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Dealmakers, Regional Social Capital and Firm Performance

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Abstract

Researchers have long been motivated to understand the links between social networks and regional economic performance. To the extent that networks are rooted in place, the suggestion is that agents in better-connected places will reap higher returns. Although a flood of theoretical and empirical studies explore these topics in the context of regional development, we do not adequately understand what constitutes a better-connected place. This paper seeks to improve this understanding by approaching the problem in two novel ways. First, rather than defining networks in aggregate, it focuses on a particular set of agents within regional economies, known as ?dealmakers:? accomplished actors who are deeply enmeshed in local social capital, and who leverage connections to make things happen. The primary research question to be investigated is: what effect do dealmakers exert upon the entrepreneurial firms with which they become affiliated? The second novelty is methodological: this study adopts a quasi-experimental approach that ought to permit more confident statements about causality. Propensity scores are estimate to permit matching of firms that receive dealmakers to those that do not. These scores are used as weights in a difference-in-differences model, yielding a measure of the causal effect of dealmakers on the performance of affiliated firms. A variety of firm outcomes are considered, such as innovation, employment, and liquidity events. By understanding whether dealmakers' attempts to organize local social capital actually enhances firm performance, this project will fill an important gap in the literature.

Jelcodes:R11,O33

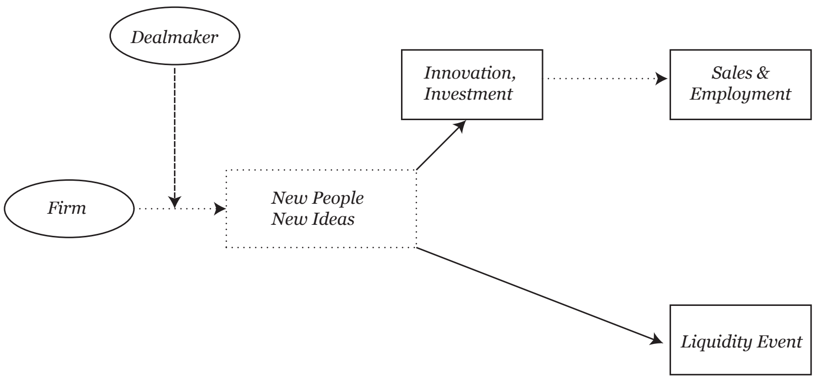
1 Introduction

Since Alfred Marshall's (1890) observations about the circulation and propagation of ideas in English industrial districts, urban-focused economists and economic geographers have been motivated to understand the links between social networks and regional economic performance (Glaeser et al., 1992; Jaffe et al., 1993; Powell et al., 1996; Saxenian, 1996; Casper, 2007; Breschi and Lissoni, 2009). This work also intersects with a profound interest throughout the social sciences in "social capital," a phrase whose constituent terms suggest something of pecuniary value whose realization depends upon community involvement (Bourdieu, 1986; Coleman, 1988; Putnam, 1995). Though social networks reach beyond individual agglomerations (Kenney and Patton, 2005), the myriad virtues of face-to-face contact suggest that by 'community', we frequently mean particular, and especially urban places (Storper and Venables, 2004). To the extent that this geographical regularity holds, the suggestion is that agents in *better connected* places will reap higher returns.

A flood of theoretical and empirical studies explore these topics in the context of regional development, but we do not adequately understand what constitutes a better connected place. In particular, the mechanisms by which local social capital may augment economic performance remain mysterious (Malecki, 2012). Several major obstacles inhibit our understanding. First, despite the work of organizational sociologists who stress the economic relevance of the behavior of individual "brokers" in the network (Hargadon and Sutton, 1997; Burt, 1995, 2004; Stam, 2010), existing studies, especially those using econometric approaches, represent regional network structures in aggregate. Brokers may bridge distinct strands of a network and enable the dissemination of new ideas, but such micro-dynamics are lost when networks are considered as aggregate entities. Relatedly, *better connected* is often taken to mean that a region or local industrial agglomeration has simply more connections, thereby ignoring the functional dimensions of social capital. Moreover, 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 paper seeks to address these gaps. Rather than defining local social capital in aggregate terms, it focuses on a particular set of agents within regional networks, known 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; in short, these are brokers with an observably local orientation. In a recent paper, Feldman and Zoller (2012) identify regional dealmakers in terms of their multiple roles as executives and board members in local firms, 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 dealmakers spur entrepreneurship and may improve regional outcomes. This would represent a discovery with considerable policy and scientific value. Another possibility is that this correlation reflects the reverse causal sequence: vibrant economies could 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 dynamism and the existence of dealmakers; this force could be a specific urban amenity, inbuilt cultural particularity, or something else entirely.

The aim of this paper is to determine whether dealmakers exert an independent causal influence on an array of economic outcomes. Building on the initial research, it shifts focus from aggregate regions to firms. As diagrammed below, the primary hypothesis to be explored in this study is that, *by lowering the costs of making connections and sharing ideas, dealmakers augment the economic performance of the individual firms to which they become connected.*



Two distinct pathways of dealmaker effects are investigated. First, this paper explores whether dealmakers' leverage their connections to influence firm performance, measured in terms of sales and/or employment. As potential mechanisms to produce these changed outcomes, the paper examines links between dealmakers and two distinct intermediate outputs: improved innovation and the infusion of new rounds of investment. Second, dealmakers' nodal positions in regional social networks could affect the trajectory of a firm by stimulating a liquidity event, such as a merger, acquisition, or initial public offering, providing original entrepreneurs and investors with a means of converting their ownership equity into cash.

In order to identify the relationship between these highly networked agents and the performance of individual firms, this study adopts a quasi-experimental research design. A typical least-squares approach, regressing firm outcomes on dealmaker presence, will lead to invalid results in this context: dealmakers ought to match with firms that demonstrate higher performance and that signal continued success, rendering dealmaker affiliations endogenous. For this reason, 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 other resemble those that do (the treatment group). This information is used in a difference-in-differences model, in which the causal effect of dealmakers on firm performance is estimated by observing the difference between the average performance of firms in the treatment and control groups after dealmaker affiliations are made, less the difference in performance before any such treatments are applied. Combining propensity score matching and difference-in-differences, this study controls for both observable firm characteristics that ought to influence the likelihood of getting a dealmaker, as well as static unobserved properties of those firms. This approach has been widely used in labor economics and other disciplines; it has not yet been used to estimate the effects of urban social networks on firm performance.

To carry out this research design, a set of approximately 4,500 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 2012. Of this collection of firms, approximately 400 become linked to a regional dealmaker by 2012. Capital IQ, a private database maintained by Standard & Poor's, provides the sampling frame of firms and dealmakers. Capital IQ is one of the more comprehensive data sources on entrepreneurial firms available in the United States, capturing firms that have received bank, private-equity or venture capital financing. Among its strengths, Capital IQ provides unique biographical detail about their executives and board members, which permits identification of agents who are highly connected to local economic activities. These data are supplemented with a wealth of firm-specific characteristics, such as their involvement in international trade, their patent applications and new product introductions, changes in corporate parents.

By understanding whether dealmakers' attempts to organize local social capital actually enhances the performances of firms to which they are connected, this project will fill an important gap in the literature. Additionally, a better understanding of the role of dealmakers may offer opportunities to intervene effectively in lagging regions.

2 Approach

This paper considers that, when a firm becomes affiliated with a dealmaker, it receives a treatment that may elicit certain kinds of behavior. There are several ways to evaluate the potential effects of such a treatment. One could, for instance, observe how the performance of firms changes before and after the treatment. But, irrespective of any causal dealmaker effects, a simple unobserved time trend in the performance outcome or an economy-wide shock could explain this result. One might also seek to compare firms that receive the treatment of linking to a dealmaker to a control group of firms for which no such links are made. This method, however, risks assigning explanatory value to dealmakers that instead 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 also bear upon their performance. Put simply, dealmakers ought to be drawn to firms that have demonstrated success, or show great promise to succeed. This selection process between dealmakers and firms would bias conventional regression approaches comparing firms that have dealmakers to those that do not.

To address these issues, this study combines the difference-in-differences (DiD) approach with kernel-based propensity score matching (PSM) techniques. The 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. If the conditional independence assumption is satisfied, there should be no significant differences between the matched treatment and control group in terms of these observed characteristics, except for the treatment itself. Propensity scores are then used as weights in the difference-in-differences model, indicating how relevant each firm in the control group is in relation to each firm that receives the treatment. The difference-in-differences estimator compares changes in firm performance between pre-and post-treatment periods across the treatment and control groups, as follows:

$$\hat{a}_{DiD} = (\bar{Y}^T_{t1} - \bar{Y}^C_{t1}) - (\bar{Y}^T_{t0} - \bar{Y}^C_{t0}) \quad (1)$$

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 $t0$ and $t1$ represents the pre- and post-treatment periods, respectively. The primary limiting assumption of the DiD 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, in that it is impossible for the treatment group to simultaneously receive, and not receive the treatment. However, successful matching using propensity scores means that, at least at one time period, the two groups are not statistically different from one another. Though similarity at one point does not demand similarity in trajectories, firms that are similar at one cross section are far more likely than those that are different to follow similar paths going forward.

PSM and DiD are complementary in other ways as well. 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, when combined DiD and PSM techniques eliminate bias from any 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 stable over time. This still leaves potential for confounding on the basis of dynamic unobservables. Though bias from source cannot be fully eliminated, Section 4 reports on a number of steps taken to minimize it.

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, defined as the lowest propensity score of a treatment observation and highest propensity score of a control observation.

3 Data

3.1 Defining Dealmakers and the Analytical Sample

Capital IQ, a private database maintained by Standard & Poor's, provides the sampling frame of firms and dealmakers. 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. Interlocks among board members, executives and their firms in these data are used to evaluate the degree to which agents are connected to multiple firms and therefore involved in the social milieu of an entrepreneurial economy. As in Feldman and Zoller (2012), dealmakers are defined as individuals who, in addition to their primary roles in finance or non-finance firms, have at least three concurrent ties as executives or board members in other firms in the region. These multiple roles and interconnections indicate an unusual degree of imbrication in regional networks; in Feldman and Zoller's (2012) original sample, while 90 percent of over 85,000 identified actors are connected to one firm in their location, less than two percent have three or more such links.

Firms are drawn from two broad industry categories: life sciences and information technology; firm headquarters must be located in one of 12 U.S. regional economies: Austin, Boston, Denver, Minneapolis, Orange County, Phoenix, Portland, Raleigh-Durham, San Diego, San Francisco, Salt Lake City, and Seattle.[1] These spatial and sectoral constraints are imposed by the real-time nature of the underlying data; Capital IQ provides no repeated cross-sections of its database. Hence, this study relies on the original sample of firms taken from Capital IQ by Feldman and Zoller, and matches it with a snapshot of these same firms three years later. Nonetheless, information technology and life sciences are industries in which inter-firm interactions, spinoffs and networks are legendarily important, and the regional economies included in the sample represent the largest concentrations of these activities in the U.S. While these constraints, especially in terms of industrial activity, may limit generalizability, results ought to be considered indicative for other sectors in which networks and inter-firm knowledge transfer are important.

To evaluate outcomes, two waves of Capital IQ data are examined: a pre-treatment wave, collected in December 2009, and a post-treatment wave

[1] 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.

from December 2012. The criteria for inclusion in the primary analytical sample is that (1) firms have zero attached dealmakers in 2009, and (2) that they continue to exist in 2012. The treatment group consists of those firms satisfying these criteria that then establish a dealmaker link between 2009 and 2012. The control group consists of those firms that continue to be unaffiliated with dealmakers by December 2012. Overall, this results in an analytical sample of 4,476 firms, including 394 firms that become affiliated with a dealmaker over the study period.

3.2 Outcomes and Matching Parameters

Outcomes are drawn from Dun & Bradstreet'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 database into a longitudinal series. D&B tracks establishments, not firms, hence identified non-headquarters establishments are dropped from the sample. D&B establishments are linked to Capital IQ firms through DUNS numbers, assigned using a proprietary matching and disambiguation algorithm by D&B.

The primary bottom-line outcomes of interest are employment and sales. These reflect basic aspects of firm performance that dealmakers could affect. Moreover, especially in information technology, profit measures are a more imperfect indicator of performance. Liquidity events result from dealmaker-enabled connections between the firm and other actors in the regional social network, and represent an exit strategy for the entrepreneurial firm by which owners and initial investors yield a financial return in exchange for surrendering or diluting their ownership stake in the company. These come in three main forms, each documented in Capital IQ. 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 a privately-held to a publicly-listed firm, with an initial public offering (IPO) of stock.

Intermediate outputs aim to capture dimensions of innovation and investment. We focus on two measures of innovative outputs: patent applications and new product introductions. United States Patent and Trademark Office (USPTO) data on patent applications comes from Delphion, which provides full bibliographic text, images and other components of all patent applications made between 2001 and the present. Individual patents and patent applications include an assignee, reflecting the organization or individual that owns the property rights conferred by the patent. For instance, while Steve Jobs and others are listed as inventors on U.S. patent number 7,479,949, which describes the invention of a touch screen that would be used in the original iPhone, the assignee for this patent is Apple Inc. We focus on applications rather than granted patents, since the study period may be too short to observe new ideas completing the process of approval. While many new ideas and products are not patented, they may still generate economic value. The patent data is supplemented with information on new product introductions, offered by ThomasNet. Capital IQ also tracks the number of current and pending bank, private or venture capital investments. An increase in the number of current or pending investments is taken to signify new round(s) of investment.

— Data on IPOs, patent applications, and new product introductions presently being collected —

Parameters used to match treatment and control firms come primarily from D&B. Selected variables 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 latter of these concerns, the data for matching comes from 2009 and earlier – before the treatments occur. On the former point, 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. Data for the matching process come from D&B, which provides a wide variety of firm characteristics.[2] Across various outcomes we select a broadly similar group of covariates, including: pre-2009 levels of sales and employment; the quartile of the firm's last three years of sales growth relative to 3-digit SIC peers; detailed industry; 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; government contracting; import, exporting or both.

4 Results

Table 1 presents descriptive statistics for the treatment, as well as for primary outcomes and outputs of interest. Nine percent of firms become linked to a dealmaker over the three-year study period. The information technology and life sciences firms that constitute this sample experience considerable employment growth, expanding, on average, by a factor of four. This is

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

likely to be partly due to the fact that firms that do not survive the three year period are eliminated from the sample. Additionally, the large growth in mean employment is a function of large positive skew in 2012: mean employment growth is approximately 150 workers, while the median is only 22. Firms in the sample also experience considerable expansion in total sales. There is less dynamism in terms of the number of current and pending investments, with an average of somewhat more than two in both periods. One in ten firms are directly acquired.

Table 1: Descriptive Statistics

Variable	Period	Observations	Mean	SD
Dealmakers	2012	4,476	0.09	0.28
Employment	2009	4,072	48.5	90.4
	2012	4,476	204.2	2,283.7
Sales ($mil)	2009	4,092	16.1	296.1
	2012	4,476	53.2	1293.3
Investment Rounds	2009	942	2.2	3.4
	2012	1,094	2.4	4.2
Patent Applications	2009			
	2012			
New Product Introductions	2009			
	2012			
Δ Immediate Parent	2009-12	4,476	0.10	0.31
Merger	2009-12			
Initial Public Offering	2009-12			

Data on patent applications, new product introductions, mergers, and IPOs being presently collected

4.1 Matching Procedure

Table 2 reports on the quality of the matching process undertaken for the estimates produced for the main employment outcome. The goal in the matching process is to achieve balance, such that, for observed covariates x, the conditional distribution of x is the same for both the treatment group and the control group (Rosenbaum and Rubin, 1983). Given satisfaction of this conditional independence and the parallel trend assumption described above, estimates produced ought to reflect the causal effect of dealmakers on those firms that become affiliated with them.

Because of differences in each estimate's common support region, somewhat different samples are used for different outcomes; however, most samples are nearly identical to the one reported below, and balance has been achieved using broadly the same set of matching covariates. The table reports means for each variable before and after matching, as well as two diagnostics that indicate matching quality. First, percent bias is presented, whose difference indicates the amount of bias is reduced as a result of the matching process. Second, and more importantly, balance is estimated using two-sample t-tests to evaluate the equality of means across the treatment and control groups for both the unmatched sample, as well as the propensity-score weighted covariates in t_0 and t_1. It is the latter of these two estimates that indicates whether balance has been achieved. Practically, significant p-values on this test for the weighted matched sample would indicate imbalance across these two subsamples, calling into question the validity of the use of the control group as a counterfactual.

The evidence presented in Table 2 suggests that balance is achieved in the matching procedure. Mean values of these variables vary insignificantly across the matched sample, despite highly significant differences in the unmatched sample. That is, there are important, raw pre-existing differences between those firms that become affiliated to dealmakers and those that do not, but, using the covariates listed in Table 2 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 2 should raise confidence in our ability to evaluate the average effect of dealmakers on the firms to which they become linked.

Table 2: Match Quality Indicators

	Unmatched/	Mean		%	t-test	
	Matched	Treated	Control	Bias	t	p
emp08	Unmatched	40.39	49.08	-10.40	-2.37	0.018
	Matched	53.03	51.65	1.60	0.17	0.864
firstyr	Unmatched	1999.10	1993.70	46.50	11.30	0.000
	Matched	1997.00	1996.10	7.30	1.26	0.207
gendceo	Unmatched	0.67	0.67	-1.20	-0.32	0.745
	Matched	0.78	0.79	-1.00	-0.13	0.895
govt	Unmatched	0.17	0.21	-11.00	-2.84	0.004
	Matched	0.28	0.28	0.20	0.02	0.984
min	Unmatched	0.04	0.06	-11.50	-2.81	0.005

	Matched	0.07	0.06	1.70	0.17	0.868
mover	Unmatched	0.16	0.18	-4.20	-1.12	0.262
	Matched	0.30	0.31	-3.80	-0.36	0.721
paydexmax09	Unmatched	76.54	76.21	4.60	1.00	0.316
	Matched	76.46	76.31	2.10	0.29	0.770
ownership	Unmatched	0.04	0.07	-12.30	-3.02	0.002
	Matched	0.04	0.05	-3.00	-0.38	0.703
paydexmin09	Unmatched	70.43	70.51	-0.60	-0.14	0.892
	Matched	69.56	69.81	-2.10	-0.24	0.807
salesmil	Unmatched	85.92	30.64	4.40	1.51	0.131
	Matched	155.98	15.25	11.20	0.99	0.321
salesgrowth r	Unmatched	2.04	2.30	-22.00	-5.73	0.000
	Matched	1.99	2.02	-2.40	-0.26	0.793
credit09	Unmatched	2.92	2.78	15.40	2.66	0.008
	Matched	2.93	2.93	-0.10	-0.01	0.988
wmnown	Unmatched	0.04	0.08	-15.90	-3.82	0.000
	Matched	0.08	0.08	1.00	0.10	0.918
city1	Unmatched	0.38	0.21	36.60	10.57	0.000
	Matched	0.35	0.33	4.60	0.51	0.612
city2	Unmatched	0.09	0.07	5.20	1.45	0.146
	Matched	0.10	0.09	1.40	0.15	0.878
city3	Unmatched	0.09	0.09	-0.30	-0.08	0.938
	Matched	0.07	0.08	-2.40	-0.30	0.767
city4	Unmatched	0.01	0.04	-20.80	-4.48	0.000
	Matched	0.00	0.00	0.00	.	.
city5	Unmatched	0.01	0.04	-17.40	-3.87	0.000
	Matched	0.01	0.01	0.40	0.08	0.935
city6	Unmatched	0.01	0.04	-17.80	-3.94	0.000
	Matched	0.01	0.02	-2.80	-0.38	0.707
city7	Unmatched	0.05	0.10	-21.10	-4.98	0.000
	Matched	0.05	0.06	-1.50	-0.20	0.842
city8	Unmatched	0.01	0.05	-20.60	-4.56	0.000
	Matched	0.01	0.02	-2.50	-0.38	0.705
city9	Unmatched	0.05	0.07	-8.90	-2.24	0.025
	Matched	0.05	0.05	1.10	0.14	0.892
city10	Unmatched	0.03	0.08	-23.40	-5.30	0.000
	Matched	0.04	0.04	-0.60	-0.08	0.938
city11	Unmatched	0.25	0.18	17.90	5.06	0.000
	Matched	0.26	0.27	-2.50	-0.27	0.788
city12	Unmatched	0.04	0.05	-2.60	-0.67	0.503

	Matched	0.04	0.03	1.50	0.18	0.854
industry1	Unmatched	0.01	0.01	-2.50	-0.63	0.528
	Matched	0.01	0.00	2.70	0.37	0.714
industry2	Unmatched	0.19	0.19	-0.20	-0.05	0.959
	Matched	0.19	0.19	-1.20	-0.14	0.891
industry3	Unmatched	0.07	0.03	18.90	6.10	0.000
	Matched	0.07	0.06	3.00	0.31	0.757
industry4	Unmatched	0.05	0.05	0.00	-0.01	0.993
	Matched	0.13	0.13	-0.10	-0.01	0.993
industry5	Unmatched	0.01	0.02	-11.30	-2.57	0.010
	Matched	0.02	0.02	-1.90	-0.19	0.852
industry6	Unmatched	0.01	0.01	3.90	1.13	0.258
	Matched	0.02	0.02	0.20	0.02	0.985
industry7	Unmatched	0.01	0.02	-9.50	-2.20	0.028
	Matched	0.00	0.01	-6.20	-0.95	0.341
industry8	Unmatched	0.01	0.03	-15.10	-3.36	0.001
	Matched	0.02	0.02	1.30	0.13	0.896
industry9	Unmatched	0.03	0.05	-12.70	-3.06	0.002
	Matched	0.03	0.02	4.60	0.62	0.537
industry10	Unmatched	0.01	0.02	-9.70	-2.24	0.025
	Matched	0.01	0.02	-4.80	-0.54	0.591
industry11	Unmatched	0.05	0.04	4.90	1.37	0.172
	Matched	0.03	0.03	-1.00	-0.14	0.888
industry12	Unmatched	0.11	0.07	14.50	4.26	0.000
	Matched	0.13	0.12	2.20	0.23	0.821
industry13	Unmatched	0.02	0.01	6.90	2.12	0.034
	Matched	0.01	0.01	0.90	0.10	0.919
industry14	Unmatched	0.01	0.01	-9.40	-2.15	0.032
	Matched	0.01	0.01	0.60	0.06	0.952
industry15	Unmatched	0.03	0.10	-29.10	-6.50	0.000
	Matched	0.05	0.06	-1.50	-0.18	0.856
industry16	Unmatched	0.01	0.01	-4.10	-1.02	0.307
	Matched	0.00	0.00	2.60	0.60	0.547
industry17	Unmatched	0.23	0.16	19.30	5.52	0.000
	Matched	0.10	0.09	0.70	0.10	0.916
industry18	Unmatched	0.01	0.02	-7.20	-1.73	0.083
	Matched	0.01	0.01	0.60	0.11	0.916
industry19	Unmatched	0.00	0.00	-6.60	-1.32	0.187
	Matched	0.00	0.00	-0.40	-0.19	0.853
industry20	Unmatched	0.04	0.03	3.00	0.83	0.408

	Matched	0.03	0.04	-1.40	-0.17	0.868
industry21	Unmatched	0.01	0.01	-3.30	-0.82	0.412
	Matched	0.01	0.01	-2.60	-0.26	0.793
industry22	Unmatched	0.03	0.02	4.50	1.29	0.197
	Matched	0.03	0.04	-2.30	-0.22	0.830
industry23	Unmatched	0.05	0.05	1.80	0.49	0.627
	Matched	0.06	0.06	1.50	0.16	0.869
industry24	Unmatched	0.01	0.03	-17.60	-3.78	0.000
	Matched	0.00	0.01	-3.80	-0.71	0.479
industry25	Unmatched	0.01	0.00	3.40	1.01	0.311
	Matched	0.01	0.01	1.00	0.10	0.919
importexp1	Unmatched	0.87	0.77	26.80	6.60	0.000
	Matched	0.73	0.74	-2.00	-0.20	0.840
importexp2	Unmatched	0.01	0.02	-8.10	-1.95	0.051
	Matched	0.02	0.02	-0.20	-0.02	0.987
importexp3	Unmatched	0.03	0.09	-23.80	-5.47	0.000
	Matched	0.07	0.07	-1.20	-0.13	0.900
importexp4	Unmatched	0.09	0.12	-11.60	-2.92	0.003
	Matched	0.18	0.17	3.50	0.33	0.744
legstat1	Unmatched	0.02	0.02	-2.20	-0.51	0.610
	Matched	0.00	0.00	0.00	.	.
legstat2	Unmatched	0.06	0.10	-14.00	-3.14	0.002
	Matched	0.08	0.08	1.30	0.15	0.879
legstat3	Unmatched	0.93	0.88	14.60	3.29	0.001
	Matched	0.92	0.92	-1.20	-0.15	0.879
legstat4	Unmatched	0.00	0.00	-7.30	-1.31	0.190
	Matched	0.00	0.00	0.00	.	.

The second major matching 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 difficult if not impossible to be entirely certain of its satisfaction. However, data from the past can help explore, if not definitively test it. To do so, I perform a 'placebo test,' in which the entire sequence of analysis is reproduced for a prior period (2006 to 2009), but for the fact that, rather than estimating the treatment effect for a set of firms that actually receive the treatment of affiliating with a dealmaker between 2006 and 2009, the treatment group is defined as those that actually receive a treatment in the subsequent, actual study period, 2009–2012. Hence, the 'treatment' firms for 2006–2009 receive a placebo dealmaker, since, in reality, they receive no such linkage until after 2009. If treatment and control firms are following a parallel path, we should expect no significant effect 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. The results of the placebo test are reported in Table 3. The average treatment effect is far from significant, suggesting that, in this earlier period, the employment pathways of the placebo-treatment group and the control group run in parallel. This still leaves open the possibility that they diverge only over the actual study period. For instance, treatment firms might make a technical or other kind of breakthrough between the pre- and post-treatment waves of data collection, and this event, not linking to a dealmaker, could produces change in employment or other outcomes of interest.

Table 3: The Placebo Test: Exploring the Parallel Trend Assumption

Outcome		Pre- (2006)	Post- (2009)
Employment	Treatment Group	59.8	72.7
	Control Group	61.0	65.3
	ATT		**8.6**
			(38.4)
	Treatment Obs.		91
	Control Obs.		1,366

Note: First two rows of each outcome present mean outcome observed in the given time period. ATT stands for average treatment effect on the treated. Standard errors in parentheses. *, **, *** indicate statistical significance at the 10, 5 and 1% levels, respectively. Estimates produced using observations within common support area only.

This possibility cannot be entirely eliminated, but it can be minimized with detailed that captures with precision the moment in which links between dealmaker and firms are forged. With such data, rather than estimating the effects of a treatment that occurs at some point between December 2009 and December 2012, it will be possible to estimate dealmaker effects in the year that the link is established, as well as (if the link occurs early enough in the three-year study period), one and two years afterward. This strategy offers a number of benefits. First, it permits more careful identification of the timeline of any observed causal effects. Second, it minimizes the heterogeneity of the treatment group, eliminating the possibility that the treatment group contains links forged in early 2010 as well as late 2012. Third, by including more granular, annual information on firm performance during the overall study period, it can reduce bias from the possibility of diverging parallel trends between 2009 and 2012. *These data are currently being collected, hence the estimation below represents preliminary results in which treatments could be distributed throughout the three-year study period. The results below will be supplanted by estimates produced using the more detailed dealmaker linkage data.*

4.2 Main Estimates

The first difference-in-differences estimates to be reported are those for outcomes that represent aspects of firms' bottom-lines: overall employment and sales. Although estimates produced without standard errors clustered at the firm level indicate a highly significant positive relationship between dealmakers and both sales and employment, individual observations are nested in firms. In the absence of clustering, the assumption of uncorrelated error terms is violated (Bertrand et al., 2004); Table 4 therefore presents results with clustered standard errors. Once the problem of the grouped error term is accounted for, dealmakers are not longer significantly related to either overall employment growth, or to sales.

It is also possible that, under some conditions, dealmakers could advise their affiliated firms to trim employment in order to become more productive. That is, dealmakers could be heterogeneous in terms of their approach, perhaps on the basis of firm or market characteristics, or even due to the dealmaker's background. If some subset of dealmakers improve performance, leading to employment growth, while others seek productivity enhancements achieved in part through reductions in employment, an insignificant relationship between dealmakers and employment may conceal a more complex, but nonetheless causal relationship. This idea is explored in the bottom panel of Table 4, which reports the average treatment effect on the treated in terms of change in sales per employee. If dealmakers affect employment through either trimmed employment associated with a broader push toward greater efficiency, or by adding more employees to achieve greater overall success, one might expect a positive influence of dealmakers on the amount of sales per employee. Yet, as with the other results in the table, dealmaker affiliations demonstrate no significant effect.

Table 5 reports the estimates of the relationship between linking to a dealmaker and receiving new rounds of investment, using Capital IQ's counts of 'current and pending investments.' Results suggest that dealmaker affil-

Table 4: The Effects of Dealmakers on Firm Sales and Employment

Outcome		Pre- (2009)	Post- (2012)
Employment	Treatment Group	61.1	507.1
	Control Group	59.3	135.2
	ATT		**370.1**
			(393.4)
	Treatment Obs.		134
	Control Obs.		1,485
Sales	Treatment Group	11.3	299.4
	Control Group	10.8	24.2
	ATT		**274.8**
			(281.9)
	Treatment Obs.		134
	Control Obs.		1,479
Sales/Employment	Treatment Group	0.19	0.15
	Control Group	0.16	0.20
	ATT		**-0.08**
			(0.07)
	Treatment Obs.		134
	Control Obs.		1,792

Note: First two rows of each outcome present mean outcome observed in the given time period. ATT stands for average treatment effect on the treated. Firm-level clustered standard errors in parentheses. *, **, *** indicate statistical significance at the 10, 5 and 1% levels, respectively. Estimates produced using observations within common support area only.

iations are unrelated to the number of investment rounds a firm receives over the study period. It should be noted that this field has considerably more missing data than the average obtained from D&B. As a result of this and the constraints of the common support area, the trajectories of only 35 treatment firms and 211 control firms are considered. It is possible that these limits prohibit effective detection of any existing relationships.

Table 5: The Effects of Dealmakers on Investment Rounds

Outcome		Pre- (2009)	Post- (2012)
Investment Rounds	Treatment Group	2.3	2.9
	Control Group	1.9	2.3
	ATT		**0.248**
			(0.319)
	Treatment Obs.		35
	Control Obs.		211

Note: First two rows of each outcome present mean outcome observed in the given time period. ATT stands for average treatment effect on the treated. Firm-level clustered standard errors in parentheses. *, **, *** indicate statistical significance at the 10, 5 and 1% levels, respectively. Estimates produced using observations within common support area only.

Table 6 presents results from liquidity event outcomes. Data on IPOs and mergers are currently being collected. The top panel of the table presents estimates of the effect of dealmaker links on changes in corporate parent. Linking to a dealmaker is not significantly related to the likelihood of being acquired by a new corporate parent.

One possible challenge to the non-significant results shown in Tables 4-6 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 emphasises 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 ten years old. It is possible that dealmakers could produce different kinds of effects in such firms than those outcomes being investigated in this paper.

Given a limited sample of firms (and the fact that very young firms

Table 6: The Effects of Dealmakers on Firm Liquidity Events

Outcome		Pre- (2009)	Post- (2012)
New Corporate Parent	Treatment Group	0.0	0.13
	Control Group	0.0	0.10
	ATT		**0.028**
			(0.030)
	Treatment Obs.		133
	Control Obs.		1,474

Note: First two rows of each outcome present mean outcome observed in the given time period. ATT stands for average treatment effect on the treated. Firm-level clustered standard errors in parentheses. *, **, *** indicate statistical significance at the 10, 5 and 1% levels, respectively. Estimates produced using observations within common support area only.

do not receive the kinds of financing required for inclusion in Capital IQ in large numbers), it is difficult to limit the analytical to younger firms. As a compromise, the definition of the treatment is relaxed, such that a firm is described as identified as receiving the treatment if it links with at least one dealmaker over the study period (rather than limiting only to firms that go from zero to one dealmaker). This provides a larger sample with which to focus on startups. Two cutoffs are explored: a start year of 2005 and later; and more generously, of 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. Table 7 presents results estimated on this sample for the employment outcome. The coefficient on employment is not statistically significant. Moreover, the actual number of firms in the common support region – for both treatment and control firms – becomes quite small. In other words, there is considerable heterogeneity between these treatment and control startups that cannot be accounted for using observed variables. For those that can be compared, dealmakers have no effect. This holds true for all other outcomes. The same procedures are run for the sample of firms whose first year is at least 2002, which produces an analytical sample of 3,540, out of which 916 receive a dealmaker. While this produces larger samples of treatment and control firms within there common support area (122 and 252, respectively), becoming affiliated with a dealmaker remains insignificantly related to changes in employment. Nor is it related to any of the other outcomes examined in this paper.

Table 7: The Effects of Dealmakers on Employment: Startups Only (First Year>=2005)

Outcome		Pre- (2009)	Post- (2012)
Employment	Treatment Group	20.4	55.44
	Control Group	18.8	51.2
	ATT		**2.6**
			(19.3)
	Treatment Obs.		27
	Control Obs.		69

Note: First two rows of each outcome present mean outcome observed in the given time period. ATT stands for average treatment effect on the treated. Firm-level clustered standard errors in parentheses. *, **, *** indicate statistical significance at the 10, 5 and 1% levels, respectively. Estimates produced using observations within common support area only.

5 Conclusion

Popular and scholarly accounts of thriving urban economies have often stressed particular qualities of their social networks, whether the size of networks, or more recently, their structure. The idea that regional social networks and social capital matter is at this point a matter of folklore, inasmuch as it is a serious research topic. Yet much of the scholarly research has considered these topics using aggregate data that risk turning networks into another black box. Moreover, most studies have been designed in ways that do not adequately overcome the many endogeneity problems that preclude confident statements about any causal effects of networks on performance. From both scientific and public policy perspectives, this is inadequate.

This paper begins to address these concerns. Applying a quasi-experimental approach, this paper has sought to explain what, if anything, happens to firms when they link to 'dealmakers': insiders who bridge disparate parts of local social networks through their multiple locally-oriented roles as executives and on boards-of-directors. In the most robust specifications, dealmakers in the 12 U.S. study regions do not appear to exert any independent causal influence on employment, sales, investment, and changes in corporate parent. Data on innovative outcomes, as well as mergers and initial public offerings is currently being collected.

The results – still preliminary– presented in this paper can be interpreted in two distinct ways. One possibility is that they confirm an underlying truth about the role of these locally-oriented agents who span networks. That is, this study's findings could reveal that dealmakers have no organising effect on local social capital, and yield no benefits for the firms to which they become affiliated. In this view, the theory asserting the importance of these network insiders could be flawed.

A second possibility is that the dealmakers concept reflects some underlying truth, but its operationalisation in this research need improvement. Along these lines, the following possibilities are considered:

- *Right concept, but too much emphasis on local links:* It may be that the ability to introduce new ideas and connect people may be less of a function of strictly local links, and instead more closely related to total links, whatever their geography. Hence, the number and range of extra-local links can be estimated for each dealmaker (and indeed each agent in the network), such that this additional information can be accounted for.

- *Right concept, too much focus on highly connected individuals:* The ability of a firm to leverage network links may be less of a function of single highly connected dealmakers, and more of a collective phenomenon, rooted in the total links available throughout a firm's executives and board members. This can also be empirically explored given the available data.

- *Right concept, wrong timeline I:* As described above, greater precision on the actual date at which affiliations are made should reduce endogeneity concerns (though they may not alter insignificant findings). More broadly, there is little theoretical guidance on how long it may take to produce hypothesised dealmaker effects. It could be that a longer timeframe is needed to observe such outcomes, though longer gaps between treatment and outcome also risks introducing more unobserved variation. Data on outcomes and the network will be gathered on an annual basis.

- *Right concept, wrong timeline II:* The role of the Great Recession, occurring precisely during the study period, may influence findings. This suggests a possible disjuncture between social networks and social capital. When dealmakers affiliate with firms they may bring their network, but may be unable to mobilise the resources that, in more robust moments in the business cycle, would produce the hypothesised benefits.

- *Right concept, wrong sample:* Much of the lore about social networks, especially in fields like information technology and biotechnology, is focused on startups. Yet the average age in the main sample is over ten years old, and average pre-treatment firm size is nearly 50. At the least, these firms are 'gazelles', but they may or may not quite reflect the scrappy upstarts that occupy the imagination. Refocusing on smaller firms could clarify the relationship, though this presents its own challenges, in terms of both sample size, as well as the likelihood that the kinds of highly-connected agents performing dealmaker-like network functions.

- *Right concept, wrong outcomes:* Dealmakers could exert a causal influence on firms to which they become affiliated, but in terms of outcomes that have not been considered. For instance, the sample as constructed selects on firms that survive in both periods, even though one of the ways that dealmakers may improve performance is by raising the likelihood of firm survival. Adjusting the sample to include firms that exit between 2009 and 2012, the effects of dealmakers on firm survival could be estimated.

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.

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