add a dealmaker, but adds one non-dealmaker. The analysis combines the difference-in-differences (DD) estimator with propensity score matching (PSM) techniques. As a first step, the Epanechnikov kernel-based PSM procedure estimates the likelihood of each firm linking to a dealmaker, conditional upon a vector of observed firm characteristics. The resulting probabilities are then used to match treatment and control firms such that, for a limited subset of cases, systematic differences across the groups can be eliminated (Dehejia and Wahba, 2002). From these probabilities, weights are generated that indicate the relevance of each control firm to each treatment firm. These weights are then applied to a regression-based difference-in-differences model. This estimator compares changes in firm performance between pre-and post-treatment periods across the treatment and control groups, as follows:

$$\hat{a}^{T} = (\bar{Y}^{T}_{t1} - \bar{Y}^{C}_{t1}) - (\bar{Y}^{T}_{t0} - \bar{Y}^{C}_{t0}) \quad (2)$$

where $\hat{a}$ measures the average effect of the treatment on the treated, *T*; *Y* represents the outcome of interest; C indicates the control group; and t_0 and t_1 represents the pre- and post-treatment periods, respectively.

Both PSM and DD come with identifying assumptions. For propensity score matching to be effective, the treatment and control group must be balanced, post-matching (Rosenbaum and Rubin, 1983). Balance, or conditional independence, is achieved when there are no significant differences in pre-treatment covariates across the matched treatment and control group, except for the treatment itself. In this manner, propensity score matching mimics random assignment (Pearl, 2000).

The primary limiting assumption of the DD approach is that the performance trajectory of the control group ought to reflect what would happen to the treatment group in the absence of the treatment. This ‘parallel trend assumption’ cannot be directly tested, since one cannot observe the evolution of the treatment group absent the treatment; firms are either treated, or they are not. Nonetheless, some confidence regarding parallel trends can be generated by estimating a placebo test, in which, for the same treatment and control groups, PSM and DD results are generated for an earlier time period during which the 'treated' group does not actually receive the treatment. In other words, this approach tests whether there are significant differences in the evolution of a given performance criterion over a prior period in which no actual treatments are assigned. While this does not eliminate the possibility that firms' trajectories shift after this earlier wave, parallel paths in the past provide the best available gauge of the similarity of subsequent pathways across the group of firms that receive dealmakers and its counterfactual.

These represent strong assumptions, but, if satisfied, PSM and DD are strongly complementary. Specifically, with PSM alone, one must assume that observable firm features sufficiently capture the important differences driving selection. And yet, although we know they matter, entrepreneurial characteristics like brand, talent, and hustle are nearly impossible to systematically observe. Fortunately, DD eliminates bias from time-invariant unobserved firm heterogeneity, as well as from broad economic shocks (Blundell and Costa Dias, 2000). This means that, even if we cannot capture the full range of hard-to-measure differences that distinguish more- and less- promising entrepreneurial firms, as long as they are rooted in enduring firm characteristics, we can account for them econometrically. Arguably, many, though not all, important firm characteristics will be relatively stationary. This still leaves potential for confounding on the basis of dynamic unobservable variables. For instance, two firms that have followed parallel trajectories, and that are endowed with identical human, physical and financial assets might still diverge as one makes a sudden and major breakthrough that both shifts their performance path and also draws the attention of a dealmaker. This caveat noted, as compared with prior work, the econometrics used here represent a considerably stronger basis upon which to consider causal effects of social networks.

For each outcome of interest, the basic sequence to be followed is: (1) estimate propensity scores; (2) evaluate matching quality with respect to balance on observables and the degree to which parallel trend assumption is likely to be upheld; (3) to produce difference-in-differences estimates on firms that fall within the common support area. If the assumptions described above can be satisfied, the results ought to efficiently estimate the average treatment effects of those firms that become linked to dealmakers.

IV. Data

Capital IQ, a private database maintained by Standard & Poor's, provides the sampling frame of firms and individual actors. Capital IQ is one of the more comprehensive data sources on private firms available in the United States, capturing those that have received bank, private-equity or venture capital financing. Crucially, these data provide extensive biographical information about firms' management and board members. For simplicity, we will refer to these individuals collectively as 'top teams.' We focus on distinguishing dealmakers and non-dealmakers and constructing regional social networks on the basis of the links between these individuals.

Networks are constructed using top team members associated with firms in two broad industry categories: life sciences and information technology.[1] These are sectors in which local inter-firm interactions, spinoffs and networks are legendarily important (Saxenian, 199; Audretsch and Feldman, 1996a; Feldman, 2000; Owen-Smith and Powell, 2004, Casper, 2007), making them apt sites at which to look for the economic effects of place-based social networks. We build such networks for 12 U.S. regional economies: Austin, Boston, Denver, Minneapolis, Orange County, Phoenix, Portland, Raleigh-Durham, San Diego, San Francisco, Salt Lake City, and Seattle.[2] These regional economies represent the largest spatial concentrations of employment in these activities in the U.S. With these constraints, Capital IQ permits consideration of networks among approximately 85,000 individuals and 22,000 firms. Some degree of completeness is important to the examination at hand; our snapshot of networks should correspond reasonably closely to actual regional networks. One potential problem arising from incompleteness is that certain individuals who we define as being only moderately connected to the network would actually emerge as dealmakers if we captured more of the underlying network. This might blur the lines between our

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[1] Capital IQ defines industries using the Global Industry Classification Standard, which is a set of codes engineered by Standard & Poor's and MSCI to facilitate effective international standardization of industry codes for the purpose of investment research and analysis. We used aggregate industry codes 35 'Health Care' and 45 'Information Technology'. The former includes detailed biotechnology industries, pharmaceuticals, and other related activities. The latter includes software, internet, IT consulting and other subsectors. Detailed listings are available at: http://www.msci.com/products/indexes/sector/gics/

[2] Austin, Portland, San Diego, and Phoenix are defined according for Office of Management and Budget (OMB) Metropolitan Area boundaries; for Orange Country, CA, only the single county is used; the remainder are defined according to Consolidated Statistical Area boundaries.

treatment group and our control group, resulting in greater odds of a false negative. To more confidently describe our networks as complete, the firm list generated by Capital IQ was compared against data from Thomson Financials Venture Xpert, a series that captures firms with similar success at securing financing.

Interlocks among top team members and their firms in these data are used to evaluate the degree to which agents are connected to multiple local firms and therefore involved in the social milieu of a local economy. Our primary definition of a dealmaker follows that of Feldman and Zoller (2012), in which dealmakers have at least three concurrent ties as executives or board members in other firms in the region. As Table 1 makes clear, these multiple roles and interconnections indicate an unusual degree of imbrication in regional networks; using data for 2009, while 90 percent of identified actors are connected to one firm in their location, just over one percent would be classified as a dealmaker. There is some variation from city to city; notably, the San Francisco Bay Area and Boston host a proportionately larger numbers of dealmakers within their absolutely larger regional networks. However, the table shows that broad patterns in the distribution of dealmakers are quite consistent across cities.

Substantively, top team members are expected to play particularly important roles in determining firm performance, and especially in terms of harnessing local social capital. Top management is tasked with the development of the organization, while boards of directors are intended to act independently to advise the executive on strategic direction (Larcker and Tayan, 2011). In the United States, public companies are legally obligated to have a board of directors. Service on boards of directors on public companies is highly regulated; and as a consequence of the Sarbanes-Oxley Act of 2002, members of the board and officers are legally liable for the direction of the firm, as a result of their substantial fiduciary obligation and connection to the firm. Privately-held organizations may also have boards, and these are especially common in biotechnology and other high technology sectors (Lerner, 1995). Board members on private firms have the opportunity to play a larger role in the direction and development of the organization. Board member are typically paid a salary, though commonly one that complements other paid work. Our focus on top team members means that we ignore possible benefits that could arise from changes in firms' workforces outside these upper echelons. We adopt this restriction for practical as well as substantive reasons. Practically, while interlocks across executives and board members represent well-mined and effective input into network-building, there exists no comparable data source available to capture inter-firm interactions among non-elites.

To evaluate outcomes, two waves of Capital IQ data are examined: a pre-treatment wave, collected in December 2009, and a post-treatment wave from December 2012. The criteria for inclusion in the primary analytical sample are that (1) firms have zero attached dealmakers in 2009; (2) that they continue to exist in 2012; (3) that treated firms add exactly one dealmaker and zero non-dealmakers between December 2009 and December 2012; and (4) that control firms add exactly zero dealmakers and one non-dealmaker between 2009 and 2012. Overall, due to attrition arising from the matching process across different datasets, this results in an analytical sample of 540 firms, including 80 firms that become affiliated with a dealmaker over the study period.

Outcomes and Matching Parameters

Outcomes are drawn from Dun & Bradstreet (D&B)'s DUNS Marketing Information database. The 2012 D&B snapshot is drawn directly from D&B. The 2009 snapshot is part of a longitudinal series from 1990 to 2011, sourced from the National Establishment Time Series (NETS), which compiles repeated cross-sections of the underlying D&B employment, sales and other data into a longitudinal series. D&B tracks establishments, not firms, hence identified non-headquarters establishments are dropped from the sample. D&B establishment records are linked to Capital IQ firms through DUNS identification numbers assigned using a proprietary matching and disambiguation algorithm by D&B.

In the analysis below, we consider that dealmakers might influence performance outcomes. Of particular interest are sales and employment. Growth in sales and employment could reflect the influence of dealmakers on the incorporation of new ideas in product or marketing; they could also indicate actual deals made with other firms. Especially in information technology, profit measures are a more imperfect performance indicator, since many firms do not make a profit for a considerable period of time. We also consider sales per employee, as an indicator of changes in productivity owing to process innovations. The rationale behind this is that dealmaker effects might be focused on extracting more value out of limited resources, which might be especially apposite given that the study period coincides with the Great Recession. Dealmaker affiliations could also stimulate liquidity events. These come in three main forms. A firm's immediate corporate parent can change, reflecting an acquisition. It can also merge with another pre-existing firm, or it may shift from privately-held to publicly-listed, with an initial public offering (IPO) of stock. Each of these represent an exit strategy for the entrepreneurial firm, enabling owners and initial investors to yield a financial return in exchange for surrendering or diluting their ownership stake in the company. Finally, we are interested in observing whether there is a relationship between dealmakers and new (and pending) rounds of investment. Unfortunately, we found that only a small number of firms experienced liquidity events or new investments over the study period, and after matching, none of these firms was deemed sufficiently comparable across the treatment and control groups. Hence, in the results below we focus on the association between dealmakers and sales, employment, sales per employee, and acquisitions.

Parameters used to match treatment and control firms should have some predictive power for both selection into the treatment and the outcome of interest. Moreover, they ought to be unaffected by the treatment. To address the former concern, a wide variety of firm characteristics ought to factor into dealmaker affiliation decisions, and these are similarly likely to be related to sales, employment and the other outcomes of interest. On the latter point, the data for matching comes from 2009 and earlier – before the treatment occurs. These data come from D&B, which captures a wide variety of establishment characteristics.[3] Across various outcomes we select a broadly similar group of covariates, including: lagged levels of sales and employment; the quartile of the firm's last three years of sales growth

[3] Unless otherwise specified, data for 2009 is used.

relative to 3-digit SIC peers; detailed industry; metropolitan region; founding year; Paydex and D&B credit scores; legal status; gender of the Chief Executive Officer; ethnic minority ownership; ownership by women; whether the firm has moved more than once between 1990 and 2009; whether the organization engages in government contracting; and importing and exporting activity.

Table 2 presents descriptive statistics for the treatment, as well as for primary outcomes and key matching parameters. Of the analytical sample of 325 firms, just under five percent of firms add one dealmaker over the three-year study period. The average firm in the sample has 72 workers, and has sales of $13 million. The average firm in the sample was started in 1993, thus reflecting not early stage startups but more established going concerns. Most of the firms are incorporated, and just over half engage in some form of international trade. A typical firm in the sample has almost 9 non-dealmaker top team members, including board of directors, and on average these individuals have a total of 9 local affiliations.[4]

V. Results

Table 3 presents difference-in-difference estimates comparing propensity-score-weighted treatment and control groups. Given satisfaction of the identifying assumptions, which we explore in depth below, the result is the average treatment effect on the treated (ATT). In this inquiry this represents estimates of the causal effects of dealmakers on firm sales, employment, sales per employee, and the likelihood of acquisition. Results are estimated only on the common support region, that is, firms in both groups that are deemed sufficiently comparable in terms of pre-treatment covariates (Heckman et al, 1998). Following the 'maxima and minima'

[4] The analytical sample resembles the overall sample drawn from Capital IQ and D&B. In two-sample t-tests across these two samples, there were significant differences in terms of employment and some measures of credit. Sales and pending investments were not dissimilar across the two samples. In most cases, even significant differences were small in absolute terms.

approach (Caliendo and Kopeinig, 2008), a treatment firm is dropped from the common support region and the regression when its estimated propensity score is higher than the maximum or less than the minimum propensity score of the controls. Though, in the current context, this represents a considerable trimming of the analytical sample, there can be no estimation of the treatment effect without it, especially when matching is performed via kernel, as against nearest-neighbor or other methods (ibid). Nonetheless, this raises is the issue of generalizability, to which we return in the conclusion.

The top left panel of Table 3 presents estimates for dealmaker effects on firm sales. In 2009, both treatment and control groups have very similar levels of sales; yet post-treatment, they have evolved quite differently. While sales levels grow for both groups, firms that become affiliated with a dealmaker experience considerably more sales growth as compared to those firms that add one non-dealmaker. The effect, as measured by the ATT, is statistically significant at a 5% level and strikingly large: an increment of just over $13 million in sales. The common support region is relatively narrow, as 9 treatment firms are compared to 22 firms in the control group, signifying that a good number of the overall sample of 80 treatment firms have no analogue in the control group.

The top right panel of Table 3 reports results for the employment outcome. Here, treatment and control groups in the common support region are fairly different in size at the outset, with firms who later become affiliated with a dealmaker being somewhat larger in the pre-treatment period than those that do not. Again, the ATT reveals large, positive and statistically significant dealmaker effects. Employment in firms that receive a dealmaker over the study period grows relatively more. In fact, while control firms add just a handful of workers over the three-year period, the dealmakers stimulate roughly a doubling of the workforces of treated firms.

The bottom left panel of Table 3 reports estimates of the average treatment effect of dealmakers on sales per employee. The rationale for this outcome was that dealmakers could stimulate efficiencies, perhaps leveraged through opportunities to outsource aspects of production previously performed within the boundaries of the firm. Results indicate that firms that get a dealmakers and those that do not share closely comparable levels of sales per employee, in both the pre- and post-treatment period. There is no detectable relationship between becoming affiliated with a dealmaker and changes in sales per employee.

The bottom right panel of Table 3 presents estimates of the causal influence of dealmakers on the likelihood of acquisition. No firms are acquired in 2009, hence values during the pre-treatment period are uniformly zero. By December 2012, 20 percent of treatment firms change their immediate corporate parent, as against only 4 percent of control firms. And although the coefficient on the ATT is large and positive, it has a standard error that is nearly as large; there are no statistically significant effects of dealmakers on this kind of liquidity event.

Overall, these results suggest that dealmakers exert an independent causal effect on the sales and employment of firms with which they become affiliated. Firms that add one dealmaker and zero non-dealmakers outperform closely comparable firms that add one non-dealmaker and zero dealmakers. To the extent that these dealmakers generate such effects through their marshaling of local social networks and social capital, this signals that such local networks do indeed have economic value. The fact that we find no significant results for acquisitions and sales per employee suggest that dealmakers do not chiefly wield influence by generating efficiency gains, nor by catalyzing formal deals in which entrepreneurial firms are acquired.

Robustness & Sensitivity

To have some confidence in interpreting these results as indicating that dealmakers *cause* beneficial changes in firm performance, we need to demonstrate the satisfaction of the conditional independence and parallel trend assumptions. Conditional independence is satisfied if, for observed pre-treatment covariates *x*, the conditional distribution of *x* is the same for both the treatment group and the control group (Rosenbaum and Rubin, 1983). Table 4 reports *t-test* comparisons on the raw (unmatched) and post-propensity-score-matched samples, for each of the four outcomes of interest. To the extent that we observe insignificant p-values on this test for the matched sample, we can conclude that balance has been achieved, affirming the validity of the use of the control group as a counterfactual for the treated.

The evidence presented in Table 4 suggests that the matching procedure achieves balance for each of the outcomes of interest. Mean values of these variables do not vary across the matched sample in a statistically significant manner, despite, at times, highly significant differences observed in the unmatched sample. This means that there are important, pre-existing differences between those firms that become affiliated to dealmakers and those that do not, but, using the covariates listed in Table 4 and their related propensity scores, it is possible to construct a counterfactual in which these differences are no longer significant. The balance reported in Table 4 should raise confidence that the main effects reported in Table 3 are derived from an appropriate comparison between firms whose primary difference is their 'assignment' to treatment.

The second major assumption to be satisfied is the parallel trend condition, requiring that treatment firms would be progressing along a comparable trajectory to control firms in the absence of treatment. This is a strong assumption, and it is never possible to be entirely certain of its satisfaction. However, data from the past can help detect, if not definitively test for a parallel trend.

In Table 5, we report the results of a placebo test, in which, for sales and employment outcomes, the entire sequence of analysis is reproduced for a prior period, 2006 to 2009. Over this period, in actuality, no firms in either the treatment group or the control group receive the treatment.[5] Put another way, we compare whether firms that receive the treatment between 2009 and 2012 have evolved differently from the control group over the previous three years. If treatment and control firms are following a parallel path, we should expect no significant effects of placebo dealmakers on firm performance. If treatment firms are on their own distinct trajectory, the placebo association with a dealmaker will appear to significantly influence the outcome of interest. Table 5 shows that average placebo treatment effects are statistically insignificant, suggesting that, in this earlier period, the sales and employment pathways of the placebo-treatment group and the control group run in parallel.

[5] Owing to lack of available data on acquisitions from this earlier period, it is not possible to conduct the placebo test for this outcome.

Given the narrow common support region, we consider some additional ways to explore the sensitivity of the main results to changes in the treatment and sample. Specifically, we first relax the strictness of the treatment, dropping consideration of changes in non-dealmakers, as well as the number of dealmakers added, such that the treatment becomes going from zero to at least one dealmaker, while control firms simply have zero dealmakers throughout the study period. This results in a sample of 394 treatment firms and 4,082 control firms. Despite the virtues of this larger size, however, the loss in the precision of the comparison results in insignificant findings for all four outcomes of interest. The same holds true when the treatment is further relaxed to include firms that receive at least one dealmaker, regardless of how many dealmakers affiliations are held in 2009.

One possible qualification of the main results is the possibility that dealmakers perform systematically different functions in firms of different ages. Firms in the startup phase might need dealmakers to plug them into the network of talent and ideas, whereas more experienced firms might link with dealmakers with other needs. Much of the literature emphasizes entrepreneurial firms, which can be interpreted as including only those that are in earlier phases of their development. And yet, as the mean values for 'first year of operation' (FIRSTYR) presented in Table 2 indicate, the average treatment and control firm included in the primary analytical sample are more than 15 years old at the start of the study period.

Acknowledging the already small common support region, an additional challenge in exploring this idea is the availability of data about younger firms. Data sources like D&B and Capital IQ tend to privilege older firms, simply because younger firms typically leave much less of a paper trail. Hence while we would like to produce estimates like those in Table 4 for only young firms, we cannot do so. The closest we can come is to use the 'relaxed' treatment described in the previous paragraph, and limit analysis to firms born after a particular cutoff. Even so, the number of relevant observations is small. Two thresholds are explored: a start year of 2005 and later; and more generously, 2002 or later. In the former case, firms are a maximum of 4 years old when the study period begins, in the latter case, seven years. With the 2005 threshold, the result is an analytical sample of 1,596 firms, out of which 476 become affiliated with a dealmaker. Again however, it appears that the imprecision of the comparison yields insignificant results: getting at least one dealmaker over the study period is not significantly associated with changes in sales, employment, sales per worker or acquisition in these younger samples.

VI. Conclusion

Accounts of thriving urban economies, both popular and scholarly, stress the importance of social capital and social networks, yet this idea has defined rigorous quantitative analysis. Academic research has mostly used aggregate data that mask the mechanisms by which social networks may influence the individual companies that make up a local economy. Moreover, most empirical studies have not been designed to account for endogeneity bias, which precludes confident statements about the causal effects of social networks on performance. From both scientific and public policy perspectives, this is inadequate.

We have begun to address these concerns in this paper. We provide a measure of local social capital that links social networks to the top management of firms. Specifically, the analysis has identified highly connected individuals who bridge disparate parts of local social networks through their multiple locally-oriented roles. This paper has then applied a quasi-experimental approach in order to examine what, if anything, happens to firms when dealmakers join the firm as executives and directors. The strength of the empirical test rests on the combination of propensity score matching and difference-in-differences, together yielding an improved counterfactual to account for selection on dynamic observables as well as stationary unobservables.

Based on this approach, we find that dealmakers in the 12 U.S. study regions exert an independent and large causal influence on employment and sales, but have no effect on sales per worker or the likelihood of getting acquired. We interpret this result to mean that dealmakers have an organizing effect on local social capital, yielding specific kinds of benefits for the firms to which they become affiliated. Dealmakers are one way that firms can become better connected in a regional economy, permitting better leverage of regional social capital that promotes firm growth.

This study represents quasi-experimental evidence on the impact of local social capital on regional economies. We hope that further work will extend this approach and explore the many unanswered remaining questions. These include deeper exploration of the relationships between dealmakers and firm age; the potential importance of not just local but also nonlocal links; potential dealmaker effects on other outcomes, including various liquidity events, as well as firm survival; and longer timeframes to explore long-run dealmaker impacts. Given the longstanding interest in the economic value of local social networks, and theoretical and anecdotal focus on highly connected individuals performing brokerage functions, these issues merit further exploration.

Tables

Table 1. Distribution of Local Affiliations Among Agents, December 2009

Region	Number of Agents	Number of Local Affiliations (%)			
		One	Two	Three	Four (Dealmaker)
Austin	3,122	93.0	5.8	0.7	0.5
Boston	15,897	89.4	7.7	1.7	1.2
Denver	4,405	94.8	4.3	05	0.4
Minneapolis	3,656	93.1	5.6	1.0	0.7
Orange County	5,500	95.9	3.8	0.3	0.0
Phoenix	2,583	95.9	3.4	0.5	0.2
Portland	2,025	95.6	3.8	0.4	0.3
Raleigh/Durham	2,520	93.9	5.3	0.6	0.3
Salt Lake City	2,243	93.9	5.1	0.6	0.3
San Francisco	31,221	86.1	9.4	2.5	2.0
San Diego	6,922	91.4	6.6	1.4	0.6
Seattle	5,485	92.2	6.1	1.0	0.7
Mean	7,132	90.1	7.2	1.6	1.1

Note: Actors are identified through positions as executives or members of boards of directors in life sciences and information technology firms, as defined by Capital IQ.

Table 2. Summary Statistics: Analytical Sample in 2009 (N=325)

Variable	Mean	Standard Deviation
Receives treatment 2009-2012	0.046	0.210
Employment	72.31	113.43
Sales ($ millions)	13.76	29.50
Sales ($mil) per Employee	0.172	0.267
Change in corporate parent 2009-2012	0.106	0.309
Number of pending/current investments	2.71	3.15
Three-year sales growth peer (Quartiles 1-4)	2.29	1.34
First year of operation	1993.1	10.67
Number of affiliated non-dealmakers	7.52	5.24
Total non-dealmaker local links	8.56	6.45
DNB rating	2.74	0.674
PayDex maximum	76.52	5.45
PayDex minimum	70.69	9.08
Male CEO (1=male)	0.763	0.43
Government Contracts (1=yes)	0.323	0.47
Minority Owned (1=yes)	0.105	0.31
Women-owned (1=yes)	0.117	0.32
Foreign-owned (1=yes)	0.077	0.27
Moved location more than once (1=yes)	0.268	0.44
International trade (0=none)	0.583	1.09
Legal Status (3=Corporation)	2.912	0.318

Note: Data come from D&B and Capital IQ. All data measured in 2009 unless otherwise specified.

Table 3. Main Estimates of the Effects of Dealmakers on Firm Performance, 2009-2012

	Sales ($ millions)			Employment		
	Control (2)	Treatment (1)	Difference (1-2)	Control (2)	Treatment (1)	Difference (1-2)
Before	5.461	5.810	0.349	63.193	108.00	44.807
	(2.602)	(1.743)	(3.132)	(18.025)	(59.742)	(62.402)
After	8.207	22.384	14.177	69.748	230.545	160.797
	(4.445)	(7.344)	(8.584)	(21.628)	(109.407)	(111.254)
ATT			13.828**			115.990**
			(6.645)			(53.945)
R^2			0.184			0.084
Common Support	22	9		18	11	

	Sales/Employee			Acquisitions		
	Control (2)	Treatment (1)	Difference (1-2)	Control (2)	Treatment (1)	Difference (1-2)
Before	0.105	0.117	0.012	0	0	0
	(0.007)	(0.028)	(0.029)			
After	0.129	0.142	0.013	0.039	0.200	0.161
	(0.015)	(0.026)	(0.030)	(0.043)	(0.132)	(0.139)
ATT			0.001			0.161
			(0.025)			(0.139)
R^2			0.046			0.122
Common Support	16	9		18	10	

Note: ATT stands for average treatment effect on the treated. Inference: *** p<0.01; ** p<0.05; * p<0.1; all estimates produced with standard errors clustered at the firm. Coefficients estimated only for firms in the common support region.

Table 4. Tests of Conditional Independence for Sales, Employment, Sales/Employment and Acquisition Outcomes

	Unmatched	Sales		Employment		Sales/Employee		Acquisition	
Variable	Matched	t	p>t	t	p>t	t	p>t	t	p>t
Employment 2008	U	-0.64	0.524	-0.64	0.524	-0.64	0.524	-0.64	0.524
	M	1.16	0.255	0.45	0.656	0.23	0.823	0.99	0.326
Employment	U	-0.58	0.562					-0.58	0.562
	M	1.86	0.072					1.66	0.105
Sales 2008	U					-0.72	0.474		
	M					-0.02	0.986		
Sales ($mil)	U			-0.74	0.459			-0.74	0.459
	M			0.85	0.399			1.49	0.146
Sales Growth Peer	U	-2.07	0.039	-2.07	0.039	-2.07	0.039	-2.07	0.039
	M	-1.53	0.136	-1.25	0.22	0.52	0.604	-1.37	0.179
Firm start year	U	2.8	0.005	2.8	0.005	2.8	0.005	2.8	0.005
	M	-1.57	0.127	0.46	0.646	0.21	0.837	-1.48	0.148
Male CEO	U	-2.58	0.01	-2.58	0.01	-2.58	0.01	-2.58	0.01
	M	-2.11	0.042	-0.73	0.474	-0.76	0.453	-1.4	0.17
Gov't Contracts	U	-2.4	0.017	-2.4	0.017	-2.4	0.017	-2.4	0.017
	M	1.8	0.081	-0.24	0.812	0.71	0.483	1.36	0.183
Minority owned	U	-1.69	0.091	-1.69	0.091	-1.69	0.091	-1.69	0.091
	M	-1.2	0.237	1.06	0.296	-0.43	0.674	-0.16	0.874
Moved location	U	-0.77	0.441	-0.77	0.441	-0.77	0.441	-0.77	0.441
	M	0.26	0.793	-1.03	0.31	0.02	0.981	0.23	0.819
DNB Rating	U	2.15	0.032	2.15	0.032	2.15	0.032	2.15	0.032
	M	0.76	0.455	-0.15	0.878	0.62	0.543	-0.06	0.956
PayDex Max	U	-1.95	0.052	-1.95	0.052	-1.95	0.052	-1.95	0.052
	M	-2.02	0.051	-0.4	0.692	-1.19	0.245	-1.7	0.097
PayDex Min	U	-2.28	0.023	-2.28	0.023	-2.28	0.023	-2.28	0.023

	M	0.48	0.634	-0.4	0.695	-0.86	0.396	0.41	0.682
Foreign-owned	U	-1.69	0.091	-1.69	0.091	-1.69	0.091	-1.69	0.091
	M	1.05	0.302	0.56	0.581	-0.8	0.429	0.81	0.426
Women-owned	U	-2.1	0.036	-2.1	0.036	-2.1	0.036	-2.1	0.036
	M	0.41	0.684	-0.74	0.464	-0.2	0.843	0.23	0.82
Non-DM	U	2.13	0.033	2.13	0.033	2.13	0.033	2.13	0.033
	M	-0.84	0.406	-0.18	0.856	0.64	0.527	-0.85	0.4
Non-DM Links	U	4.56	0.000	4.56	0.000	4.56	0.000	4.56	0.000
	M	-0.98	0.336	-0.44	0.662	0.27	0.786	-1.01	0.319
No trade	U	2.66	0.008	2.66	0.008	2.66	0.008	2.66	0.008
	M	-1.1	0.281	-0.46	0.648	0.19	0.854	-0.88	0.383
Imports & Exports	U	-1.59	0.112	-1.59	0.112	-1.59	0.112	-1.59	0.112
	M	.	.	.	.	.	.	.	.
Exports only	U	-2.14	0.033	-2.14	0.033	-2.14	0.033	-2.14	0.033
	M	0.85	0.402	-0.22	0.83	0.35	0.73	0.81	0.422
Imports Only	U	-0.8	0.421	-0.8	0.421	-0.8	0.421	-0.8	0.421
	M	0.63	0.535	0.75	0.46	-0.53	0.603	0.4	0.691
Proprietorship	U	2.06	0.04	2.06	0.04	2.06	0.04	2.06	0.04
	M	.	.	.	.	.	.	.	.
Partnership	U	-1.41	0.16	-1.41	0.16	-1.41	0.16	-1.41	0.16
	M	.	.	.	.	.	.	.	.
Corporation	U	0.72	0.472	0.72	0.472	0.72	0.472	0.72	0.472
	M	.	.	.	.	.	.	.	.
Non-profit	U	-0.38	0.703	-0.38	0.703	-0.38	0.703	-0.38	0.703
	M	.	.	.	.	.	.	.	.

Note: Unless otherwise specified, all measures are for values of variables measured at 2009. Each matching procedure also included dummy variables for each of the 12 regional economies and 25 industry classes.

Table 5. Placebo Test Estimates of the Effects of Dealmakers on Firm Sales and Employment, 2006-2009

	Sales ($ millions)			Employment		
	Control (2)	Treatment (1)	Difference (1-2)	Control (2)	Treatment (1)	Difference (1-2)
Before	6.474	10.638	4.164	56.42	53.58	-2.837
	(1.163)	(5.885)	(5.998)	(10.753)	(14.253)	(17.854)
After	7.522	15.234	7.712	61.705	99.33	37.628
	(1.256)	(10.395)	(10.471)	(11.386)	(55.92)	(55.109)
ATT			3.548			40.466
			(4.650)			(42.537)
R^2			0.024			0.032
Common Support	68	11		62	12	

Note: ATT stands for average treatment effect on the treated. Inference: *** p<0.01; ** p<0.05; * p<0.1; all estimates produced with standard errors clustered at the firm. Coefficients estimated only for firms in the common support region.

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