# DIRECTOR HISTORIES AND THE PATTERN OF AQUISITIONS

by

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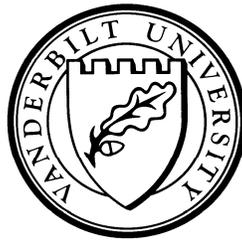
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# Director Histories and the Pattern of Acquisitions

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October 5, 2011

### Abstract

We trace directors through time and across firms to study whether acquirers' exposure to non-public information about potential targets through board service histories affects the market for corporate control. In a sample of publicly-traded U.S. firms from 1996 through 2006, we find that acquirers are about five times more likely to buy firms at which their directors once served. These effects are stronger when the acquirer has better corporate governance, the interlocked director has a larger ownership stake at the acquirer, or the director played an important role during past service at the target. The findings are robust to endogeneity of board composition and to controls for network connectivity and conventional inter-firm interlocks.

KEYWORDS. Interlocking directorates; board networks; social networks; corporate governance.

*JEL* CLASSIFICATION NUMBERS: G34.

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It is well-known in finance and economics that firms possess private information about their own operations and fundamental values.[1] We contribute to this literature by examining, in the context of the market for corporate control, how the transmission of non-public information about potential targets influences mergers and acquisitions. This is interesting because, despite extensive research on the determinants of acquisitions, we can still only imperfectly predict which firms will choose to initiate acquisitions and, for those that do, how they choose among potential targets.

We capture a potential acquirer's exposure to target-specific non-public information by tracking the service histories of its directors over time. This is reasonable because the theoretical literature on boards of directors (Adams and Ferreira (2007), Harris and Raviv (2008), Song and Thakor (2006)) and survey evidence summarized in Adams, Hermalin, and Weisbach (2010) both indicate that most directors see themselves as playing an active role in setting corporate strategy (see also Demb and Neubauer (1992), pp. 43-44). It is therefore not surprising that SEC merger documents offer countless cases confirming the view that directors play an active role in acquisitions.[2]

Specifically, if a given firm has a current director who formerly sat on the board of another firm but who no longer serves there, we say that the two firms have a "historical interlock," and propose that these interlocks confer information about the firm where the current director once served. For example, A. Clinton Allen served as a director at Psychemedics Corporation, Inc. from 1989 to 2002. After leaving Psychemedics, he joined Brooks Automation, Inc. in 2003, where he has been on the board ever since. In this case, we would consider Brooks Automation to be historically interlocked with Psychemedics from 2004 to the present, but not prior to that. At the same time, given that no current director of Psychemedics had ever served on the board of Brooks Automation, Psychemedics would not have a historical interlock with Brooks.

In this paper we estimate the impact of historical interlocks on the decision to initiate acquisitions of linked firms. In particular, our main analysis considers whether a *specific*

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[1] See, for example, Myers and Majluf (1984), Travlos (1987), and Eckbo, Giammarino, and Heinkel (1990).

[2] For example, the S-4 form submitted in 2003 by Plug Power (a fuel cell system manufacturer operating mainly in Illinois, Indiana, Michigan and Ohio) in regard to its acquisition of H Power (a fuel cell developer and manufacturer) reports that during the Fall of 2001 and Spring of 2002 the board established a special committee of three directors to consider potential acquisitions. This committee oversaw the process that eventually led to the November 11, 2003 announcement that Plug Power intended to acquire H Power.

historical interlock from an acquirer to a potential target increases the probability that the two firms merge, and we find in a sample of publicly-listed U.S. firms from 1996 to 2006 that a given firm is about five times more likely to initiate an acquisition of a historically-interlocked target than some other unlinked firm.

One advantage of historical interlocks over standard contemporaneous linkages (in which a director simultaneously sits on the two firms' boards) is that historical interlocks are *directional.* Under our proposed mechanism this feature offers a clear and testable prediction about the flow of information from potential target to acquirer, and we show that target-to-acquirer flows affect merger pairings but that acquirer-to-target flows do not. This is consistent with the hypothesis that the ability to overcome information frictions is important when acquirers search for suitable targets. We also find that the effects of historical interlocks are stronger when the acquirer has better corporate governance, the director has a larger ownership stake at the acquirer, or the director played a more important role during past service at the potential target, all of which are more likely when the director has been exposed to deal-relevant information. The effect of historical interlocks on acquisitions is also stronger when the linked firms have less access to non-public information about one another through alternate channels such as social connections.

Since directors are not randomly assigned to firms, persistent and unobserved network connectedness between firms could pose a threat to the validity of our informational mechanism. Unobserved factors that might influence common director selection include governance structures, social connections, CEO entrenchment, board size, industry positioning, firm size, and organizational strategy, among others. We control for such unobserved and time-invariant factors with fixed-effects that include a unique dummy variable for each firm and firm-pair in our dataset. This analysis indicates even stronger effects of historical interlocks on the propensity to merge, and suggests that firm-pair specific network effects are not driving the results. We proceed to estimate the extent to which time-varying unobserved factors might further influence director selection and find that they are also unlikely to explain our results.

There are many studies in which experience in executing particular corporate actions passes across firms through director linkages. These actions include the use of poison pills (Davis (1991)), option backdating (Bizjak, Lemmon, and Whitby (2009)), repeat acquisi-

tions (Haunschild (1993)), and taking firms private (Stuart and Yim (2010)).[3] But gaining know-how from another firm in executing a corporate action does not necessarily imply that information was passed about that firm. Other studies more related to ours use contemporaneous director linkages to examine how cross-firm information may affect the decision to initiate an acquisition or joint venture (e.g., Gulati and Westphal (1999), Schonlau and Singh (2009)), yet contemporaneous director linkages could be forged as part of a plan for acquisition and thus not necessarily reflect the transfer of information about the target to the acquirer.

There is a large literature on social connectivity and how it affects corporate decisions, but it is not obvious whether social networks are channels for information flows or sources of agency problems (Freeman (1979)). Our use of historical interlocks controls for observed contemporaneous inter-firm connectedness, and we show across a variety of specifications that historical interlocks still exert an independent and positive influence on acquisitions.

The rest of the paper is organized as follows. Section I develops our hypotheses and discusses a firm's exposure to non-public information through the service histories of its directors. Section II describes the data, presents summary statistics, and lays out the estimation framework and identification strategy. Section III presents the main results on pair-specific merger probabilities and supplemental evidence on the probability of entering a new industry through an acquisition. Section IV considers a number of potential channels through which information can pass from a potential target to an acquirer and assesses the available evidence for each. Section V conducts robustness tests related to alternative hypotheses, potential endogeneity and functional form assumptions. Section VI concludes.

## I. Literature and Hypothesis Development

In this section we review the literature on cross-firm information asymmetries to motivate our own measure of information exposure (i.e., the "historical interlock"). Then, taking its construction as given, we discuss the predictions for acquisition patterns that historical interlocks imply.

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[3]See also Davis and Greve (1997), Haunschild and Beckman (1998), and Sorenson and Stuart (2001).

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*A. Measuring cross-firm information asymmetries*

A well-developed literature quantifies the degree of information asymmetry between a given firm and other market participants. Some authors propose that asymmetries are largest when analysts disagree about a firm's prospects (Krishnaswami and Subramaniam (1999), Thomas (2002), and Moeller, Schlingemann, and Stulz (2007)), while others relate valuation difficulties to the idiosyncratic component of stock prices (Dierkens (1991)), the quality of a firm's accounting information (McNichols and Stubben (2009)), or the existence and size of a defined benefit pension plan (Cocco and Volpin (2009)).

In contrast, we focus on a firm's decision to acquire specific targets. To do this, we develop a measure of information frictions that varies across potential acquirers for each potential target and vice-versa. By supposing that directors at an acquirer are exposed to information about a potential target through past service on the target's board of directors, we define a "historical interlock" for a potential acquirer as a binary indicator that varies at the *firm-pair* level. We then build a comprehensive set of firm-pairs, some of which actually merge and many others that do not, and assess how historical interlocks and other measures of target desirability affect the propensity to merge.

Our historical interlocks by construction isolate instances where a current director at a given firm served on the board of a prospective target two or more years ago. An alternative would be to measure inter-firm information transfers with contemporaneous interlocks. Indeed, while it is natural to suppose that information about corporate practices can be transmitted through current linkages, they are more likely than historical interlocks to reflect agency conflicts or strategic effects unrelated to the transfer of private information. Among other possibilities, these factors could include knowledge about a firm's current negotiating position or a tendency to "stack" the target's board with directors from the acquirer shortly before announcing a merger.[4] The existence of such agency conflicts, if only in the minds of investors, may alter acquisition decisions in ways that are unrelated to the transfer of information. Since contemporaneous interlocks may simultaneously confer information and involve strategic effects, their interpretation in this context is unclear.

Historical interlocks are distinct from contemporaneous ones and we propose that they

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[4]These practices under certain circumstances could be seen as a breach of the director's fiduciary responsibility. For example, the S-4 form submitted by Capitol Bancorp is connection with its 2001 acquisition of Sun Community Bancorp Ltd. reports that concerns were raised during the pre-announcement negotiations about conflicts of interest because the two firms had a common director.

isolate informational effects more cleanly. This is because historical interlocks are directed in that if firm $i$ is historically-interlocked with firm $j$ in year $t$ through a director $k$, it follows that director $k$ has a fiduciary responsibility to only one firm at any given point in time. It is also unlikely that director $k$ would have obtained non-public information about firm $i$ while serving on the board of firm $j$, since his service on firm $i$'s board had not yet begun. This directedness allows us to test whether private information matters more for an acquirer seeking a target or for a target seeking an acquirer.

At the same time, we recognize that directors are not randomly assigned to firms and that historical interlocks could influence mergers through other channels. We turn next to describe our main hypothesis and some of these alternatives.

*B. Main hypothesis and alternatives*

An ideal measure of an acquiring firm's access to information about a potential target would isolate this from other factors affecting mergers. While no ideal measure exists, our "historical interlocks" are more likely than the common alternatives to meet this criterion. To guide the analysis, we now review the predictions associated with various interpretations of historical interlocks.

We propose that information transmission affects acquisition patterns. This might occur if information allows acquirers to more accurately identify synergies associated with potential targets where an interlocked director once served, making these firms more likely targets than unlinked firms.[5] For example, given that acquisitions typically involve large capital outlays, risk-averse firms may avoid negative outcomes by bidding for a known target even if the expected synergy is lower than that associated with a random unknown firm. Alternatively, as suggested by Rhodes-Kropf and Robinson (2008), firms may engage in an iterative and costly search for suitable targets in which access to non-public information would, by lowering search costs, lead firms to prioritize known targets in the search queue.[6]

Similarly, information about potential targets may also improve an acquirer's ability to negotiate favorable deal terms (Custodio and Metzger (2010)). In this case, firms are again

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[5]This prior is supported by Bruner (2004) p. 183, who suggests that acquisition search is an information-gathering process focused on non-public "deal-rich" information.

[6]In this case, a prioritized target would be acquired only if the expected gain is higher than the next-best investment option. This mechanism is distinct from one induced by psychological biases that would lead acquirers to bid for familiar targets regardless of the expected gains.

more likely to buy known targets, but with the additional implication that acquirers should pay lower deal premia on average. The ability to use information to improve negotiating is likely stronger for acquirers with interlocks that are either contemporaneous or very recent. We say this because investment bank affiliations and the compositions of management and legal teams can change rapidly, and the acquirer will be able to negotiate more effectively if the potential target has maintained the same business relationships that were in place during an interlocked director's prior service. Our historical interlocks by construction therefore reflect negotiating power less strongly than contemporaneous interlocks, and we will test this implication empirically. Finally, information about targets gained by interlocked directors could be useful for integrating the two firms' operations after the deal has closed. If the acquirer were to recruit the target's directors or place its own directors on the target's board at this time, the action would appear as a contemporaneous interlock. But there remains the possibility that the target's directors are moved to the acquirer two or more years before the announcement, and it is only in this unlikely case that such "board stacking" or "director take-on" would appear as a historical interlock.[7]

All of these variants of the information hypothesis predict that historical interlocks will affect target choice most when the experienced director once played an important role at the potential target, since in these cases he likely had better access to relevant information. They also predict that past experience at the target will be more valuable when informational asymmetries are potentially large, as might be the case if the two firms are not closely situated geographically. The ability of informed directors to influence mergers should also increase with the effectiveness of the firm's corporate governance since firms with good governance are more likely to take advantage of available board-level information and are less likely to allow biased directors to influence merger decisions. Finally, given their informational advantage, acquirers should get better deals on historically-interlocked targets as measured by the market's reaction to a merger announcement.[8]

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[7]This could also involve significant conflicts of interest since the recruited director could pass information about the target's operations and outside options, allowing the acquirer to obtain more favorable terms (see, for example, p. 14 of *Corporate Director's Guidebook*, 5th ed).

[8]An important caveat to this last prediction is that acquirers less skilled in identifying suitable targets might disproportionately rely on director experience relative to firms with superior target identification skills who might, for example, have an effective standing acquisitions team. In such cases, we would observe a negative correlation across firms between returns and historical interlocks even if director experience improved target selection for those firms that relied upon it. More generally, endogeneity of the event being studied

A broad set of alternative interpretations of historical interlocks implies that directors with past employment at potential targets are influenced by agency conflicts or psychological biases that affect their decision-making (Asch (1951), Freeman (1979), Mizruchi (1996), Uzzi (1996), Gulati and Westphal (1999)). For example, psychological biases could lead directors influenced by familiarity (i.e., social connections) to pressure their boards to bid on firms where they once served, resulting in deals that are on average less favorable for the acquirer. Similarly, salience bias might lead directors to approve or suggest acquisitions of targets about which they are familiar with less regard for the effectiveness of the deal. We will refer to these variants collectively as "psychological biases," although it should be remembered that they could reflect agency concerns as well. Unlike the information hypotheses, psychological bias suggests that the impact of informed directors depends on the influence they can exert on their boards, which is likely greatest for an executive or inside director. Similarly, because board inefficiencies are more likely to affect decision-making adversely in poorly-run firms, psychological bias could be more severe for acquirers with ineffective corporate governance. In these cases, the effects of psychological bias could be reduced if the historically-interlocked director has a significant ownership stake in his current firm.

Another broad possibility could arise if similar firms hire the same directors, since optimal board structure depends on firm characteristics (Hermalin and Weisbach (2003), Adams, Hermalin, and Weisbach (2010)). Because similar firms are more likely to merge, an observed correlation between director connections and acquisitions could be driven by common characteristics. For example, directors with experience in a particular industry are likely to be hired by firms in that industry.[9] This "director-firm matching" or "spurious correlation" hypothesis predicts that the effect of historical interlocks on mergers will be weakest for firm-pairs that are similar. In these cases, inter-firm director connections would also be random after conditioning on firm-pair similarities, and thus make either firm as likely as the other to become the acquirer. Finally, the spurious correlation hypothesis predicts

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renders complicated the interpretation of returns in this context (see, for example, Eckbo, Macksimovic and Williams (1990), Li and Prabhala (2007)).

[9]Even outside the context of directors, an observed relationship between firm actions and mergers could reflect such unobserved firm-pair similarity. For example, Higgins and Rodriguez (2006) study the wealth effects of mergers among firms that have prior business alliances. Gompers and Xuan (2009) measure the extent to which problems of asymmetric information are smaller among common pools of venture capital investors relative to independent venture capitalists, and find that having common venture capital investors increases the probability of a merger.

no effect of director experience on market reactions to a merger announcement or on deal premia.

## II. Data and Experience Measures

*A. Directors and historical interlocks*

Data on directors is drawn for 1996 through 2006 from the Investor Responsibility Research Center, Inc. (IRRC) *Director's Database,* which includes all firms listed in the Standard and Poor's (S&P) 1500. The data set was collected from company annual reports and websites and covers approximately 90 percent of U.S. stock market capitalization.

Our main measure of inter-firm director connections is the historical interlock, which takes a value of unity for a potential acquirer in year $t$ if at least one of its current directors served on the potential target's board in the past, but not in the past two years. In other words, for each firm in our sample there are potential matches with a universe of possible targets, each of which is characterized by the presence or absence of a historical interlock. Historical interlocks are thus meant to isolate situations where information about a target is all that is likely to remain with the potential acquirer and where the reverse (i.e., information transfer from acquirer to target) is much less likely.

Figure 1 illustrates the historical interlock. The three horizontal timelines reflect board service at different firms by a single director. In the figure, our hypothetical director served on the board of firm A up to 2001 at which point he left firm A and joined the board of firm B. He also served from 1996-2005 at firm C.[10] The definitions given above imply that firm B has a historical interlock with firm A from 2003 onward and that firm C has a historical interlock with firm A from 2003 to 2005. Further, firm B would have a historical interlock with firm C from 2007 onward.

As discussed in Section I, the historical interlock stands in contrast with standard (i.e., contemporaneous) interlocks, which are defined for an acquirer $i$ in year $t$ as the presence of a current board member sitting, at period $t$, on a potential target's board. This necessarily implies that one of the potential target's current directors also sits on the acquirer's board so that, unlike historical interlocks, every contemporaneous linkage reflects *two* interlocks,

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[10]This example is somewhat contrived because, as we will see, directors in most cases serve on only one board at a given point in time.

## Figure I
### Director Movements and the Historical Interlock
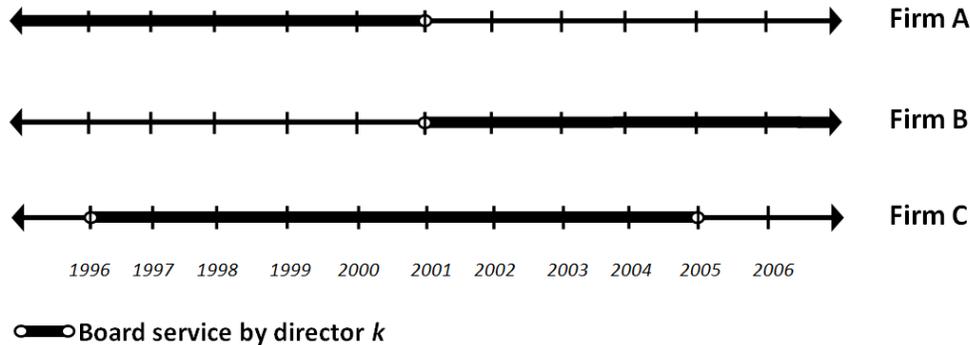


| Ordered pair | Historical Interlock | Contemporaneous Interlock |
|:---:|:---:|:---:|
| A to B | none | none |
| A to C | none | 1997-2000 |
| B to A | 2003- | none |
| B to C | none | 2002-2004 |
| C to A | 2003-2005 | 1997-2000 |
| C to B | none | 2002-2004 |

with one in each direction. Figure 1 also illustrates the contemporaneous interlock. Here, firm B is contemporaneously interlocked with firm C (and vice-versa) from 2002-2004 and firm A is contemporaneously interlocked with firm C (and vice-versa) from 1997-2000.[11]

### B. Acquisitions and firm-level characteristics

We obtain information from Thomson's *SDC Platinum* database on all announced acquisitions by firms in the *Director's Database* between January 1, 1996 and December 31, 2006 and filter the results by retaining information on acquisitions of publicly-listed firms that satisfy the following criteria:

- The deal value exceeds $1 million;

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[11]The annual data reported by Riskmetrics obscures the fact that annual meetings and thus director appointments are held throughout the year, complicating the decision of how to classify directors given that in some instances no cross-firm overlap occurs within years while in others there are varying degrees of overlap. We take a conservative approach and count firms as contemporaneously interlocked only in those cases where the director served on both boards in the prior year, as illustrated in Figure 1.

- The acquirer buys 20 percent or more of the target's shares;

- The acquirer owns a majority of the target's shares after the deal.

Since our focus is on the acquirer's intent to buy the target, we do not condition on the outcomes of announced mergers, though nearly 99 percent of in-sample mergers are completed and our main results are unaffected by this restriction. For the pair-specific analysis we require that at least two years of financial data for both potential acquirer and target is available from Standard and Poor's *Compustat* database and that the history of both boards' memberships is available for the duration of each firm's existence in the sample period.

*C. Descriptive Statistics*

Table 1 shows summary statistics on cross-firm seatings obtained by tracking the 26,797 directors in our sample through time and across firms. Panel A, which shows the distribution of the total number of firms at which each director served, indicates that 22.4 percent of directors served at only one firm over the 1996-2006 period, thus generating no interlocks. At the same time, 69 percent of directors served on five boards or fewer. To understand whether the multiple board seatings resulted primarily from simultaneous appointments or from director movements from firm to firm, Panel B shows the distribution of simultaneous director seatings at the director-year level. The majority (81.1 percent) of director seatings involve directors who serve on the board of a single firm, 12.8 percent involve a contemporaneous interlock between two firms, and 5.9 percent involve contemporaneous interlocks among three to five firms. This indicates that most cross-firm director connections occur when directors move across firms over time, and thus are historical in nature rather than contemporaneous.

[**Table 1 here**]

Panel C of Table 1 indicates the roles played at the potential acquirer and target for both historically and contemporaneously-interlocked directors. For example, over a third of historically-interlocked directors were executives at the potential acquirer. A fourth were inside directors at the acquirer, while a third were inside directors during their earlier service at the potential target. Contemporaneously-interlocked directors, on the other hand, were more likely to be executives and less likely to be professional directors (i.e., "career directors") than were historically-interlocked directors.

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Panel A of Table 2 lists the breakdown of in-sample acquisitions from 1996-2006. These include 4,846 acquisitions totaling more than $1 trillion in market value. The inverted U-shaped time pattern of deals reflects the merger wave of the late 1990s. Panel B, which presents the distribution of deals broken down by target industry, shows that 78.7 percent of all acquisitions involve targets in the manufacturing, services and financial sectors. The uneven distribution of deals across time and industries suggests that our empirical estimates should condition on the year and industry in which acquisitions occur. Panel C shows that 9.1 percent of in-sample merging firm pairs were historically interlocked and that 6.3 percent of merging firm-pairs were contemporaneously interlocked.

[**Table 2 here**]

Table 3 compares acquirer and target characteristics of in-sample firms with the average for all S&P 1500 firms. Consistent with previous studies, acquirers are larger, have higher free cash flow, are less leveraged, and have lower sales-to-asset ratios but higher market-to-book ratios, while targets are more similar to the average S&P 1500 firm.

[**Table 3 here**]

### III. Baseline Results

In this section we present our baseline findings, describing the estimation approach as we proceed.

*A. Are firms more likely to enter industries in which their directors have experience?*

Before moving to our main analysis we check to see whether there is evidence in support of a diffuse role for directors' industry experience. The business press has long suggested that firms pay particular attention to industry and M&A experience when recruiting new directors since these may be useful for evaluating large corporate initiatives such as diversifying acquisitions (see, for example, pp. 32-43 in Ward (2000), pp. 25-50 in Shultz (2007), and pp. 136-142 in Bowen (2008)). A finding in support of the more general hypothesis that industry experience influences the choice of industry that an acquirer enters through

acquisition would suggest that director experience is a source of useful information for acquiring firms, especially since experience in this context does not necessarily entail strategic conflicts.[12] To do this we estimate the following partial effect:

$$\Pr(ACQ_{ik} = 1 \mid H_{ik} = 1) - \Pr(ACQ_{ik} = 1 \mid H_{ik} = 0), \tag{1}$$

where $ACQ_{ik}$ takes a value of unity if firm $i$ announces an acquisition of a target in industry $k$, which differs from firm $i$'s own industry, and zero otherwise, and $H_{ik}$ is an indicator set to unity if acquirer $i$ has a current director with board experience at *any* firm in industry $k$.

Following Rhodes-Kropf and Robinson (2008), we render equation (1) estimable by comparing firms that initiated acquisitions of targets in industry $k$ with those choosing not to acquire a target in industry $k$. The universe for each firm is thus the set of all potential target industries. We limit attention to 2-digit SIC industries to construct a counterfactual set that is computationally feasible and retain only the first announcement made by each acquirer for a target in a given 2-digit industry. This yields 4,846 actual acquirer-industry mergers among 177,130 potential acquirer-industry pairs over the sample period.

We estimate the average partial effect using the logit equation:

$$\Pr(ACQ_{ikt} = 1) = G(\beta_1 H_{ikt} + \beta_2 I_{ikt} + \gamma X_{it} + \tau_t + w_k + w_l + \varepsilon_{ijt}),$$

where $I_{ikt}$ is an indicator taking a value of unity if acquirer $i$ has a director currently on the board of a firm in industry $k$ and $X_{it}$ is a vector of acquirer-specific control variables that may affect the propensity to initiate acquisitions. The $\tau_{t,}$, $w_k$, and $w_l$ are fixed effects for years, the two-digit SIC code of the potential acquirer, and the two-digit code of the target industry, respectively, and $G(z) = \exp(z)/[1 + \exp(z)]$.[13] The coefficient of interest, $\beta_1$, measures the effect of director industry experience on the probability that his current

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[12] Our data does not permit us to disentangle the relative magnitudes of target-specific and industry-specific experience since the directors' data does not cover all potential targets.

[13] Shumway (2001) demonstrates the equivalence between the multi-period binary response model that we use and hazard models, implying that the marginal effect associated with $\beta_1$ can be interpreted as that of a historical interlock on the probability that acquirer $i$ acquires a target in industry $k$ in year $t$.

firm enters industry $k$ by buying a target there relative to other new industries.

## [Table 4 here]

Table 4 reports the results. Column (1) shows that firms are more likely to enter an industry where a current director has past board experience. The regression reported in column (2), which includes contemporaneous industry experience, indicates that this is also positively associated with the entry decision. To control for the role of other firm characteristics, we add the leverage ratio and the log of the firm's total assets to the regression in column (3). In column (4) we add the firm's market-to-book ratio as well as the log of its sales and cash.[14] Including these controls lowers the estimated coefficients on both historical and contemporaneous interlocks but they remain highly statistically significant. The marginal effects indicate that an acquirer with industry-specific historical director experience is 2.6 times more likely to buy a target in industry $k$ relative to other new industries. Similarly, an acquirer with a contemporaneously-interlocked director is 2.7 times more likely to buy a firm in industry $k$ relative to others.[15]

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[14]Total assets is *Compustat* item 6, sales is item 12, and cash is item 1. The leverage ratio is the sum of short and long-term debt (*Compustat* items 34 and 9) divided by item 6. We measure the numerator of the market-to-book ratio as the value of a firm's common equity at current share prices (the product of items 24 and 25), to which we add the book values of preferred stock (item 130) and short- and long-term debt (items 34 and 9). We use book values of preferred stock and debt in the numerator because prices of preferred stock are not available on *Compustat* and we do not have information on issue dates for debt from which we might better estimate market value. We note that book values of these components are reasonable approximations of market values in stable interest-rate environments such as the U.S. during our sample period. We compute the denominator in the same way except that we use the book value of common equity (item 60) rather than its market value. We eliminate firms with negative values for net common equity since they imply negative market-to-book ratios, as well as observations with market-to-book ratios in excess of 500 since many are likely to be serious data errors.

The sales-to-asset ratio (*Compustat* item 12 divided by item 6) is our proxy for TFP because individual firm output is not available on *Compustat* to form the numerator implied by the basic $AK$ model. Leverage is given by the ratio of short- and long-term debt to total assets (the sum of *Compustat* items 34 and 9 divided by item 6).

[15]Marginal effects of covariates on the dependent variable are obtained in the usual manner given coefficient estimates for a $dx_i$ change in covariate $i$: $\frac{\partial \Pr(Acq_{it}=1)}{\partial x_{it}} = G'(odds(X))\gamma_i$ where $odds(X)$ is the odds ratio constructed using the estimated coefficients and is evaluated at the mean value of covariates: $odds(X) = \exp(\alpha_1 H_{it}^C + \gamma X_{i,t} + \tau_t + w_k)$.

*B. Are firms more likely to acquire specific historically-interlocked targets?*

Having found that director industry experience has predictive power for an acquirer's choice of industry, we proceed to our main analysis which asks whether firm-specific historical interlocks influence merger patterns between individual firms. The baseline specification is

$$\Pr(ACQ_{ijt} = 1) = G(\beta_1 H_{ijt} + \beta_2 I_{ijt} + \gamma X_{ijt} + \tau_t + w_k + \varepsilon_{ijt}), \tag{2}$$

where $ACQ_{ijt}$ now takes a value of unity if firm $i$ announces an acquisition of firm $j$ in year $t$ and zero otherwise, and $H_{ijt}$ is an indicator set to unity if firm $i$ had a historical interlock with potential target $j$ in year $t$ and zero otherwise. $X_{ijt}$ represents a vector of firm-pair specific control variables that may affect the propensity to merge. In this case our intent is to measure the probability that firm $i$ acquires firm $j$ in year $t$. We include year ($\tau_t$) and industry ($w_k$) fixed effects because merger waves tend to cluster along these dimensions (Harford (2005)), and we account for arbitrary correlations in acquisition probabilities between individual firm pairs by clustering standard errors at the ordered-pair level. In this case, the counterfactual universe for each firm is the set of all potential targets in the sample. We make the analysis computationally feasible by restricting attention to all firm pairs within major SIC divisions, generating a panel of 1,471,098 matched firm pairs, 612 of which represent actual merger announcements.[16]

Table 5 presents the results. Column (1) shows that the coefficient for a historical interlock is positive and statistically significant, consistent with the hypothesis that acquirers are more likely to buy firms about which they have access to non-public information through directors' service histories. Column (2) adds contemporaneous interlocks, which also enter positively and are statistically significant. We add several controls in column (3). These include measures of relative size (i.e., the ratio of acquirer-to-target total assets), relative sales productivity (sales-to-assets ratios), and relative market valuation (market-to-book ratios), all of which could reflect increased investment opportunities or overvaluation and lead to a greater propensity for merger (Jovanovic and Rousseau (2002, 2008), Harford (2005), Dong et al. (2006)). We add two measures of network connectedness among firm pairs in column

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[16]These estimates understate the effects of historical interlocks because binary choice models such as the logit are downward-biased in rare-events contexts such as the present application (see, for example, King and Zeng, (2001)). We address this further in Section VI by implementing procedures that relax the restriction on the set of actual merging pairs and restrict the size of the counterfactual set.

(4), namely dummy variables set to unity when the acquirer and potential target are in the same 4-digit SIC industry or are headquartered in the same county (Hoberg and Phillips (2010)). Our main specification in column (5) includes fixed effects for major SIC industries along with the other controls.[17] The key finding is that including these controls does not affect the sign or statistical significance of the estimated coefficient on historical interlocks, though its size is slightly attenuated in columns (4) and (5), which suggests that historical interlocks are positively correlated with our measures of network connectedness. Turning to the other controls, a potential acquirer is more likely to buy a given potential target when the former is relatively larger and more highly valued than the latter. Interestingly, the relative sales-to-assets ratio does not seem to affect matching.

[**Table 5 here**]

As before, we assess the economic magnitudes of the estimated effects using the odd's ratio evaluated at the covariate means. First and foremost, acquirers are 4.2 times more likely to initiate purchases of potential targets with which they are historically interlocked. The probability of merging rises by 0.6 percent in response to a one standard deviation increase in relative firm size and by 2.9 percent for a one standard deviation increase in relative market-to-book ratios. The same-county effect is strong as well, with two firms from the same county being 23 percent more likely to merge relative to an average firm pair, while within-industry pairs are twice as likely to merge relative to across-industry pairs.

## IV. Potential Channels

We now examine whether historical interlocks are consistent with the information hypothesis or whether the data are better described by one of the alternative interpretations. The exercises conducted in this section also strengthen the argument made later in Section V that director selection is not driving the results.

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[17]This specification includes industry fixed effects for the acquirer and target pair. Since using more disaggregated industries would not be computationally feasible because it would require a separate industry effect for every possible combination of industry codes, we include industry effects at the major (i.e., 16 category) SIC level.

*A. Does the direction of information flow matter?*

Our prior is that information available to acquirers through a current board member's earlier service can be useful in selecting targets. An obvious alternative is that the relevant flow of information runs in the opposite direction. For example, if a target believes that acquirers are generally overvalued and a proposed merger involves an exchange of shares, information gained by one of the target's current directors during past service at the acquirer could reduce uncertainty about the value of the shares being offered relative to an unknown acquirer, making the target more likely to accept this particular offer. Alternatively, the target of a hostile takeover may be more likely to recruit as white knights those acquirers with which they have a historical interlock.

We test for the direction of information flow by constructing "reverse historical inter-locks," which are simply historical interlocks from targets to potential acquirers. Formally, we define a reverse historical interlock for an acquirer when, in year $t$, one of the potential *target*'s current directors once served on the acquirer's board but has not in the past two years. In constructing the counterfactual set, we match both actual and potential acquirers with actual and potential targets so that there is no distinction a priori between direct and reverse historical interlocks in terms of acquirer or target characteristics. Including reverse historical interlocks in our regressions also tests indirectly for whether our historical inter-locks reflect network connectivity or spurious director-firm matching, since both of these alternative interpretations predict a positive and equivalent effect of either type of interlock.

Table 6 presents the results. The first row shows that historical interlocks remain associated with a higher probability that, for firm pair $ij$ in year $t$, firm $i$ will attempt to acquire $j$. The second row, on the other hand, shows that reverse historical interlocks are not statistically significant whether or not historical interlocks are included in the regression. Comparing Table 6 with Table 5, there is also no evidence that the inclusion of reverse historical interlocks attenuates the effect of historical interlocks. These results suggest that the direction of inter-firm connections *does* matter, and is consistent with our hypothesis that access to non-public information is more important for acquiring firms than for targets. This is perhaps unsurprising given that targets, at least in the case of cash deals, need only care about the price received rather than acquirers who must also consider synergies associated with a particular acquisition.

[**Table 6 here**]

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*B. Does a director's role influence the effect of historical interlocks?*

We now consider a number of director characteristics and how they affect the operation of historical interlocks. We do this because understanding how various forms of director heterogeneity influence target choice can help to distinguish whether historical interlocks reflect the transfer of information or other factors. For example, while the information view of historical interlocks predicts that non-public information about potential targets will affect merger decisions, the psychological bias and negotiation interpretations are more closely related to the extent of an interlocked director's influence over the acquirer's board. To explore these effects, we first interact historical interlocks with indicators set to unity if a historically-interlocked director is an executive or inside director at the acquirer.[18] This of course assumes that executives and insiders can influence acquisitions more than other board members.

Columns (1) and (2) of Table 7 report estimates from adding these interactions to the baseline specification (i.e., the logit model in column (4) of Table 5). A positive coefficient for either interaction would suggest that director influence, and thus psychological bias, can explain at least part of the effect of historical interlocks on target choice. The coefficient estimates are both negative and not statistically significant, however, and their inclusion does not affect the sign, statistical significance or overall magnitude of the direct coefficients on historical interlocks.

[**Table 7 here**]

Second, if the information hypothesis is correct, evidence for it should be strong when a historically-interlocked director at the acquirer has a significant ownership stake there. This is because stakeholding directors are less likely to let factors such as inattention, psychological bias, or maintaining social connections influence their decisions. We test this by interacting historical interlocks with an indicator set to unity if the director has a stake of less than one percent in the acquirer and zero otherwise. Column (3) of Table 7 shows that this interaction is indeed negative and statistically significant and that the direct effect of historical interlocks becomes larger.

Third, the information hypothesis predicts that information about potential targets will affect merger decisions more when the historically-interlocked director played an impor-

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[18]Information on individual directors' roles and ownership stakes is available from the IRRC *Director's Database.*

tant role on the target's board during service there. This follows because access to relevant non-public information about the target is likely to have been greater. In this case, we interact historical interlocks with indicators set to unity when the historically-interlocked director was once an executive, inside director or former employee of the target. Columns (4), (5) and (6) of Table 7 add these interactions to the baseline specification, and in all three cases the effect of historical interlocks continues to be positive and statistically significant. At the same time, and consistent with the information interpretation, the interaction terms are also positive and statistically significant.

*C. Historical interlocks and corporate governance*

Psychological bias and network connections predict that motives other than maximizing synergies drive the effects of historical interlocks on merger decisions. As additional tests of these possibilities, we use data from the IRRC *Corporate Takeover Defenses Database*, which includes an index of overall governance quality originally constructed by Gompers et al. (2003) as well as information on individual governance provisions. If historical interlocks affect merger decisions more strongly for firms with weak governance provisions regarding agency conflicts between directors and shareholders, this would offer support for one or both of the alternative theories. We examine three characteristics of the acquiring firm's board. First, under "duties" provisions, directors are not precluded from acting in the interests of non-shareholders when evaluating an acquisition. Second, when the firm has indemnification contracts in place, directors are protected from certain legal actions such as lawsuits filed by shareholders for perceived breaches of fiduciary responsibility. Finally, "care" provisions limit director liability arising from breaches of their "duty of care."[19]

The requirement that governance data be available for each potential acquirer reduces the number of observations in our sample by 70 percent, given that the Gompers at al. (2003) index covers the S&P 500 (i.e., the largest firms in our sample) or firms that are included in the annual lists of the largest corporations published by *Forbes*, *Fortune*, and *Businessweek*. To allow comparisons with our earlier results, we therefore re-estimate the baseline regression reported in column (4) of Table 5 using only those observations for which governance data is available for the acquirer. These results appear in column (1) of Table

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[19] A director's "duty of care" involves a responsibility to be informed and active when making corporate decisions and to do so using prudence and sound judgment (see, for example, pp. 18-21 of *Corporate Director's Guidebook* 5th ed.).

8, and indicate that the effect of historical interlocks on the propensity to merge is still positive and highly significant statistically but smaller in magnitude than that obtained with the unrestricted sample. Columns (2), (3) and (4) add interactions of historical interlocks with indicators for the "duties," "contract," and "care" provisions. These interactions are not statistically significant and the direct effect of historical interlocks is largely unaffected by their inclusion, which is opposite to the predictions of the network connections and psychological bias theories.

[**Table 8 here**]

Next, we interact historical interlocks with the index of corporate governance, which is defined so that larger values reflect worse overall corporate governance. In doing so, we ask whether the effect of historical interlocks on target selection is stronger when the acquirer has poor governance. A positive coefficient on the interaction term would answer in the affirmative and offer support for the network connections or psychological bias theories. The regression reported in column (5), however, indicates a negative and significant coefficient on the interaction term, which is consistent with a stronger effect of historical interlocks for acquirers with *good* governance.

In Columns (6) and (7) we interact historical interlocks with two additional measures of governance that address acquisitions in particular. Control-share acquisition laws deny voting rights to newly-qualified large shareholders unless approved by a majority of disinterested shareholders, while supermajority provisions allow increases in the voting threshold needed to approve deals beyond state-level mandates so that mergers are less easily approved by shareholders. Neither of these terms are statistically significant when interacted with historical interlocks, however, and neither affects the magnitude of the direct effect of a historical interlock. These tests offer additional evidence that historical interlocks do not lead to acquisitions driven by value-destroying motives.

*D. Historical interlock exposure and difficulty of valuation*

If historical interlocks reflect information that is useful in valuing targets, we would expect them to affect target choice more in environments where the deal is difficult to value. Although an imperfect indicator of informational proximity, difficulties in valuation could correspond to geographic positioning such as when the two firms are headquartered in

different counties. For example, it is common for executives in California's "Silicon Valley" to share ideas through social and network connections, and their relative proximity facilitates such exchanges.

To explore these implications, we interact historical interlocks with an indicator set to unity when the potential acquirer and target are headquartered in the same county. The information hypothesis predicts that these interactions will have a negative effect on target selection since the degree of overlapping non-public information provided by directors is likely to be smaller.

[**Table 9 here**]

Table 9 presents the results. In the first row we re-estimate the baseline specification of equation (2) including the interaction of historical interlocks with the same county indicator on the right-hand side. This term enters negatively and is statistically significant, indicating that the effect of historical interlocks is greater for firm-pairs where information asymmetries are likely to be largest. Column (2) performs an analogous exercise for contemporaneous interlocks and finds no such effect. In column (3) we include the interactions of both historical and contemporaneous interlocks with the same-county dummy. In this case we find that the interaction with historical interlocks continues to enter negatively and with a high degree of statistical significance while the interaction of contemporaneous interlocks with geographic proximity now enters positively at the ten percent level.

*E. Historical interlock exposure and deal outcomes*

We now examine the effect of historical interlocks on market reactions to acquisitions by estimating the following regression of abnormal returns around the announcement day:

$$CAR_{ijt}^m = \alpha_1 + \alpha_2 H_{ijt} + \alpha_4 I_{ijt} + \alpha_5 Z_{ijt} + \tau_t + \varepsilon_{ijt}^m,$$

where $CAR_{ijt}^m$ is the cumulative abnormal return to counterparty $m \in \{$acquirer, target$\}$ of the announced merger between firm $i$ and firm $j$ in year $t$, the $Z_{ijt}$ are deal characteristics and the $\tau_t$ are fixed effects for years.

We calculate abnormal percentage returns using standard event study methods (e.g., Brown and Warner (1985)) over a three-day window around the announcement date using the difference between stock returns from the market model and those from the equally weighted

index created by the University of Chicago's Center for Research in Securities Prices (CRSP). We estimate the market model parameters for each acquisition using data over the 200 days preceding the start of the announcement window. Control variables include the ratio of total assets for each firm pair (e.g., Jarrell and Poulsen (1989), Shleifer and Vishny (2003), Rhodes-Kropf and Viswanathan (2004)), the total value of the transaction (e.g., Moeller (2004)), and binary indicators for whether the deal was unsolicited (e.g., Schwert (2000)) and whether payment was made primarily in cash (e.g., Huang and Walking (1987), Travlos (1987)).

Table 10 presents the results. Column (1) indicates that historical interlocks do not affect excess returns accruing to the acquirer, but this is not the case for contemporaneous interlocks, which are associated with returns that are 2.6 percent *lower*. This is consistent with market participants associating contemporaneous interlocks with an increase in the potential for conflicts of interest, and illustrates how contemporaneous interlocks differ fundamentally from historical ones. Consistent with earlier studies, majority cash deals are related with abnormal returns that are 2.1 percent higher for acquirers while large transactions are associated with lower returns.

In Column (2) we interact historical interlocks with the indicator for a cash deal. The regression indicates that cash deals with a historical interlock are related to abnormal returns for the acquirer that are 3.7 percent higher than cash deals without a historical interlock. This offers indirect evidence that cash deals, which are likely more arms-length in nature than share exchanges, are less likely to be motivated by social connections, psychological biases, or other value-destroying motives. In columns (3) and (4) we repeat the analysis for abnormal returns among target firms and find no effect of either historical or contemporaneous interlocks.

**[Table 10 here]**

As discussed in Section I, the negotiation hypothesis, as a special case of the information interpretation of historical interlocks, predicts that a historically-interlocked director has information that helps the acquirer negotiate more effectively and obtain better deal terms. To test this prediction, we estimate the effects of historical and contemporaneous interlocks on deal premia measured as the per-share price paid for the target divided by the target share price four weeks prior to the merger announcement. Columns (5) and (6) of Table 10 report the results. Column (5) shows that historical interlocks have no measurable effect on

deal premia, but at least do not lower them, while contemporaneous interlocks are associated with acquirers paying 9 percent higher premia on average. This raises the possibility that contemporaneous director connections lower the bargaining power of acquirers, and offers further evidence that historical interlocks differ fundamentally from contemporaneous ones.

In column (6) we explore this effect further by interacting historical interlocks with the indicator for cash payment and we find that historical interlocks are associated with an *increase* in deal premia for majority cash deals, which is inconsistent with the negotiation hypothesis. This finding, combined with our earlier result that acquirers with historical interlocks obtain higher abnormal returns in cash deals, could mean that such historically-interlocked deals involve higher synergies for which acquirers must pay higher premia, which is consistent with the transfer of information from targets to potential acquirers.

*F. Summary*

We now pause to summarize the evidence so far about how historical interlocks matter in merger decisions. And while the transfer of information across firms is intuitive and appealing, it is possible that other channels are at work. The psychological bias hypothesis, which predicts that directors choose targets based on agency conflicts stemming from social or network connections, is perhaps the main contender. We have conducted several tests for psychological biases, but none indicated that they have a measurable effect on the operation of historical interlocks.[20] Indeed, if historical interlocks operated through social connections, those from targets to potential acquirers (i.e., reverse historical interlocks) would also affect acquisition decisions, but Table VI shows that this is not the case. Psychological biases could also come into play if socially-connected directors, even if only historically interlocked, have greater influence over acquisitions when they hold more important positions at the acquirer. We test this in Table VII and show that this also is not the case.

As discussed above, director-firm matching predicts a spurious correlation between inter-firm director connections and mergers due to similarities among firms. The failure of reverse historical interlocks to predict acquisitions in Table VI, however, provides no support for this alternative, and we offer further evidence against a role for director-firm matching in Section V below. Finally, cross-firm director connections may be related to mergers because an acquirer's directors are sometimes placed on the target's board to facilitate the deal. But

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[20] Although historical interlocks capture board service at a target that ended more than two years in the past, psychological biases could persist even if no observable current connection remains across firms.

since our historical interlocks specifically omit firms that shared a director within the past two years, they are unlikely to be associated with director take-on.

On the other hand, director experience may well confer information that is useful for identifying suitable targets or negotiating better deal terms. And consistent with the information hypothesis, Table VII shows that historical interlocks matter more when the interlocked director played a larger role on the potential target's board. We also found that historical interlocks affect merger decisions more when the historically-interlocked director has a larger stake in the acquiring firm (Table VII), when his current firm has better corporate governance (Table VIII), and the firm pairs are not in close proximity (Table IX). These results are again consistent with the information hypothesis and inconsistent with agency conflicts driving our main results.

With respect to abnormal returns and deal premia, the psychological bias hypothesis predicts that director-connected deals will perform worse than the average deal. We saw in Table X that this is true for contemporaneously-interlocked firms, but also that historical interlocks have a positive effect on abnormal returns for cash deals, which are precisely those transactions that are less likely to be driven by motives other than capitalizing on acquisition synergies.

The negotiation hypothesis predicts that information relevant for negotiating favorable terms is transmitted from potential targets to acquirers, but Table X shows that historical interlocks are associated with higher deal premia, if anything. It would also predict that a target's access to information about potential acquirers would help targets to obtain better deal premia, leading targets to solicit acquirers where their directors once worked. And yet we have already seen that reverse historical interlocks do not affect acquisition patterns.[21] Finally, the operations hypothesis suggests that recruited directors are useful for running the integrated firm. And while we cannot test this hypothesis directly, we do not find it a compelling explanation for the role of historical interlocks because acquirers can easily recruit target directors and management after a deal has closed.

Taken together, our findings are supportive of the information hypothesis and difficult to reconcile with the alternatives, and especially with the psychological bias perspective.

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[21]Custodio and Metzger (2010) find that *industry* experience seems, for CEOs, to be associated with better negotiating outcomes for the acquirer, a finding that is very reasonable in that context.

## V. Identification and Robustness

The above exercises suggest that historical interlocks reflect the transfer of information from potential targets to acquirers. But it is also well known that directors are not randomly assigned to firms, and this leaves open the possibility that alternative mechanisms are driving the observed correlation between historical interlocks and acquisitions. Our strategy to address these concerns has four components. First, as discussed in Sections I and II, our historical interlocks by construction omit director seatings where factors other than information transfer are more likely to affect merger decisions. Second, we conducted a variety of tests in Section IV for alternative theories identified by the literature on directors and acquisitions.

In this section we address broad concerns that firms' director choices are outcomes of optimization problems that take firm characteristics as their inputs, and that these characteristics also affect merger pairings. For example, since one responsibility of a director is to monitor management, firms in industries that use managerial capital more intensely may require more outside directors, leading to more historical interlocks, while firms in these same industries may, for some other reason, also be more likely to acquire one another. Since many unobservable firm-specific characteristics likely to be relevant for director selection are also persistent, we begin by controlling for a wide variety of time-invariant unobservable characteristics with fixed effects (e.g., Ábrahám and White (2006), Foster, Haltiwanger, and Syverson (2008)). We then consider the possibility that the unobservables are time-varying and calculate how large the effect of such unobserved forces would need to be to completely explain the effect of historical interlocks on acquisition patterns. We conclude by non-parametrically estimating of the effect of historical interlocks on acquisition patterns in a way that does not impose a priori restrictions on the set of potential targets.

### A. Time-invariant director selection

We now address the "spurious correlation" that would arise from director-firm matching using firm-pair specific fixed effects to control for unobservable and time-invariant features that are specific to an acquirer, target, or acquirer-target pair. These persistent common factors could include industry positioning, product lines, investment advisors, corporate governance structures, network connectedness, CEO entrenchment, board and firm size, presence of antitrust pressure, and profitability, among many others. Because the number of firm

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pairs increases proportionally with the sample size,, constructing these fixed effects is computationally infeasible for binary response models due to the incidental variables problem in which the number of parameters increases in proportion to the number of firm pairs.[22] Fortunately, consistent estimates can be obtained by de-meaning a linear probability model, which is econometrically equivalent to including these fixed effects.

Table 11 presents the estimated marginal effects of historical interlocks. Column (1) includes only historical interlocks, column (2) adds contemporaneous interlocks to the regression, and column (3) includes the complete vector of controls from our full specification in Table 5. In all three equations, historically interlocked firms are about 7.5 times more likely to merge relative to average firms. These estimates are larger than those obtained from our logit models, which is expected given that binary response models are downward-biased in rare-events data.

[**Table 11 here**]

*B. Time-varying director selection*

While theory suggests that time-invariant unobserved heterogeneity is the most likely form of director selection, in principle time-varying unobserved heterogeneity could be driving the results. To address this, we use the insights developed in Altonji, Elder, and Taber (2005) to gauge the degree to which time-varying director selection might be at play. The technique quantifies the amount of selection bias that would be required to explain the entire effect of director experience on acquisition patterns.[23]

Bellows and Miguel (2009) develop a general statistic that makes no assumptions on the shape of the error distribution. This statistic measures how much greater the influence of unobservables on selection must be relative to the influence of observables on selection to fully remove the estimated effect of the variable of interest. The statistic is $\theta_r = \hat{\beta}^f / (\hat{\beta}^r - \hat{\beta}^f)$, where $\hat{\beta}^r$ is the estimated coefficient from a regression with a restricted set of controls and

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[22]Even if these effects were computable, latent variable frameworks such as logit and probit do not permit computation of the variance of individual effects, so the estimated coefficients are identified only up to a scale factor. This prohibits comparative estimates of how controls for pair-specific effects alter parameter values (Wooldridge (2002, p. 470)).

[23]This procedure is motivated by the insight that the amount of selection on observables conveys information about the amount of selection on unobservables. All that is required for this procedure is that the amount of selection on observables be at least as large as the amount of selection on unobservables. Altonji, Elder and Taber (2005) argue that this assumption is no less implausible than the assumptions required for OLS estimation.

$\hat{\beta}^f$ is the coefficient from a regression with the full set of controls. The key choice in constructing this statistic is to select the restricted set of controls appropriately, so we work with several different restricted models. An estimated ratio of unity means that selection on unobservables must be at least as strong as selection based on observable characteristics to account for the entire baseline estimate. Similarly, a number greater than one, say 3, would mean that selection on unobservables must be three times greater than selection on observables to attribute the main effect to director selection. Numbers less than one imply that negative selection is present and that the true effect of director experience is in fact larger than the baseline estimates.

Panel B of Table 11 presents the results. The first column shows the estimate when the restricted set consists of a constant term only. In this case, the statistic indicates that selection on unobservables would need to be ten times greater than selection on observables to fully explain the effect of historical interlocks on acquisition patterns. A similar result obtains in columns (2) and (3), which include year fixed effects and contemporaneous interlocks. Column (4), which removes the effect of all time-invariant unobservables, measures the extent to which time-varying unobservables might influence the main result. The estimated statistic in this case is almost 60, indicating that once director selection based on time-invariant firm-pair effects has been taken into account, further selection on unobservables must be at least 60 times as large as selection on observables to explain fully the effect of historical interlocks on acquisition patterns.

The finding that firm-pair fixed effects do not lead to attrition in the effect of historical interlocks, along with the finding that time-varying unobserved heterogeneity is unlikely to pose a threat to empirical validity, together provide strong evidence that director selection does not play a crucial role in explaining the observed effect of historical interlocks on acquisition decisions.

## C. Matching Estimators

As a final set of robustness checks we now obtain estimates of the effects of historical interlocks on merger decisions using matching methods. Matching provides a simple way to form treatment and control groups by selecting on observable covariates and comparing similar firms that received the treatment (those with historical interlocks) with those that were not treated (e.g., Moffitt (2004), Cameron and Trivedi (2005)). Here, we apply more structure by implementing a procedure where for each ordered firm-pair with a historical

interlock we construct a control group with similar observed characteristics but no historical interlock. The difference in acquisition patterns between these two groups is an empirical estimate of the treatment effect of historical interlocks. Matching also allows for estimation of average partial ("treatment") effects when the presence of historical interlocks is driven by self-selection, and unlike probit or logit methods, provides nonparametric estimates of such effects. This allows us to assess the importance of the parametric forms used in our earlier analyses.

The propensity score matching is implemented in two stages. First, we run a probit regression for the effects of a historical interlock on firm or firm-pair characteristics. We then use the matched sample to correct for selection on observables and estimate the effect of historical interlocks on acquisition outcomes. We estimate the average treatment effect using the unmatched average partial effect to compare with the estimates obtained from nearest neighbor matching as suggested by Abadie and Imbens (2006) using one, two and three nearest neighbors.

[**Table 12 here**]

The matching estimator results are presented in Table 12. Column (1) presents the unmatched treatment effect for comparison. From columns (2)-(4), we can see that the matching estimates are also positive and statistically significant and yield treatment effects of historical interlocks that increase the propensity to acquire by a factor of about 15, which is much larger than the increase implied by the logit estimates in our main analysis (see Table V). The results suggest that functional form assumptions are not critical for our results and that the effects of historical interlocks remain even when the set of potential targets is assumed to be much smaller.

## VI. Conclusion

The transfer of information about potential merger targets through the past experience of an acquirer's director could have an important impact on decisions in the market for corporate control. We investigate these relationships by creating measures of inter-firm director linkages from potential targets to potential acquirers. Using a current director's past service at a potential target as a proxy for this information, we find that firms with historical interlocks are more likely to initiate acquisitions with potential targets to which they are

linked than those for which they have no historical interlock. Our results indicate an effect of information transfer on acquisition patterns that is independent of other factors such as network connectedness, board take-on, and observed and unobserved firm characteristics. A series of robustness tests confirm our main findings.

The suggestion that firms can generate synergies through acquisitions but that their ability to do so is inhibited by informational asymmetries appears to be born out in the data (see, for example, Rhodes-Kropf, Robinson, and Viswanathan (2005)). We refine this notion by showing that information frictions specific to firm pairs have a quantifiable effect on acquisition patterns. Though we do not consider the implications for optimal portfolio selection here, the fact that director histories are traceable, combined with the tendency for share prices of merger targets to rise, suggests that there may be some value to investors in better understanding these linkages.

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## Table I
## Inter-firm Director Connections

Panel A shows the distribution of the total number of firms on whose boards directors served from 1996 to 2006. Panel B shows, at the director-year level, the distribution of the number of simultaneous board seatings held by a given director at a point in time. Panel C shows the percent of historically and contemporaneously-interlocked directors at the potential acquirer or target that during their tenure served as an executive, inside director, were ever a former employee, or were identified as a 'professional director' by the IRRC, where a professional director is an individual whose primary occupation is to hold corporate directorates.

*Panel A. Director firm count*

| # | Frequency | Percent |
|---|---|---|
| 1 | 6005 | 22.4% |
| 2 | 4862 | 18.1% |
| 3 to 5 | 7635 | 28.5% |
| 6 to 10 | 5940 | 22.2% |
| 10+ | 2355 | 8.8% |
| All | 26,797 | 100% |

*Panel B. Annual seat count*

| # | Frequency | Percent |
|---|---|---|
| 1 | 104,860 | 81.1% |
| 2 | 16,573 | 12.8% |
| 3 to 5 | 7,644 | 5.9% |
| 6 to 10 | 254 | 0.2% |
| All | 129,331 | 100% |

*Panel C: Role of interlocked directors*

| | Historically-interlocked | | | | Contemporaneously-interlocked | |
|---|---|---|---|---|---|---|
| | Potential acquirers | | Potential targets | | | |
| | Freq. | Percent | Freq. | Percent | Freq. | Percent |
| Executive | 6,525 | 38.7% | 5,775 | 36.4% | 9,984 | 43.6% |
| Inside director | 4,232 | 25.1% | 5,188 | 32.7% | 6,480 | 28.3% |
| Former Emp. | 742 | 4.4% | 1,095 | 6.9% | 1,259 | 5.5% |
| Prof. director | 1,079 | 6.4% | 1,253 | 7.9% | 962 | 4.2% |
| Other | 4,283 | 25.4% | 2,554 | 16.1% | 4,213 | 18.4% |
| Total | 16,861 | 100.0% | 15,865 | 100.0% | 22,898 | 100.0% |

## Table II
## Acquisitions 1996-2006

The table presents data on announced acquisitions between January 1, 1996 and December 31, 2006 by S&P 1500 firms with a total deal value exceeding $1 million and where the acquirer obtains at least 20% of the target's shares and owns a majority of the target's shares after the deal. Panel A lists all announced mergers by year of announcement, with median and total amounts in millions of U.S. dollars. Panel B shows the breakdown of announced acquisitions by industry of the target, where manufacturing corresponds to SIC codes 2000-3999, services corresponds to SIC codes 7000-8999, finance corresponds to SIC codes 6000-6799, retail/wholesale trade corresponds to SIC codes 5000-5999, utilities corresponds to SIC codes 4000-4999, and primary inputs corresponds to SIC codes 0001-1799. Panel C shows, at the firm-pair level, the frequency of historical and contemporaneous interlocks alone and based on whether the two firms are headquartered in the same county, where historical and contemporaneous interlocks are defined in Section II.

*Panel A: Classification by time period and size of the deal*

| Year | N | % | Median | Total |
|---|---|---|---|---|
| 1996 | 463 | 9.6 | 48.3 | 50,543 |
| 1997 | 555 | 11.5 | 31.6 | 57,953 |
| 1998 | 583 | 12.0 | 59.4 | 94,831 |
| 1999 | 546 | 11.3 | 50.0 | 65,449 |
| 2000 | 469 | 9.7 | 65.0 | 81,695 |
| 2001 | 397 | 8.2 | 53.9 | 64,273 |
| 2002 | 373 | 7.7 | 42.3 | 45,833 |
| 2003 | 326 | 6.7 | 62.0 | 45,925 |
| 2004 | 363 | 7.5 | 104.7 | 171,861 |
| 2005 | 385 | 7.9 | 92.2 | 192,396 |
| 2006 | 386 | 8.0 | 90.0 | 153,169 |
| Total | 4,846 | 100 | 59.3 | 1,023,928 |

*Panel B: Acquisitions by target firm industry*

| Industry | N | % | Industry | N | % |
|---|---|---|---|---|---|
| Manufacturing | 1,581 | 32.6 | Utilities | 240 | 4.9 |
| Services | 1,440 | 29.7 | Primary inputs | 218 | 4.5 |
| Finance | 794 | 16.4 | Other | 13 | 0.3 |
| Retail/wholesale trade | 560 | 11.6 | Total | 4,846 | 100% |

*Panel C: Cross-firm connections for merging pairs*

| Measure | % | & same county | N |
|---|---|---|---|
| Historical interlocks | 9.1% | 3.9% | 809 |
| Contemporaneous interlocks | 6.3% | 1.7% | 809 |
| No inter-firm connection | 84.6% | 94.4% | 809 |

## Table III
## Firm-Level Descriptive Statistics

The table compares characteristics of all in-sample actual acquirers and targets with the mean for all S&P 1500 firms over the period 1996-2006. Assets is total book value of assets, market capitalization is the total market value of equity computed as common shares outstanding times the fiscal year closing price, cash-to-assets is measured as the ratio of cash to total book assets. Debt-to-assets is measured as the ratio of short and long-term debt to total assets and sales-to-assets is total sales revenue divided by total assets. The construction of the market-to-book ratio is described in detail in Section III.A. All amounts are in millions of U.S. dollars.

|  | Acquirers | | Targets | | S&P 1500 | |
| --- | --- | --- | --- | --- | --- | --- |
|  | Mean | Median | Mean | Median | Mean | Median |
| Total assets | 31,020 | 8,493 | 5,617 | 1,300 | 5,037 | 231.6 |
| Market capitalization | 48,684 | 11,342 | 5,355 | 1,714 | 3,988 | 250.5 |
| Cash-to-assets | 0.07 | 0.04 | 0.09 | 0.04 | 0.12 | 0.05 |
| Debt-to-assets | 0.23 | 0.21 | 0.25 | 0.23 | 0.27 | 0.17 |
| Sales-to-assets | 0.76 | 0.71 | 0.90 | 0.72 | 0.91 | 0.73 |
| Market-to-book | 3.03 | 2.03 | 2.36 | 1.71 | 2.90 | 1.55 |

## Table IV
### Effect of Director Experience on Industry Choice

The table reports estimates from logit regressions where the dependent variable is unity if a firm initiates an acquisition of a target in industry $k$ in year $t$. The historical interlock is an indicator set to unity if the firm has a current director who is historically interlocked with a target in industry $k$ as defined in Section II.A. The regression in column (2) adds an indicator set to unity if the acquirer has a director with a contemporaneous interlock with a firm in industry $k$. The regression in column (3) adds controls for leverage and the log of total assets, while column (4) adds the logs of sales and cash, as well as the market-to-book ratio. All regressions include fixed effects for years. Robust standard errors clustered at the firm level appear in parentheses beneath the coefficient estimates. *, ** and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

| Variable | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Historical interlock (industry) | 1.778*** (0.047) | 1.241*** (0.053) | 1.159*** (0.053) | 1.136*** (0.053) |
| Contemporaneous interlock (industry) | | 1.326*** (0.041) | 1.233*** (0.041) | 1.209*** (0.041) |
| Total assets | | | 0.112*** (0.008) | 0.027* (0.015) |
| Leverage ratio | | | -0.426*** (0.072) | -0.286*** (0.071) |
| Sales | | | | 0.072*** (0.015) |
| Cash | | | | 0.033*** (0.010) |
| Market-to-book ratio | | | | 0.189*** (0.032) |
| Year effects | yes | yes | yes | yes |
| Observations (thousands) | 1,445 | 1,445 | 1,445 | 1,301 |
| Pseudo $R^2$ | 0.015 | 0.030 | 0.033 | 0.034 |

## Table V
## Effects of a Historical Interlock on Match Pairing

This table reports estimates from logit regressions for the pair-specific match propensity where the dependent variable is unity if firm $i$ announced an acquisition of potential target $j$ in period $t$. The variable of interest is the historical interlock (defined in Section II.A). Column (2) adds an indicator for whether there is a contemporaneous interlock between the two firms. Column (3) adds controls for relative size, relative sales-to-assets, and relative market-to-book ratios. Column (4) includes binary indicators set to unity when the potential pair are in the same 4-digit SIC industry or are headquartered in the same county. Column (4) adds industry fixed effects defined at the major SIC level. All regressions include fixed effects for years. Robust standard errors clustered at the firm-pair level appear in parentheses beneath the coefficient estimates. *,**, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

| Variable | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Historical interlock | 4.448*** | 4.274*** | 4.305*** | 3.872*** | 3.821*** |
| | (0.158) | (0.177) | (0.177) | (0.180) | (0.184) |
| Contemporaneous interlock | | 1.442*** | 1.460*** | 1.397*** | 1.366*** |
| | | (0.244) | (0.245) | (0.233) | (0.235) |
| Relative size | | | 0.005*** | 0.005*** | 0.006*** |
| | | | (0.001) | (0.001) | (0.001) |
| Relative sales to assets | | | 0.000 | 0.004 | 0.004 |
| | | | (0.016) | (0.010) | (0.010) |
| Relative market to book | | | 0.003 | 0.023** | 0.030*** |
| | | | (0.015) | (0.010) | (0.008) |
| Same 4-digit SIC | | | | 3.170*** | 3.111*** |
| | | | | (0.084) | (0.106) |
| Same county | | | | 1.142*** | 1.201*** |
| | | | | (0.137) | (0.146) |
| Year fixed effects | yes | yes | yes | yes | yes |
| Industry fixed effects | no | no | no | no | yes |
| Observations (thousands) | 12,634 | 12,634 | 12,634 | 12,634 | 12,633 |
| Pseudo $R^2$ | 0.034 | 0.037 | 0.040 | 0.135 | 0.141 |

## Table VI
### Effects of Interlocks on Match Pairing:
### Reverse Historical Interlocks

The table reports estimates from logit regressions for the pair-specific match propensity where the dependent variable equals unity if an acquisition is announced by potential acquirer $i$ for potential target $j$ in period $t$. The variables of interest are historical interlocks in the first row and reverse historical interlocks from the potential target to a potential acquirer (defined in Section IV.A) in the second row. Column (1) includes reverse historical interlocks only and Column (2) adds historical interlocks, while Column (3) adds contemporaneous interlocks. Column (4) adds controls for relative size, sales-to-assets, and market-to-book ratios, and binary indicators for the potential pair being in the same 4-digit SIC industry and for being headquartered in the same county. Column (5) adds fixed effects for industries at the major SIC level. All estimating equations include fixed effects for years. Robust standard errors clustered at the firm-pair level appear in parentheses beneath the estimated coefficients. *,**, and *** denote statistical significance at the 10%, 5%, and 1% levels.

| Variable | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Historical interlock | | 4.453*** | 4.284*** | 3.899*** | 3.849*** |
| | | (0.159) | (0.177) | (0.180) | (0.183) |
| Reverse historical interlock | 0.431 | -0.333 | -0.739 | -1.274 | -1.318 |
| | (1.000) | (1.012) | (1.026) | (1.060) | (1.071) |
| Contemporaneous interlock | | | 1.458*** | 1.441*** | 1.413*** |
| | | | (0.245) | (0.233) | (0.236) |
| Relative size | | | | 0.006*** | 0.006*** |
| | | | | (0.001) | (0.001) |
| Relative sales-to-assets | | | | 0.004 | 0.004 |
| | | | | (0.010) | (0.010) |
| Relative market-to-book | | | | 0.023** | 0.030*** |
| | | | | (0.010) | (0.008) |
| Same 4-digit SIC | | | | 3.173*** | 3.115*** |
| | | | | (0.084) | (0.106) |
| Same county | | | | 1.138*** | 1.198*** |
| | | | | (0.138) | (0.146) |
| Year fixed effects | yes | yes | yes | yes | yes |
| Industry fixed effects | no | no | no | no | yes |
| Observations (thousands) | 12,634 | 12,634 | 12,634 | 12,634 | 12,633 |
| Pseudo $R^2$ | 0.008 | 0.034 | 0.037 | 0.135 | 0.141 |

## Table VII
## Firm Pairings and Director Characteristics

The table reports estimates from logit regressions for the pair-specific match propensity where the dependent variable equals unity if an acquisition is announced by firm $i$ for potential target $j$ in period $t$. Each column estimates the full specification of equation (2) and interacts historical interlocks (HI) with an indicator variable for an individual director characteristic. In each case, the indicator variable for the characteristic is also included in the vector of controls. Columns (1)-(3) interact historical interlocks with characteristics of the director at the acquirer and columns (4)-(6) include interactions with characteristics of the director during past service at the potential target. Columns (1), (2) and (3) employ binary indicators set to unity if a historically-interlocked director is also an executive, an inside director, or holds an ownership stake of less than 1 percent at the potential acquirer in year $t$. Columns (4), (5) and (6) include indicators set to unity if a historically-interlocked director was also an executive, an inside director, or a former employee at the potential target during past service there. Robust standard errors clustered at the firm-pair level appear in parentheses beneath the coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

| Variable | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Historical interlock (HI) | 3.967*** | 3.875*** | 4.041*** | 3.268*** | 3.396*** | 3.736*** |
| | (0.199) | (0.197) | (0.206) | (0.332) | (0.274) | (0.194) |
| HI × Executive (acquirer) | -0.514 | | | | | |
| | (0.357) | | | | | |
| HI × Inside director (acquirer) | | -0.236 | | | | |
| | | (0.359) | | | | |
| HI × Low ownership (acquirer) | | | -0.828** | | | |
| | | | (0.408) | | | |
| HI × Executive (target) | | | | 0.729** | | |
| | | | | (0.321) | | |
| HI × Inside director (target) | | | | | 0.777** | |
| | | | | | (0.373) | |
| HI × Former employee (target) | | | | | | 0.751* |
| | | | | | | (0.435) |
| Full set of controls | yes | yes | yes | yes | yes | yes |
| Year fixed effects | yes | yes | yes | yes | yes | yes |
| Industry effects | yes | yes | yes | yes | yes | yes |
| Observations (thousands) | 12,633 | 12,633 | 12,633 | 12,633 | 12,633 | 12,633 |
| Pseudo R$^2$ | 0.141 | 0.141 | 0.141 | 0.141 | 0.141 | 0.141 |

Table XIII

Historical Interlocks and Corporate Governance

The table reports estimates from logit regressions for the pair-specific match propensity where the dependent variable equals unity if an acquisition is announced by firm $i$ for potential target $j$ in period $t$. Each column estimates the full specification of equation (2) and includes an interaction term of the presence of historical interlocks (HI) with a characteristic of the acquirer's corporate governance. In each case, the variable for corporate governance is included in the vector of controls. These measures are described in Section IV.C. Column (1) re-estimates the specification from column (4) of Table 5 using only observations for which corporate governance data are available. Columns (2), (3) and (4) interact the presence of a historical interlock with indicator variables for duties, contract and care (also discussed in Section IV.C). Column (5) interacts historical interlocks with the governance index from Gompers et. al (2003). Columns (6) and (7) interact the presence of a historical interlock with indicators for the presence of control-share acquisition laws and supermajority provisions, respectively. Robust standard errors clustered at the firm-pair level appear in parentheses beneath the coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

| Variable | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Historical Interlock | 1.250*** | 1.283*** | 1.237*** | 1.333*** | 1.858*** | 1.255*** | 1.272*** |
| | (0.090) | (0.103) | (0.094) | (0.116) | (0.299) | (0.093) | (0.094) |
| HI × Duties | | 1.283*** | | | | | |
| | | (0.103) | | | | | |
| HI × Contract | | | 0.164 | | | | |
| | | | (0.307) | | | | |
| HI × Care | | | | -0.193 | | | |
| | | | | (0.177) | | | |
| HI × G-index | | | | | -0.067** | | |
| | | | | | (0.032) | | |
| HI × Control-share | | | | | | -0.041 | |
| | | | | | | (0.369) | |
| HI × Supermajority | | | | | | | -0.239 |
| | | | | | | | (0.347) |
| Full vector of controls | yes | yes | yes | yes | yes | yes | yes |
| Year effects | yes | yes | yes | yes | yes | yes | yes |
| Industry Effects | yes | yes | yes | yes | yes | yes | yes |
| Observations (thousands) | 3,729 | 3,729 | 3,729 | 3,729 | 3,729 | 3,729 | 3,729 |
| Pseudo R-squared | 0.161 | 0.164 | 0.163 | 0.165 | 0.164 | 0.164 | 0.164 |

## Table IX
## Historical Interlocks and Information Environment

The table reports estimates from logit regressions for the pair-specific match propensity where the dependent variable equals unity if an acquisition is announced by firm $i$ for potential target $j$ in period $t$. The variables of interest are historical interlocks in the first row, contemporaneous interlocks in the second row, and their respective interactions with a binary indicator set to unity if the two firms are headquartered in the same county. All columns estimate the full specification of equation (2) while at the same time including either or both of the interaction terms. Column (1) includes the interaction of the presence of a historical interlock and the same county dummy. Column (2) includes, instead, the interaction of the presence of a contemporaneous interlock and the same county dummy. Column (3) includes both interaction terms. All estimating equations include fixed effects for years and for major SIC industries. Robust standard errors clustered at the firm-pair level appear in parentheses beneath the estimated coefficients. *,**, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

| Variable | (1) | (2) | (3) |
|---|---|---|---|
| Historical interlock (HI) | 4.192*** | 3.824*** | 4.224*** |
|  | (0.182) | (0.184) | (0.182) |
| Contemporaneous interlock | 1.339*** | 1.212*** | 1.104*** |
|  | (0.233) | (0.283) | (0.287) |
| HI × Same county | -2.082*** |  | -2.207*** |
|  | (0.558) |  | (0.587) |
| CI × Same county |  | 0.549 | 0.864* |
|  |  | (0.472) | (0.455) |
| Full vector of controls | yes | yes | yes |
| Year fixed effects | yes | yes | yes |
| Industry fixed effects | yes | yes | yes |
| Observations (thousands) | 12,633 | 12,633 | 12,633 |
| Pseudo $R^2$ | 0.143 | 0.142 | 0.143 |

## Table X
## Deal Returns

The table reports estimates of regressions where the dependent variable for an announced acquisition by acquirer $i$ of target $j$ in year $t$ is the cumulative abnormal return (defined in Section IV.E) in columns (1)-(4) and where the dependent variable equals the deal premium on announced acquisitions in columns (5)-(6), defined as the ratio of the price paid per share for the target divided by the target's price four weeks prior to the announcement of the deal. The variables of interest are the historical and contemporaneous interlocks (defined in Section II.A), the majority cash payment indicator, and the interactions of the interlock variables with the majority cash indicator. The control variables, described in Section IV.E, are indicators for whether the deal was a tender offer or was unsolicited, as well as the ratio of total assets for the two firms and the money value of the transaction. All specifications include fixed effects for years. Robust standard errors appear in parentheses beneath the coefficient estimates. *,**, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

| | Acquirer returns | | Target returns | | Deal premia | |
|---|---|---|---|---|---|---|
| Variable | (1) | (2) | (3) | (4) | (5) | (6) |
| Historical interlock (HI) | -0.002 | -0.008 | 0.003 | 0.005 | -0.030 | -0.066 |
| | (0.007) | (0.008) | (0.022) | (0.024) | (0.042) | (0.048) |
| Contemporaneous interlock (CI) | -0.026* | -0.026* | 0.019 | 0.019 | 0.091* | 0.088* |
| | (0.012) | (0.012) | (0.027) | (0.027) | (0.053) | (0.053) |
| Cash × HI | | 0.037** | | -0.013 | | 0.149* |
| | | (0.015) | | (0.038) | | (0.083) |
| Cash | 0.022** | 0.020** | 0.064*** | 0.064*** | 0.008 | -0.005 |
| | (0.008) | (0.008) | (0.014) | (0.014) | (0.034) | (0.037) |
| Unsolicited | 0.011 | 0.011 | -0.040* | -0.040* | 0.101** | 0.103** |
| | (0.007) | (0.007) | (0.019) | (0.019) | (0.041) | (0.041) |
| Relative size | 0.003 | 0.003 | 0.024** | 0.024** | 0.057*** | 0.056*** |
| | (0.002) | (0.002) | (0.010) | (0.010) | (0.015) | (0.015) |
| Transaction value | -0.006** | -0.006** | -0.001 | -0.001 | 0.003 | 0.001 |
| | (0.003) | (0.002) | (0.004) | (0.004) | (0.013) | (0.013) |
| Year effects | yes | yes | yes | yes | yes | yes |
| Observations | 424 | 424 | 424 | 424 | 382 | 382 |
| R-squared | 0.141 | 0.145 | 0.143 | 0.149 | 0.198 | 0.202 |

## Table XI
## Linear Fixed Effect Models and
## Estimates of Director Selection

Panel A reports estimates from linear probability models for the pair-specific match propensity expressed as the percent increase in the probability that acquirer $i$ announces an acquisition of potential target $j$ in period $t$. Column (1) includes the historical interlock and year effects only. Column (2) includes contemporaneous interlocks and column (3) includes the full vector of controls from Table 4. All estimating equations include fixed effects for years and for ordered firm pairs. T-statistics derived from robust standard errors clustered at the firm-pair level appear in parentheses beneath the coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Each cell of Panel B reports ratios based on the coefficient for historical interlocks from regressions on the probability that firm $i$ acquires firm $j$ in year $t$. The ratio is calculated as $\beta^f/(\beta^r - \beta^f)$, where $\beta^r$ is the estimate on historical interlocks obtained from an equation using a restricted set of controls and $\beta^f$ is the estimate on historical interlocks from the full regression which includes firm-pair specific fixed effects. The restricted set of controls is a constant (column (1)), year fixed effects (column (2)), year fixed effects and contemporaneous interlocks (column (3)), and year effects, contemporaneous interlocks, and the full set of controls less firm-pair fixed effects (column (4)).

*Panel A: Effect of historical interlock from fixed effects model*

| Variable | (1) | (2) | (3) |
|---|---|---|---|
| Historical interlock | 740.8*** | 759.0*** | 758.9*** |
|  | (4.84) | (4.89) | (4.89) |
| Contemporaneous interlock |  | 34.2*** | 34.2*** |
|  |  | (3.14) | (3.14) |
| Full vector of controls | no | no | yes |
| Year fixed effects | yes | yes | yes |
| Firm-pair fixed effects | yes | yes | yes |

*Panel B: Extent of unobserved heterogeneity*

| Variable | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| $\theta_r$ | 9.77 | 9.86 | 10.18 | 59.60 |
| Year fixed effects | no | yes | yes | yes |
| Contemporaneous interlock | no | no | yes | yes |
| Vector of controls | no | no | no | no |
| Firm-pair fixed effects | no | no | no | yes |

## Table XII
## Matching Estimators

This table reports matching estimates for the pair-specific match propensities described in Section V.C. The first column reports unmatched estimates of the average treatment effect. Columns (2), (3) and (4) report one, two and three nearest-neighbor matching estimates of the effect of historical interlocks on acquisitions. Average treatment effects are expressed as percentage increases relative to the control group. T-statistics appear in parentheses. *, ** and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

|  | Unmatched | 1 neighbor | 2 neighbor | 3 neighbor |
|---|---|---|---|---|
|  | (1) | (2) | (3) | (4) |
| Treatment Effect | 2,501 | 2,071 | 2,069 | 1,551 |
| $t$-statistic | [31.62] | [10.14] | [10.14] | [10.00] |