Summer 2012

Essays in empirical corporate finance: CEO compensation, social interactions, and M&A

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ESSAYS IN EMPIRICAL CORPORATE FINANCE:

CEO COMPENSATION, SOCIAL INTERACTIONS, AND M&A

by

Feng Jiang

An Abstract

Of a thesis submitted in partial fulfillment of the
requirements for the Doctor of Philosophy
degree in Business Administration
in the Graduate College of
The University of Iowa

July 2012

Thesis Supervisor: Professor Erik Lie

ABSTRACT

This thesis consists of three essays and studies CEO compensation and mergers and acquisitions in empirical corporate finance. The first essay is sole-authored and is titled 'The Effect of Social Interactions on Executive Compensation.' The second essay 'The Role of Investment Banker Directors in M&A: Can Experts Help?' is a joint work with Qianqian Huang, Erik Lie, and Ke Yang. The third essay is titled 'The Strategic Use of CEO Compensation in Labor Contract Negotiations' and is coauthored with Erik Lie and Tingting Que.

In the first essay, I examine how executives' social interactions affect their compensation. Using the social networks among 2,936 chief executive officers (CEOs) during 1999–2008, I report that the compensation of a pair of socially connected CEOs is significantly more similar than that of a pair of non-connected CEOs. I further find that CEO compensation responds to a peer's change in pay caused by industry performance, especially if that change in pay is positive rather than negative and when the firm is suffering from weak corporate governance. I interpret these results as consistent with the notion that relative earnings concerns within social networks affect negotiations about compensation. Finally, I find that the past practice of backdating stock option grants spread across social networks, suggesting that social networks serve as a conduit for interpersonal information flow about compensation practices. Taken together, I show that CEOs' peer interactions have a substantial impact on executive pay.

In the second essay, we examine how directors with investment banking experience affect firms' acquisition behavior. We find that firms have a higher probability of acquisition when an investment banker is a director. Furthermore, acquirers with investment banker directors on the board have significantly higher announcement returns, especially if the deal is relatively large and the bankers' experience and/or network is current. We also find evidence that investment banker directors help reduce the takeover premium and advisory fees paid to outside consultants. Finally, the presence of investment banker directors is positively related to long-run operating and stock performance.

Lastly, in the third essay, we study whether firms strategically alter CEO compensation to improve their bargaining position with labor unions. We conjecture that (i) firms in heavily unionized industries offer lower compensation packages to their CEOs than do their non-union counterparts, (ii) unionized firms temporarily curtail CEO compensation before union contract negotiations, and (iii) the curtailment in compensation is most pronounced for option grants due to their discretionary nature. Our results support these conjectures. We also find that CEOs are more likely to sacrifice compensation if they hold a relatively large stake in the company whose value depends on the contract negotiations. Finally, we report evidence that curtailing CEO compensation helps reduce the negotiated salary growth.

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CERTIFICATE OF APPROVAL

PH.D. THESIS

This is to certify that the Ph.D. thesis of

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has been approved by the Examining Committee for the thesis requirement for the Doctor of Philosophy degree in Business Administration at the July 2012 graduation.

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To my parents

ACKNOWLEDGEMENTS

I would like to gratefully and sincerely thank Erik Lie, my dissertation committee chair, for his guidance, patience and enormous support during my graduate studies at the University of Iowa. This dissertation would not have been possible without the expert guidance of Professor Erik Lie. I am also grateful to valuable advice from my dissertation committee: Jon Garfinkel, Paul Hribar, Shagun Pant, and Yiming Qian. I especially want to thank Qianqian Huang and Tong Yao, my wonderful friends and colleagues, for spending uncountable hours with me on developing my thesis, practicing my job talks and interviews. I also thank Matt Billett, Tyler Leverty, Wei Li, Amrita Nain, Thomas Rietz, Ashish Tiwari, and Yilei Zhang for their helpful comments and suggestions on my job market paper. Finally, my sincere gratitude goes to my wife, Xiaoyan Yang, for her continuous support during my doctoral study and for her insightful comments on my dissertation. My son, Andrew, and my daughter, Annie, have been the greatest motivation for me to strive in this profession. I am indebted to my parents, Ronghua and Miaolan, for their encouragement and unconditional love. I dedicate this dissertation to my family. I am responsible for any remaining errors.

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CHAPTER 1
THE EFFECT OF SOCIAL INTERACTIONS ON EXECUTIVE COMPENDATION

1.1 Introduction

Social Executive compensation is regularly debated in the popular press and is often viewed as excessive, unfair, and ineffective. For example, on April 4, 2011, *USA Today* writes, "Says Kevin Murphy, professor of finance at the University of Southern California, 'We have the recipe for controversy over CEO pay: big increases in CEO pay that show up following run-ups in stock prices coupled with high unemployment rates.'" Academia has followed with extensive research on the trends in and determinants of executive compensation. The empirical results suggest that executive compensation levels depend on various firm characteristics such as firm size and performance (e.g., Murphy, 1999; Jensen and Murphy, 1990; Rosen, 1982) and executive characteristics such as CEO talent, general skills, and unobservable time invariant characteristics (e.g., Claudia, Ferreira, and Matos, 2010; Cremers and Grinstein, 2010; Engelberg, Gao, and Parsons, 2010; Graham, Li, and Qiu, 2009; Li, Falato, and Mibourn, 2010; and Murphy and Zabojnik, 2004 and 2007).

Presumably, executive compensation decisions are not only based on characteristics of the firms and executives themselves, but also on concurrent trends in the larger labor market. Indeed, research indicates that compensation decisions are influenced by similar decisions in benchmark firms (e.g., Faulkender and Yang, 2009; Bizjak, Lemmon, and Naveen, 2008 and 2010). In addition, executives commonly have a broad personal network that influences perceptions about the labor market and their own value. I find that, on average, chief executive officers (CEOs) in S&P 1500 firms are connected to fifteen CEOs from other S&P 1500 firms via educational background, past and/or current employment, and social activities. The purpose of this study is to examine how such connections affect executive compensation.

There are several channels through which CEOs' peer interactions might affect executive compensation. First, social networks serve as a conduit for information flow about compensation levels and changes, how to bargain with a board of directors, how to structure compensation, and how to choose compensation consultants.[1] This information might, in turn, enter into bargaining and decisions about compensation. Second, peer interactions could induce envy about compensation, resulting in efforts to "keep up with the Joneses" when negotiating compensation. This is based on the broader phenomenon of "relative earnings concerns," in which individuals place utility on both their own income as well as how that income compares to that of those in their social network (Frank, 1985; Luttmer, 2005; Card, Mas, Moretti, and Saez, 2011).[2] Third, executives' perceived value of their human capital and/or outside options is likely to move with the performance of socially connected CEOs. For example, peers' industry performance or shocks may alter the outside options of executives if job referrals operate through social networks.

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[1] There is growing literature suggesting that information and beliefs travel through social networks. For example, Hong, Kubik, and Stein (2005) report the evidence that fund managers spread information about stocks to one another by word of mouth. Cohen, Frazzini, and Malloy (2008) show that mutual fund managers gain informational advantages when investing in firms managed by those in their education networks. Cohen, Frazzini, and Malloy (2010) also show that analysts gain information advantages when they have an educational link to the company. Engelberg, Gao, and Parsons (forthcoming) find that personal connections between employees at firms and banks lead to better information flow.

[2] For early classical references, see Veblen (1899), Duesenberry (1949), Becker (1974), Pollak (1976), and Easterlin (1974). Akerlof and Yellen (1990) provide an extensive review of the literature (mostly outside of Economics) on the impact of relative pay comparisons.

In this study, I first examine whether two socially connected CEOs receive more similar compensation than two non-connected CEOs. Regardless of how peer influence occurs, I expect that CEOs' social interactions lead compensation of socially connected CEOs to become more similar than that of non-connected CEOs. Using the social networks among 2,936 CEOs over the period of 1999 to 2008, I find that, relative to non-connected CEOs, socially connected CEOs receive significantly more similar compensation, even after controlling for executive- and firm-fixed effects. For example, the variation in compensation among socially connected CEOs is 15 to 18 percent less than that among non-connected CEOs. I further show that the greater similarity in compensation for socially connected CEOs is not attributable to industry connections or geographic proximity. Nor are they attributable to similarities in unobserved executive characteristics and/or management styles.

Motivated by a recent paper by Shue (2011), I conduct a test of "pay for friend's luck" to better understand how peer interactions influence CEO compensation. Here, "lucky pay" is defined as the part of CEO compensation that can be predicted using mean industry returns (Bertrand and Mullainathan, 2001; Garvey and Milbourn, 2006), and the change in lucky pay can therefore be both positive and negative. I find that CEO compensation responds significantly more to the change in lucky pay of socially connected CEOs than to that of non-connected CEOs, even after controlling for own firm and industry performance.[3] This is consistent with the idea that relative earnings within executive social networks enter the utility function, such that a change in compensation

[3] Following Shue (2011), the analysis is restricted to peers working in distant (as defined using BEA input and output tables), highly-aggregated industries to reduce the likelihood that shocks to peers in different industries will have significant direct unobserved effects on CEOs.

caused by lucky industry performance for one CEO triggers connected CEOs to negotiate for higher compensation as well.

An alternative explanation for the response to a peer's lucky pay is that the CEO's outside options change with the industry performance of peers in one's social network. To differentiate the relative earnings from the outside options explanation, I investigate whether the CEO compensation response to a peer's change in lucky pay (i) is asymmetric and (ii) depends on the quality of firm governance. I first argue that if CEOs value relative earnings, they will primarily respond to friends' change in lucky pay if it is positive (hence, lucky in the traditional sense of the word). I find that CEO compensation responds more prominently to peer's change in lucky pay when it is positive than when it is negative, which suggests that "pay for friend's luck" is at least partially, if not entirely, driven by CEOs valuing relative earnings within their social networks. Next, I conjecture that the ability for a CEO to negotiate higher compensation in response to a peer's lucky pay is greater among poorly governed firms. Alternatively, I conjecture that if a CEO's outside options move with the industry performance of peers, the response in CEO compensation to a peer's lucky pay should be at least as great in well-governed firms as in poorly governed firms. Consistent with the former conjecture, I report that "pay for friend's luck" is more pronounced in poorly governed firms, according to the measure introduced by Gompers, Ishii, and Metrick (2003).

To determine whether social networks serve as a conduit for interpersonal information flow and beliefs, I test whether CEOs' social networks contribute to the spread of executive option grant backdating across firms. If information on compensation practices spread through CEOs' social networks *and* such information influences compensation decisions, I expect that CEOs are more likely to engage in option backdating if their peers are also involved in such a practice. Using the option grant data from 1996 through 2002, I find that CEOs' social networks play an important role in the spread of option backdating.

I report additional evidence consistent with the argument that CEOs value relative compensation within their social networks. I show that a CEO's compensation will significantly (both statistically and economically) increase if her compensation is below the median of her social peers, especially when governance is poor. The results are robust to the introduction of a "naïve" (industry- and size-matched) peer group into the model (Bizjak, Lemmon, and Nguyen, 2008 and 2010), thus alleviating the concern that the increase in pay is driven by those CEOs who are underpaid relative to their industry- and size-matched peers.

Finally, I find that my results hold for several categories of social connections, including those arising from a common educational history, crossroads in employment, and other (social) activities connections. The results seem overall weaker for educational connections than for the two other categories, but I am cautious about interpreting these relative effects, because the measures for the various connections are all noisy.

In summary, I document substantial peer effects in top executive compensation and shed some light on the potential channels through which endogenous peer interactions lead to more similar compensation practices for socially connected CEOs. In particular, I conclude that the peer effect is due to both sharing of compensation matters and efforts to "keep up with the Joneses" within the network.

This paper is related to a recent paper by Shue (2011). Using historical random assignment of MBA students to sections at Harvard Business School, Shue shows that executive peer networks are important determinants of managerial decision-making and firm policies. She finds that, within a class, executive compensation and acquisitions strategies are significantly more similar among graduates from the same section than among graduates from different sections. She also demonstrates the important role of ongoing social interactions by showing that peer effects are more than twice as strong in the year immediately following staggered alumni reunions. Finally, she shows that peer effects in compensation are not driven by similarities in underlying managerial productivity using a test of “pay for friend’s luck.”

This paper differs from Shue (2011) in at least four aspects. First, while Shue examines a sample of top executives (CEO/CFO) who graduated from Harvard Business School, which accounts for less than 6 percent of all top executives (less than 4 percent of all CEOs) in the ExecuComp dataset, I examine more than 90 percent of the CEOs in ExecuComp, thereby facilitating a more systematic and generalized study of the peer influences in executive pay. Second, Shue examines only the effect of educational connections, whereas I also examine the effect of job connections and other social connections.[4] To alleviate the concern that peer effects in CEO compensation might be driven by unobservable time invariant executive and/or firm characteristics, I control for executive- and firm-fixed effects in the first-stage, which is a unique feature of my research design. Third, I extend the analysis of “pay for friend’s luck” by separating positive versus negative luck and good versus poor governance to disentangle the

[4] The results seem overall weaker for educational connections than for the two other categories.

channels through which the peer effect works. This part of my analysis shows that the peer effect is at least partially attributable to envy among peers affecting compensation negotiations. Fourth, unlike Shue, I examine a specific form of stealth compensation, namely backdating of stock options, to test the joint hypothesis that information about compensation practices is shared in social networks and that such sharing information affects decisions about compensation. My results support this joint hypothesis.

My study also contributes to the executive compensation literature in general. More specifically, this study adds to our understanding of several controversial compensation practices in recent years. The first practice is that CEOs are paid for firm performance beyond their control. In other words, CEOs are paid for luck (Bertrand and Mullainathan, 2001). Garvey and Milbourn (2006) extend this research and document evidence that the pay-for-luck relationship is asymmetric in that CEOs are rewarded for good luck but not punished for bad luck. Harford and Li (2007) show that the asymmetry is much stronger following an acquisition. The evidence in this paper suggests that there exists a contagious effect in lucky pay among connected CEOs, i.e., CEOs are also paid for their friend's lucky industry performance, especially if that lucky pay is up.

The second practice is that a large number of firms, perhaps thousands, have granted backdated options to their executives (Lie, 2007; Heron and Lie, 2007; Heron and Lie, 2009; Bizjak, Lemmon, and Whitby, 2009; Collins, Gong, and Li, 2009; Bebchuk, Grinstein, and Peyer, 2009). It is not clear how this practice started and how it became so pervasive. Board interlock is one important channel (Bizjak, Lemmon, and Whitby, 2009). But this study identifies another important channel (i.e., executives' social networks) through which the practice of option backdating spread across firms.

The third practice is that compensation in peer firms is used as a benchmark when setting executive compensation, which gives an incentive to select a peer group with particularly high compensation (Faulkender and Yang, 2009; Bizjak, Lemmon, and Naveen, 2008 and 2010).[5] The evidence in this paper reveals that there might exist another reference point that CEOs tend to benchmark—compensation level of their social peers.

This paper also contributes to the growing literature of social network study in finance. Prior studies have shown that (i) social networks help information transfer between agents (e.g., Cohen, Frazzini, and Malloy, 2008; Cohen, Frazzini, and Malloy, 2010; Engelberg, Gao, and Parsons, forthcoming; Hong, Kubik, and Stein, 2005); (ii) social connections between CEOs and board members tend to weaken internal corporate governance (e.g., Hwang and Kim, 2009; Fracassi and Tate, 2010); and (iii) social connections have considerable impact on firm policies, including CEO compensation and executive turnover (e.g., Butler and Gurun, 2009; Engelberg, Gao and Parsons, 2010; Hwang and Kim, 2009; Liu, 2008), mergers and acquisitions (Cai and Sevilir, 2009; Fracassi and Tate, 2010; Ishii and Xuan, 2010; Schmidt, 2008), and firm investment (Fracassi, 2008).

The remainder of the paper is organized as follows. Section 2 describes the data and sample selection and Section 3 presents the empirical model. Section 4 provides the results. Section 5 concludes.

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[5] Faulkender and Yang (2009) argue that the highly paid peer groups are chosen to inflate CEO compensation, while Holmstrom and Kaplan (2003) and Bizjak, Lemmon, and Naveen (2008, 2010) argue that this practice is mainly consistent with competitive compensation to CEOs.

1.2 Data and Sample Selection

The data to construct social connections among CEOs are collected from various sources. I start with all publicly traded U.S. companies in CRSP and restrict that set to firms with common shares only (share code 10 and 11, according to CRSP). In order to construct the social networks among CEOs, I merge the sample with the BoardEx database, which provides extensive biographical information, such as educational background, employment history, and other social activities of corporate directors and senior executives in major public firms.[6] To ensure the quality of the data integration procedure, I manually check all matches and make necessary adjustments. For example, the same firm might be assigned different identifiers in BoardEx because it collects individuals' biographical information from various public sources that sometimes use different spellings or abbreviations.[7] I go through the BoardEx database to ensure that each firm is associated with a unique identifier. My matching procedure yields a sample of 8,007 unique publicly traded firms between the BoardEx and CRSP/COMPUSTAT databases.[8]

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[6] Each publicly traded company in the United States is required by the SEC to provide information about the board of directors and the top five earners. BoardEx of Management Diagnostics Limited, an independent, privately owned corporate research company, collects and classifies such information and supplements it with additional publicly-available information. Several papers have used BoardEx to examine the role of social networks. My procedure is very similar to that used by Cohen, Frazzini, and Malloy (2008); Fracassi (2008); Fracassi and Tate (forthcoming); Ishii and Xuan (2010); Engelberg, Gao and Parsons (2010, forthcoming).

[7] For example, *American Tower Corp.* shows up twice in BoardEx with two different company IDs: 1954 and 460743, and *American Tower Corporation* is coded as yet another company with company ID 743135.

[8] Using a similar procedure, Engelberg, Gao and Parsons (2010) yield a sample of 8,428 unique firms between the BoardEx and CRSP/COMPUSTAT database.

1.2.1 CEOs' Social Networks

After matching firms in BoardEx to PERMNOs and GVKEYs, I again use the built-in algorithm in SAS to match CEO names in BoardEx with CEO names in ExecuComp (after an initial match of their firms by GVKEY). Then, I manually check the matches to ensure the quality of matching. My final sample consists of 2,936 unique CEOs between 1999 and 2008.[9] Since BoardEx supplies biographical information on education, past and current employment, and other activities for each CEO, I define the social networks that represent the social ties among CEOs like so:

Education Network: Two CEOs are socially connected through their education network if they went to the same school and graduated within one year of each other with the same professional, master's, or doctoral degree.

Employment Network: Two CEOs are socially connected through their past or current employment networks if they work in the same company and sit together either in the top management group or on a board of directors.

Other Activities Network: Two CEOs are socially connected through their other activity network if they share membership and have active roles in clubs, organizations, or charities.

In this study, I define two CEOs are socially connected if they are connected through any of the above three social networks.[10] Table 1.1 reports summary statistics of

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[9] Similar to other studies using the BoardEx dataset, the sample in this study starts with 1999 since BoardEx only began collecting information of corporate directors and senior executives in major public information in 1999.

[10] Other studies use a similar way to define social connections between individuals. See, for example, Fracassi (2008), Fracassi and Tate (forthcoming), Ishii and Xuan (2010), Engelberg, Gao and Parsons (2010, forthcoming), and Liu (2008).

the sample over the period 1999–2008. Panel A includes CEO compensation, demographic information, and firm characteristics. Panel B includes information and characteristics of CEOs' social peers. CEO compensation data comes from ExecuComp. Total compensation is the sum of base salary, bonus, value of restricted stock grants, and the Black Scholes value of option grants and long-term incentive plans (*TDC1* in ExecuComp). Total payout is the sum of base salary, bonus, value of restricted stock grants, and the Black Scholes value of option exercised and long-term incentive plans (*TDC2* in ExecuComp). Summary statistics for salary, salary plus bonus, total compensation, and total payout consist of winsorized means and winsorized standard deviations at the 1 percent level of both tails. CEO age, CEO tenure, and percent chair and female data come from ExecuComp. Financial information, including firm size (log of total assets), sales revenue, financial leverage, market to book, and return on assets come from Compustat. Firm and industry returns are matched to firm fiscal year month end dates and come from CRSP. Definitions of the variables are presented in the Appendix, and all dollar values are adjusted to 2008 dollars.

From Panel A, the average CEO earns $1,523 thousand in salary and bonuses. The total compensation is nearly four times that amount, $5,891 thousand. The difference indicates the large fraction of a CEO's pay is due to equity and options grants. The average CEO is roughly 55 years old and has been CEO of her firm for seven years.

From Panel B, a typical CEO in my sample is socially connected to around fifteen CEOs in other companies. I also calculate the ratio of a peer's compensation to sample CEO's pay and report the summary statistics of the ratio. It shows that, on average, a CEO's compensation is slightly below the median of her peer groups, which is not surprising since CEOs in larger firms receive higher compensation and are more likely to be socially connected to other CEOs. Since CEOs from the same industry or the same state are more likely to be socially connected, one potential concern of my analysis is that the expected peer effects could be driven by pairs of CEOs from the same industry or state. Hence, I also report the percentage of a CEO's social peers from the same industry and the same state. I find around 21 percent of the peers are from the same industry and 20 percent of the peers are from the same state.

1.2.2 Executive Stock Option Grants Data

For To provide evidence that information transfers through CEOs' social networks, I examine whether CEOs' social networks contribute to the spread of executive option grant backdating across firms. I obtain the sample of stock option grants to CEOs from the Thomson Financial Insider Filing database. This database captures insider transactions reported on SEC forms 3, 4, 5, and 144. I restrict the sample to transactions that occurred from January 1996 to December 2002.[11] I further require that stock returns be available from 20 trading days before to 20 trading days after the grant date. Finally, following Heron and Lie (2009), I only include grants to the CEO, President, or Chairman of the Board. I include all three categories because in many instances, CEOs identify themselves by an alternate title (such as the President) in their SEC filings (Heron and Lie, 2009).

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[11] Similar to Bizjak, Lemmon, and Whitby (2009), I begin with 1996 since it is the first year Thomson began collecting data on option grants, and I end my sample period in 2002 since Heron and Lie (2009) report that the incidence of backdating drops dramatically after the implementation of new insider reporting guidelines associated with the passage of SOX in August of 2002.

I eliminate any duplicate grants that occur on a given grant date, so that there is only one grant for a given grant date and company combination. Like other studies, I focus on unscheduled awards because these grants are much more likely to be manipulated (Heron and Lie, 2007 and 2009). A grant is identified as scheduled if a grant is issued on the same date, plus or minus one day, in the preceding year; otherwise, it is classified as unscheduled. My final CEO option grants sample consists of 29,421 grants across 4,326 companies over the period 1996–2002.

1.3 Empirical Model

The main empirical model in this study to measure the influence of CEOs' social interactions on executive pay (including other corporate policies) is similar to that used in Fracassi (2008) and Shue (2011). I use the Pair Model to test whether two socially connected CEOs receive more similar compensation (both the level and the change) than two non-connected CEOs. The unit of observation in the Pair Model is each pair of two CEOs. The Pair Model is a two-stage econometric model:

1st Stage: $Y_{i,t} = \alpha_0 + \alpha_1 X_{i,t} + \varepsilon_{i,t}$ (1)

2nd Stage – Compensation Level: $abs\ (\varepsilon_{i,t} - \varepsilon_{j,t}) = \beta_0 + \beta_1 I_{i,j,t} + \eta_{i,j,t}$ (2.1)

2nd Stage – Compensation Change: $abs\ ((\varepsilon_{i,t} - \varepsilon_{i,t-1}) - (\varepsilon_{j,t} - \varepsilon_{j,t-1})) = \gamma_0 + \gamma_1 I_{i,j,t} + \delta_{i,j,t}$ (2.2)

In the first stage, CEO *i*'s log annual compensation $Y_{i,t}$ is regressed over the typical control variables $X_{Pi,t}$ in the related literature.[12] One unique feature of my research

[12] For CEOs' annual compensation, the control variables in the first stage include firm size (lagged natural log of sales), lagged book-to-market, lagged financial leverage, current and lagged annul stock return, current and lagged return on assets, stock price volatility, CEO tenure, current and lagged annul industry stock return, executive and firm dummies, and year dummies. These control variables are suggested by prior research in executive compensation literature (e.g., Smith and Watts, 1992; Core, Holthausen, and Larcker, 1999; Murphy, 1999; Core, Guay, and Larcker, 2008; Graham, Li, and Qiu, 2009).

design is that I control for executive- and firm-fixed effects in the first stage, which is important as it helps alleviate the concern that peer effects in CEO compensation might be driven by unobserved time invariant executive and/or firm characteristics. The residual of the regression is the unexplained part of the compensation in which we are primarily interested. For each pair of CEOs *i* and *j* at time *t*, I take the absolute value of the difference in their residuals, $|\Delta\varepsilon| = abs\ (\varepsilon_{i,t} - \varepsilon_{j,t})$. This variable is a proxy for the difference in the CEOs' pay. I also compute the absolute value of the first difference of the change in residuals, $|\Delta\Delta\varepsilon| = abs\ ((\varepsilon_{i,t} - \varepsilon_{i,t-1}) - (\varepsilon_{j,t} - \varepsilon_{j,t-1}))$. This variable is a proxy for the difference in the changes of compensation between the two CEOs.

The unit of observation in the second stage is a pair of CEOs in a given fiscal year. If we are interested in peer similarity in level of pay, the dependent variable is equal to $|\Delta\varepsilon|$. Alternatively, if we are interested in peer similarity in change in pay, the dependent variable is the absolute value of the difference in changes in the first stage residual $|\Delta\Delta\varepsilon|$. The variables $|\Delta\varepsilon|$ and $|\Delta\Delta\varepsilon|$ are then regressed on a dummy variable $I_{i,j,t}$ for whether CEO *i* and *j* are socially connected at fiscal year *t-1*.

Two issues make it complicated to estimate standard errors and significance levels in the second stage models. First, observations in the Pair Model represent pairs of CEOs, so each CEO can appear in multiple paired observations. Second, residuals of CEO compensation panel data may exhibit serial correlation.[13] I account for the serial correlation by allowing for clustering of the error term at the CEO level for both *i* and *j*

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[13] The autocorrelation of residuals of total compensation is only -0.01 as the first stage regression controls for executive- and firm-fixed effects.

using the double-clustering algorithm from Cameron, Gelbach, and Miller (2006) and Peterson (2008).

1.4 Empirical Results

1.4.1 Baseline Results

I start by examining whether two socially connected CEOs receive more similar levels of compensation than two non-connected CEOs. For brevity, the results of the first stage regression are omitted here, but they are available upon request. Table 1.2 only presents the results of the second stage regression. My analysis focuses on two measures of CEO compensation: total compensation and total payout (*TDC1* and *TDC2* in the ExecuComp dataset, respectively).[14] The dependent variables are residuals of total compensation in columns (1) to (3) and residuals of total payout in columns (4) to (6). The coefficient β_1 in the second stage model (2.1) is expected to be negative. All standard errors are allowed to be double clustering observations by each member in an executive pair.

In columns (1) and (4), I first present the results including the dummy variable (*connection dummy*) $I_{i,j,t}$ that takes the value of one if two CEOs are socially connected at time *t-1* and zero otherwise. I also include some control variables to address potential concerns. First, I introduce a dummy variable (*same industry*) that equals one if the pair of CEOs is from the same Fama-French 49 industry at time *t-1*. Even though I already

[14] I extend the analysis to salary plus bonus, pay scheme (ratio of cash pay over total compensation), and CEO pay slice (Bebchuk, Cremers, and Peyer, 2010) and find similar results. These results are omitted for brevity, but they are available upon request.

control for executive and firm-fixed effects in the first stage, the industry control in the second stage controls for possible heteroscedasticity in the second moments of the compensation across industries. Such heteroscedasticity can influence and bias the second-stage results. For example, if the idiosyncratic variance of executive pay differs across industries, then pairs of CEOs in the same industry could have both stronger social connections and smaller difference in residuals. Second, using a similar argument, I add another dummy variable (*same state*) that takes the value of one if the headquarters of two firms are located in the same state.[15] Finally, I also add year dummies to control for idiosyncratic differences in the second moments across years. After controlling for industry, state, and year, the effect of social interactions remains (highly) statistically significant at the one percent level.

A remaining concern is that the similarities in compensation for connected CEOs are driven by the similarities in underlying executive characteristics and/or management styles (Manski, 1993). For example, it could be the case that CEOs that went to MIT together may have similar characteristics, backgrounds, and experiences, and therefore will behave and manage their companies more similarly, which leads them to receive similar compensation. Alternatively, for example, Microsoft may hire managers or board of directors with similar management characteristics, styles, or experiences. This concern is mitigated through the use of a CEO's residual compensation after controlling for executive- and firm-fixed effects. The specification in columns (2) and (5) further alleviate this concern. I introduce a control variable (*same school*) that takes the value of

[15] Bouwman (2010) and Ang, Nagel, and Yang (2010) find evidence that geography affects CEO compensation.

one if two CEOs went to the same school and earned the same professional degree, but graduated more than a year apart from each other. Similarly, I define another control variable (*same firm*) that takes the value of one if two CEOs worked for the same company as top managers or served on the board of directors, but not at the same time. These two control variables can be considered a proxy for the executive characteristics and/or management style associated with going to the same school and/or working for the same company. Results in columns (2) and (5) show that, even after controlling for these variables, the coefficient of the interested variable is still negative and statistically significant. The peer influence is also economically meaningful. The amount of variation in total compensation among socially connected CEOs is estimated to be around 16 percent less than the variation among non-connected CEOs.

To ensure that the greater similarity in pay among socially connected CEOs is not driven by board interlock between firms, I include a dummy variable (*board interlock*) which equals one if there is board interlock (but not through CEO) between the pair of firms in columns (3) and (6). Consistent with the findings in Bouwman (2011), the coefficient of board interlock is statistically negative. But more importantly, the coefficient of $I_{i,j,t}$ remains negative and highly statistically significant.

Thus far, I have provided evidence suggesting that CEOs' peer interactions lead to a similar level of executive pay between two socially connected CEOs. A natural follow-up question is whether CEOs' peer interactions lead to more similar change in compensation among connected CEOs than non-connected CEOs. In the spirit of peer interactions, the evolution of executive compensation should also be affected by social networks. To identify that peer interactions affect changes in compensation is arguably more important since changes are more useful for identifying responses to peer's shocks over time.

Table 1.3 presents the results of whether CEOs' peer interactions affect changes in pay over time. The dependent variable in the Pair Model is the difference in difference in residuals from the first-stage regression and the coefficient γ_1 is expected to be negative. The results are consistent with the findings of the Pair Model comparing compensation levels. In columns (1) and (4), I find, after controlling for possible heteroscedasticity in industry, state, and year, the coefficient of dummy variable $I_{i,j,t}$ is negative and statistically significant at the one percent level. In columns (2) and (5), I account for the possible alternative explanation that the results are driven by peer similarities in underlying managerial characteristics and/or styles. The coefficient of $I_{i,j,t}$ is still negative and statistically significant. In columns (3) and (6), I address the potential concern that the results are driven by board interlock. The coefficient of $I_{i,j,t}$ is still negative and statistically significant. Again, the impact of peer influence in the evolution of executive pay is economically significant.

The overall evidence in Tables 1.2 and 1.3 indicates a substantial peer influence in executive compensation, although I cannot completely rule out the possible alternative explanations. Therefore, in order to highlight the importance of social interactions in explaining executive compensation, in the next several sections I investigate the potential mechanisms underlying the peer similarity in pay for socially connected CEOs.

1.4.2 Pay for Friend's Luck

To better understand how social interactions influence CEO compensation and further mitigate potential concerns, I conduct a test of "pay for friend's luck" in this section. The test of "pay for friend's luck" is motivated by Shue (2011). A reaction to a peer's change in compensation caused by industry performance shocks might generate a social multiplier effect with respect to policies or shocks that affect peer compensation while leaving peer fundamentals unchanged.

The "pay for friend's luck" tests in Table 1.4 explore how CEO pay reacts to peers' lucky compensation in different industries and states. For the specifications in Table 1.4, I adopt a modified form of the second stage of the Pair Model:

Pay for friend's luck: $abs\,((\varepsilon_{i,t} - \varepsilon_{i,t-1}) - (\hat{Y}_{j,t} - \hat{Y}_{j,t-1})) = \lambda_0 + \lambda_1 I_{i,j,t} + \zeta_{i,j,t}$ (2.3)

Here, ε is the residual from the first stage regression of log compensation levels on the controls for executive, firm, and industry fundamentals in the literature (including executive- and firm-fixed effects as well). $\hat{Y}$ is the peer's predicted "lucky" compensation from a regression, estimated using the full ExecuComp sample, of log compensation levels on the peer's current and lagged fiscal-year value-weighted Fama-French 49 industry returns (calculated excluding the peer's firm returns).[16] All standard errors in the second stage are allowed to be double clustering by each member in an executive pair. The variable of interest is the connection dummy ($I_{i,j,t}$), and the coefficient λ_1 is expected to be negative if CEO pay responds more sensitively to a socially connected CEO's change in lucky pay than a non-connected CEO's change in lucky pay.

A potential concern with the pay for friend's luck tests is that, for an executive pair *i* and *j* in different industries, *j*'s industry returns may have a direct impact on *i*'s compensation if *i* and *j* work in related industries. For example, if socially connected CEOs belong to more related industries than non-connected CEOs, the direct impact of

[16] I find similar results if I use SIC3 industry returns to estimate lucky compensation.

peers' industry shocks would lead to positive (biased) estimates of peer effects even in the absence of peer influence. This concern is mitigated through the use of *i*'s residual compensation after controlling for *i*'s own firm and industry current and lagged fiscal year returns. In column (3), I also limit bias by excluding all pairs of CEOs belonging to the same broad Fama-French 49 industry classification and firms with headquarters in the same state. To further ensure the robustness of the results, in column (4), I take the more conservative approach of excluding all CEOs working in the financial sector (SIC codes 6000-6999) and all pairs of CEOs in linked industries. Using the BEA input-output tables and following Ahern and Harford (2010), industries are considered linked if a customer industry buys at least 1 percent of a supplier industry's total output or if a supplying industry supplies at least 1 percent of the total inputs of a customer industry. Table 1.4 presents the pay for friend's luck tests for total compensation.[17] The coefficient of connection dummy ($I_{i,j,t}$) is significantly negative with the p-value less than 1 percent. Socially connected CEOs are around 17 percent more similar than non-connected CEOs, even when peers' change in pay is due to industry performance shock (which, arguably, is less likely to be correlated with executive characteristics and/or productivities) and detailed controls are included for own firm and industry performance.

These results of pay for friend's luck have two important implications. First, it helps mitigate the concern that peer similarities in compensation are driven by similarities in unobservable time variant managerial skills or executive fundamentals. Similarities in underlying executive characteristics may lead executives to select similar types of firms

[17] Results for total payout (*TDC2* in ExecuComp), which are qualitatively similar to the results of total compensation, are omitted for brevity, but they are available upon request.

or industries, but it is unlikely to cause CEO compensation to vary with lucky compensation to peers over time. Second, and perhaps more importantly, the evidence of pay for friend's luck suggests that relative earnings within a CEO's social networks directly affects executive compensation and highlights the existence of contemporaneous social interactions in CEO pay.

These results of pay for friend's luck provide suggestive evidence supporting the idea that relative earnings concerns directly enter into each executive's utility function. For example, executives might bargain harder when their friends receive pay increase purely due to lucky industry performance. However, an alternative explanation is that a CEO's outside options are changing with the industry performance of peers in one's social network. Instead of arguing against this alternative explanation, I directly test one implication of relative earnings concerns by exploring whether CEO compensation reacts asymmetrically to a peer's change in lucky pay. Motivated by Garvey and Milbourn (2006), I argue that if top managers value relative earnings within their social networks, they will respond to a friend's change in lucky pay only when peers' lucky pay is up. Here, "lucky pay is up/down" means that the annual change of the lucky pay is positive/negative. On the other hand, if instead it is the case that executives' outside options vary with the industry performance of peers in one's social network, then CEOs are expected to react symmetrically to a peer's change in lucky pay, regardless of whether it is up or down. Column (1) in Table 1.5 reports the results. Based on the empirical model in Table 1.4, I introduce an extra term that interacts the connection dummy with an indicator variable taking the value of one if a peer's lucky pay is up. The coefficient on the interaction term is negative and different form zero at one percent level.

The results reveal that changes in CEO compensation respond more aggressively to a peer's lucky pay when that peer's lucky pay is up than it is down, which suggests that "pay for friend's luck" is at least partially (if not all) driven by that fact that CEOs value relative earnings within their social networks.

To further our understanding of the process whereby pay for friend's luck takes place, I examine how corporate governance affects this practice. Since poorly governed-firms increase the CEO's ability to capture the pay process, pay for friend's luck should be more prevalent in poorly-governed firms if relative earnings concerns lead CEOs to bargain more aggressively with the board of directors. Column (2) in Table 1.5 reports the results. In these regressions, I allow the response to a peer's lucky compensation to depend on the corporate governance. Results show that pays for friend's luck is more pronounced in poorly governed firms, according to the measure introduced by Gompers, Ishii, and Metrick (2003). If it is the case that a CEO's outside options moves with the industry performance of peers, we would have expected CEO compensation in well-governed firms to react to friend's lucky pay as much as (if not more than) in poorly governed firms. These findings suggest that at least some of the pay for friend's luck in poorly governed firms is driven by the fact that CEOs value relative earnings.

Evidence of pay for friend's luck is consistent with the findings in Shue (2011), both of which support the argument that executives are rewarded for more than their effort or skill (Bertrand and Mullainathan, 2001). Bertrand and Mullainathan find that executives are paid for lucky industry shocks in their own industry. This paper shows that executives (after controlling for own firm performance) are paid more when their friends in different industries receive lucky shocks to their compensation. More importantly, the newly documented CEOs' asymmetrical response to a peer's lucky shock is in line with the findings in Garvey and Milbourn (2006) and Harford and Li (2007). Garvey and Milbourn show that the pay-for-luck relationship uncovered by Bertrand and Mullainathan is asymmetric. That is, CEOs are only paid for good luck but not blamed for bad luck. Harford and Li extend this study and show that the asymmetry is much stronger following an acquisition. Finally, the evidence that pay for friend's luck is more pronounced in poorly governed firms helps single out the underlying mechanisms.

1.4.3 The Spread of Executive Option Grant Backdating

In this section, I make the effort to investigate whether executives' social networks serve as a conduit for interpersonal information flow and beliefs. I use executive option grant backdating to test this conjecture. If information and/or beliefs do spread across CEOs' social networks, then CEOs' social networks are expected to contribute to the spread of option backdating practices.[18] Following the influential work by Lie (2005) and Heron and Lie (2007), it has been shown that over 30 percent firms were involved in the option backdating practice (e.g., Heron and Lie, 2009; Bizjak, Lemmon, and Whitby, 2009; Collins, Gong, and Li, 2009; Bebchuk, Grinstein, and Peyer,

[18] Because the value of an option is higher if the exercise prize is lower, executives should prefer being granted options when the exercise prize is at its lowest. The backdating of stock options is a practice through which CEOs (and other top executives) choose a favorable date (i.e., when the stock price was low) that precedes the current date for when stock options were supposedly granted. Backdating would not be illegal if it were clearly communicated to shareholders, adequately accounted for in both earnings and taxes, and no document was forged. However, this is rarely true in practice, making most instances of backdating illegal.

2009). Option backdating was not a well-known practice outside the firms that adopted it, which makes it a perfect setting to examine how information spread among agents.[19]

I start by identifying companies involved in executive option grant backdating. My sample of executive option grants consists of 29,421 grants across 4,326 firms over the period of 1996 to 2002. In the option grant backdating literature, several approaches have been proposed to identify firms involved in the backdating practice. My approach follows Bizjak, Lemmon, and Whitby (2009), but the results are robust to other methods to identify firms involved in the backdating (e.g., Collins, Gong, and Li, 2009; Bebchuk, Grinstein, and Peyer, 2009).

The key assumption used by Bizjak, Lemmon, and Whitby (2009) to identify firms involved in the option grant backdating is that if option grant dates are chosen randomly instead of manipulated, then there will not be any unusual performance pattern in the stock price surrounding the grant date. Alternatively, if firms look backward and choose low points in the firm's stock-price history to award option grants, then option grants that have been backdated in this manner will exhibit a stock-price reversal around the reported grant date. Consistent with backdating, Lie (2005); Heron and Lie (2007); Narayanan and Seyhun (2008); Bizjak, Lemmon, and Whitby (2009) find that, on average, stock option grants are preceded by a fall in the stock price, with a subsequent increase in the stock price following the reported grant date.

In order to identify individual grant dates that are likely to have been manipulated, I employ the following statistical approach which is very similar to that used by Bizjak,

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[19] There is almost no public information about the option backdating practice until 2004, when an early version of Lie (2005) was circulated.

Lemmon, and Whitby (2009). First, I randomly select 1,000,000 trading days from my final sample of firm years and define these as hypothetical option grant dates. I calculate the cumulative raw stock returns over the 20 trading-day periods prior to and following the randomly selected grant dates. To measure reversals around the hypothetical grant dates, I compute the difference between the post-grant and pre-grant 20-day cumulative returns. Next, I sort firms into quartiles based on the monthly standard deviation of stock returns calculated over the two-year period preceding the hypothetical grant date. Separating firms into groups based on the volatility of returns controls for the fact that firms with higher stock-price volatility will exhibit more frequent and larger reversals on average, even in the absence of backdating. For the sample of random grant dates in each volatility quartile, I identify the magnitude of the post- to pre-grant return difference that corresponds to a pre-specified confidence level (e.g., 95% or 99%).

Finally, to identify whether an actual option grant date is likely to have been backdated, I compute the difference in the post-grant and pre-grant 20-day cumulative stock returns around the actual reported grant date and compare this value with the cutoff level corresponding to the desired confidence level based on randomly assigned grant dates. If the magnitude of the return difference around the actual grant date exceeds the cutoff level, the grant is classified as having been backdated. At the firm level, I classify a firm as having backdated options in a given year if I classify any of the option grant dates by that firm in that year as having been backdated.

The results are presented in Table 1.6. Panel A in Table 1.6 presents the cutoff levels for returns around the grant date that correspond to a given confidence level within each of the volatility quartiles. In general, the magnitude of the forty-one-day cumulative returns (–20 to +20) that are necessary to identify a grant as having been backdated are large and increase significantly with return volatility. For example, to identify backdated grants at the 95% confidence level, cumulative returns around the grant date must be larger than 18.39% for firms with low volatility and must exceed 61.95% for firms with high volatility. At the 99% confidence level, the corresponding return cutoffs are 30.60% and 113.71% for low and high volatility firms, respectively.

Panel B of Table 1.6 reports the total number of firms in the sample each year and the number of firms identified as backdaters using different confidence levels to identify backdated grants. Across years, the number of firms in the sample remains relatively constant ranging between a high of 2,519 firms in 1998 and 2,223 firms in 1996. The number of firms identified as backdaters fluctuates much more from year to year. The number of firms identified as backdating option grants is highest in 1998, 2000, and 2001, and is lowest in 1996 and 2002. Of the 16,735 firm years in our sample, approximately 12% are classified as backdaters based on a 95% confidence level. In other words, nearly three times as many firm years are classified as backdating option grants than would be expected if options were granted on randomly chosen dates. Similarly, based on a 97.5% confidence level, 6.4% of firm years are classified as backdaters, and 2.7% of firm years exhibit evidence of backdating based on a 99% confidence level.

Having identified firms involved in the option grant backdating, I use a modified two stage Pair Model to investigate whether CEOs' social networks contribute to the spread of option grant backdating. In the first stage, I run a logistic regression where the dependent variable is one if the firm has been identified as a backdater in time *t* and the independent variables include control variables in the option backdating literature. In the second stage, the dependent variable is the absolute value of the difference in the residuals from the first stage regression for each pair of CEOs and the control variables are the same as the model specifications in Table 1.2.

Table 1.7 presents the results of my analysis.[20] In column (1), the coefficient of connection dummy $I_{i,j,t}$ is negative and statistically significant at the one percent level, suggesting CEOs' social networks do contribute to the spread of option grant backdating. In column (2), I investigate which of the social networks components (education, employment, and other activities) contribute more to the spread of option backdating practice. The results suggest that employment and other activities networks significantly contribute to the backdating activities among connected CEOs.

A potential concern of my analysis is that the finding is not robust to other methods to identify firms involved in the backdating practice. Hence, as a robustness check, I reconduct all the analysis using different approaches to identify firms that have manipulated the option grants (Collins, Gong, and Li, 2009; Bebchuk, Grinstein, and Peyer, 2009) and present the results in columns (3)–(6) in Table 1.7. The coefficient of $I_{i,j,t}$ is persistently negative and statistically significant with the p-value less than one percent. The overall evidence in Table 1.7 suggests that information does travel through CEOs' social networks, which serve as an important channel through which peer interactions affect executive pay.

[20] For brevity, the results of the first stage regression are omitted, but they are available upon request.

1.4.4 Further Evidence on Relative Earnings

In this section, I conduct an additional test to support the argument that top managers value relative earnings within their social networks, inducing top executives to "keep up with the Joneses" in terms of compensation.

The empirical model is motivated by two lines of research. First, a recent paper by Card, Mas, Moretti, and Saez (2010) show job satisfaction depends directly on relative pay comparisons. More specifically, using a subset of employees of the University of California, they find that workers with salaries below the median of their peers (the same pay unit and occupation) report lower pay and job satisfaction as well as a significant increase in the likelihood of looking for a new job. Second, Faulkender and Yang (2009) and Bizjak, Lemmon, and Nguyen (2008, 2010) highlight the importance of the pay of the peer group median in explaining the level and change in executive compensation. I conjecture that the pay of one's peer median will serve as a reference point that a typical CEO particularly cares about. CEOs with compensation below the median of their social peers might bargain more aggressively with their firms to increase their pay. Hence, I expect that a CEO's compensation will significantly increase if her compensation is below the median of her social peers.

The results are reported in Table 1.8. I employ a modified model specification as in Bizjak, Lemmon, and Nguyen (2010). The dependent variable in the model is the annual change in the logs of total compensation. Independent variables include log sales, change in log sales, and firm volatility. Other variables include measures of current and lagged stock and accounting performance (*ROA*). The variable of interest is a dummy variable, which takes the value of one if a CEO's total compensation is below the median of her peer group at fiscal year *t-1*. The coefficient of this dummy variable is expected to be positive. In column (1), the coefficient of the dummy variable is significantly positive at the one percent level, consistent with the conjecture that a CEO's compensation will significantly increase if her compensation is below the median of her social peers.

One potential concern of the above analysis is that the documented increase in pay for those below median CEOs is driven by the fact that those CEOs are simply underpaid relative to their industry peers, regardless of whether they value relative earnings or not. In order to alleviate this concern, I introduce a "naïve" peer group (CEOs from firms of similar size and the same industry) into the empirical model. The naïve peer group is based on industry and size (Bizjak, Lemmon, and Nguyen, 2008 and 2010). More specifically, I add another dummy variable into the model specification that takes the value of one if a CEO's compensation is below the median of her "naïve" peer group. The result is reported in column (2). Consistent with the findings in Bizjak, Lemmon, and Nguyen (2008), the coefficient of the newly included dummy variable is significantly positive. More importantly, however, I find the coefficient of the variable of interest is still positive and significant at the one percent level even controlling for the potential effect of underpay.

I also compute the difference in log compensation between the median CEO in the social peer group and the sample CEO. This distance measure captures how much the manager is paid relative to the median CEO in her peer group. The coefficient estimate on this variable measures how a CEO's compensation evolves as a function of the manager's pay relative to the peer group. To control the effect of underpayment, I also compute the difference in log compensation between the median pay in the "naïve" peer group and the sample CEO. In columns (3) and (4), the coefficient of interested variables is persistently positive and significantly different from zero at one percent level. Overall, the results in Table 1.8 are consistent with the argument that executives value relative earnings, which serves as the mechanism thorough which CEOs' social networks influence executive compensation.

The results are related to a recent controversial executive compensation practice, in which firms tend to manipulate peer groups to inflate CEO compensation. Both Faulkender and Yang (2009) and Bizjak, Lemmon, and Naveen (2008, 2010) argue that the pay of the peer group median has a substantial influence on executive compensation. For example, Bizjak, Lemmon, and Naveen (2010) report that CEO compensation will significantly increase if her total compensation is below the median of her (industry and size matched) peers. Pay of the peer group median is generally believed to serve as a reference point that a typical CEO/firm would target. The evidence in Table 1.8 reveals that there might be another reference point that CEOs tend to benchmark—the median compensation level of their social peers.

1.4.5 Extensions and Robustness

I extend my analysis to examine whether peer effects also exist among CEOs if they are indirectly connected through a third CEO. I refer to this type of connection as a "second-degree connection". Table 1.9 reports the results. More specifically, based on the empirical model in previous tables, I include a dummy variable (*second-degree connection*) which equals one if a pair of CEOs are not directly connected while are indirectly connected through a third CEO and zero otherwise. Column (1) extends the analysis in Table 1.2, column (2) extends the analysis in Table 1.3, and columns (3) and

(4) extend the pay for friend's luck tests in Table 1.4. Two interesting findings emerge from Table 1.9. First, the negative coefficient of second-degree connection (p-value < 0.01) highlights the existence of peer influence among indirectly connected CEOs. Second, the magnitude of the peer influence of second degree connections is weaker than that of first-degree connection.

Table 1.10 modifies the pay for friend's luck specification to test the relationship between a CEO's change in residual compensation and a peer's lagged change in predicted lucky compensation:

Modified pay for friend's luck:

$$abs\,((\varepsilon_{i,t} - \varepsilon_{i,t-1}) - (\hat{Y}_{j,t-1} - \hat{Y}_{j,t-2})) = \lambda_0 + \lambda_1 I_{i,j,t} + \zeta_{i,j,t} \qquad (2.4)$$

This test explores whether the pay for friend's luck results hold with a one-year lag between leaders (represented by executives with the predicted lucky compensation) and followers. Estimates of peer effects in this table are very similar to those in Table 1.4. Evidence of lagged responses to peer's lucky shocks are strongly consistent with a theory of leaders and followers in peer compensation.

A recent paper by Gopalan, Milbourn, and Song (2010) argue that, at least in multi-segment firms, the observed relationship between CEO compensation and industry performance is consistent with optimal contracting as CEOs select and implement the firm's strategy and this strategy choice manifests itself in realized exposures to sector returns. This raises the concern that the change in pay caused by industry performance might be correlated with underlying managerial productivity, especially in firms with multiple segments. To address this concern, I redo the pay for friend's luck test by excluding observations in multi-segment firms. I find that the coefficient of connection dummy is still (highly) significantly negative. For brevity, the results are omitted here, but they are available upon request.

1.4.6 Do Peer Interactions Affect Other Corporate Policies?

The evidence in previous sections suggests that substantial peer influence exists in executive compensation and that the similarities in pay for socially connected CEOs are not driven by the similarities in underlying managerial characteristics and/or styles. A natural follow-up question is: Do CEOs' peer interactions affect firm policies beyond executive pay? The mechanisms through which peer interactions affect executive pay can arguably be extended to other firm policies (e.g., Fracassi, 2008; Goel and Thakor, 2010; Leary and Roberts, 2010; Shue, 2011). Hence, in this section, I test the conjecture that CEOs' social interactions lead to more similar corporate policies among connected CEOs than non-connected CEOs. The corporate policies I examine here include mergers and acquisitions, capital structure, R&D, and cash holdings.

The empirical method employed to study the influences of peer interactions on corporate policies is similar to that used to study executive pay. In the first stage, CEO *i*'s corporate policy $Y_{i,t}$ is regressed over the typical control variables $X_{Pi,t}$ in the related literature. The residual of the regression is the unexplained part of the firm policy in which I am primarily interested. For each pair of CEOs *i* and *j* at time *t*, I take the absolute value of the difference in their residuals, $|\Delta\varepsilon| = abs\ (\varepsilon_{i,t} - \varepsilon_{j,t})$. This variable is a proxy for the difference in the corporate policy of the CEOs. In the second stage, the unit of observation is each pair of CEOs in a given fiscal year. The variable $|\Delta\varepsilon|$ is then regressed on a dummy variable $I_{i,j,t}$ for whether CEO *i* and *j* are socially connected at fiscal year *t*. All standard errors are allowed to be double clustering observations by each member in an executive pair. For brevity, the results of the first stage regression are omitted but are available upon request.

The results in the second stage are reported in Table 1.11. From columns (1) through (4), the policies of interest are mergers and acquisitions, capital structure, R&D, and cash holdings.[21] The coefficient of connection dummy $I_{i,j,t}$ is consistently negative and statistically significant at the one percent level for all firm policies examined, indicating the existence of strong peer influence in firm policies among socially connected CEOs. However, it is important to note that the similarities in firm policies among socially connected CEOs are not the underlying driving force that leads to the similarities in pay for connected CEOs. Remember that in the Pair Model to test the peer similarity in pay, firm fundamentals have already been controlled in the first stage. Hence, the documented peer similarities in pay are essentially the peer similarities in *excess* compensation, which is unrelated to the peer similarities in firm policies.

1.5 Conclusion

Considerable evidence suggests that CEOs matter for pay, performance, and firm policies (e.g., Bertrand and Schoar, 2003; Bertrand, 2009; Graham, Li, and Qiu, 2009). I extend this literature to explore the role of top executives' social interactions in explaining executive compensation. CEOs' peer interactions might influence executive compensation through several mechanisms. First, social networks serve as a conduit for interpersonal information flow and beliefs. Second, peer interactions may induce top executives to "keep up with the Joneses" in terms of compensation. Third, a CEO's

[21] I further find that the peer similarities in acquisitions are more pronounced if peers are involved in large deals in terms of relative deal value and/or receive large bonuses afterwards.

human capital and/or outside options might correlate with industry performance of socially connected CEOs. Regardless of exactly how peer influence occurs, CEOs' social interactions will lead compensation of socially connected CEOs to become more similar than that of non-connected CEOs.

Using the social networks among 2,936 CEOs over the period 1999–2008, I find socially connected CEOs receive significantly more similar levels of compensation and also experience significantly more similar change in executive pay than non-connected CEOs. More importantly, I find CEO compensation responds to peers' lucky pay and reacts more strongly when peers' lucky pay is up than down, both of which help alleviate the concern that peer effects are driven by similarities in underlying managerial skill and/or characteristics. The peer influence is also economically significant. The amount of variation in compensation among socially connected CEOs is around 7 to 10 percent less than the variation among non-connected CEOs.

To investigate the underlying mechanism, I find that (i) CEO compensation responds asymmetrically to a peer's lucky pay, i.e., CEO pay reacts more strongly to friend's luck pay if the luck is up rather than down and when the firm is suffering from weak corporate governance; and (ii) a CEO's pay will significantly increase if her compensation is below the median of her peer group. I interpret these results as consistent with the notions that envy within social networks affects negotiations about compensation. I further find that the past practice of backdating stock option grants spread across social networks, suggesting that social networks serve as a conduit for interpersonal information flow and beliefs.

Table 1.1 Summary Statistics

Panel A

	Mean	Q1	Median	Q3	STD
CEO characteristics:					
Salary ($ thousand)	789	526	744	1,015	355
Salary & Bonuses ($ thousand)	1,523	692	1,057	1,803	1,429
Total compensation (TDC1, $ thousand)	5,891	1,642	3,408	6,983	7,192
Total payout (TDC2, $ thousand)	6,094	1,258	2,727	6,478	9,404
CEO age	55.05	50.00	55.00	60.00	7.09
CEO tenure	7.68	2.67	5.42	10.01	7.20
Dummy (CEO is chair)	0.58	0.00	1.00	1.00	0.49
Dummy (CEO is female)	0.02				
Firm characteristics:					
Assets ($ million)	12,373.90	691.69	2,021.84	7,424.90	37,027.99
Sales ($ million)	5,626.31	600.07	1,547.22	4,825.49	11,447.54
Leverage	0.223	0.060	0.210	0.339	0.182
Market-to-book	2.051	1.145	1.563	2.215	1.297
Investment	0.241	0.128	0.202	0.314	0.156
Cash	0.147	0.024	0.073	0.213	0.172
R&D	0.042	0.000	0.000	0.033	0.095
Acquisitions	0.039	0.000	0.000	0.025	0.098
ROA	0.049	0.014	0.050	0.097	0.102
Stock return	0.110	-0.193	0.059	0.308	0.501

Panel B

	Mean	Median	STD
Number of peers	15.34	9	17.37
Median (Peer / CEO salary)	1.455	1.094	1.802
Median (Peer / CEO salary & bonuses)	1.678	1.121	2.489
Median (Peer / CEO total compensation)	2.408	1.264	3.776
Median (Peer / CEO total payout)	2.648	1.250	5.112
Dummy (Peer from the same industry)	0.209	0.067	0.294
Dummy (Peer from the same state)	0.201	0.067	0.282
Dummy (Assets within 50 - 200%)	0.257	0.242	0.234
Dummy (Sales within 50 - 200%)	0.292	0.273	0.244

Table 1.1 Continued

Note: This table reports summary statistics of the sample over the period of 1999-2008. The Chief Executive Officer (CEO) sample includes 2,936 CEOs covered by both the BoardEx dataset and the ExecuComp dataset over the sample period. The total CEO-year observation is 12,672 over the sample period. Panel A reports CEO compensation and firm characteristic and Panel B reports CEO's peer information and characteristics. CEO compensation data comes from ExecuComp. Total compensation is the sum of salary, bonus, value of restricted stock grants, and the Black Scholes value of options and long term incentive plans (*TDC1* in ExecuComp). Summary statistics for base salary, base salary plus bonus, and total compensation consist of winsorized means and winsorized standard deviations at the 1% level of both tails. CEO age, CEO tenure, and percent chair and female data come from ExecuComp. Firm financial information including firm size (log of total assets), sales revenue, financial leverage, market to book, and return on assets come from COMPUSTAT. Firm and industry returns are matched to firm fiscal year month end dates and come from CRSP. Definitions of the variables are presented in the Appendix and all dollar values are adjusted to 2008 dollars.

Table 1.2 Peer Influence in the Level of Executive Compensation

	Total compensation			Total payout		
	(1)	(2)	(3)	(4)	(5)	(6)
Connection dummy	-0.072***	-0.072***	-0.070***	-0.045***	-0.046***	-0.045***
	[6.995]	[7.039]	[6.897]	[3.482]	[3.559]	[3.498]
Same industry dummy	0.001	0.001	0.001	0.003	0.003	0.003
	[0.189]	[0.192]	[0.207]	[0.492]	[0.498]	[0.504]
Same state dummy	-0.024*	-0.024*	-0.024*	-0.019	-0.019	-0.019
	[1.727]	[1.736]	[1.743]	[1.217]	[1.226]	[1.238]
Same school dummy		-0.013	-0.013		-0.018	-0.018
		[0.301]	[0.309]		[0.437]	[0.424]
Same firm dummy		-0.029	-0.025		-0.019	-0.018
		[1.613]	[1.421]		[0.525]	[0.501]
Board interlock			-0.028***			-0.022**
			[3.933]			[2.344]
Constant	0.429***	0.430***	0.430***	0.599***	0.599***	0.599***
	[44.653]	[44.699]	[44.696]	[45.466]	[45.481]	[45.473]
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
R square	0.016	0.017	0.017	0.009	0.011	0.011
# of observations	8,042,174	8,042,174	8,042,174	8,042,174	8,042,174	8,042,174
First stage model:						
Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Executive characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Industry characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Executive and firm dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.2 Continued

Note: This table reports results of whether two socially connected CEOs receive more similar level of compensation than non-connected CEOs. Specification follows the Pair Model described in Section 3. The dependent variable, $|\Delta\varepsilon| = abs\ (\varepsilon_{i,t} - \varepsilon_{j,t})$, is the absolute value of the difference in their residuals from the first stage regression. For the first stage regression, the dependent variable is the natural log of compensation and the control variables include firm size (lagged natural log of sales), lagged book-to-market, lagged financial leverage, current and lagged annul stock return, current and lagged return on assets, stock price volatility, current and lagged annul value-weighted industry (Fama-French 49 industry) stock return, CEO tenure, executive- and firm-fixed effects, and year-fixed effects. For brevity, the results of the first stage regression are omitted but are available upon request. Definitions of the control variables in the second stage are in the Appendix. The variable of interest is a dummy variable (*connection dummy*) which takes value of one if the pair of CEOs is socially connected. All regressions control for year-fixed effects whose coefficients are suppressed for brevity. Standard errors are corrected for clustering of the error term at both executive level using the double-clustering algorithm from Peterson (2008) and are reported in brackets. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.3 Peer Influence in the Change of Executive Compensation

	Total Compensation			Total Payout		
	(1)	(2)	(3)	(4)	(5)	(6)
Connection dummy	-0.134***	-0.134***	-0.132***	-0.104***	-0.104***	-0.103***
	[11.683]	[11.740]	[11.651]	[6.971]	[7.008]	[6.970]
Same industry dummy	-0.005	-0.005	-0.005	-0.004	-0.004	-0.004
	[0.831]	[0.837]	[0.834]	[0.483]	[0.474]	[0.479]
Same state dummy	-0.034**	-0.034**	-0.034**	-0.015	-0.015	-0.015
	[2.414]	[2.428]	[2.391]	[0.726]	[0.729]	[0.723]
Same school dummy		-0.018	-0.018		-0.018	-0.018
		[0.496]	[0.487]		[0.474]	[0.478]
Same firm dummy		-0.043*	-0.042*		-0.023	-0.026
		[1.814]	[1.793]		[0.506]	[0.556]
Board interlock			-0.041***			-0.031***
			[4.303]			[2.796]
Constant	0.611***	0.611***	0.611***	0.916***	0.916***	0.917***
	[39.070]	[39.069]	[39.073]	[40.349]	[40.341]	[40.345]
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
R square	0.013	0.013	0.013	0.005	0.005	0.005
# of observations	5,419,572	5,419,572	5,419,572	5,419,572	5,419,572	5,419,572
First stage model:						
Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Executive characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Industry characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Executive and firm dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.3 Continued

Note: This table reports results of whether two socially connected CEOs experience more similar change in compensation than non-connected CEOs. Specification follows the Pair Model described in Section 3. The dependent variable, $|\Delta\Delta\varepsilon| = abs\ ((\varepsilon_{i,t} - \varepsilon_{i,t-1}) - (\varepsilon_{j,t} - \varepsilon_{j,t-1}))$, is the absolute value of the first difference of the change in residuals from the first stage regression. For the first stage regression, the dependent variable is the natural log of compensation and the control variables include firm size (lagged natural log of sales), lagged book-to-market, lagged financial leverage, current and lagged annul stock return, current and lagged return on assets, stock price volatility, current and lagged annul value-weighted industry (Fama-French 49 industry) stock return, CEO tenure, executive- and firm-fixed effects, and year-fixed effects. For brevity, the results of the first stage regression are omitted but are available upon request. Definitions of the variables in the second stage are in the Appendix. The variable of interest is a dummy variable (*connection dummy*) which takes value of one if the pair of CEOs is socially connected. All regressions control for year-fixed effects whose coefficients are suppressed for brevity. Standard errors are corrected for clustering of the error term at both executive level using the double-clustering algorithm from Peterson (2008) and are reported in brackets. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.4 Pay for Friend's Luck

	(1)	(2)	(3)	(4)
Connection dummy	-0.072***	-0.072***	-0.072***	-0.065***
	[7.265]	[7.214]	[7.103]	[5.366]
Same industry dummy	-0.009	-0.009		
	[1.447]	[1.435]		
Same state dummy	-0.024**	-0.025**		
	[2.113]	[2.214]		
Same school dummy	-0.018	-0.018	-0.018	-0.014
	[0.599]	[0.593]	[0.569]	[0.485]
Same firm dummy	-0.040*	-0.038*	-0.024	-0.036
	[1.889]	[1.777]	[0.945]	[1.273]
Board interlock		-0.029***	-0.029***	-0.032**
		[3.186]	[3.213]	[3.263]
Constant	0.401***	0.401***	0.403***	0.391***
	[29.607]	[29.608]	[29.859]	[28.594]
Year dummy	Yes	Yes	Yes	Yes
Exclude pairs in same FF49 industry and state	No	No	Yes	Yes
Exclude pairs in linked industries, financials	No	No	No	Yes
R square	0.014	0.014	0.014	0.015
# of observations	10,601,836	10,601,836	9,827,987	5,966,960
First stage model:				
Firm characteristics	Yes	Yes	Yes	Yes
Executive characteristics	Yes	Yes	Yes	Yes
Industry characteristics	Yes	Yes	Yes	Yes
Executive and firm dummy	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes

Note: This table examines the relationship between an executive's change in compensation and her peer's "lucky" change in compensation, where peers' lucky changes in compensation are predicted using the peers' industry returns. The sample exclude executives working in the financial sector (SIC codes 6000-6999) as well as pairs of peers in linked industries (using BEA input-output tables and following Ahern and Harford (2010), industries are considered linked if a customer industry buys at least 1% of a supplier industry's total output or if a supplying industry supplies at least 1% of the total inputs of a customer industry). Specification follows the modified Pairs Model described in Section 3. The dependent variable is $abs\,((\varepsilon_{i,t} - \varepsilon_{i,t-1}) - (\hat{Y}_{j,t} - \hat{Y}_{j,t-1}))$. ε is the residual from the standard first stage regression of compensation levels (log total compensation) on the set of controls indicated in the bottom panel (the same as the first-stage regression in Table 1.2). $\hat{Y}$ is the predicted "lucky" compensation from a regression of log compensation levels on the firm's value-weighted Fama-French 49 industry current and lagged

Table 1.4 Continued

fiscal year returns (calculated excluding the relevant firm's own returns). Consider a pair of executives A and B in a given year. This pair will account for two observations. The dependent variable in the first observation is the absolute difference between A's change in residual compensation and B's change in predicted compensation. The dependent variable in the second observation is the absolute difference between A's change in predicted compensation and B's change in residual compensation. All other variables are as described in Appendix. Standard errors in parentheses are allowed to be double clustered by each member of an executive pair. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.5 More Evidence on Pay for Friend's Luck

	(1)	(2)
Connection dummy	-0.048***	-0.023
	[3.653]	[1.123]
Connection dummy x dummy (Lucky pay is up)	-0.034***	
	[4.037]	
Connection dummy x dummy (GIM index ≥ 12)		-0.088***
		[3.290]
Same school dummy	-0.014	-0.003
	[0.485]	[0.103]
Same firm dummy	-0.036	-0.031
	[1.273]	[1.033]
Board interlock	-0.032***	-0.026**
	[3.263]	[2.557]
Constant	0.391***	0.361***
	[28.594]	[25.606]
Year dummy	Yes	Yes
Exclude pairs in same FF49 industry and state	Yes	Yes
Exclude pairs in linked industries, financials	Yes	Yes
R square	0.015	0.016
# of observations	5,966,960	5,193,372

Note: This table examines whether an executive's change in compensation react asymmetrically to her peer's "lucky" change in compensation and how corporate governance affects the practice of pay for friend's luck. The dependent variable is *abs* $((\varepsilon_{i,t} - \varepsilon_{i,t-1}) - (\hat{Y}_{j,t} - \hat{Y}_{j,t-1}))$. ε is the residual from the standard first stage regression of compensation levels (log total compensation) on the set of controls indicated in the bottom panel of Table 1.4. $\hat{Y}$ is the predicted "lucky" compensation from a regression of log total compensation levels on the firm's value-weighted Fama-French 49 industry current and lagged fiscal year returns (calculated excluding the relevant firm's own returns). Specification in column (1) identical to that in Table 1.4 except that I allow the connection dummy to interact with a dummy variable that take value of one if the change in peer's lucky pay is up. Here, peers' lucky pay is up/down means the change in predicted "lucky" compensation is positive/negative. Specification in column (2) is identical to that in Table 1.4 except that I allow the connection dummy to interact with a dummy value that takes value of one if the GIM index is great than 11. All other variables are as described in Appendix. Standard errors in parentheses are allowed to be double clustered by each member of an executive pair. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.6 Firms Identified in the Executive Option Grant Backdating

	Confidence level for defining backdating		
	95%	97.5%	99%
Panel A: Return cutoff levels for defining backdaters			
Monthly standard deviation of returns	Post- to pre-grant cumulative stock return cutoff level		
Less than or equal to 8.92%	18.39%	23.54%	30.60%
8.92% < STD <= 13.45%	29.16%	37.01%	48.05%
13.45% < STD <= 20.27%	42.61%	55.15%	73.75%
Greater than 20.27%	61.95%	82.26%	113.71%

Panel B: Number of backdating firms by sample year

Year	Total number of firms	Number of firms identified as backdaters		
1996	2,223	181	78	24
1997	2,447	206	103	33
1998	2,519	361	196	87
1999	2,416	282	154	68
2000	2,404	416	241	106
2001	2,465	354	195	87
2002	2,261	182	108	44
Total firm years	16,735	1,982	1,075	449

Table 1.6 Continued

Note: Summary statistics for defining backdating firms and for the frequency of backdating by year using various return cutoffs. Panel A reports the required level of return reversals for a firm to be identified as a backdater in each volatility quartile. Volatility is measured as the monthly standard deviation of stock returns calculated over the two-year period preceding the grant date. Return levels are reported for the 95, 97.5, and 99% confidence levels. Confidence levels are derived from 1,000,000 randomly selected trading days that are assigned as hypothetical option grant dates. Panel B reports the number of firms in our sample each year and the number of firms identified as backdaters using different confidence levels. Firms are classified as backdaters using cumulative raw returns over the forty-one-day period beginning twenty days prior to through twenty days following the option grant date. To be a backdater, the cumulative stock returns around the grant date must be larger than the cutoff for a given confidence level and volatility quartile.

Table 1.7 Do CEOs' Social Networks Contribute to the Spread of Option Grant Backdating?

	Bizjak, Lemmon, and Whitby (2009)		Collins, Gong, and Li (2009)		Bebchuk, Grinstein, and Peyer (2010)	
	(1)	(2)	(3)	(4)	(5)	(6)
Connection dummy	-0.035***		-0.042***		-0.049***	
	[3.325]		[3.382]		[4.464]	
School connection dummy		-0.049		-0.080**		-0.086***
		[1.338]		[2.270]		[4.196]
Social connection dummy		-0.033***		-0.039***		-0.049***
		[2.744]		[2.970]		[4.347]
Employment connection dummy		-0.043**		-0.053**		-0.037
		[2.012]		[2.286]		[1.432]
Same school dummy	-0.001	-0.001	-0.044*	-0.044*	-0.046**	-0.046**
	[0.041]	[0.040]	[1.789]	[1.791]	[2.266]	[2.263]
Same firm dummy	-0.041	-0.041	-0.046	-0.046	-0.043	-0.043
	[1.252]	[1.255]	[1.349]	[1.352]	[1.525]	[1.514]
Board interlock	-0.032***	-0.032***	-0.037***	-0.037***	-0.036**	-0.036***
	[3.364]	[3.364]	[3.542]	[3.549]	[2.572]	[2.610]
Constant	0.199***	0.199***	0.233***	0.233***	0.183***	0.183***
	[19.578]	[19.578]	[19.327]	[19.326]	[15.514]	[15.514]
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes
Exclude pairs in same FF49 industry	Yes	Yes	Yes	Yes	Yes	Yes
Exclude pairs in same state	Yes	Yes	Yes	Yes	Yes	Yes
R square	0.011	0.011	0.019	0.019	0.005	0.005
# of observations	1,467,122	1,467,122	1,467,122	1,467,122	1,467,122	1,467,122
First stage model:						
Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes
CEO characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Governance characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.7 Continued

Note: This table reports results of whether CEOs' social networks contribute to the spread of executive option grant backdating. Specification follows the modified Pair Model described in Section 3. The dependent variable, $|\Delta\varepsilon| = abs\ (\varepsilon_{i,t} - \varepsilon_{j,t})$, is the absolute value of the difference in their residuals from the first stage regression. For the first stage regression, I run a logistic regression where the dependent variable is one if the firm has been identified as a backdater and the independent variables include control variables in the option backdating literature. For brevity, the results of the first stage regression are omitted but are available upon request. Definitions of the control variables in the second stage are in the Appendix. The variable of interest is a dummy variable (*connection dummy*) which takes value of one if the pair of CEOs is socially connected. All regressions control for year-fixed effects whose coefficients are suppressed for brevity. Standard errors are corrected for clustering of the error term at both executive level using the double-clustering algorithm from Peterson (2008) and are reported in brackets. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.8 Further Evidence on Whether CEOs Value Relative Earnings

	Change in log(Total compensation)			
	(1)	(2)	(3)	(4)
Dummy if compensation below the median of the social peer group	0.417***	0.296***		
	[16.77]	[13.07]		
Dummy if compensation below the median of the naive peer group		0.300***		
		[15.53]		
Difference in log compensation between the social peer group and the firm			0.293***	0.188***
			[15.06]	[11.45]
Difference in log compensation between the naïve peer group and the firm				0.221***
				[12.27]
Log of sales	0.065***	0.072***	0.106***	0.091***
	[11.15]	[12.08]	[13.60]	[11.33]
Change in log sales	0.131**	0.154***	0.125**	0.155**
	[2.18]	[2.60]	[2.07]	[2.49]
ROA	0.019	0.021	0.021	0.023
	[0.11]	[0.14]	[0.13]	[0.15]
Lag ROA	-0.152	-0.115	-0.086	-0.051
	[0.96]	[0.75]	[0.62]	[0.41]
Stock return	0.228***	0.221***	0.217***	0.208***
	[6.64]	[6.57]	[6.44]	[6.56]
Lag stock return	0.092**	0.095***	0.105***	0.109***
	[2.50]	[2.62]	[2.80]	[3.15]
Stock price volatility	-0.718***	-0.605***	-0.638***	-0.568**
	[3.30]	[2.75]	[2.92]	[2.45]
Intercept	-0.906**	-1.002**	-0.954***	-0.832**
	[2.28]	[2.43]	[2.69]	[2.50]
Year dummy	Yes	Yes	Yes	Yes
R square	0.108	0.144	0.179	0.227
# of observations	8,238	8,238	8,238	8,238

Table 1.8 Continued

Note: This table reports results of whether CEO's compensation will significantly increase if her compensation is below the median of her social peers. The dependent variables are the change in logs of total compensation. Independent variables include log of sales revenue, change in logs of sales revenue, ROA, prior year ROA, stock return, prior stock return, and stock return volatility. The variable of interest is a dummy variable which takes value of one if CEO's compensation in year t-1 is below the median of her social peers. I include another dummy variable which takes value of one if CEO's compensation in year t-1 is below the median of her naïve peer group. A naïve peer group consists of all firms similar in size in the same two-digit SIC code. If a firm's sales is above (below) the median sales of its two-digit SIC industry then the naïve peer group includes all firms in this industry with sales higher (lower) than the median. In column (3) and (4) the variable of interest is a measure of the distance in the firm's pay from the peer group median. All regressions control for year-fixed effects whose coefficients are suppressed for brevity. Standard errors in brackets are corrected for clustering of the error term at the firm level. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.9 Extension to Second-degree Connection

	(1)	(2)	(3)	(4)
First-degree connection	-0.069***	-0.128***	-0.068***	-0.065***
	[5.757]	[8.794]	[6.007]	[5.455]
Second-degree connection	-0.034***	-0.068***	-0.032***	-0.033***
	[3.895]	[6.446]	[4.159]	[3.870]
Same school dummy	-0.012	-0.019	-0.004	-0.002
	[0.312]	[0.535]	[0.140]	[0.078]
Same firm dummy	-0.018	-0.022	-0.007	-0.017
	[0.954]	[0.707]	[0.317]	[0.609]
Board interlock	-0.026***	-0.039***	-0.019**	-0.020**
	[3.698]	[3.882]	[2.478]	[2.089]
Constant	0.435***	0.620***	0.401***	0.395***
	[45.010]	[39.962]	[32.017]	[28.781]
Year dummy	Yes	Yes	Yes	Yes
Exclude pairs in same FF49 industry and state	Yes	Yes	Yes	Yes
Exclude pairs in linked industries, financials	No	No	No	Yes
R square	0.017	0.015	0.015	0.016
# of observations	7,551,742	5,040,200	9,827,987	5,966,960

Note: This table reports the results of whether peer effects also exist among CEOs if they are indirectly connected through a third CEO. This type of connection is defined as a "second-degree connection". "First-degree connection" is the same as the connection dummy in previous tables. Specification in column (1) identical to that in Table 1.2 except that I include a dummy variable (*second-degree connection*) which equals one if a pair of CEOs are not directly connected while are indirectly connected through a third CEO and zero otherwise. Specification in column (2) is identical to that in Table 1.3 and specifications in column (3) and (4) are identical to those in Table 1.4. All other variables are as described in Appendix. Standard errors in parentheses are allowed to be double clustered by each member of an executive pair. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.10 **Extension to Pay for Friend's (Lagged) Luck**

	(1)	(2)	(3)	(4)
First-degree connection	-0.067***	-0.075***	-0.076***	-0.084***
	[5.767]	[5.553]	[5.896]	[5.614]
Second-degree connection			-0.041***	-0.043***
			[4.417]	[4.115]
Same school dummy	-0.009	-0.014	-0.012	-0.008
	[0.293]	[0.405]	[0.376]	[0.224]
Same firm dummy	-0.026	-0.034	-0.022	-0.029
	[1.153]	[1.243]	[0.981]	[1.114]
Board Interlock	-0.029***	-0.030***	-0.024***	-0.025**
	[3.118]	[2.856]	[2.794]	[2.343]
Constant	0.401***	0.394***	0.406***	0.400***
	[29.821]	[26.280]	[30.111]	[26.540]
Year dummy	Yes	Yes	Yes	Yes
Exclude pairs in same FF49 industry and state	Yes	Yes	Yes	Yes
Exclude pairs in linked industries, financials	No	Yes	No	Yes
R square	0.011	0.012	0.012	0.013
# of observations	9,338,606	5,680,427	9,338,606	5,680,427

Note: This table examines the relationship between an executive's change in compensation and her peer's lagged "lucky" change in compensation, where peers' lucky changes in compensation are predicted using the peers' industry returns. The sample exclude executives working in the financial sector (SIC codes 6000-6999) as well as pairs of peers in linked industries (using BEA input-output tables and following Ahern and Harford (2010), industries are considered linked if a customer industry buys at least 1% of a supplier industry's total output or if a supplying industry supplies at least 1% of the total inputs of a customer industry). Specification is identical to that in Table 1.4 except for the use of lagged changes in peer predicted compensation as the dependent variable: *abs* $((\varepsilon_{i,t} - \varepsilon_{i,t-1})-(\hat{Y}_{j,t-1} - \hat{Y}_{j,t-2}))$. ε is the residual from the standard first stage regression of compensation levels (log total compensation) on the set of controls indicated in the bottom panel (the same as the first-stage regression in Table 1.2). $\hat{Y}$ is the predicted "lucky" compensation from a regression of log compensation levels on the firm's Fama-French 49 industry current and lagged fiscal year returns (calculated excluding the relevant firm's own returns). Consider a pair of executives A and B in a given year. This pair will account for two observations. The dependent variable in the first observation is the absolute difference between A's change in residual compensation and B's change in predicted compensation. The dependent variable in the second observation is the absolute difference between A's change in predicted compensation and B's change in residual compensation. All other variables are as described in Appendix. Standard errors in parentheses are allowed to be double clustered by each member of an executive pair. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

Table 1.11 Peer Influence in Other Firm Policies

	(1)	(2)	(3)	(4)
Connection dummy	-0.008***	-0.032***	-0.029***	-0.011***
	[7.782]	[5.478]	[6.376]	[4.391]
Same industry dummy	-0.003***	-0.010***	-0.006***	-0.007***
	[4.924]	[3.571]	[3.710]	[3..832]
Same state dummy	-0.002**	-0.003	-0.005*	-0.004**
	[2.286]	[1.032]	[1.851]	[2.374]
Same school dummy	-0.003	-0.016**	-0.025**	-0.004
	[1.158]	[2.223]	[2.372]	[1.513]
Same firm dummy	-0.004*	-0.017**	-0.022***	-0.006**
	[1.641]	[2.366]	[3.159]	[2.318]
Constant	0.067***	0.201***	0.128***	0.046***
	[9.896]	[20.941]	[15.923]	[17.333]
Year dummy	Yes	Yes	Yes	Yes
R square	0.022	0.018	0.034	0.019
# of observations	8,042,174	8,042,174	7,948,836	8,042,174

Note: This table reports results of peer influence in other firm policies including mergers and acquisitions, capital structure, R&D, and cash holdings. Specification follows the Pair Model described in Section 3. The dependent variable, $|\Delta\varepsilon| = abs\ (\varepsilon_{i,t} - \varepsilon_{j,t})$, is the absolute value of the difference in their residuals from the first stage regression. For the first stage regression, the dependent variable is the measure of corporate policies of interest and the independent variables are control variables in related literature. For brevity, the results of the first stage regression are omitted but are available upon request. Definitions of the control variables in the second stage are in the Appendix. The variable of interest is a dummy variable (*connection dummy*) which takes value of one if the pair of CEOs is socially connected. All regressions control for calendar year-fixed effects whose coefficients are suppressed for brevity. Standard errors are corrected for clustering of the error term at both executive level using the double-clustering algorithm from Peterson (2008) and are reported in brackets. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

CHAPTER 2
THE ROLE OF INVESTMENT BANKER DIRECTORS IN M&A: CAN EXPERTS HELP?

2.1 Introduction

Following a wave of accounting scandals in early 2000s, regulators adopted several new rules and stressed the need for more financial experts on the board. [1] The underlying assumption is that financial experts can provide better oversight of financial reporting and, thereby, prevent similar failures of corporate governance. A large body of research finds evidence in support of this argument. For instance, the presence of financial expertise on the board is negatively related to the likelihood of artificial earnings management, fraud and restatement (McMullen and Raghunandan, 1996; Xie, Davidson, and DaDalt, 2003; Agrawal and Chadha, 2005; Abbott, Parker, and Peters, 2004), and the market reacts favorably when firms name new audit committee members with accounting expertise (Defond, Hann, and Hu, 2005). [2]

The influence of the board members' financial expertise on corporate policies, however, extends beyond monitoring. Both the Business Roundtable and the American Law Institute list advising as another central function of the board of directors. Some

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[1] For example, the Sarbanes-Oxley Act of 2002 requires that the audit committee of public firms should be entirely composed of independent directors and should have at least one financially knowledgeable member. Since 2003, all major stock exchanges (NYSE, NASDAQ, and AMEX) have required that each member of the audit committee must be financially literate.

[2] Another set of papers find that financial expertise leads to higher financial statement quality, more conservative accounting and a propensity to provide or update managerial forecasts containing adverse rather than favorable news (Felo, Krishnamurthy, and Solieri 2003, Krishnan and Visvanathan 2008, and Karamanou and Vaeas 2005). Moreover, greater expertise is associated with more timely dismissal of Arthur Andersen, less-frequent suspicious auditor switching, and lower likelihood of material weaknesses in internal controls (Archambeault and DeZoort 2001, Chen and Zhou 2007, and Zhang, Zhou, and Zhou. 2007).

recent studies have found evidence that boards provide valuable advice, but the evidence on the advisory role of the board of directors with financial expertise is mixed.[3] On the positive side, Dionne and Triki (2005) find that financially educated directors encourage corporate hedging, and Brochet and Welch (2011) find that top executives with working experience in investment banking or the auditing sector are more likely to report goodwill impairment when there is a director with a similar functional background on the board. On the negative side, Güner, Malmendier, and Tate (2008) show that bank executives on boards can affect corporate decisions, but sometimes to benefit themselves rather than the firm, and Minton, Taillard, and Williamson (2010) find that financial experts on banks' board of directors failed to alleviate the effect of the recent financial crisis.

We extend this literature by examining the advisory role of outside directors with investment banking background. More specifically, we investigate the effect of directors who once held or currently hold senior positions at investment banks (henceforth, IB directors) on firms' acquisition decisions and performance. We focus on acquisitions for two main reasons. First, they are one of the most value relevant corporate events that require the involvement of the board of directors. From a legal perspective, board decisions in takeovers are subject to enhanced scrutiny. For example, in situations where the board adopts defensive tactics, courts often apply the more stringent "Unocal Standard", rather than the traditional Business Judgment Rule.[4] Hence, board needs to deliberate thoroughly before making any critical decision related to acquisitions. Second,

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[3] See, Schmidt (2009), Adams (2009), Goldman, Rocholl, and So (2009), and Duchin, Matsusaka, and Ozbas (2010).

[4] Under the Business Judgment Rule courts will not second-guess business decisions of the board, so long as the members of the board acted in compliance with established standards of conduct.

the uncertainty of the target value to the acquirer, the complexity of the deal, and the negotiation make directors' investment banking background particularly relevant.[5]

We conjecture that IB directors use their expertise and network to affect a firm's acquisition decisions in two ways. First, IB directors might improve the screening of the target candidates. On the one hand, they might assist in identifying good targets that otherwise would not have been pursued, in which case the probability of making acquisitions would increase with the presence of IB directors. On the other hand, they might assist the firm in dodging value-destroying acquisitions, in which case the probability of making acquisitions would decrease with the presence of IB directors. These alternatives are not mutually exclusive, and both effects might be at work simultaneously, in which case the acquisition probability could be unrelated to the presence of IB directors. Second, we conjecture that IB directors assist in negotiating acquisition terms, especially the acquisition premium, thereby increasing the share of merger gains towards the acquiring firm.

We start our analysis by identifying IB directors for a large set of public firms. IB directors are defined as outside directors who have past or concurrent working experience as either top executives or senior managers in one of the most active M&A advising firms. Using a sample of 41,393 firm-year observations from 1998-2008, we document a positive relation between the presence of IB directors and the firms' probability of making acquisitions. *Ceteris paribus*, firms with IBs on the board are 13.6% more likely to make acquisitions in the following year, suggesting that IB directors help firms to be

[5] Board members might serve as generalists and lack the financial knowledge needed to understand some firm policies. For example, Buckley and Van Der Nat (2003) reported disturbing levels of ignorance among independent directors in the matter of derivatives policy.

more active in the takeover market. Furthermore, we show that our findings are not driven by the reverse causality where firms appoint directors with investment banking experience in anticipation of acquisition activities. Our results remain robust when we remove observations where the IB directors were appointed in the three years leading up to the announcement of the acquisition.

Next, we examine whether acquirers with IB directors make better acquisitions. Using a sample of 2,465 acquisitions announced during 1999-2008, we find that acquiring firms with IB directors experience significantly higher abnormal stock returns around the acquisition announcements. Acquirers with IB directors are associated with 0.8% higher abnormal announcement returns. This translates into $36 million in enhanced shareholder value for the mean-sized acquirer. The effect is more pronounced when (i) the relative deal size is larger and (ii) at least one outside director on the acquirer's board holds a concurrent senior position at an investment bank. These results suggest that IB directors are especially valuable when the deal is economically more significant to the acquirer, and when the director has concurrent affiliation with an investment bank.

The more favorable market reaction towards acquisitions by firms with IB directors is consistent with our conjecture that directors' investment banking experience helps firms make better acquisition decisions. We next investigate the source of the value gains by examining target announcement returns, target premium, the advisory fees paid by acquirers, acquirers' long-run operating performance, and the acquirers' buy-and-hold abnormal returns. We find the presence of IB directors is associated with a significantly lower takeover premium when the relative size of the target is large, lending support to the view that IB directors assist in determining and/or negotiating the price for their shareholders in important deals. We also find that acquirers with IB directors pay lower advisory fees than do other acquirers, suggesting that IB directors assist in negotiating a lower advisory fee and/or help reduce the firm’s reliance on outside advisory services in making acquisitions. Finally, we find the presence of IB directors is positively related to the operating and stock performance of the firm in the long-run. Taken together, our results suggest that IB directors help firms identify better targets and negotiate the deals.

The work most related to our study is Güner, Malmendier, and Tate (2008). Using a novel data set on the board composition of 282 large firms during 1988-2001, they study how directors with financial expertise affect corporate policies. They focus their analysis on commercial banker directors and document that having commercial bankers on the board leads to increased external funding and decreased investment-cash flow sensitivity. They also examine the impact of IB directors on firms’ acquisition performance, but find no evidence that having IB directors leads to better acquisitions. As pointed out by Güner, Malmendier, and Tate (2008), their sample consists of large and mature firms, and the results might not be generalized to a larger population. Our analysis, based on a much larger and more recent sample, in addition to the different measure of the directors’ investment banking financial expertise, suggests that directors with investment banking background help firms make better acquisitions.

Our study contributes to the literature by providing new insights on the influence of financial experts on corporate policies. We find that, in addition to offering more vigilant monitoring as documented by prior studies, directors with financial expertise benefit shareholders through their advisory roles. Our analysis also complements a large literature that relates corporate governance to a firm's decision to acquire, with particular attention to the impact of board independence and board size on acquisition performance (Byrd and Hickman 1992; Cotter, Shivdasani, and Zenner, 1997; Harford 2003; Moeller, 2005; Masulis, Wang, and Xie, 2007). More importantly, our study adds to a growing body of research that analyzes the effects of directors with specific attributes. Masulis and Mobbs (2010) find that firms with inside directors holding outside directorships make better acquisition decisions. Fahlenbrach, Low, and Stulz (2010) document that CEO directors have no impact on firms' acquisition performance. Our analysis reveals that directors' current and past professional experience can be valuable to shareholders in the context of acquisitions.

The remainder of the paper is structured as follows. Section 2.2 discusses the data and provides descriptive statistics. Section 2.3 presents empirical results for the relation between the presence of IB directors and firms' acquisition propensity. Section 2.4 presents empirical evidence on the impact of IB directors on firms' acquisition performance. Section 2.5 explores the sources of acquisition value gains. Section 2.6 concludes.

2.2 Data and Variables

The data in this study are collected from various sources. We start with all U.S. publicly traded firms in 1998-2008. To obtain directors' background information, we merge the sample with the BoardEx database, which provides extensive biographical information, such as employment history and educational background, of corporate directors and senior executives in major public firms. To ensure the quality of the data integration procedure, we manually check all matches and make necessary adjustments.[6] For example, the same firm might be assigned different identifiers in BoardEx, because it collects individuals' biographical information from various public sources which sometimes use different spellings or abbreviations. We go through the BoardEx database to make sure that each firm is associated with a unique identifier. Our matching procedure yields a sample of 8,007 unique public firms, of which 1,128 financial and utility firms are eliminated. This initial sample corresponds to 41,393 firm-year observations.

To identify directors with investment banking experience, we first aggregate the deal values of U.S. mergers and acquisitions for investment bank advisers from 1980 to 2008. We then merge these M&A advising firms with the BoardEx data and compile a list of the 100 most active investment banks. A director serving on the board of a public firm in our sample is identified as an IB director if she, at some point in her career, held a senior position at any of these 100 investment banks. A senior position is defined as a top executive position (e.g., CEO, CFO, Chairman or President) or a senior manager position (e.g., managing director, Regional CEO, Regional CFO, or executive president). Junior job titles—such as *division VP, analyst, associate* or *consultant*—are not included. Table 2.1 provides a list of the ten most active M&A advisors by the aggregate deal value and by the number of connected directors at public firms with whom they once shared an employment relation. As expected, there is a large overlap between the two.

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[6] There are several papers using BoardEx to examine the role of social networks. Our procedure is very similar to those in Cohen, Frazzini, and Malloy (2008), Fracassi and Tate (2010), Ishii and Xuan (2010) and Engelberg, Gao, and Parsons (2010).

Table 2.2 presents summary statistics for the 41,393 firm-year observations. Panel A reports the presence of IB directors by year. The proportion of firms appointing IB directors to the board increases monotonically over time. For example, while 17.3% of the firms have at least on IB director on the board in 1998, the ratio increases to 29.7% in 2008. On average, 24% of the firm-year observations have at least one IB director on their board. Panel B describes the presence of IB directors by industry. Our sample covers ten Fama-French industries, as financial and utility firms are excluded. The Consumer Nondurables industry has the highest ratio of IBs on their board (33.0%), followed by the Telephone and Television industry (31.5%).

To examine the influence of IB directors on a firm's acquisition decisions, we collect deal information from SDC's M&A database. Following the previous literature, we exclude all transactions labeled as spinoffs, recapitalizations, self-tender offers, exchange offers, repurchases, minority stake purchases, acquisitions of remaining interest, or privatizations. We further require that the deal be completed with a deal value greater than $1 million and that the acquirer possess more than 95% of the target's stock after the transaction. This procedure gives us a total of 2,057 firm-years with at least one acquisition. We obtain financial information from COMPUSTAT and stock returns from CRSP. For a subsample, we supplement our data with CEO information from ExecuComp and firms' governance characteristics from RiskMetrics.

2.3 IB Directors and the Probability of Making Acquisitions

We first investigate whether IB directors affect a firm's likelihood of making an acquisition. Panel A of Table 2.3 reports the number and percent of firms that make at least one acquisition in a year. It also reports these values for firms with and without an

IB Director. For 9 out of the 10 years, the percentage of firms making acquisitions is significantly higher in the subsample of firms with IB directors. Therefore, the univariate results suggest a positive relation between the presence of IB directors and the firms' likelihood of making an acquisition.

We next conduct the analysis in a multivariate setting. In particular, we estimate a probit regression in which the dependent variable equals one if a firm announces at least one acquisition in the year and zero otherwise. The primary variable of interest is an indicator variable (*IB Director*) that equals one if the firm has an IB director in the previous year and zero otherwise. Similar to the prior literature (Asquith, Bruner, and Mullins 1983; Harford 1999), we control for a number of other determinants of a firm's acquisition likelihood, including firm size, market-to-book ratio, leverage, previous acquisition experience, cash, sales growth, noncash working capital, price-to-earnings ratio, and average abnormal return. Panel B of Table 2.3 provides the summary statistics for these control variables. We also control for calendar year and industry (Fama-French 48 industry) fixed effects in the regression.

Model 1 in Table 2.4 presents the results of the probit regression. Consistent with Asquith, Bruner, and Mullins (1983) and Harford (1999), we find that firms with higher abnormal returns, higher sales growth, higher market-to-book ratio, or larger asset base are more likely to make acquisitions. Turning to our variable of interest, we find that the coefficient on the *IB Director* dummy is 0.057 and it is statistically significant. The effect on the acquisition likelihood is also economically meaningful. *Ceteris paribus*, firms with IBs on the board are 13.6% more likely to make an acquisition than other firms.[7]

A potential concern for our analysis is the endogeneity of board composition, as pointed out by Hermalin and Weisbach (1998, 2003). In particular, it is possible that firms appoint directors with investment banking experience in anticipation of acquisition activities. To address this potential concern, we employ a two-stage regression model. In the first stage, we estimate the probability of having an IB on the board. Since geographic proximity increases the chance that there are personal and professional ties between executives of the firm and investment bankers and decreases the personal cost (primarily travel time) for investment bankers to serve on the board, we expect firms located in states close to financial centers to have more IB directors.[8] We also include variables that could affect a firm's decision to appoint IBs to the board. Large boards are usually composed of directors with different backgrounds and thus are more likely to have directors with investment banking experience. Accordingly, we include board size in the regression. In addition, the Sarbanes-Oxley Act of 2002 requires firms to have financial experts on the board, so we expect the presence of IB directors to be more common after 2002. Moreover, firms might hire IB directors to provide advice on other capital market activities, such as equity offerings and debt issuances, so we control for these corporate

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[7] Prior research has shown that CEO and board characteristics have significant effects on firms' acquisition policy (Lewellen, Loderer, and Rosenfeld, 1985; Cotter, Shivdasani, and Zenner, 1997; Bertrand and Schoar, 2003; Yim, 2010). In untabulated regressions, we include CEO and board variables, and find similar results. The effect of investment banker directors remains the same when we add Gompers, Ishii, and Metrick (2003) G-index as an additional control. We also use total deal value as our alternative dependent variable and find that the positive relation between investment banker presence and acquisitions likelihood still holds.

[8] Fahlenbrach, Low, and Stulz (2010) and Masulis, Wang, and Xie (2010) use a similar instrumental strategy in their study.

events. In the second stage, we re-estimate the probit regression by replacing the *IB Director* dummy with the predicted probability of having at least one IB director on firms' boards.

Model 2 in Table 2.4 presents the two-stage regression results. In the first stage, we find most of our explanatory variables are significant in predicting the presence of IB directors. The coefficient on the predicted *IB Director* in the second stage is positive and significant at the one percent level, confirming the positive association between the presence of IB directors and a firm's acquisition propensity.

In untabulated analyses, we conduct additional tests to further confirm that our results are not driven by the reverse causality. [9] First, if an IB director is appointed to the board to facilitate anticipated acquisitions, the deal is likely to be announced shortly after the director's appointment. Accordingly, we exclude deals where IB directors have three or fewer years of tenure on the board. Our results are robust to this exclusion. Second, since directors who gain investment banking experience after joining the board should be free of such selection bias, we re-estimate the probability of acquisition using this subsample of IB directors. We still find a positive association between this type of IB director and a firm's likelihood of acquisition. Overall, the evidence indicates that the positive impact of IB directors on the probability of acquisitions is not driven by the reverse causality.

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[9] Several papers use similar methods to alleviate reverse causality concerns (Güner, Malmendier, and Tate 2008; Stuart and Yim 2010).

2.4 IB Directors and the Acquirer Announcement Returns

If directors with investment banking experience provide valuable advice to firms in making acquisition decisions, we expect such firm to make better acquisitions and receive more favorable market reactions around the acquisition announcements. In this section, we examine whether acquiring firms experience higher abnormal announcement returns when they have at least one IB director on their board.

2.4.1 Acquisition Sample

Our sample of M&A deals consists of 2,465 acquisitions of U.S targets by 1,390 unique U.S public acquirers during 1999-2008. Among these deals, 808 deals (33%) have at least one IB director on the acquirer's board. Panel A of Table 2.5 shows the distribution of acquisitions by announcement year. The deals are roughly evenly distributed over the ten-year period**.** We further divide the sample of acquisition into two subgroups based on whether the acquiring firm has at least one IB director in the year prior to the acquisition announcement. The percentage of deals by acquirers with IB directors increases from 26% in 1999 to 39% in 2008. The aggregate deal value by such acquirers is $1.14 trillion, representing 51% of the aggregate transaction value over our sample period.

Panel B of Table 2.5 reports the distribution of acquisitions by acquirer industry. The industry distribution is similar across the two subsamples. The most active industry is Business Equipment (1,031 acquisitions). Acquisitions by acquirers with an IB director are more prevalent in some industries than others. For example, 49% of deals in the Telephone and Television industry are announced by acquirers with IB directors, but only 28% of the deals in the Business Equipment industry.

Panel C and Panel D contain descriptive statistics for control variables used in this section. On average, acquirers with IB directors are larger and have lower market-to-book ratios, higher leverage and higher cash flows. The average transaction value is $908 million, or 27% of the acquirer's total assets. About 42% of the target firms are publicly traded. Acquirers with IB directors are more likely to target public firms and use cash as payment.

Panel E describes our measures of the presence of IB directors. Besides the *IB Director* dummy, we construct two other variables: *IB Director_Size*, defined as the total number of IB directors, and *IB Director (%)*, defined as the proportion of IB directors on the acquirer's board. As we showed earlier, 33% of acquiring firms have investment bankers on the board. The average number of investment bankers is 0.41, representing 5% of board members. For the subsample of deals by acquirers with an IB director, a typical acquiring firm has one IB director, representing 15% of the board size. Among these acquirers, more than 20% have multiple IB directors.

2.4.2 Acquirer Announcement Returns

To calculate the abnormal returns around the acquisition announcements, we use the CRSP value-weighted return as the market return and estimate the market model for each deal over a 200-day period ending 11 days before the announcement dates. We then use the estimated parameters to calculate the cumulative abnormal returns (CAR) over the three-day event window centered on the announcement date.

Table 2.6 reports the mean and median CAR for acquiring firms over the three-day event window. On average, the acquirers' stock reaction is positive but not significantly different from zero. We then split the sample into two groups based on the presence of IB directors on the acquiring firms' board. The mean CAR for acquirers without IB directors is not significantly different from zero. In contrast, the mean CAR for acquirers with IB directors is 0.7% and it is statistically significant from zero at the 5% level. This suggests that, unconditionally, the presence of IB directors is associated with an increase in acquirers' shareholders wealth upon the acquisition announcement. The table also shows that the higher mean excess announcement returns for acquirers with IB director persist across size quartiles.

Next, we estimate regressions of acquisition returns to control for the determinants of acquirer announcement returns documented in previous studies (Asquith, Bruner, and Mullins, 1983; Travlos, 1987; Byrd and Hickman, 1992; Yermack, 1996; Chang, 1998; Moeller, Schlingemann, and Stulz, 2005). These control variables include firm and deal characteristics, such as acquirer board size, board independence, firm size, the market-to-book ratio, leverage, cash flow, whether the acquirer owns more than 5% of the target's stock prior to the announcement date, method of payment, and identifiers for deal competition, conglomerate deals, tender offers, and target public status. We also control for year and industry fixed effects in all of our regressions.

Table 2.7 presents the OLS regression results. The dependent variable is the three-day CAR for acquirers. The primary explanatory variables of interest are the *IB Director* dummy or *IB Director(%)*. The results in Columns 1 and 2 suggest that both *IB Director* and *IB Director(%)* have positive and significant effects on acquirer announcement returns. The presence of an investment banker on the board increases the acquirer's three-day CAR by 80 basis points in comparison with the sample average of 30 basis points. Increasing *IB Director(%)* by one standard deviation raises the three-day CAR by 64 basis points. For other control variables, our estimated coefficients are similar to those reported in earlier studies. We find a strong negative correlation between acquirer size and acquirer abnormal returns, consistent with Moeller, Schlingemann, and Stulz (2004). We also find that deals involving cash financing or tender offers have higher acquirer abnormal returns, whereas deals involving public targets or competing offers are associated with lower returns.

Li and Prabhala (2007) argue that a firm's decision to attempt an acquisition is not random and that deal anticipation might affect market reactions to acquisition announcements. Cai, Song, and Walkling (2010) find evidence consistent with such an anticipation effect. They find that when deals are less anticipated, returns to bidders are less negative (or more positive). Because our results suggest that firms with IB directors have a higher propensity to make acquisitions, deals by such firms might be less surprising to the market and thus are associated with a more favorable market reaction as suggested by the anticipation hypothesis. To control for a potential anticipation effect, we employ a two-stage Heckman Selection model. The first stage employs a probit regression of the acquisition likelihood as shown in Table 2.4 Model 1. In the second stage, we add the Inverse Mills ratio as an additional independent variable in our estimation of acquirer announcement returns. Columns 3 and 4 of Table 2.7 present the regression results. The coefficients on our key explanatory variables, *IB Director* and *IB Director(%)* remain positive and significantly different from zero.

Güner, Malmendier, and Tate (2008) examine the effect of IB directors on acquisition performance by studying 526 acquisitions made by large and mature firms during 1988–2001. They show that having investment banker executives on the board has no significant impact on acquirer announcement returns.[10] In Columns 5 and 6, we repeat our regressions in Columns 1 and 2 using firms within the largest size quartile. The coefficient on the *IB Director* dummy is not significantly different from zero, similar to what Güner, Malmendier, and Tate (2008) report. However, the coefficient on *IB Director(%)* is still positive and significant, suggesting that *IB Director(%)* better captures the effect of IB directors on acquirer returns. In a subsample of acquisitions by acquirers with IB directors (i.e., *IB Director* = 1), we also find significant positive relation between *IB Director(%)* and acquirer returns, indicating that the proportion of IB directors matters. Therefore, in our following analyses, we only report results for *IB Director(%)*. Our results, however, are qualitatively similar if we use the *IB Director* dummy instead.

In an untabulated analysis, we add CEO ownership, CEO age, CEO gender, and GIM-index/BCF-index in our baseline regression for a subsample of 850 deals, for which we have information available from ExecuComp and RiskMetrics. The estimated coefficients of *IB Director* and *IB Director(%)* are persistently positive, though the significance level decreases from 1% to 5% level, presumably due to a significant reduction in sample size. We also verify that our findings are not driven by outliers, as our results remain robust when we winsorize the dependent variables at the 1st and 99th percentile.

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[10] For deals involving private targets, Güner, Malmendier, and Tate (2008) find a negative relation between the presence of investment banker director and acquirer announcement returns.

2.4.3 Identification Concern

The positive relation between the presence of IB directors and acquirer abnormal announcement returns might be subject to selection bias. For instance, firms that foresee good acquisition opportunities might decide to hire IB director to facilitate the acquisition process. Such an endogeneity concern is mitigated by the observation that the mean tenure of IB directors in our acquisition sample is 5.9 years when the deal is announced. It is unlikely that these directors are appointed to the board to facilitate a planned acquisition. Nevertheless, we examine more carefully whether selection bias drives our results.

First, we remove deals where the acquiring firms' IB directors are appointed in the three years leading up to the deal (i.e. IB directors with short tenure) or deals where the IB directors gained investment banking experience before they joined the board. The results (not tabulated) are largely unchanged. Second, instead of excluding the aforementioned acquisitions, we construct dummy variables identifying the IB directors associated with such deals. We do not find that such IB directors have a significantly different impact on the acquirer announcement returns than other IB directors. Finally, we employ a two-stage regression model. In the first stage, we use the probit regression from Table 2.4 to predict the presence of investment banker directors. We then use *IB Director (predicted)* in the second stage acquirer CAR regression. The coefficient on *IB Director (predicted)* is still significantly positive. Overall, our findings suggest that the positive relation between the presence of IB director on the acquirer board and the acquirer acquisition announcement returns is not driven by the selection bias discussed above.

2.4.4 Supplemental Results

To the extent that the financial expertise of IB directors is valuable to firms in making acquisition decisions, their influence is likely to be more pronounced in acquisitions where the target's size constitutes a significant proportion of the combined entity. To test this conjecture, we construct an indicator variable for deals with a relative target size above the sample median and interact this indicator variable with our IB director measure, *IB director(%)*. We then repeat our baseline regression of acquirer CARs with this additional interaction term and report the results in Table 2.8.[11] The positive and significant coefficient on the interaction term suggests that IB directors are especially helpful when the deal is economically significant. Second, we conjecture that directors with current experience and/or a current network have a greater influence on the deal process. Accordingly, we account for the director's employment status at the time of the announcement of the deal. If the director is currently employed by an investment bank when the deal is announced, then we define him/her as an investment banker director with *Current experience*. The results, reported in Column 2, show current investment bankers have a stronger positive effect.

In untabulated analyses based on a subsample of 1,082 deals for which we have acquirer CEO information, we find that acquirers with young and short-tenured CEOs benefit more from the presence of IB directors, suggesting that the impact of the IB directors increases with the importance of their advisory roles. We also predict that directors with more recent investment banking experience should have a greater impact in

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[11] We only report the coefficients on key independent variables for simplicity, as the coefficients on control variables are similar to those reported in our baseline regression in column 2 of Table 7.

this subsample of firms. We consider an IB's experience to be recent if it was gained in the 10 years prior to the announcement of the deal. We find that recently gained experience does indeed matter more.

Furthermore, we partition IB directors based on their employment history at different tiers of investment banks. A director is classified as having top investment bank experience if she, at some time in her career, worked for one of the top 10 most active investment banks serving as M&A advisors listed in Table 2.1. The two groups of directors have a similar impact on acquirer returns, suggesting our results are not driven by IB directors with top investment banks experience. In addition, we identify all directors that have junior job experience in investment banks and examine whether they exert any influence. We find that junior directors have no significant impact on acquirer announcement returns. Finally, we investigate whether conflicts of interest hamper a director's advisory role. An IB director is denoted as having conflicts of interest if he/she is currently employed by acquirer or target financial advisors. We do not find any evidence that affiliated directors destroy value for the shareholders of the acquiring firms to benefit themselves.

Overall, we find that the acquiring firms with IB directors are associated with higher abnormal announcement returns. This favorable market reaction is consistent with investment bankers improving either the screening process or the implementation of the deal. It is also consistent with an alternative argument that the observed positive relation is purely driven by a certificate effect, whereby the market reacts more favorably to acquirers with investment banker directors, even though these directors have no significant impact on the process. We next investigate the potential sources of the value gains associated with the acquisitions by examining target announcement returns, target premium, advisory fee paid by acquirers, long-run operating performance, and buy-and-hold abnormal return.

2.5 Sources of the Value Gain

2.5.1 Target Announcement Returns and Takeover Premium

Table 2.9 presents our analysis of the effect of IB directors on target announcement returns and takeover premiums for deals involving public targets. In Columns 1 – 3, the dependent variable is the three-day target CAR. In Columns 4 – 6, the dependent variable is the acquisition premium defined as the difference between the price paid per share and the target share price four weeks prior to the deal announcement date. All regressions control for the acquiring firm characteristics and deal characteristics as specified in the acquirer CAR regressions as well as target firm characteristics such as target market to book ratio, leverage, and cash flow.

In Column 1, we find that the presence of an IB director does not seem to influence the target's stock reaction to the acquisition announcement on average. *IB Director(%)* is negatively related to the target abnormal returns, but not significantly different from zero. In Column 2, we add an interaction term between *IB Director(%)* and the *Large Deal* dummy. We find that for the sample of large deals relative to the acquirers' size, the presence of investment banker directors is negatively related to the target abnormal returns. For large deals, a one standard deviation increase in *IB Director(%)* is associated with a 3.5 percentage point decrease in the target three-day CAR. In Column 3, we add an additional interaction to test whether directors with current investment banking experience have any different impact on target returns. The results show that current investment bankers do not have stronger effects than other investment banker directors.

If acquiring firms benefit from the financial expertise of the IB directors in better evaluating the target, such acquirers are less likely to overpay. Therefore, we expect acquirers with IB directors, on average, to pay lower acquisition premium. Columns 4 to 6 in Table 2.9 report the regression results of the takeover premium. We find that the presence of IB directors is negatively related to the takeover premium, though the effect is not statistically significant. For the subsample of deals where the relative target size is large, we find the presence of the IB directors significantly reduces the takeover premium. For example, a one standard deviation increase in *IB Director(%)* is associated with a 6.3% decrease in target premium. In the last column, we find that directors with current investment banking experience do not have significantly greater ability to reduce the premium.

2.5.2 Acquirer Advisory Fees

To facilitate M&A transactions, firms generally hire investment bankers to provide professional advice. If acquirers have investment bankers on the board, their need for outside financial advisors is likely to be lower and they might be in a better position to negotiate the fee. Thus, we expect that advisory fees are lower for acquirers with IB directors.

We collect the M&A advisor data from SDC and investigate the dollar amount of financial advisory fees paid by the acquirers. Table 2.10 reports the results. The dependent variable is the natural logarithm of the financial advisory fees. [12] In all regressions, we control for acquirer firm characteristics and deal characteristics, as well as industry and year fixed effects.

Consistent with our conjecture, the presence of an IB director is associated with significantly lower advisory fees paid by the acquirers. For example, a one standard deviation increase in *IB Director(%)* is associated with a 12% decrease in the advisory fees paid by acquirers. Furthermore, the negative relation between the presence of the IB directors and the advisory fees is more pronounced when the relative target size is large and when the IB directors have a concurrent affiliation with an investment bank.

2.5.3 Post-acquisition Operating Performance

In addition to helping reduce the takeover premium and advisory fees, IB directors might also help acquirers pick targets with greater synergy potential. We test this conjecture by investigating the post acquisition performance of the combined firms.

We use two operating performance measures. The first one is the raw operating performance, calculated as earnings before the deduction of interest, tax and amortization expenses (EBITDA) scaled by sales. [13] The second measure is industry benchmark-adjusted operating performance. Barber and Lyon (1996) show that tests of changes in operating performance are only well specified when the sample firms are matched to control firms of similar pre-event performance. We construct the industry-performance benchmark for each sample firm following Barber and Lyon (1996) and Vijh and Yang

[12] We also use percentage advisory fees, defined as the amount of fees scaled by deal value, as another dependent variable and obtain similar results.

[13] Results are similar based on cash flow return on assets.

(2009). For each acquirer (target), we first identify all firms with the same two-digit SIC code in the same year, but excluding the acquirer and target firms. Among these firms, we select those that have operating performance between 90% and 110% of the acquirer/target firm's operating performance during the year before the acquisition announcements. If no firm meets the industry-performance criteria, we apply the 90% to 110% filter without imposing the same industry requirement. If still no matching firm is found, we select the single firm with operating performance closest to that of the sample firm. The benchmark-adjusted operating performance is then defined as the difference between the performance of the sample firm and the median performance of the control group described above.

For the pre-acquisition years, we calculate operating performance as the weighted-average performance of the acquirer and target firms, where the weights correspond to the relative sales of the two firms. The calculation of benchmark operating performance uses the same weighting procedure. For the post-acquisition years, the calculation of benchmark operating performance follows the same weighing procedure, where the weights correspond to the total sales of the acquirer and the target firms during the year before acquisition announcement. The calculation of the post acquisition operating performance for the combined firms is obvious and does not require weighting procedure.

We then compare changes in operating performance for acquiring firms from pre-acquisition to post-acquisition years across subgroups. We focus on changes rather than levels because Barber and Lyon (1996) show that the change models dominate the level models in detecting abnormal operating performance. Table 2.11 reports median changes in operating performance.[14] We find that acquirers with IB directors experience greater improvement in operating performance than other firms.

2.5.4 Long-run Buy-and-Hold Abnormal Returns

We next examine whether acquirers with IB directors outperform other firms in terms of buy-and-hold returns. We analyze buy-and-hold abnormal returns (BHARs) over 36 months for each acquisition, compounding monthly over the relevant period. Two buy-and-hold abnormal returns are calculated: market-adjusted BHARs and Fama-French adjusted BHARs. To calculate the market-adjusted BHARs, we subtract market returns (CRSP value-weighted index) from the monthly raw returns before compounding. To calculate the Fama-French adjusted BHARs, we first regress monthly returns on the Fama-French three factors using five-year data leading up to the acquisition event, and then use the estimated coefficients to calculate monthly abnormal returns before compounding.

Table 2.12 reports the median buy-and-hold abnormal returns from quarter 1 to quarter 12. Focusing on market-adjusted BHARs, we find that acquirers with investment bank directors outperform other acquirers. The difference between the two subsamples is statistically different. Over the three-year period, acquirers with IB directors outperform the other group by 8.1%. We find similar results when we use Fama-French adjusted BHARs. Figure 2.1 depicts the median Fama-French adjusted BHARs for two subsamples, as well as the difference between them.

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[14] Mean changes have a similar pattern.

We reported earlier that deals in the two subsamples are different in both deal size and method of payment. Accordingly, in Table 2.13 we divide the whole sample into several subsamples based on deal characteristics and report the median buy-and-hold abnormal returns. The results show that within each subsample, acquirers with IB directors still outperform other acquirers, and most of the differences are statistically significant. Overall, the long-run performance evidence is consistent with the idea that IB directors enhance the firm's ability to identify suitable targets and, hence, generate greater synergy in the long run.

2.6 Conclusion

We analyze how investment banker directors affect firms' acquisition behavior and acquisition performance. We test several hypotheses using the employment history of outside directors serving on boards of U.S public firms. Our results indicate that firms having directors who, at some time in their career, held senior positions at investment banks (i.e., IB directors) are more likely to make acquisitions and experience higher abnormal returns upon their acquisition announcements. On average, having an IB director on the board increases the acquirer's three-day CAR by 80 basis points. The positive wealth effect of IB directors is more prominent when the target size constitutes a significant proportion of the combined entity, suggesting that the importance of the financial expertise of IB directors increases with the economic significance of the acquisition. Our results are robust to tests for endogeneity.

We next explore potential sources of the value gains to the acquirers with IB directors. We find that when the relative target size is large, the presence of IB directors on the acquiring firms' board is associated with lower acquisition premium and advisory fees and greater improvement in the operation performance after the acquisitions. Our findings suggest that directors with investment banking experience help the acquiring firms in (i) selecting better target candidates, (ii) better evaluating the target valuation, and (iii) reducing the firms' reliance on the outside M&A advisory service and/or negotiating lower advisory fees.

Our study contributes to the literature with further insights on the roles of boards of directors. We provide additional evidence on the advisory roles of the board of directors in the context of mergers and acquisitions, which are one of the most value relevant corporate decisions. In particular, we document the benefit of the financial expertise of board members.

Table 2.1 Top 10 Investment Banks

Ranking	Ranked by Aggregate Deal Value	Ranked by Number of Affiliated Directors
1	GOLDMAN SACHS	MORGAN STANLEY
2	MORGAN STANLEY	LEHMAN BROTHERS
3	MERRILL LYNCH	GOLDMAN SACHS
4	J.P. MORGAN	BEAR STEARNS & CO INC
5	CITIGROUP	SALOMON BROTHERS
6	CREDIT SUISSE	J.P. MORGAN
7	BARCLAYS CAPITAL	MERRILL LYNCH
8	UBS	CITIGROUP
9	LAZARD	CREDIT SUISSE
10	DEUTSCHE BANK AG	LAZARD

Note: This table presents two lists of investment banks. The first is the 10 most active M&A advisors in terms of aggregate deal value in the U.S. market during 1980-2008, based on data from SDC's M&A database. The next list is the 10 most commonly affiliated investment banks, ranked based on the total number of affiliated directors in our sample.

Table 2.2 Summary Statistics for the Aggregate Sample

Panel A: Distribution of Observations by Year

Year	Number of Firms	IB Director = 1
1998	3,827	17.30%
1999	3,989	18.40%
2000	4,084	19.50%
2001	3,906	20.90%
2002	3,787	22.60%
2003	3,728	25.30%
2004	3,794	26.40%
2005	3,782	27.30%
2006	3,713	28.40%
2007	3,570	28.90%
2008	3,213	29.70%
Total	41,393	23.90%

Panel B: Distribution of Observations by Industry

Fama-French Industry	Number of Firm Years	IB Director = 1
Consumer nondurables	2,654	33.0%
Consumer durables	1,120	18.4%
Manufacturing	5,172	22.6%
Oil, gas and coal	1,887	25.1%
Chemical products	1,116	25.7%
Business equipment	10,637	19.6%
Telephone and television	1,624	31.5%
Wholesale and retail	5,115	27.2%
Healthcare	5,820	21.7%
Other	6,248	25.8%
Total	41,393	23.9%

Note: This table reports the summary of our firm-year observations. Panel A presents the distribution of observations by year. Number of firms in each year is reported, followed by the percentage of firms with investment banker directors. Panel B presents the distribution of observations by industry. Industries are defined by the Fama-French 12-industry category. Our sample covers ten Fama-French industries, as financial and utility firms are excluded from the sample. Number of firm years in each industry is reported, followed by the percentage of firm years with investment banker directors.

Table 2.3 Preliminary Results of Acquisitions Propensity

Panel A: Percentage of Firms Making Acquisitions

	Firms with IB Director		Firms without IB Director	
Year	Number of firms	% of Firms Making Acquisitions	Number of Firms	% of Firms Making Acquisitions
1999	661	6.66%	3,166	5.09%**
2000	733	9.41%	3,256	7.00%**
2001	795	5.91%	3,289	4.93%
2002	818	5.38%	3,088	4.11%*
2003	857	6.18%	2,930	3.28%***
2004	944	6.36%	2,784	4.99%*
2005	1,001	7.49%	2,793	5.05%***
2006	1,032	7.56%	2,750	5.16%***
2007	1,054	7.50%	2,659	5.45%**
2008	1,030	5.73%	2,540	4.25%**
Total	8,925	6.81%	29,255	4.95%***

Panel B: Summary Statistics for Some Control Variables

	Acquisition = 1 (N = 2,057)		Acquisition = 0 (N = 36,123)	
	Mean	Median	Mean	Median
IB Director	0.30	-	0.23***	-
Acquisition Dummy	0.38	-	0.17***	-
Size	6.68	6.59	5.77***	5.68***
Market-to-Book	2.99	2.06	2.26***	1.58***
Leverage	0.18	0.14	0.22***	0.17***
Cash	0.24	0.16	0.21***	0.11***
Avg. Abnormal Return	-0.34	-0.78	-0.28**	-0.65**
Sales Growth	0.19	0.14	0.14***	0.07***
Noncash Working Capital	0.05	0.04	0.05	0.04
Price-to-Earnings	19.61	18.25	12.03***	11.39***

Note: This table reports some univariate results for the relation between investment banker directors and acquisition behavior, and provides descriptive statistics for some control variables. Panel A reports the percentage of firms in each year that make at least one acquisition for subsamples based on the presence of investment banker directors. Panel B reports mean and median differences in some control variables broken out by acquisition dummy. Variable definitions are provided in the Appendix A. All unbounded variables are winsorized at the 1st and 99th percentiles and all dollar values are adjusted to 2009 dollars. Asterisks denote statistically significant differences between the two sub-samples at the 1% (***), 5% (**), or 10% (*) level, based on t-tests for differences in mean and on Wilcoxon tests for differences in median.

Table 2.4 Probit Regressions of Acquisition Propensity

	Predicting Acquisitions (Acquisition = 1)		
	(1)	(2)	
		First stage	Second stage
IB Director	0.057**		
	[0.027]		
IB Director (predicted)			1.432***
			[0.000]
Size	0.128***	0.117***	0.069***
	[0.000]	[0.000]	[0.000]
Market-to-Book	0.052***	0.004	0.048***
	[0.000]	[0.313]	[0.000]
Leverage	-0.261***	0.198***	-0.334***
	[0.000]	[0.000]	[0.000]
Firm Age		-0.059***	
		[0.000]	
Board Size		0.077***	
		[0.000]	
Post-SOX		0.081***	
		[0.000]	
Location		0.168***	
		[0.000]	
SEO Dummy		0.066***	
		[0.000]	
Debt issuance Dummy		0.008	
		[0.687]	
Acquisition Dummy	0.399***	0.021	0.362***
	[0.000]	[0.298]	[0.000]
Cash	0.167**		0.187**
	[0.023]		[0.014]
Avg. Abnormal Return	0.208***		0.223***
	[0.008]		[0.002]
Sales Growth	0.073***		0.069***
	[0.001]		[0.001]
Noncash Working Capital	0.417***		0.449***
	[0.000]		[0.000]
Price-to-Earnings	0.001***		0.001***
	[0.002]		[0.001]
Intercept	-2.843***	-2.218***	-2.351***
	[0.000]	[0.000]	[0.000]
Industry dummy	Yes	Yes	Yes
Year dummy	Yes	No	Yes
N	38,180	41,393	38,180
Pseudo R-squared	0.036	0.102	0.043

Table 2.4 Continued

Note: This table reports results of probit regressions of the probability that a firm has at least one acquisition in a given year. The dependent variable is one if a firm completes an acquisition and zero otherwise. In Model 1, we report the regular probit regression results. In Model 2, the two-stage regression results are reported, where we replace the *IB Director* dummy in the second-stage probit with its predicted value. Definitions of the independent variables are in the Appendix A. Both regressions control for calendar year-fixed effects and industry (Fama-French 48 industry) fixed effects whose coefficients are suppressed for brevity. P-values are reported in brackets. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.5 Summary Statistics for the Acquisition Sample

Panel A: Number of Acquisitions by Year

	All Deals		Non-IB Director Deals		IB Director Deals	
Year	N	%	N	%	N	%
1999	326	13.23%	240	14.48%	86	10.64%
2000	384	15.58%	277	16.72%	107	13.24%
2001	227	9.21%	163	9.84%	64	7.92%
2002	187	7.59%	132	7.97%	55	6.81%
2003	188	7.63%	114	6.88%	74	9.16%
2004	228	9.25%	155	9.35%	73	9.03%
2005	257	10.43%	167	10.08%	90	11.14%
2006	252	10.22%	155	9.35%	97	12.00%
2007	247	10.02%	151	9.11%	96	11.88%
2008	169	6.86%	103	6.22%	66	8.17%
Total	2,465	100.00%	1,657	100.00%	808	100.00%

Panel B: Number of Acquisitions by Industry of Acquirer

	All Deals		Non-IB Director Deals		IB Director Deals	
Fama-French Industry	N	%	N	%	N	%
Consumer nondurables	117	4.75%	75	4.53%	42	5.20%
Consumer durables	44	1.78%	30	1.81%	14	1.73%
Manufacturing	259	10.51%	169	10.20%	90	11.14%
Oil, gas and coal	110	4.46%	62	3.74%	48	5.94%
Chemical products	35	1.42%	21	1.27%	14	1.73%
Business equipment	1,031	41.83%	742	44.78%	289	35.77%
Telephone and television	82	3.33%	42	2.53%	40	4.95%
Wholesale and retail	165	6.69%	108	6.52%	57	7.05%
Healthcare	310	12.58%	211	12.73%	99	12.25%
Other	312	12.66%	197	11.89%	115	14.23%
Total	2,465	100.00%	1,657	100.00%	808	100.00%

Panel C: Acquirer Characteristics

	All Deals		Non-IB Director Deals		IB Director deals	
	Mean	Median	Mean	Median	Mean	Median
Size	6.81	6.65	7.42	7.34	6.51***	6.36***
Market-to-Book	3.08	2.09	3.19	2.20	2.82***	1.98***
Leverage	0.18	0.14	0.16	0.11	0.21***	0.18***
Cash Flow	0.10	0.13	0.09	0.13	0.13***	0.14***
Board Size	8.32	8.00	7.74	7.00	9.51***	9.00***
Board Independence	0.75	-	0.74	-	0.77***	-
Acquisition Dummy	0.39	-	0.37	-	0.44***	-

Table 2.5 Continued

Panel D: Deal Characteristics

	All Deals		Non-IB Director Deals		IB Director deals	
	Mean	Median	Mean	Median	Mean	Median
Transaction Value	907.94	94.20	661.34	83.96	1,413.65***	121.99***
Relative Transaction Value	0.27	0.09	0.27	0.09	0.25	0.08**
Related Deal	0.62	-	0.63	-	0.60	-
Toehold	0.03	-	0.03	-	0.04	-
Cash Deal	0.41	-	0.38	-	0.48***	-
Stock Deal	0.24	-	0.26	-	0.20***	-
Tender Offer	0.09	-	0.07	-	0.11***	-
Competition	0.02	-	0.02	-	0.02	-
Public Target	0.42	-	0.39	-	0.49***	-
Private Target	0.49	-	0.52	-	0.41***	-
Subsidiary Target	0.09	-	0.09	-	0.10	-

Panel E: Investment Banker Directorship Characteristics

	All Deals (N = 2465)		IB Director Deals (N = 808)	
	Mean	Median	Mean	Median
IB Director	0.33	0.00	1.00	1.00
IB Director_Size	0.41	0.00	1.25	1.00
IB Director (%)	0.05	0.00	0.15	0.13

Note: The acquisition sample consists of 2,465 completed U.S. mergers and acquisitions between 1999 and 2008. This table reports the distribution of acquisitions by year and by acquirer industry, and provides some summary statistics for acquirer and deal characteristics. In Panel A, year is defined as the year when the deal is announced. In Panel B, industries are defined by the Fama-French 12-industry category, and acquisitions are assigned to one of the industry based on the SIC code of the acquirer. Our sample covers ten Fama-French industries, as financial and utility firms are excluded from the sample. Panel C presents mean and median values for acquirer characteristics and Panel D presents mean and median values for deal characteristics. For all panels, numbers are first reported for the full sample and then for subsamples based on the presence of investment banker directors. *Non_IB Director Deals* refer to deals where acquirer has no investment banker director on the board when the deal is annunced. *IB Director Deals* refer to deals where acquirer has at least investment banker director when the deal is announced. All variable definitions are in the Appendix A. All unbounded variables are winsorized at the 1st and 99th percentiles and all dollar values are adjusted to 2009 dollars. Asterisks denote statistically significant differences between the two subsamples at the 1% (***), 5% (**), or 10% (*) level, based on t-tests for differences in mean and on Wilcoxon tests for differences in median.

Table 2.6 Acquirer Cumulative Abnormal Returns around Acquisition Announcement

	All Deals			Non-IB Director Deals			IB Director Deals			Difference	
	N	Mean	Median	N	Mean	Median	N	Mean	Median	Mean	Median
Total	2465	0.003	0.001	1657	0.001	-0.001	808	0.007**	0.005**	*	**
Q1 (Small acquirers)	616	0.014**	0.007**	486	0.011**	0.005*	130	0.024***	0.014**	*	**
Q2	617	0.009**	0.005**	445	0.007*	0.004*	172	0.015**	0.012**	-	**
Q3	616	-0.001	0.001	399	-0.006	-0.002	217	0.009*	0.011**	**	***
Q4 (Large acquirers)	616	-0.010***	-0.006**	327	-0.013***	-0.008***	289	-0.007**	-0.002	-	**

Note: The acquisition sample consists of 2,465 completed U.S. mergers and acquisitions between 1999 and 2008. This table presents mean and median of acquirer cumulative abnormal returns (CAR) over the three-day event windows around acquisition announcement dates. CAR are reported for the full sample and then for subsamples based on the presence of investment banker directors. The market returns are measured by the CRSP value-weighted index returns. Data over the period from event day -211 to event day -11 are used to estimate the market model. CAR for size quartile subsamples are also reported. Asterisks denote statistically significant from zero at the 1% (***), 5% (**), or 10% (*) level, based on t-tests for mean and on Wilcoxon tests for median. In the last two columns, asterisks denote statistically significant differences between the two sub-samples.

Table 2.7 Regressions of Acquirer Cumulative Abnormal Returns

	[-1, +1] Acquirer CARs					
	Full Sample		Heckman Correction		Large Acquirer	
	(1)	(2)	(3)	(4)	(5)	(6)
IB Director	0.008**		0.008**		0.006	
	[0.031]		[0.034]		[0.154]	
IB Director (%)		0.072***		0.072***		0.056**
		[0.002]		[0.004]		[0.036]
Acquirer Characteristics:						
Acquirer Size	-0.004***	-0.004***	-0.004**	-0.004**	-0.001	-0.001
	[0.008]	[0.006]	[0.017]	[0.015]	[0.825]	[0.768]
Acquirer Market-to-Book	0.001	0.001	0.000	0.000	-0.001	-0.001
	[0.439]	[0.450]	[0.674]	[0.707]	[0.412]	[0.422]
Acquirer Leverage	0.017	0.016	0.015	0.015	0.013	0.015
	[0.173]	[0.195]	[0.249]	[0.282]	[0.493]	[0.455]
Acquirer Cash Flow	-0.022	-0.022	-0.022	-0.022	0.057	0.056
	[0.363]	[0.354]	[0.388]	[0.381]	[0.104]	[0.106]
Board Independence	0.002	0.001	0.002	0.002	0.008	0.007
	[0.802]	[0.898]	[0.832]	[0.748]	[0.461]	[0.517]
Board Size	0.001	0.001	0.001	0.001	0.000	0.000
	[0.189]	[0.212]	[0.307]	[0.153]	[0.844]	[0.625]
Acquisition Dummy	-0.005	-0.005	-0.009	-0.009	-0.008	-0.008
	[0.242]	[0.251]	[0.142]	[0.154]	[0.107]	[0.109]
Deal Characteristics:						
Relative Transaction Value	0.014*	0.013*	0.013*	0.013*	-0.017	-0.017
	[0.065]	[0.078]	[0.084]	[0.083]	[0.038]	[0.038]
Toehold	0.004	0.004	0.004	0.004	-0.001	-0.001
	[0.541]	[0.542]	[0.544]	[0.547]	[0.897]	[0.920]
Competition	-0.024	-0.025*	-0.025	-0.025	-0.016	-0.016
	[0.108]	[0.097]	[0.110]	[0.103]	[0.196]	[0.201]
Stock Deal	-0.008	-0.008	-0.008	-0.008	0.008	0.008
	[0.276]	[0.263]	[0.235]	[0.224]	[0.338]	[0.354]
Cash Deal	0.011**	0.010**	0.011**	0.011**	0.022***	0.021***
	[0.017]	[0.038]	[0.017]	[0.018]	[0.001]	[0.001]
Conglomerate	-0.001	-0.001	-0.001	-0.001	0.000	-0.001
	[0.872]	[0.863]	[0.864]	[0.857]	[0.951]	[0.862]
Tender Offer	0.020***	0.019***	0.019***	0.019***	0.008	0.008
	[0.001]	[0.001]	[0.002]	[0.002]	[0.188]	[0.205]
Private Target	0.003	0.003	0.004	0.004	0.014*	0.015*
	[0.653]	[0.673]	[0.519]	[0.547]	[0.079]	[0.068]
Public Target	-0.036***	-0.036***	-0.034***	-0.035***	-0.001	-0.001
	[0.000]	[0.000]	[0.000]	[0.000]	[0.872]	[0.940]
Inverse Mills Ratio	No	No	Yes	Yes	No	No
Industry Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummy	Yes	Yes	Yes	Yes	Yes	Yes
N	2465	2465	2396	2396	616	616
R-squared	0.067	0.071	0.066	0.069	0.142	0.145

Table 2.7 Continued

Note: The acquisition sample consists of 2,465 completed U.S. mergers and acquisitions between 1999 and 2008. This table reports results of OLS regressions for acquirer cumulative abnormal returns. The dependent variable is the three-day cumulative abnormal returns for the acquirer. Definitions of the independent variables are in the Appendix A. All regressions control for calendar year-fixed effects and industry (Fama-French 48 industry) fixed effects whose coefficients are suppressed for brevity. P-values based on standard errors adjusted for firm clustering are reported in brackets. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.8 Supplemental Tests for Acquirer Cumulative Abnormal Returns

	[-1, +1] Acquirer CAR		
	(1)	(2)	(3)
IB Director(%)	0.039* [0.092]	0.046* [0.057]	0.008 [0.842]
IB Director(%) x Large Deal	0.092** [0.019]		0.090** [0.021]
IB Director(%) x Current		0.095** [0.016]	0.094** [0.018]
Acquirer Characteristics:	Yes	Yes	Yes
Deal Characteristics:	Yes	Yes	Yes
Industry dummy	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes
N	2,465	2,465	2,465
R-squared	0.076	0.071	0.079

Note: The acquisition sample consists of 2,465 completed U.S. mergers and acquisitions between 1999 and 2008. This table reports results of OLS regressions for acquirers with different characteristics. The dependent variable is the three-day cumulative abnormal returns for the acquirer. Acquirer Characteristics include acquirer board size, board independence, firm size, market-to-book ratio, leverage, and cash flow. Deal Characteristics include relative transaction value, whether the acquirer owns more than 5% of the target's stock prior to the announcement date, deal competition, method of payment, whether the acquisition is diversifying, whether the deal involves a tender offer, and target public status. All regressions control for calendar year-fixed effects and industry (Fama-French 48 industry) fixed effects whose coefficients are suppressed for brevity. P-values based on standard errors adjusted for firm clustering are reported in brackets. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.9
Regressions of Target Cumulative Abnormal Returns and Takeover Premium

	[-1, +1] Target CAR			PREM4WK from SDC		
	(1)	(2)	(3)	(4)	(5)	(6)
IB Director (%)	-0.023	0.166	0.198	-0.059	0.343	0.381
	[0.857]	[0.351]	[0.285]	[0.796]	[0.306]	[0.259]
IB Director (%) x Large Deal		-0.381**	-0.397**		-0.781**	-0.803**
		[0.039]	[0.033]		[0.024]	[0.029]
IB Director(%) x Current			-0.101			-0.168
			[0.531]			[0.466]
Acquirer Characteristics:						
Acquirer Size	0.012	0.011	0.010	0.007	0.006	0.006
	[0.201]	[0.244]	[0.264]	[0.627]	[0.732]	[0.841]
Acquirer Market-to-Book	0.003	0.002	0.002	0.011*	0.010	0.010
	[0.419]	[0.495]	[0.496]	[0.074]	[0.101]	[0.138]
Acquirer Leverage	-0.018	-0.012	-0.012	-0.126	-0.111	-0.121
	[0.774]	[0.847]	[0.849]	[0.271]	[0.328]	[0.295]
Acquirer Cash Flow	-0.046	-0.056	-0.055	-0.221	-0.195	-0.204
	[0.618]	[0.545]	[0.546]	[0.193]	[0.248]	[0.227]
Board Independence	-0.006	-0.004	-0.004	-0.009	-0.004	-0.005
	[0.839]	[0.890]	[0.890]	[0.884]	[0.952]	[0.931]
Board Size	-0.003	-0.002	-0.002	-0.009	-0.008	-0.008
	[0.531]	[0.583]	[0.583]	[0.137]	[0.173]	[0.156]
Acquisition Dummy	-0.031	-0.029	-0.029	-0.051	-0.055	-0.056
	[0.118]	[0.136]	[0.135]	[0.303]	[0.272]	[0.265]
Target Characteristics:						
Target Market-to-Book	-0.011	-0.011	-0.011	-0.009	-0.010	-0.009
	[0.116]	[0.107]	[0.108]	[0.388]	[0.368]	[0.410]
Target Leverage	-0.001	-0.006	-0.006	0.055	0.041	0.044
	[0.993]	[0.913]	[0.914]	[0.604]	[0.696]	[0.675]
Target Cash Flow	0.026	0.026	0.025	0.034	0.034	0.033
	[0.714]	[0.728]	[0.764]	[0.329]	[0.318]	[0.0331]
Deal Characteristics:						
Relative Transaction Value	-0.047***	-0.038**	-0.038**	-0.011	-0.032	-0.031
	[0.005]	[0.025]	[0.024]	[0.778]	[0.384]	[0.417]
Toehold	-0.074	-0.071	-0.071	-0.069	-0.061	-0.049
	[0.227]	[0.235]	[0.235]	[0.389]	[0.426]	[0.520]
Competition	-0.074**	-0.075**	-0.075**	-0.081	-0.078	-0.085
	[0.038]	[0.035]	[0.037]	[0.184]	[0.181]	[0.152]
Stock Deal	-0.009	-0.010	-0.010	0.019	0.018	0.018
	[0.716]	[0.711]	[0.711]	[0.689]	[0.695]	[0.700]
Cash Deal	0.099***	0.093***	0.093***	0.166***	0.151**	0.152**
	[0.003]	[0.001]	[0.001]	[0.005]	[0.012]	[0.012]
Related Deal	0.038*	0.041*	0.040*	0.019	0.026	0.024
	[0.081]	[0.062]	[0.062]	[0.569]	[0.450]	[0.477]
Tender Offer	0.065*	0.069*	0.069*	0.006	0.014	0.012
	[0.072]	[0.060]	[0.059]	[0.931]	[0.845]	[0.861]
Industry Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummy	Yes	Yes	Yes	Yes	Yes	Yes
N	843	843	843	843	843	843
R-squared	0.229	0.233	0.233	0.134	0.144	0.146

Table 2.9 Continued

Note: This table reports results of OLS regressions for target cumulative abnormal returns and takeover premium. In regression 1 – 3, the dependent variable is the three-day target cumulative abnormal returns. In regression 4 – 6, the dependent variable is PREM4WK from the SDC database. Definitions of the independent variables are in the Appendix A. All regressions control for calendar year-fixed effects and industry (Fama-French 48 industry) fixed effects whose coefficients are suppressed for brevity. P-values based on standard errors adjusted for firm clustering are reported in brackets. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.10 Regressions of Financial Advisory Fees Paid by Acquirer

	Log (Advisory Dollar Fees)		
	(1)	(2)	(3)
IB Director(%)	-1.350*	0.562	1.114
	[0.068]	[0.564]	[0.210]
IB Director(%) x Large Deal		-2.669**	-2.826**
		[0.026]	[0.017]
IB Director(%) x Current			-2.849**
			[0.019]
Acquirer Characteristics:			
Acquirer Size	0.096	0.139**	0.124*
	[0.106]	[0.049]	[0.080]
Acquirer Market-to-Book	-0.038	-0.033	-0.032
	[0.190]	[0.271]	[0.269]
Acquirer Leverage	-0.734*	-0.800**	-0.709*
	[0.059]	[0.039]	[0.065]
Acquirer Cash Flow	0.139	0.172	0.185
	[0.663]	[0.614]	[0.583]
Board Independence	0.093	0.102	0.126
	[0.647]	[0.608]	[0.529]
Board Size	0.024	0.019	0.024
	[0.173]	[0.254]	[0.178]
Acquisition Dummy	-0.259**	-0.243**	-0.236**
	[0.019]	[0.027]	[0.030]
Deal Characteristics:			
Log (Deal Value)	0.618***	0.578***	0.577***
	[0.000]	[0.000]	[0.000]
Toehold	0.005	-0.007	-0.021
	[0.987]	[0.981]	[0.944]
Competition	0.165	0.221	0.209
	[0.522]	[0.379]	[0.415]
Stock Deal	-0.091	-0.078	-0.059
	[0.479]	[0.546]	[0.649]
Cash Deal	-0.087	-0.036	-0.017
	[0.612]	[0.839]	[0.924]
Related Deal	0.074	0.069	0.089
	[0.537]	[0.551]	[0.447]
Tender Offer	0.092	-0.043	0.024
	[0.659]	[0.839]	[0.914]
Private Target	-0.424	-0.386	-0.483
	[0.288]	[0.316]	[0.210]
Public Target	0.013	0.038	-0.066
	[0.967]	[0.898]	[0.822]
Industry dummy	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes
N	211	211	211
R-squared	0.853	0.858	0.861

Table 2.10 Continued

Note: The table presents OLS regression results of financial advisory fees paid by acquirer. The dependent variable is the natural logarithm of dollar value of advisory fees paid by acquirer. Definitions of the independent variables are in the Appendix A. All regressions control for calendar year-fixed effects and industry fixed effects whose coefficients are suppressed for brevity. P-values based on standard errors adjusted for firm clustering are reported in brackets. ***, **, and * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2.11 Changes in Operating Performance

	IB Director Deals			Non-IB Director Deals		
	N	Raw Performance	Benchmark-Adjusted	N	Raw Performance	Benchmark-Adjusted
[-1, +1]	253	0.25	1.12*	423	-0.11	0.89
[-1, +2]	229	0.23*	1.10*	394	-0.82	0.63
[-1, +3]	187	0.44**	1.43**	350	-1.25	0.77
[-1, +4]	152	0.69**	2.08**	308	-0.44	1.38
[-1, +5]	123	1.16***	3.31***	265	-0.55	1.45

Note: This table reports the median changes in operating performance of combined firms for subsamples based on the presence of investment banker directors. Performance measure is based on earnings before the deduction of interest, tax and amortization expenses (EBITDA) scaled by sales. Changes in both raw performance and benchmark-adjusted performance are reported. To obtain benchmark firms, each acquirer and target firm is paired with a set of matching firms following the procedure of Barber and Lyon (1996), which involves selecting firms with the same 2-digit SIC code in year 0 and operating performance within 90% to 110% of the operating performance of sample firms in year -1. Benchmark-adjusted performance is the calculated as the difference between the performance of sample firm and the median performance of matching firms. Asterisks denote statistically significant differences between the two sub-samples at the 1% (***), 5% (**), or 10% (*) level based on Wilcoxon tests for differences in median.

Table 2.12 Buy-and-Hold Abnormal Returns

	Market-Adjusted Buy-and-Hold Return			Fama-French Adjusted Buy-and-Hold Return		
	Non-IB Director deal	IB Director Deal		Non-IB Director Deal	IB Director Deal	
Quarter	Median	Median	Difference	Median	Median	Difference
1	-0.018	0.008	**	-0.011	-0.005	
2	-0.035	0.019	**	-0.037	-0.016	*
3	-0.070	-0.020	***	-0.062	-0.024	*
4	-0.079	-0.025	***	-0.070	-0.040	*
5	-0.109	-0.030	***	-0.083	-0.039	**
6	-0.123	-0.058	***	-0.108	-0.064	**
7	-0.127	-0.045	***	-0.116	-0.059	**
8	-0.142	-0.049	***	-0.123	-0.067	**
9	-0.159	-0.060	***	-0.136	-0.069	***
10	-0.160	-0.060	***	-0.142	-0.070	***
11	-0.166	-0.087	***	-0.160	-0.084	***
12	-0.185	-0.104	***	-0.157	-0.086	***

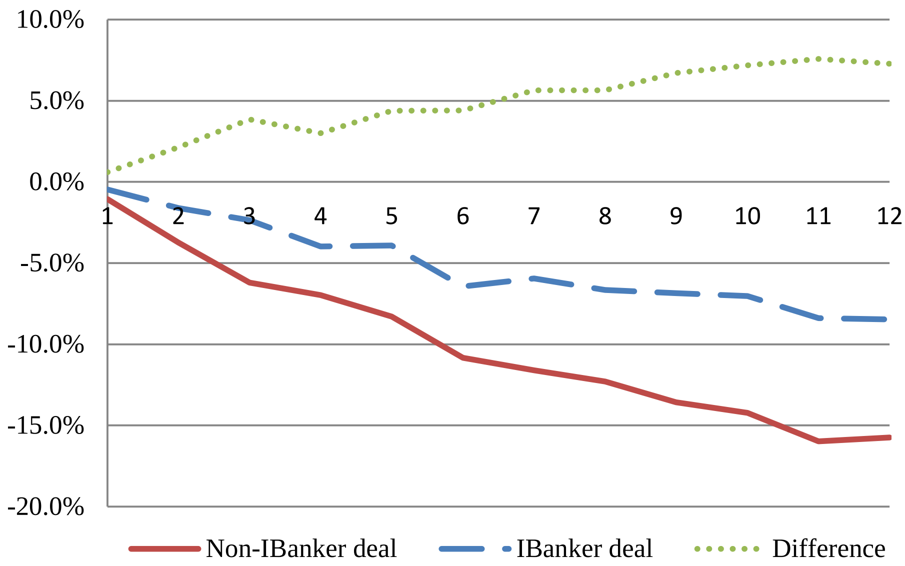
Note: This table reports the median buy-and-hold abnormal returns for subsamples based on the presence of investment banker directors. Starting on the day after the acquisition announcement date, a buy-and-hold return is calculated for the acquirer for up to 3 years (or 12 quarters) after the acquisition. Two buy-and-hold abnormal returns are reported: market-adjusted BHARs, where market returns (CRSP value-weighted index) are subtracted from the monthly raw returns before compounding, and Fama-French adjusted BHARs, where 5-year monthly returns leading up to merger events are regressed on the Fama-French three factors, and the estimated coefficients are then used to calculate the monthly abnormal returns before compounding. Asterisks denote statistically significant differences between the two sub-samples at the 1% (***), 5% (**), or 10% (*) level based on Wilcoxon tests for differences in median.

Table 2.13 Buy-and-Hold Abnormal Returns for Subsamples

		Fama-French Adjusted Buy-and-Hold Return		
	Year	Non-IB Director Deal Median	IB Director Deal Median	Difference
All	1	-0.070	-0.040	*
(N = 2,465)	2	-0.123	-0.067	**
	3	-0.157	-0.086	***
Large deal	1	-0.065	-0.026	**
(N = 1,232)	2	-0.174	-0.067	***
	3	-0.186	-0.085	**
Small deal	1	-0.076	-0.051	
(N = 1,233)	2	-0.090	-0.069	*
	3	-0.133	-0.088	**
Cash	1	-0.054	-0.034	
(N = 1,016)	2	-0.073	-0.048	*
	3	-0.097	-0.047	*
Stock	1	-0.072	-0.068	
(N = 600)	2	-0.147	-0.097	*
	3	-0.282	-0.062	**
Mixed	1	-0.103	-0.034	**
(N= 849)	2	-0.182	-0.104	**
	3	-0.221	-0.124	**

Note: This table reports the median buy-and-hold abnormal returns for different deal characteristics sorted subsamples, broken out by IB Director dummy. Starting on the day after the acquisition announcement date, a buy-and-hold return is calculated for the acquirer for up to 3 years after the acquisition. Fama-French adjusted BHARs are reported, where 5-year monthly returns leading up to acquisition events are regressed on the Fama-French three factors, and the estimated coefficients are then used to calculate the monthly abnormal returns before compounding. Asterisks denote statistically significant differences between the two sub-samples at the 1% (***), 5% (**), or 10% (*) level based on Wilcoxon tests for differences in median.

Figure 2.1 Fama-French Risk-Adjusted Buy-and-Hold Abnormal Returns



Note: This figure depicts the Fama-French risk-adjusted buy-and-hold abnormal returns (BHARs) over 36 months after the deal announcement. It first plots the median BHARs for subsamples based on the presence of investment banker directors, and then the differences between two subsamples. Five-year monthly returns leading up to the acquisition events are used to estimate the Fama-French three factor model. Returns are calculated using the estimated coefficients and then compounded monthly over the relevant period.

CHAPTER 3
THE STRATEGIC USE OF CEO COMPENSATION IN LABOR CONTRACT NEGOTIATIONS

> The United Auto Workers says it knows it needs to help Detroit's automakers cut labor costs to reduce the gap in production expenses with Asian rivals. But as talks continue on new contracts, the union also is questioning why top executives at the automakers are paid what they are.
>
> USA Today, October 10, 2007

3.1 Introduction

A growing literature examines how strategic considerations arising from bargaining between firms and their unionized workers affect corporate policy. Extant research suggests that firms often use high financial leverage, low cash holdings, and downward earnings manipulation to gain a bargaining advantage over labor unions. Bronars and Deere (1991) and Matsa (2010) provide evidence that firms use financial leverage to shelter income from labor unions' demands. Klasa, Maxwell, and Ortiz-Molina (2009) document a strong negative relation between industry unionization rate and corporate cash reserves, and that unionized firms decrease their cash holdings prior to labor contract negotiations. Finally, DeAngelo and DeAngelo (1991) report that firms manage their earnings downward prior to labor negotiations.

While financial policies that involve high financial leverage and low cash holdings might induce concessions from unions, they might also endanger the firms' competitive viability. We examine an alternate strategy of curbing CEO compensation leading up to negotiations with labor unions. This action might serve as a symbolic concession that engenders a willingness among rank-and-file employees to also sacrifice for the well-being of the company. Moreover, the decreased CEO compensation might be interpreted as a signal that executives view future prospects to be dismal, thus requiring everybody to show moderation. Consequently, we conjecture that unionized firms pay their executives less than do non-unionized firms, and that unionized firms temporarily curtail CEO compensation prior to union contract negotiations to strengthen their bargaining position.

The growing disparity between CEO compensation and average employee pay has been drawing media and political attention since the early-1990s (see, e.g., Murphy, 1999). Labor unions have been paying particular attention. For example, in 1997, the AFL-CIO launched a website focusing exclusively on "exorbitant pay schemes that have created unprecedented inequities in the American workplace" and described as a "working families' guide to monitoring and curtailing the excessive salaries, bonuses and perks in CEO compensation packages."[1]

Anecdotal evidence further suggests that top executives' compensation affects collective bargaining with unions. For instance, in 2007, just as American Airlines' (AA) contract with its flight attendants' union was about to expire, the president of the flight attendants' union expressed dissatisfaction with AA's intention to award millions in bonuses to executives. More recently, Ford angered its United Auto Workers (UAW) after granting more than $100 million in stock to its CEO, Alan Mulally, and Executive Chairman, Bill Ford. According to a fund manager, "The size of the stock awards could make it difficult for Ford to negotiate a new contract this year with the United Auto Workers union" (Bloomberg, March 8, 2011). UAW President Bob King called the awards "outrageous and unfair to the automaker's employees" which will "help the union

[1] Quotes are taken from the AFL-CIO website, http://afocio.paywatch.org/ceopay.

get a better deal at the bargaining table this year" as it will give UAW "more traction and more support from the membership to make sure they get a very substantial share in the upside" (Bloomberg, March 22, 2011). Five months later, when "union leaders discussed the equality of sacrifice grievance and its connection to contract talks at a meeting," Gary Walkowicz, a bargaining committeeman with UAW stated that "exorbitant and excessive pay has people upset and feeling they are owed something by the company" (Bloomberg, August 23, 2011).[2]

Using the industry unionization rate as a proxy for individual firms' unionization rate, we first study the relation between CEO compensation and the extent of unionization. Based on a sample of 5,398 CEO-year observations over the period 1993 through 2008, we document a strong negative relation between CEO compensation and unionization. We estimate that a one-standard deviation increase in the industry unionization rate is associated with roughly 20% decrease in total CEO compensation. One explanation for this relation is that firms in unionized industries strategically curtail CEO compensation to improve their bargaining position vis-à-vis unions. An alternative explanation is Jensen and Murphy's (1990) conjecture that labor unions affect contracts between managers and shareholders, including compensation contracts. A third explanation is that the observed relationship between compensation and unionization is spurious.

[2] While these examples show that unions scrutinize executive compensation in preparation for negotiations, the examples do not speak to whether executives strategically consider the union response to executive compensation. In fact, our examples show what happens when executives fail to behave strategically. If executives generally behave strategically, as we conjecture, executive compensation should be sufficiently modest not to cause any uproar among unions. In this sense, the lack of broad discontent among unions about executive compensation is consistent with our conjecture on strategic behavior.

To distinguish among these possibilities, we examine CEO compensation surrounding union contract expirations to gauge whether firms strategically curb CEO compensation to gain bargaining power leading into new negotiations with labor unions. Using a sample of 114 union contract negotiations that involve a non-trivial portion of the total labor force (ranging from 10% to more than 50%), we find pervasive evidence that firms curtail CEO compensation during the fiscal year prior to union negotiations. The CEO compensation curtailment prior to union contract negotiations is both statistically and economically significant in our univariate analysis. For instance, when at least 30% of employees are involved in union negotiations, we document that the median CEO total compensation decreases from $4.17 million to $2.90 million before the negotiations. Furthermore, our multivariate analysis establishes that these compensation trends cannot be explained by variation in firm performance alone. For example, firms having at least 30% of its employees involved in union contract talks are estimated to, *ceteris paribus*, decrease their CEO's total compensation by around 16% prior to the negotiations.

Option grants represent a key part of discretionary compensation to executives. In particular, the board often has significant leeway in both how many options to grant and when to grant these options. We examine individual option grants more closely because of their discretionary character and because we can pinpoint the dates of such grants, thus allowing us to more precisely specify the chronology of events and compensation trends surrounding union contract negotiations. Our analysis reveals that, for both company-reported and Black-Scholes option values, the value of option grants is significantly lower in the two quarters preceding union contract negotiations.

We also conjecture that CEOs are more likely to sacrifice compensation if they hold a relatively large stake in the company whose value depends on the contract negotiations. Consistent with this conjecture, we report a positive relation between CEO ownership and the likelihood of a CEO pay cut before labor contract negotiations.

Finally, we examine the effect of CEO compensation cuts on labor contract negotiations outcomes for a small sample for which we have sufficient information. We find that the average annual salary growth is about one percent less when negotiations are preceded by CEO compensation cuts. For the average company, this implies an annual saving of more than $70 million.

The most related prior work is that of DeAngelo and DeAngelo (1991). DeAngelo and DeAngelo use a sample of seven major steel firms from 1980 through 1988 to examine managerial compensation, financial reporting, and dividend policy during a period in which managers sought concessions from union members. They document a significant CEO salary and bonus cut prior to union negotiations. Our study differs from that of DeAngelo and DeAngelo in several respects. First, a large fraction of the negotiations that DeAngelo and DeAngelo study were opened early as a result of the looming problems in the steel industry. The financial difficulties of the firms presumably contributed to the CEO salary cuts, so it is not clear what role the union negotiations *per se* played. In contrast, the union negotiations in our sample are primarily scheduled, thus allowing us to study the strategic games leading up to negotiations that are not contaminated by financial difficulties. (Incidentally, our results are robust to the exclusion of financially distressed firms from the sample.) Second, while DeAngelo and DeAngelo focus on base salary and bonus, we also examine option grants. This is critical because these grants are arguably the most discretionary component of compensation in recent years in terms of both magnitude and timing. Third, our much larger and more general sample allows us to conduct a more systematic and generalized study of the strategic role of CEO compensation in collective bargaining, including the effect of CEO holdings on such compensation strategies. Fourth, unlike DeAngelo and DeAngelo, we examine the effect of executive compensation cuts on union negotiation outcomes.

The remainder of the paper is organized as follows. Section 2 discusses related literature and our empirical approach. Section 3 describes the data and sample selection. Section 4 provides empirical results. Section 5 concludes.

3.2 Related Literature and Empirical Approach

3.2.1 Theory

Most strategic models on collective bargaining between firms and unions consider a general two-party situation (bilateral monopoly) in which a union tries to maximize the utility of its members, and a firm attempts to maximize its value.[3] The desire of a particular party to concede or hold out depends on both objective factors (e.g., the state of product demand and capital-labor substitution) and subjective factors (e.g., the assessment of the bargaining strategy of the other party). These models differ in a number of ways, including their specification of: (1) the precise nature of the unions' objectives; (2) the information sets of each of the parties; (3) whether firms and unions negotiate over wages, employment, or both; (4) whether the bargaining is static or dynamic; and (5)

[3] Klasa et al. (2009) present a review of bargaining theory between firms and labor unions.

how the disagreements are resolved in practice.[4] Because the failure to agree on a settlement prior to a contract's expiration is costly to the firm (loss of profits) and workers (loss of wages), most collective bargaining models embed a tendency for the parties to come to an agreement in time to avert a strike. Moreover, regardless of specific modeling details, a party's ability to increase its share of firm surplus is directly related to its bargaining power.

3.2.2 Empirical Evidence

Labor costs usually represent a large fraction of firms' total costs. Consequently, firms have the incentive to take strategic actions to improve their bargaining position against unionized workers. Prior empirical work shows that unionized firms use high financial leverage (Bronars and Deere (1991), Hanka (1998), and Matsa (2010)), low cash holdings (Klasa et al. (2009)), downwardly managed earnings (DeAngelo and DeAngelo (1991), and D'Souza, Jacob, and Ramesh (2001)), and pension underfunding (Benmelech, Bergman, and Enriquez (2009)) to gain bargaining power against unionized workers.

DeAngelo and DeAngelo (1991) provide evidence from the restructuring of the steel industry during the period 1980 to 1988 using a sample of seven firms. Firms often try to make the case that the firm's competitive viability is threatened by current

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[4] In traditional static models, such as those in Leontief (1946) and McDonald and Solow (1981), unions have control over wages, while firms choose employment levels. Earle and Pencavel (1990) model the bargaining over hours of work, whereas both Clark (1990) and Johnson (1990) model the bargaining over work rules. Other models rely on the Nash (1950) bargaining solution where each party receives its payoff in case of disagreement plus a fraction of the joint surplus that is increasing in the party's bargaining power. Sequential bargaining models based on Rubinstein (1982) incorporate the cost of delays in reaching agreements. For instance, in war of attrition models (e.g., Fudenberg and Tirole, 1986), a party's ability to endure strikes endows it with a strong bargaining position.

economic conditions in order to gain concessions from unions. Their findings suggests that prior to union contract negotiations, firms manage their earnings downward in order to help their case for union concessions. Specifically, they show that unionized firms report lower net income during negotiation than non-negotiation years. This difference is attributable to managers using their discretion to report one-time special charges during negotiation years. Similarly, D'Souza et al. (2001) find that firms actively manage earnings to improve their bargaining position in union negotiations. In particular, they document that prior to reducing retirement benefits, unionized firms are likely to adopt new accounting standards that allow them to reduce current net income.

Bronars and Deere (1991), Hanka (1998), and Matsa (2010) suggest that firms can also improve their bargaining power against unions by increasing financial leverage. By committing themselves to repaying a larger portion of future cash flows to creditors, firms set a ceiling on the revenues that labor can extract without triggering bankruptcy. Consistent with this argument, Bronars and Deere (1991) show that more unionized industries hold more debt than do less unionized industries. However, this result might be driven by an omitted variables bias in which industries with high unionization rates also have higher debt capacity for reasons unrelated to bargaining between the firm and union. Matsa (2010) alleviates this concern in his research design by exploiting exogenous variation in state-level labor laws. He reports that when states adopt legislation that reduces union bargaining power, firms with concentrated labor markets reduce debt relative to otherwise similar firms in other states. Furthermore, Hanka (1998) reports that higher debt results in decreased labor costs. More specifically, he shows that high debt is associated with more frequent employment reductions, greater reliance on part time and seasonal employees, low wages, and reduced pension funding.

Klasa et al. (2009) show that corporate cash holdings are negatively related to industry unionization rates. This suggests that unionized firms strategically hold small cash reserves so as to shelter corporate income from the demands of labor unions. They also find evidence that unionized firms manage their cash reserves downward prior to union contract negotiations, but they do not find any evidence that firms increase their debt levels prior to these negotiations. Their results indicate that while firms can use both permanently higher debt levels and lower cash balances to improve their bargaining position, they are more likely to temporarily manipulate cash levels than debt levels immediately before a negotiation, perhaps because of the relative ease with which firms can reduce cash levels.

3.2.3 CEO Compensation

Our study also relates to a line of research that examines how politics and external pressure influence the practice of CEO compensation. Jensen and Murphy (1990) emphasize that CEO compensation contracts are “not a private matter between a principal and an agent.” They contend that “third parties such as rank-and-file employees, labor unions, consumer groups, Congress, and the media affect the type of contracts written between management and shareholders.”

There is ample evidence that politics and public perception play an important role in determining the structure and level of executive compensation. DeAngelo and DeAngelo (1991) document that CEOs in the US steel industry in the 1980s received lower cash compensation in union-negotiation years than in other years, and interpret the lower compensation as representing "symbolic sacrifices that encourage all stakeholders to participate in the concessions needed to salvage the firm." Joskow, Rose, and Wolfram (1996) analyze the relation between CEO pay and firm characteristics in the electric utility industry, and conclude that political pressures constrain CEO pay levels in that industry. Murphy (1996) finds that managers adopt disclosure methodologies with reduced reported or perceived compensation. He suggests that this supports the hypothesis that managers bear non-pecuniary costs from high reported levels of compensation. Dial and Murphy (1995) document the pressures on pay at General Dynamics, leading the company to replace a controversial bonus plan with conventional stock options. Rose and Wolfram (1997) and Perry and Zenner (2001) analyze the impact of the $1 million "cap" on deductibility of non-performance pay. They find that while companies subject to the cap have reduced relative levels of base salaries, they have increased relative levels of stock options and other performance-related pay. Core, Guay, and Larcker (2008) use more than 11,000 press articles about CEO compensation to study the press' role in monitoring and influencing executive compensation practice. They document that negative press coverage is more strongly related to excess annual pay than to raw annual pay, and that negative coverage is greater for CEOs with more option exercises, which suggests that "the press engages in some degree of sensationalism." However, they find little evidence that firms respond to negative press coverage by decreasing excess CEO compensation or increasing CEO turnover.

3.3 Data

3.3.1 Sample

We study the population of firms covered by ExecuComp during the 1993-2008 period. Following Klasa et al. (2009) and Matsa (2010), we focus on the manufacturing sector, because labor costs represent a significant fraction of total costs in this sector. We require the sample firms to have available information on key variables used in our analysis, including CEO compensation and industry unionization rates. Our data requirements yield an initial sample of 5,398 firm-year observations from 1993 to 2008.

3.3.2 Industry Unionization Data

A union's bargaining power is highly correlated with the fraction of unionized employees in that firm. Thus, labor economists often use unionization rates as a proxy for union bargaining power. However, it is difficult to obtain reliable information on unionization rates at the firm-level because firms are not required to provide union membership information about their workers in their public filings. Consequently, most previous studies on labor unions make the assumption that industry unionization rates are a reasonable proxy for the unionization rates of individual firms within an industry, and therefore use industry-level data on unionization rates as a measure for union bargaining power at the firm level.[5] We do the same in our analysis.

Following Klasa et al. (2009), we collected data on annual industry unionization rates for the 1993-2008 period from the Union Membership and Coverage Database

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[5] Rosen (1969), Karier (1985), Connolly, Hirsch, and Hirschey (1986), and Bronars and Deere (1991), among others, use industry unionization rates provided by Bureau of Labor Statistics as a proxy for union bargaining power.

maintained by Barry Hirsch and David Macpherson, which is publicly available at www.unionstats.com. This database reports industry unionization rates for three-digit Census Industry Classification (CIC) industries. The unionization rates represent the percentage of total workers in a CIC industry that are represented by unions in collecting bargaining agreements. Our unionization data span 77 three-digit CIC industries in the manufacturing sector with CIC codes between 100 and 392.

Like Klasa et al. (2009), we find that there is a large cross-sectional variation in the unionization rates across industries, as shown in Figure 3.1, Panel A. Mean industry unionization rates over our sample period range from 2.95% (electronic computing equipment) to 47.98% (pulp, paper, and paperboard mills). Pulp, paper, and paperboard mills, blast furnaces, steelworks, rolling and finishing mills, motor vehicles and motor vehicle equipment, leather tanning and finishing are the most unionized industries, with mean unionization rates above 40% over the 1983-2008 periods. On the other end of the spectrum, 13 three-digit CIC industries have mean unionization rates under 10%. Figure 3.1, Panel B, further reveals that industry unionization rates exhibit a decreasing temporal trend. Specifically, mean unionization rates decreased from 31.7% in 1983 to only 12.8% in 2008.

3.3.3 Labor Contract Negotiations

We obtained data on labor contract expirations from the BNA Labor Plus database maintained by the Bureau of National Affairs (BNA). The BNA have collected data on union contract expirations from 1990 onwards from notices that parties to collective bargaining agreements up for renewal are required to file with the Federal

Mediation & Conciliation Service.[6] We also collect information on contract settlements from the BNA Labor Plus database maintained by the BNA. BNA PLUS collects information on contract settlements reported through newspapers, union publications, and direct reports to BNA. Wage and benefit changes negotiated under collective bargaining agreements are summarized, along with basic information about the contracts.[7]

We include collective bargaining agreements involving 500 workers or more. In comparison, Klasa et al. (2009) include contract negotiations that cover at least 1,000 workers. We choose a lower break point than do Klasa et al., because there might have been multiple contract negotiations in a given year that individually involve less than 1,000 workers but aggregate to more than 1,000 employees. For example, in 2007 BAE Systems negotiated two labor contracts involving 747 and 700 employees, respectively.

To identify years during which major contract negotiations took place, we first compute a "spike ratio," defined as the ratio of the number of employees involved in all contract negotiations for a firm in a given fiscal year to total employees in the firm. A contract negotiation year is defined as a year with a spike ratio that is at least 10% *and* at least twice as high as the ratios for the two preceding years. We also analyze smaller samples of contract negotiation years in which the spike ratio exceeds 20%, 30%, 40%, or 50%.

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[6] The database includes both contentious and non-contentious negotiations. According to industry insiders with whom we have communicated, only a small minority (less than 5%) of negotiations are not filed because a new contract is agreed upon more than 30 days before the previous one expires.

[7] A typical union contract includes employee salary growth rate during the term of the contract and health and pension benefits. We focus our analysis on the salary growth rate since health and pension benefits are not readily quantifiable and/or converted into dollar figures, making it difficult to compare contract outcomes over time.

3.4 Empirical Results.

3.4.1 Unionization and CEO Compensation

Table 3.1 reports summary statistics (Panel A) and results of univariate tests of the relation between industry unionization and CEO pay (Panel B). Two measures of CEO compensation are used: (1) salary plus bonus and (2) total compensation, defined as the sum of base salary, bonus, long-term incentive payouts, the value of restricted stock grants, and the value of option grants.[8] Considering "the best-documented stylized fact regarding CEO pay: CEO pay is higher in larger firms" (Murphy 1999), we scale both measures by total assets. Panel A shows that over our sample period, mean salary plus bonus scaled by total assets and mean total compensation scaled by total assets are 0.19% and 0.51%, respectively. The discrepancy between the means demonstrates the importance of including information on option grants in our analysis.

Panel B presents the results of our univariate tests on these relationships between industry unionization rates and CEO pay. For this analysis, we first sort firms into quartiles according to their annual industry's unionization rates, and then compute mean and median compensation values for each quartile. Both the mean and median for the two measures of CEO pay decrease monotonically from the first to the fourth unionization quartile. The differences in CEO pay between the bottom and top quartiles are economically and statistically significant, with compensation in the bottom quartile being at least double that in the top quartile. Overall, the univariate results suggest that CEO

[8] Our compensation measures fail to capture various perks, such as executive loans, which might not be observable and the value of which is opaque. In general, opaque compensation is particularly suited for unionized firms, and it is conceivable that unionized firms rely more heavily on opaque compensation leading up to union negotiations. In this sense, our study suffers some inherent shortcomings.

compensation is decreasing in firm unionization rate. However, differences in industry unionization rates are likely to be associated with differences in firm characteristics. Consequently, we turn to multivariate tests, which allow us to further explore the relationship between CEO pay and unionization rates while accounting for these firm-level differences.

Table 3.2 provides the results of regressions of CEO pay on industry unionization rates and control variables. CEO pay is measured as the natural logarithm (Log) of our measures of CEO compensation. The main independent variable of interest is the unionization rate in a firm's three-digit CIC industry. We control for other economic determinants of CEO pay as suggested by prior research in this area (e.g., Smith and Watts, 1992; Core, Holthausen and Larcker, 1999; Murphy, 1999; and Core, Guay and Larcker, 2008), including firm size, growth opportunities, stock return, and accounting return. For comparability purposes, all variables have been adjusted to 2009 constant dollars.

The first two models in Table 3.2 are pooled regressions with alternative measures of CEO pay as dependent variables. The third and fourth models are Fama-Macbeth regressions. Fixed effects for years and two-digit SIC codes are included in all four regressions. Because the primary source of variation in unionization rates is across industries, we also remove the fixed effect for two-digit SIC codes from models 1 through 4 and report these results in models 5 through 8.

The results from all models show that there is a negative relation between CEO pay and industry unionization rates. The relation is statistically significant in all cases except the specification that employs Fama-Macbeth regressions with no year fixed effects and salary plus bonus as the dependent variable[9]. This might have to do with the downward slope in the time series industry unionization rates discussed earlier and shown in Figure 3.1, Panel B. In particular, the significance is muted by coefficients estimated from the years 2004 to 2008, when both the level and cross sectional variation in unionization rates were at their lowest.

The coefficients on the control variables are generally in line with extant research and have the expected signs. For example, both measures of compensation are positively related to firm size, growth opportunities (as proxied by the market-to-book ratio), and stock returns.

The impact of unionization on CEO pay is also economically significant. For a one standard deviation increase in unionization, the salary plus bonus component is reduced by 4.78% and total compensation is reduced by 19.70%. For comparison, we estimate that a one standard deviation increase in contemporaneous and lagged annual stock returns increases total compensation by 14.25% and 10.48%, respectively. Thus, the economic impact of unionization on CEO pay seems comparable to that of several well known determinants of CEO pay (Jensen and Murphy, 1990; Joskow and Rose, 1994; and Boschen and Smith, 1995).

Overall, the evidence in Tables 3.1 and 3.2 is consistent with the notion that firms in more unionized industries strategically curtail their CEO pay to improve their bargaining position against unions. However, the results are also consistent with the related possibility that labor unions condemn the pay disparity between top executives

[9] Our results are robust if the standard errors of the coefficients are adjusted for the clustering of observations at the industry level.

and average workers, thereby constraining CEO pay in more unionized industries. This is, in turn, consistent with Jensen and Murphy's (1990) broader conjecture that politics and external constituencies play an important role in monitoring and influencing the level and structure of CEO pay. A last possibility is that the relation is simply spurious, that is, unidentified factors increase unionization and decrease compensation in certain industries. To disentangle these possibilities, we now turn to a more direct analysis of CEO compensation during the period surrounding contract negotiations.

3.4.2 CEO Compensation around Contract Negotiations

Generally On the one hand, if unionized firms strategically use CEO compensation to gain a bargaining advantage over labor unions, we expect that firms temporarily curtail CEO compensation leading up to union contract negotiations. On the other hand, if either firms merely appease union demands for less pay disparity or the relationship between compensation and unionization is spurious, we expect compensation in unionized firms to be low in all years, irrespective of whether negotiations are imminent.

We begin our analysis by examining firm characteristics surrounding labor contract negotiations. Table 3.3 reports descriptive statistics of firm characteristics for years -2 to +2 relative to the fiscal year in which the negotiation takes place. We focus on a spike ratio of 10% in this table. We examine the following economic determinants of CEO pay: firm size, growth opportunities, stock return, and accounting return. Except for the annual stock return, the results show that there are no significant reductions in these determinants of CEO pay from year -2 to year 0. Moreover, consistent with Klasa et al. (2009), our evidence suggests both that firms decrease their cash holdings during the fiscal year preceding labor contract negotiations and that unionized firms generally hold less cash than non-unionized firms. The mean (median) ratio of cash and short-term investment to total assets is roughly 6% (3%), compared to a mean (median) of 22% (11%) for all manufacturing firms in Compustat during 1993-2008. The trend of financial leverage prior to union contract negotiations is also consistent with Klasa et al. (2009). In particular, we find no evidence that firms increase financial leverage prior to union negotiations.

Next, we employ univariate tests in to determine whether unionized firms temporarily manage CEO compensation downward prior to labor contract negotiations. More specifically, we examine median CEO compensation from year -2 to year +2, relative to the negotiation fiscal year spike, based on the following: (1) unadjusted compensation, (2) industry-adjusted compensation; (3) relative compensation, defined as compensation in the year of interest scaled by the level of compensation in year -2; and (4) relative compensation for the sample firms versus relative industry-adjusted compensation. Table 3.4 reports the results of our analysis. Panels A through E correspond to different spike ratio criteria ranging from 10% to 50%. Once again, we adjust the annual CEO compensation to 2009 constant dollars.

The evidence based on the raw CEO compensation surrounding union contract negotiation spikes is consistent with our hypothesis that unionized firms temporarily curtail CEO compensation leading up to union contract negotiations. The median CEO total compensation falls prior to contract negotiations, and the magnitude of the drop is increasing in the spike ratio.

Compensation might change over time as a result of a steady trend. Hence, we also analyze whether the change in CEO compensation leading up to the contract negotiations is different from that observed in the other three years (years 0, +1, and +2). We find that the change in annual CEO compensation in year -1 is significantly lower than that in the other three years at the 0.10 significance level for the spike ratio of 10% and at the 0.05 level for higher spike ratios.

A remaining concern is that the documented compensation pattern could be the result of industry-level trends, independent of any strategic use of CEO compensation in collective bargaining. Thus, we examine industry-adjusted median CEO compensation around union negotiations. We define industry-adjusted CEO compensation as CEO compensation minus the median value of CEO compensation for other ExecuComp firms sharing the same primary four-digit SIC code.[10] The control firms we use to calculate the median industry CEO compensation exclude firms that face labor contract negotiations in the same year as the original sample firms. The results in Table 3.4 reveal that the industry-adjusted compensation is generally positive, presumably because our firms are relatively large. More importantly, the industry-adjusted compensation trends downward prior to negotiations and upward after negotiations. This suggests that our results based on raw compensation are not driven by industry-wide compensation trends.

Our analysis thus far has focused on unscaled compensation, which naturally places more weight on the firms in our sample with the highest compensation levels. To place similar weight on all observations in our sample, we also examine annual CEO

[10] We use a firm's four-digit SIC code if there are at least five firms in the firm's four-digit SIC code for all five years around the negotiation. If this criterion is not met, we try to use the firm's three-digit SIC code. As a last resort, we use the two-digit code.

compensation scaled by the level of CEO compensation in year -2. We find that the growth rate in CEO compensation during the year prior to labor contract negotiations is lower than the growth rate in other years. For example, the results based on a spike ratio of 10% show that total CEO compensation increases by only one percent in year -1, compared to more than six percent for each of the other three years.

Finally, to rule out the possibility that overall industry trends contribute to the relative compensation pattern leading up to the negotiation, we compare the relative compensation (i.e., annual compensation relative to compensation in year -2) of the sample firms to the relative compensation of industry peers for observations for which we can identify suitable industry peers. The results for our slightly smaller sample of firms in labor negotiations are confirmatory. That is, the relative compensation growth decelerates significantly prior to the negotiation years. More importantly, the industry control sample does not exhibit the same deceleration in compensation growth. On this basis, we can conclude that the compensation pattern in relative compensation around labor negotiations is unique to the firms that are involved in union contract negotiations.

Overall, the univariate results in Table 3.4 strongly suggest that unionized firms temporarily curtail CEO compensation prior to union contract negotiations. However, other firm characteristics might also play a role in determining CEO pay surrounding labor contract negotiations, so we turn to multivariate tests for more robustness tests.

3.4.3 Multivariate Analysis

To control for other determinants of compensation, we regress compensation levels and growth rates during the years around union negotiations (years -2 to +2 relative to the fiscal year in which the negotiation takes place) on an indicator variable that takes a value of one for year -1 ("prior-year dummy") as well as various control variables. Table 3.5 provides the results from these regressions.

We first examine the determinants of compensation levels, and report the results in Panel A. The dependent variable is the natural logarithm (log) of annual CEO compensation (in millions). As before, we examine two measures of CEO compensation: salary plus bonus and total compensation. The primary independent variable of interest is the prior-year dummy. We control for some commonly used variables from the CEO compensation literature, including the natural logarithm of sales, annual stock return, return on assets, and CEO tenure.[11] We also include year and industry dummy variables. Estimated standard errors in all regressions are corrected for clustering at the firm level.

The coefficient on the prior-year dummies is consistently negative across the regression models, and statistically significant at the ten (one) percent level in seven (four) of the ten models. Furthermore, the absolute magnitude of the coefficient is consistently larger when we use total compensation instead of just salary plus bonus. It is also generally increasing in the spike ratio. The results are economically significant. For example, for firms with at least 30% unionized employees in contract negotiations, we estimate that the total compensation in year -1 is 16% lower than it is in other years, while the salary and bonus is 8% lower in year -1.

Next, following DeAngelo and DeAngelo (1991), we examine the determinants of annual growth rate in CEO compensation surrounding union contract negotiations, and report the results in Panel B. The variables are otherwise the same as those used in Panel A. The coefficient on the prior year dummy shows that, *ceteris paribus*, compensation

[11] Core, Holthausen and Larcker (1999) and Murphy and Sandino (2008) use similar specifications.

growth is consistently lower in the year leading up to the negotiations than it is in other years. These relations are statistically significant at levels ranging from 0.01 to 0.10, depending on the specification. Moreover, consistent with our earlier findings, the results are most pronounced when we focus on total compensation and/or higher spike ratios. Overall, our results suggest that firms curtail executive compensation, especially total compensation (which includes option grants), leading up to union contract negotiations.[12]

3.4.4 CEO Option Grants surrounding Union Negotiations

Option grants represent a key component of discretionary compensation to executives. In particular, a firm's board of directors often has significant leeway in determining both how many options to grant and when to grant these options. Consistent with this discretionary view of option grants, as noted above, the evidence of CEO compensation curtailment prior to union negotiations is more pronounced when the compensation measure includes the value of option grants. Furthermore, we can pinpoint the dates of option grants, thus allowing us to make a finer time grid of compensation around union contract negotiations. Consequently, we examine individual option grants more closely.

We study option grant activities on a quarterly basis. Using even shorter periods than quarters would be feasible, but the downside is that there would be fewer option grants in each period, making the pattern in option grant values more erratic. Because our earlier sample was designed with annual data in mind, we generate a somewhat different sample for our option grant analysis. Specifically, instead of annualizing the contract

[12] Our results are similar if we include industry median CEO compensation or industry median CEO compensation growth in multivariate analysis.

negotiations, we study the individual contract negotiations that involve a significant portion of the labor force of the firm, where a significant portion is defined as 10%, 15%, 20%, or 25%. Because a number of firms have multiple unions (with different contract negotiation dates), we further require that contract negotiations be preceded by at least two years during which no other labor contracts expired.

We employ two measures of option grant values: (1) the Black-Sholes values from Execucomp, and (2) those reported by the company. One might argue that the evidence from the value of option grants reported by the company would be more pertinent here to the extent that they are more readily observable to the public and the unions, but the Black-Scholes values are arguably the better estimates of the true values of the options. A concern might be that systematic changes in the underlying stock values around negotiations could bias these values. However, because the options are granted at the money, and the two measures of option values are based on the combination of the exercise price and the market price on the grant date (along with assumptions about volatility, etc.), we believe that this bias is small. For example, if the stock price were to rise after a successful labor contract negotiation, the Black-Scholes value of grants prior to the negotiations would be unaffected, and so would largely the Black-Scholes value of grants afterward because both the exercise price and the market price would be correspondingly higher.

Table 3.6 reports the results of our option grant analysis. For each observation in our sample, we aggregate the value of option grants in each quarter and then scale this value by the total value of option grants in all eight quarters to obtain a relative value of the grants in each quarter. Panels A through D correspond to varying portions of the labor force involved in the negotiations, ranging from 10% to 25%, as noted above.

The results indicate that option grant values in the two quarters prior to contract expirations are significantly lower than those in other quarters. For example, Panel A, which is based on negotiations involving at least 10% of the labor force, shows median fractional values of less than 44% in quarters -2 and -1 (where quarter -1 is the quarter of the negotiation), compared to 49% or more in each of the other six quarters. The values in quarters -2 and -1 are statistically different from those in other quarters at the 0.01 and 0.05 levels, respectively.

Figure 3.2 depicts the option grant values reported by the company over an eight-quarter period surrounding union contract negotiations. The figure reveals a visual trough in option grant values in quarter -2 across all subsamples. This supports our earlier findings that CEOs are given less valuable option grants in the period immediately prior to union negotiations.

3.4.5 CEO Ownership and Excessive Compensation

We noted earlier that strategic models on collective bargaining generally assume that executives seek to maximize firm value. This explains why CEOs would accept a pay cut before collective bargaining. If CEOs instead seek to maximize their personal utility, they would be more willing to accept a pay cut when their interests are closely aligned with those of the shareholders. Thus, we conjecture that the probability of a strategic pay cut increases with CEO ownership. We also conjecture that a cut is more likely if past compensation seems to have been excessive, because unions and the public are be particularly outraged by such excess. To test these conjectures, we regress the probability of a pay cut before collective bargaining against CEO ownership in the firms, a measure for CEO excessive compensation, and control variables. Our measure for excessive compensation is the ratio of actual compensation minus expected compensation to expected compensation, where expected CEO compensation is obtained from an OLS regression of the natural logarithm of compensation on natural logarithm of lag sales, annual stock return, lag annual stock return, return on assets, lag return on assets, lag book to market, natural logarithm of CEO tenure, and industry controls.

The signs on the regression coefficients, which are reported in Table 3.7, are as we expected. Regardless of whether we analyze only salary and bonus or total compensation, the effect of CEO ownership on the likelihood of a pay cut is positive. Further, a pay cut is more likely if CEO compensation is excessive, although this relation is only statistically significant when we analyze total compensation. These results reinforce our general conclusion that the executive pay trends around union negotiations are designed to improve the firms' bargaining situation.

3.4.6 The Effect on Labor Contract Negotiation Outcomes

Our analysis thus far suggests that firms strategically cut CEO compensation before negotiations with unions. A natural follow-up question is whether this strategy affects the outcome of the negotiations. This is a hard to answer based on empirical data for at least three reasons. First, we do not know what the contract outcomes would have been in the absence of the preceding CEO compensation cuts. We could, and we will, assume that the terms of the contracts would otherwise evolve similarly in cases with and without CEO compensation cuts. However, we recognize that this could introduce bias. For example, firms might resort to strategic CEO compensation cuts when the union pressure for improved employment terms is particularly great. This would bias against finding evidence that CEO compensation cuts temper employee salary growth. Second, there are many dimensions to a union contract, many of which are not readily quantifiable and/or converted into dollar figures, making it difficult to compare contract outcomes over time. Third, we do not always have information about the outcome of the negotiations, which could limit the generalization and statistical power of the results.

For our sample, we identified 28 cases for which we have information about employee salary in the negotiated contract. In panel A of table 3.8, we report descriptive statistics for annual salary growth in contracts preceded by CEO compensation cuts versus other contracts. The mean (median) salary growth rate in the labor contracts when the negotiations are preceded by a cut in total CEO compensation is 1.62% (1.83), compared to 2.53% (2.87%) when there is no prior compensation cut. The difference in means of 0.91% is statistically different at the 0.05 level, and the difference in medians of 1.04% is statistically different at the 0.10 level. These differences are also economically meaningful. Assuming that the CEO compensation cut curbs salary growth by 0.90%, the average number of employees is 135,000, and the average salary is $60,000, the average firm will save $72.9 million annually.

In panel B, we report results from a multivariate analysis in which we also attempt to control for the overall economic conditions of the firms. This analysis suggests that the univariate differences in salary growth rate of about 1% between contracts preceded by CEO compensation cuts and others are robust. If anything, the difference is somewhat larger, as suggested by the coefficient on the compensation cut dummy of -1.1%.

3.4.7 Additional Robustness Checks

DeAngelo and DeAngelo (1991) report executive pay cuts before union negotiations for a small set of distressed firms. To test whether the effect we have documented here is a general phenomenon that is not driven by labor negotiations of financially distressed firms, we redo our analysis excluding financially distressed firms. In doing so, we define firms to be financially distressed if they their Altman Z-score is below 1.8 before labor contract negotiations. The results, which are not tabulated, are similar to those reported here for the full sample, suggesting that we are not merely documenting an effect that is specific to negotiations during distress.

We also examine compensation for executives other than the CEO. We conjecture that firms will curtail not only CEO compensation, but also that of other top executives before important negotiations. Thus, we examine the annual compensation to the five highest-paid executives during the period surrounding union contract negotiations. Untabulated results indicate that top executives as a group experience temporary compensation curtailment prior to union contract negotiations.

As a final robustness check, we re-run our analysis on the subsample for which there is no CEO replacement during the five–year period under study. The results, yet again not tabulated, are qualitatively similar to the results for the larger sample.

3.5 Conclusion

Extant evidence suggests that firms adopt corporate policies that strengthen their bargaining position vis-à-vis labor unions. In particular, firms might inflate financial leverage, lower cash reserves, and even manipulate earnings in an attempt to dodge union demands. We extend this literature by examining whether firms also strategically manipulate CEO compensation as part of their negotiation efforts. Unions might interpret the current level of CEO compensation as the willingness of executives to sacrifice for the good of the firm and/or an indicator of the firm's expected future financial performance. If so, firms have the incentive to curb CEO compensation in the presence of strong unions, especially in anticipation of important negotiations with such unions.

We find that unionized firms curtail CEO compensation in the fiscal year preceding union contract negotiations, which is consistent with our hypothesis that firms strategically use CEO compensation to improve their bargaining position with unionized workers. The curtailments in compensation are both statistically and economically significant and cannot be explained by variation in firms' performance or time trends. We also document a temporary drop in the value of option grants during the two quarters before union contract negotiations. Finally, we find some evidence that curtailing CEO compensation serves to slow down the negotiated salary growth.

Our study adds to the understanding of how strategic considerations arising from collective bargaining between a firm and its labor unions affect corporate policy. In addition to manipulating actual financial flexibility, which might threaten a firm's viability, firms manipulate CEO compensation to improve their bargaining position with unions. In this sense, executive compensation is not only used to incentivize executives, but also to create goodwill among constituencies and/or signal firm prospects to less informed parties.

Table 3.1 Descriptive Statistics of CEO Compensation and Univariate Tests

Panel A: Descriptive statistics for CEO compensation

	Mean	Stdev	Q1	Median	Q3
Salary plus bonus	0.190	0.425	0.039	0.097	0.202
Total compensation	0.513	1.099	0.108	0.234	0.505

Panel B: Compensation for industry unionization rate based subsamples

		Unionization Quartiles					
		Q1	Q2	Q3	Q4	Q4-Q1	p-value
Salary plus bonus	Mean	0.270	0.192	0.159	0.094	-0.176	0.000
	Median	0.122	0.113	0.104	0.056	-0.069	0.000
Total compensation	Mean	0.730	0.548	0.412	0.196	-0.534	0.000
	Median	0.324	0.290	0.235	0.125	-0.199	0.000

Note: The sample consists of firm-year observations for ExecuComp CEOs during the 1993-2008 period with non-missing data for unionization and the main control variables specified in Table 3.2. The industry unionization rate is the fraction of an industry's workers that are represented by labor unions in the collective bargaining with the firm, where each industry corresponds to a 3-digit Census Industry Classification (CIC). Our data span 77 different CIC industries. Two measures of CEO compensation are used. Salary plus bonus is the sum of base salary and bonus scaled by total assets. Total compensation is the CEO's total compensation (*TDC1* in the ExecuComp database), defined as the sum of base salary, bonus, long-term incentive payouts, the value of restricted stock grants, and the value of option grants, scaled by total assets. Panel A reports summary statistics for salary plus bonus and total compensation. In Panel B, during each year we sort firms into quartiles according to their industry's unionization rate, and then compute the mean (median) values of CEO compensation for each of the unionization rate-based subsamples. The last column of the table reports p-values for the significance of the difference between the compensation values for quartiles 1 and 4. p-values for whether means differ are based on the difference in means test, while p-values for whether medians differ are based on the Wilcoxon rank-sum test.

Table 3.2 The Effect of Unionization on CEO Compensation

Model	Log (S&B)	Log (TC)	Log (S&B)	Log (TC)	Log (S&B)	Log (TC)	Log (S&B)	Log(TC)
Industry unionization rate	-0.368	-1.416	-0.078	-1.253	-0.271	-1.095	-0.188	-1.034
	(0.015)	(0.000)	(0.692)	(0.001)	(0.000)	(0.000)	(0.018)	(0.000)
Lag_Sales	0.276	0.388	0.279	0.388	0.275	0.377	0.277	0.376
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lag_BM	-0.101	-0.327	-0.234	-0.461	-0.156	-0.449	-0.275	-0.601
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lag_RET	0.060	0.134	0.074	0.125	0.049	0.126	0.077	0.117
	(0.000)	(0.000)	(0.006)	(0.017)	(0.000)	(0.000)	(0.003)	(0.022)
RET	0.111	0.178	0.169	0.187	0.113	0.193	0.181	0.213
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lag_ROA	-0.185	-0.275	-0.308	-0.441	-0.201	-0.333	-0.353	-0.499
	(0.000)	(0.011)	(0.003)	(0.001)	(0.000)	(0.004)	(0.004)	(0.000)
ROA	0.152	-0.135	0.009	-0.080	0.132	-0.194	0.029	-0.132
	(0.000)	(0.115)	(0.937)	(0.573)	(0.002)	(0.028)	(0.781)	(0.297)
CEO_tenure	0.003	-0.004	0.003	-0.005	0.004	-0.005	-0.003	-0.005
	(0.000)	(0.212)	(0.071)	(0.003)	(0.000)	(0.175)	(0.045)	(0.001)
Lag_Leverage	0.130	-0.064	0.020	-0.044	0.183	0.175	0.067	-0.092
	(0.004)	(0.585)	(0.808)	(0.503)	(0.000)	(0.307)	(0.408)	(0.230)
Intercept	-2.199	-1.348	-1.757	-1.325	-2.250	-1.144	-1.856	-1.216
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
year-fixed effect	Yes	Yes	No	No	Yes	Yes	No	No
industry-fixed effect	Yes	Yes	Yes	Yes	No	No	No	No
R-squared	0.540	0.472	0.544	0.475	0.513	0.437	0.518	0.440
N	5,417	5,398	16	16	5,417	5,398	16	16

Table 3.2 Continued

Note: This table reports OLS regressions of the natural logarithm (log) of CEO compensation on industry unionization rates and control variables. The sample consists of 5,398 firm-year observations in the manufacturing sector during the 1993-2008 period. The first and second models are pooled OLS regression models. The third and fourth models are Fama-MacBeth models. The dependent variable in the first, third, fifth and seventh models is the log of the sum of base salary and bonus (S&B). The dependent variable in the second, fourth, sixth and eighth models is the log of the CEO's total compensation (TC), defined as the sum of base salary, bonus, long-term incentive payouts, the value of restricted stock grants, and the value of option (*TDC1* in the Execucomp database). The dependent variables are winsorized at the 1^{st} and 99^{th} percentiles. Industry unionization rates are for 3-digit CIC industries and represent the fraction of total workers in an industry that are represented by unions in the collective bargaining with the firm. Lag_Sales is the log of firm sales for year t-1. Lag_BM is the ratio of book value to market value of assets at the end of year t-1. RET is the firm's stock return for year t. Lag_RET is the firm's stock return for year t-1. ROA is the ratio of net income to book value of total assets for year t. Lag_ROA is the ratio of net income to book value of total assets for year t-1. Lag_leverage is the ratio of total debt to total debt plus market capitalization at the end of year t-1. Compensation and sales are adjusted to 2009 constant dollars. Fixed effects for 2-digit SIC codes are included in the first four regressions. Year fixed effects are included in four cross-sectional regressions. Significance levels for whether coefficient estimates are different from zero are given in parentheses. The standard errors of the coefficients are adjusted for the clustering of observations at the firm level.

Table 3.3
Descriptive Statistics of Firm Characteristics around Union Contract Negotiations

		Year relative to contract expirations				
		-2	-1	0	1	2
Sales ($ millions)	Mean	18814.6	17727.9	17843.0	18531.7	18642.2
	Median	3397.8	3443.5	3853.2	4155.6	3773.8
Assets ($ millions)	Mean	33525.2	33433.5	35887.0	38163.5	38098.4
	Median	3163.9	3001.8	3010.0	3512.6	3444.4
Book to market	Mean	0.677	0.686	0.670	0.681	0.702
	Median	0.689	0.683	0.679	0.672	0.698
Cash/Assets	Mean	0.059	0.057	0.058	0.061	0.066
	Median	0.029	0.027	0.031	0.031	0.030
Leverage	Mean	0.301	0.309	0.310	0.327	0.334
	Median	0.265	0.267	0.285	0.278	0.292
Stock Return	Mean	0.118	0.072	0.185	0.096	0.097
	Median	0.093	0.059	0.169	0.055	0.079
ROA	Mean	0.040	0.056	0.052	0.046	0.050
	Median	0.041	0.051	0.053	0.052	0.049

Note: The table reports the descriptive statistics of firm characteristics from fiscal years -2 to +2 relative to the year when there are at least 10% of employees in contract negotiations and there is no more than 5% of total employees in contract negotiations during the two prior years. In order to be included in our sample, we also require the firm to have CEO compensation data in all five of the years. Data on labor contract negotiations are for manufacturing firms collected from the BNA Labor Plus database maintained by the Bureau of National Affairs. There are 114 observations over the 1995-2005 period. Assets is the book value of total assets in millions. Book to market value is the ratio of book value to market value of assets. Cash to assets is the ratio of cash and short-term investments to book assets. Leverage is the ratio of total debt to total debt plus market capitalization. Stock return is cumulative annual stock return. ROA is the ratio of net income to book value of total assets. Sales and Assets are adjusted to 2009 constant dollars.

Table 3.4 CEO Compensation around Union Contract Negotiations

Year relative to contract negotiation year	N1	Median CEO compensation ($ million)		Industry-adjusted median CEO compensation ($ million)		Median relative compensation		N2	Median relative compensation for observations with industry peers		Median relative compensation for industry peers	
		S&B	TC	S&B	TC	S&B	TC		S&B	TC	S&B	TC
Panel A: spike ratio = 10%												
-2	114	1.42	3.47	0.30	0.95	1.00	1.00	101	1.00	1.00	1.00	1.00
-1	114	1.57	3.37*	0.26	0.91*	1.02	1.01*	101	1.03	1.01*	1.08	1.11
0	114	1.67	4.48	0.29	1.13	1.09	1.09	101	1.10	1.11	1.13	1.19
1	114	1.74	4.72	0.30	1.31	1.14	1.16	101	1.16	1.17	1.18	1.23
2	114	1.61	4.89	0.17	1.68	1.13	1.23	101	1.13	1.24	1.22	1.31
Panel B: spike ratio = 20%												
-2	75	1.36	3.67	0.16	1.04	1.00	1.00	68	1.00	1.00	1.00	1.00
-1	75	1.49	3.03**	0.23	0.48**	1.02	0.99**	68	1.04	1.00**	1.07	1.07
0	75	1.52	4.53	0.12	0.78	1.09	1.08	68	1.10	1.12	1.13	1.14
1	75	1.52	4.63	0.17	1.08	1.13	1.15	68	1.17	1.18	1.17	1.22
2	75	1.57	4.44	0.14	1.15	1.10	1.21	68	1.10	1.21	1.20	1.26

Table 3.4 Continued

Panel C: spike ratio = 30%													
-2	51	1.39	4.17	0.22	1.48	1.00	1.00	48	1.00	1.00	1.00	1.00
-1	51	1.49	2.90**	0.25	0.59**	1.04	1.01**	48	1.04	1.01**	1.06	1.08
0	51	1.62	3.93	0.29	1.33	1.11	1.11	48	1.12	1.13	1.12	1.14
1	51	1.52	5.52	0.19	1.56	1.21	1.21	48	1.24	1.22	1.16	1.20
2	51	1.70	4.73	0.18	2.29	1.22	1.35	48	1.19	1.33	1.19	1.21
Panel D: spike ratio = 40%													
-2	36	1.39	3.95	0.20	1.48	1.00	1.00	34	1.00	1.00	1.00	1.00
-1	36	1.40	2.75**	0.23	0.49**	1.00	1.01*	34	1.00	1.00*	1.06	1.09
0	36	1.65	4.48	0.18	1.02	1.13	1.11	34	1.13	1.12	1.12	1.16
1	36	1.52	5.13	0.19	1.32	1.17	1.18	34	1.17	1.18	1.16	1.20
2	36	1.69	4.58	0.13	2.29	1.22	1.30	34	1.19	1.30	1.19	1.24
Panel E: spike ratio = 50%													
-2	25	1.23	2.11	0.12	0.50	1.00	1.00	24	1.00	1.00	1.00	1.00
-1	25	1.27	1.88**	0.23	-0.58*	0.99	0.90*	24	0.99	0.95*	1.06	1.09
0	25	1.62	3.60	0.36	0.89	1.17	1.14	24	1.20	1.15	1.13	1.19
1	25	1.41	2.92	0.19	0.54	1.15	1.05	24	1.17	1.10	1.16	1.21
2	25	1.67	3.07	0.11	-0.08	1.17	1.12	24	1.12	1.16	1.14	1.34

Note: This table reports CEO compensation over the five-year window surrounding union contract negotiations. Panels A through E correspond to different spike ratios ranging from 10% to 50%. We study two measures of CEO compensation: base salary plus bonus (S&B) and total compensation (TC). Base salary plus bonus is the sum of salary and bonus, and total compensation is *TDC1* in the ExecuComp database. CEO annual compensation is adjusted to 2009 constant dollars. Industry-adjusted CEO compensation is firm-level CEO compensation minus median industry CEO compensation, where industry CEO compensation is defined as the CEO compensation of other ExecuComp firms that share the same primary four-digit SIC as a sample firm, but that do not have a labor contract negotiation in the same fiscal year as that of a sample firm. A firm's 4-digit SIC code is used to calculate this statistic if there are at least five firms in the firm's 4-digit SIC code for all five of the years. If this criterion is not met, the firm's 3-digit SIC code is used. If this criterion is not met, the 2-digit SIC code is used. For the year immediately preceding the contract negotiation spike, we test whether CEO compensation change (industry-adjusted CEO compensation change) in year -1 is lower than those in the other three years using a Wilcoxon rank-sum test. We also test whether CEO compensation growth rate in year -1 is lower than those in the other three years using a Wilcoxon rank-sum test. * denot

Table 3.4 Continued

significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level. Relative CEO compensation is calculated by scaling CEO compensation by the level of compensation in year -2 relative to the fiscal year when there is a labor contract negotiation spike. We also report median relative CEO compensation of industry controls.

Table 3.5 Multivariate Analysis of CEO Compensation around Union Contract Negotiations

Panel A: (Log) Annual compensation as dependent variable

Spike ratio	10%		20%		30%		40%		50%	
	S&B	TC	S&B	TC	S&B	TC	S&B	TC	S&B	TC
Prior-year dummy	-0.043*	-0.057*	-0.042*	-0.105**	-0.079**	-0.163***	-0.109***	-0.189**	-0.141***	-0.224**
	(0.09)	(0.08)	(0.10)	(0.03)	(0.02)	(0.01)	(0.01)	(0.03)	(0.01)	(0.02)
Financial controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year/Industry dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	533	533	352	352	242	242	169	169	116	116
R-squared	0.755	0.746	0.801	0.781	0.811	0.821	0.861	0.841	0.815	0.813

Panel B: Annual compensation growth rate as dependent variable

Spike ratio	10%		20%		30%		40%		50%	
	S&B	TC	S&B	TC	S&B	TC	S&B	TC	S&B	TC
Prior-year dummy	-0.044	-0.177**	-0.025	-0.291***	-0.084*	-0.391***	-0.096*	-0.446***	-0.076	-0.422*
	(0.17)	(0.05)	(0.32)	(0.01)	(0.09)	(0.01)	(0.07)	(0.01)	(0.30)	(0.08)
Financial controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year/Industry dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	507	507	334	334	231	231	162	162	111	111
R-squared	0.255	0.142	0.301	0.164	0.331	0.246	0.323	0.267	0.366	0.330

Table 3.5 Continued

Note: This table reports a multivariate analysis on CEO compensation around union contract negotiations. Two measures of CEO compensation are studied: Base salary plus bonus (S&B) and Total compensation (TC). Base Salary plus bonus is the sum of salary and bonus, and total compensation is *TDC1* in the ExecuComp database. The dependent variable in the first, third, fifth, seventh, and ninth models of Panel A is the annual growth rate in CEO's base salary plus bonus, and the dependent variable in the second, forth, sixth, eighth, and tenth models of Panel A is the annual growth rate in CEO's total compensation. The dependent variable in the first, third, fifth, seventh, and ninth models of Panel B is the natural logarithm (log) of CEO base salary plus bonus, and the dependent variable in the second, forth, sixth, eighth, tenth models of Panel B is the natural logarithm (log) of CEO total compensation. The primary independent variable of interest is a dummy variable (prior-year dummy) that equals one if the year of observation is year -1 relative to union contract negotiation spike; otherwise it equals zero. Other control variables in Panel A are annual sales growth rate, lag annual sales growth, change in annual stock return, lag change in stock return, change in return on assets, lag change in return on assets, change in financial leverage, lag change in financial leverage, change in book to market, lag change in book to market, and CEO tenure. Other control variables in Panel B are the natural logarithm of lag sales, annual stock return, lag annual stock return, return on assets, lag return on assets, lag book to market and CEO tenure. CEO compensation and sales are adjusted to 2009 constant dollars. Fixed effects for 2-digit SIC codes and years are included in all models. Significance levels for whether coefficient estimates are different from zero are in parentheses. The standard errors of the coefficients are adjusted for the clustering of observations at the firm level. * denotes significance at the 10% level; ** denotes significance at the 5% level ; *** denotes significance at the 1% level.

Table 3.6 CEO Option Grants around Union Contract Negotiations

Quarter relative to contract expiration	N	# of firms making option grant	Median value of option grant (Black-Scholes)	Median value of option grant (company)
		Panel A: spike ratio = 10%		
-4	98	23	48.9%	50.0%
-3	98	38	51.6%	49.8%
-2	98	20	36.7%***	35.3%***
-1	98	18	43.2%**	43.7%**
1	98	22	49.7%	49.6%
2	98	33	49.8%	53.9%
3	98	18	49.1%	53.3%
4	98	19	51.2%	51.0%
		Panel B: spike ratio = 15%		
-4	68	17	48.8%	49.0%
-3	68	28	53.2%	55.9%
-2	68	12	32.8%**	32.7%**
-1	68	11	47.6%	47.1%
1	68	16	53.2%	53.0%
2	68	22	51.7%	52.4%
3	68	10	51.9%	55.6%
4	68	11	48.8%	48.1%
		Panel C: spike ratio = 20%		
-4	54	13	48.8%	49.0%
-3	54	25	53.6%	56.0%
-2	54	9	31.8%**	31.2%*
-1	54	8	36.9%**	35.5%**
1	54	11	51.1%	51.9%
2	54	19	49.8%	50.9%
3	54	8	51.9%	55.6%
4	54	9	48.8%	48.1%
		Panel D: spike ratio = 25%		
-4	46	12	48.1%	48.7%
-3	46	20	53.7%	53.4%
-2	46	8	38.4%*	29.8%**
-1	46	8	36.9%**	35.5%**
1	46	11	51.1%	51.9%
2	46	15	55.2%	57.4%
3	46	7	49.1%	54.8%
4	46	7	47.0%	46.5%

Note: This table reports option grant activities over the eight-quarter window surrounding union contract negotiations. In order to be included in this sample, we require the number of employees in contract negotiations to represent at least 10% of the total labor force and that contract negotiations are preceded by at least two years during which a firm has no other labor contract that expired. Two measures of option grants are studied: the Black-Scholes value and the firm-reported value. For each observation in our sample, we aggregate the value of option grants in each quarter and then we scale by the total value of option grants in all eight quarters. Panels A through D correspond to different spike ratio criteria ranging from 10% to 25%. For quarters -2

Table 3.6 Continued

to -1 relative to contract expirations, we test whether the value of option grants is lower than those in the other six quarters using a Wilcoxon rank-sum test. * denotes significance at the 10% level; ** denotes significance at the 5% level ; *** denotes significance at the 1% level.

Table 3.7 Excess Pay, CEO Ownership, and the Likelihood of Compensation Cuts

	Dependent variable:	
	Cut in S&B	Cut in TC
Excess CEO compensation	0.46	0.64**
	(0.17)	(0.02)
CEO ownership	6.52**	6.37**
	(0.03)	(0.05)
Financial controls	Yes	Yes
Year/Industry dummy	Yes/Yes	Yes/Yes
Number of observations	93	93
Likelihood ratio	16.94	20.25

Note: This table reports a multivariate probit analysis on the effect of excess CEO compensation and CEO ownership on the likelihood of a CEO compensation cut prior to labor contract negotiations. The dependent variable is a dummy variable equal to one if CEO's base salary plus bonus (S&B) or total compensation (TC) drops prior to labor contract negotiations. Excess CEO compensation is measured as the ratio of actual compensation minus expected compensation to expected compensation, where expected CEO compensation is obtained from an OLS regression of the natural logarithm of compensation (either S&B or TC) on natural logarithm of lag sales, annual stock return, lag annual stock return, return on assets, lag return on assets, lag book to market, natural logarithm of CEO tenure, and industry controls. CEO ownership is measured by the number of shares and options owned by the CEO scaled by the number of shares outstanding prior to labor contract negotiations. Other control variables include annual stock return, lag annual stock return, return on assets and lag return on assets. Fixed effects for 2-digit SIC and years are included in all models. p-values from tests of whether the coefficient estimates are different from zero are given in parentheses. * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

Table 3.8 CEO Compensation Cuts and Labor Contract Negotiation Outcomes

Panel A: Descriptive statistics on the annual salary growth rate in labor contracts

	N	Mean	Median	Minimum	Maximum
Cut in TC	12	1.62%**	1.83%*	0.00%	3.50%
No cut in TC	16	2.53%	2.87%	1.13%	4.00%

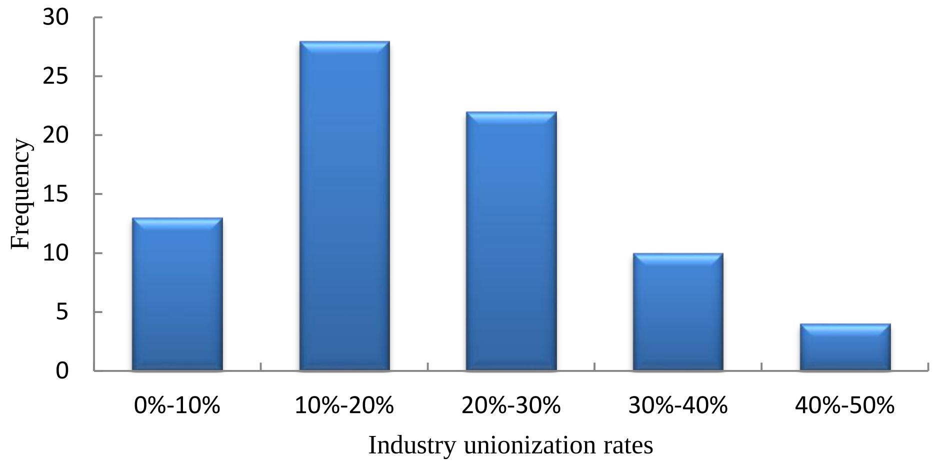
Panel B: Regressions of the annual salary growth rates in labor contracts

	(1)	(2)
Cut in TC	-0.011**	
	(0.03)	
Growth rate in TC		0.010*
		(0.09)
Annual stock return	0.009	0.005
	(0.35)	(0.58)
Return on assets	0.018	0.020
	(0.68)	(0.66)
Book value of assets	-0.001	-0.001
	(0.61)	(0.56)
Cash / Assets	0.088	0.120
	(0.19)	(0.13)
Debt / Assets	0.012	0.018
	(0.63)	(0.50)
Number of observations	27	27
R-squared	0.280	0.209

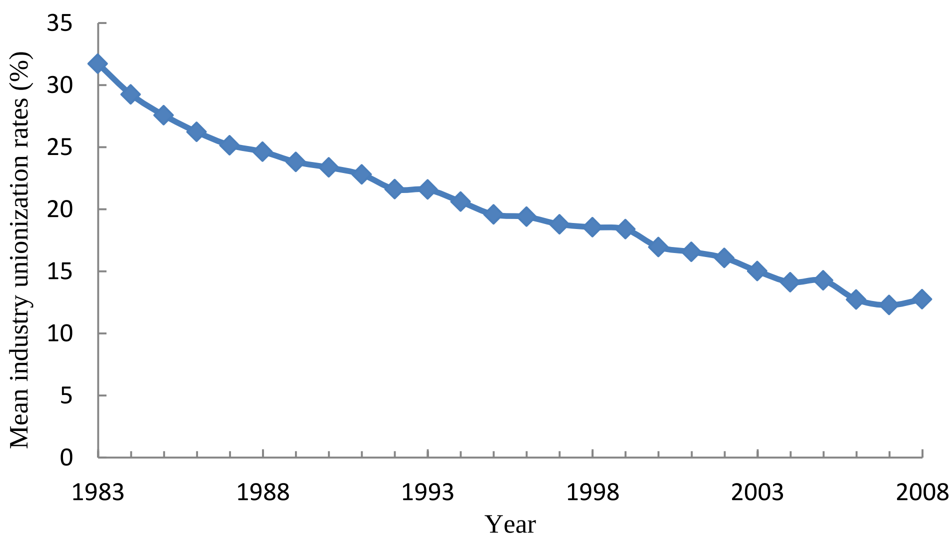
Note: This table reports the effect of CEO compensation cuts on the outcome of labor contract negotiations. The sample consists of 28 labor contract observations with information about annual salary growth rates. A typical labor contract provides information about salary for multiple future years, and we define the annual salary growth rate to be the average growth rate across these years. In Panel A, we partition the sample into two groups based on whether CEOs' total compensation (TC) drops prior to labor contract negotiations and report summary statistics for the annual salary growth rate for each group. We test whether the mean and median salary growth rates differ across the groups using a t-test and Wilcoxon rank-sum test, respectively. Panel B reports results from multivariate regressions of the annual salary growth rate. In model (1), the independent variable of interest is a dummy variable (Cut in TC) that equals one if the CEOs' total compensation drops before labor contract negotiations, and in model (2), the variable of interest is the growth rate in CEOs' total compensation before labor contract negotiations. Other control variables include the annual stock return and return on asset during the fiscal year prior to labor contract negotiations, and book value of assets, cash scaled by assets, and debt scaled by assets at the end of the fiscal year prior to labor contract negotiations. * denotes significance at the 10% level; ** denotes significance at the 5% level.

Figure 3.1 Industry Unionization Rates

Panel A: Cross-sectional variation of industry unionization rates

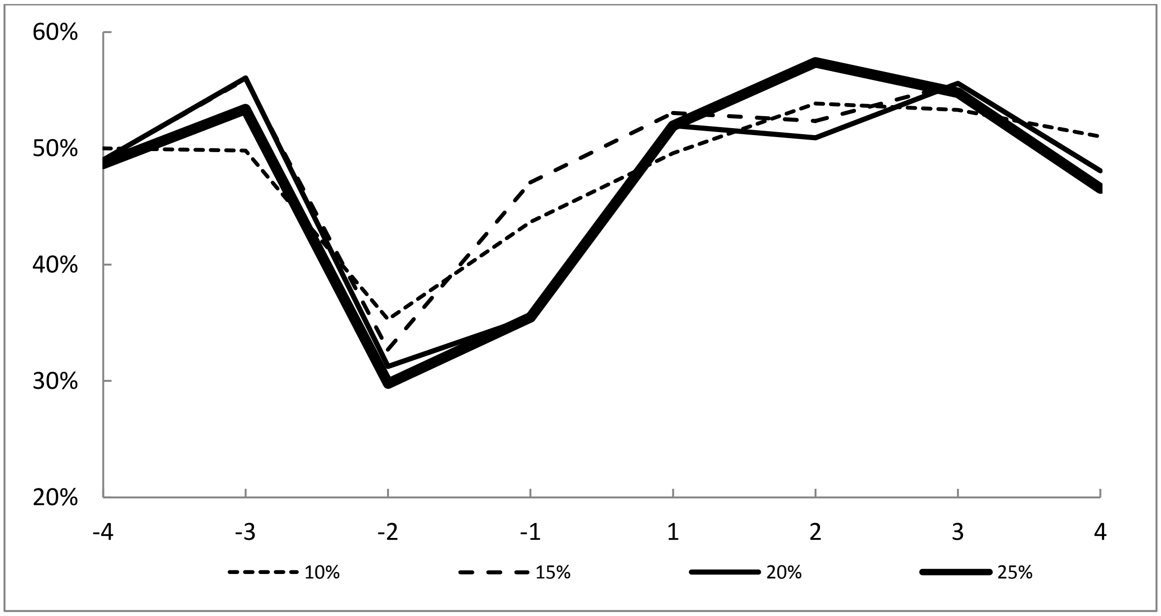


Panel B: Industry unionization rates over the 1983-2008 period



Note: The industry unionization rate is the fraction of an industry's workers that are represented by labor unions in the collective bargaining with the firm, where each industry corresponds to a 3-digit Census Industry Classification (CIC). Panel A depicts cross-sectional variation in industry unionization rates. We compute the mean industry unionization rates over the 1983-2008 period and report the number of 3-digit CIC industries that fall in each decile. Panel B depicts the time-series trend of industry unionization rates over the 1983-2008 period.

Figure 3.2 CEO Option Grants surrounding Union Contract Negotiations



Note: This figure depicts option grant activities over the eight-quarter window surrounding union contract negotiations. In order to be included in this sample, we require that the number of employees in contract negotiations scaled by the total labor force (the spike ratio) is at least 10% and that contract negotiations are preceded by at least two years during which a firm has no other labor contract that expired. For each observation in our sample, we aggregate the value of CEO option grants in each quarter and then scale it by the total value of CEO option grants in all eight quarters. We plot the median option grants for firms with spike ratios exceeding values ranging from 10% to 25%.

APPENDIX
VARIABLE DEFINISIONS

<= 3 years tenure	Dummy variable equal to one if there is one investment banker director who has 3 or fewer years of tenure when the deal is announced and 0 otherwise. Source: BoardEx.
Acquisition dummy	Dummy variable equal to one if the firm makes any acquisitions in past three years. Source: SDC.
Acquisitions	Expenditures on acquisitions over the lagged book value of total assets. Source: Compustat.
Advisory dollar fees	Total advisory fees paid by acquirer. Source: SDC.
Assets	Total assets ($ million). Source: Compustat.
Avg. abnormal return	Average daily market-model abnormal return. Source: CRSP.
BCF index	Governance index based on 6 antitakeover provisions, taken from BCF (2004).
Board independence	Dummy variable equal to one if over 60% of directors are independent. Source: BoardEx.
Board size	Number of directors on the board. Source: BoardEx.
Board tenure	The average number of years that directors have served on the board. Source: BoardEx.
CAR [-1,1]	Three-day cumulative abnormal return calculated using the market model estimated over the 200-day period ending 11 days before the announcement dates, with the CRSP value-weighted return as the market index. Source: CRSP.
Cash	Cash and cash equivalent holdings over book value of total assets. Source: Compustat.
Cash deal	Dummy variable equal to one for deals are paid for 100% by cash. Source: SDC.
Cash flow	Operating income before depreciation (EBITDA) over book value of total assets. Source: Compustat.
CEO age	CEO's age at the end of the fiscal year. Source: ExecuComp.
CEO gender	Dummy variable equal to one if acquirer CEO is a male, 0 otherwise. Source: RiskMetrics.
CEO ownership	Acquirer CEO's percentage ownership of the firm, including both stock and stock options. Source: RiskMetrics.
CEO tenure	CEO's tenure in years at the end of fiscal year. Source: ExecuComp
Competition	Dummy variable equal to one if a deal has competing bidders. Source: SDC.

Conglomerate deal	Dummy variable equal to one if the target and the acquirer have the same two-digit SIC code. Source: SDC.
Current appointment	Dummy variable equal to one if there is one investment banker director who still works as an investment banker when he or she is appointed to the board. Source: BoardEx.
Current experience	Dummy variable equal to one if there is one investment banker director who still works as an investment banker when the deal is announced. Source: BoardEx.
Debt issuance dummy	Dummy variable equal to one if the firm has any debt issuance in past three years. Source: SDC.
Dummy (Assets within 50 - 200%)	Dummy variable equal to one if the ratio of two CEOs' firm assets is within 50 - 200%, zero otherwise. Source: Compustat.
Dummy (CEO is chair)	Dummy variable equal to one if CEO is the chairman of the board, zero otherwise. Source: ExecuComp.
Dummy (CEO is female)	Dummy variable equal to one if CEO is a female, zero otherwise. Source: ExecuComp.
Dummy (Sales within 50 - 200%)	Dummy variable equal to one if the ratio of two CEOs' sales is within 50 - 200%, zero otherwise. Source: Compustat.
Dummy (same firm)	Dummy variable equal to one if two CEOs worked for the same company as top managers or board of directors, but not at the same time, zero otherwise. Source: BoardEx.
Dummy (same industry)	Dummy variable equal to one if two CEOs are from the same Fama-French 49 industries, zero otherwise. Source: ExecuComp.
Dummy (same school)	Dummy variable equal to one if two CEOs went to the same school and earned the same professional degree, but graduated more than a year apart from each other, zero otherwise. Source: BoardEx.
Dummy (same state)	Dummy variable equal to one if the headquarters of two firms are located in the same state, zero otherwise. Source: ExecuComp.
Experienced	Dummy variable equal to one if there is one investment banker director who has gained investment banking experience when he or she is appointed to the board. Source: BoardEx.
Financial leverage	Sum of long-term debt and debt in current liabilities over book value of total assets. Source: Compustat.
Firm age	Number of years a firm has been listed. Source: CRSP.
GIM index	Governance index based on 24 antitakeover provisions, taken from GIM (2003).
IB Director	Dummy variable equal to one if there is at least one director having investment banking experience when the deal is announced. Source: BoardEx.

IB Director(%)	Percentage of outside directors with investment banking experience on board. Source: BoardEx.
IB Director_size	Number of directors with investment banking experience. Source: BoardEx.
Investments	Capital expenditures over net property, plant and equipment at the beginning of the fiscal year Source: Compustat
Large deal	Dummy variable equal to one if relative transaction value is above the median value. Source: SDC.
Leverage	Sum of long-term debt and debt in current liabilities over book value of total assets. Source: Compustat.
Location	Dummy variable equal to one if the firm is located in NY, NJ, CA, IL, or MA. Source: Compustat.
Market-to-book	(Total Assets - Book Equity + Market Value of Equity) / Total Assets. Source: Compustat.
Noncash working capital	Net working capital minus cash and cash equivalents over total assets. Source: Compustat.
Number of peers	Number of socially connected CEOs. Source: BoardEx.
Peer / CEO salary	A peer's salary over CEO salary. Source: BoardEx and ExecuComp
Peer / CEO salary and bonus	A peer's salary and bonus over CEO salary and bonus. Source: BoardEx and ExecuComp
Peer / CEO total compensation	A peer's total compensation over CEO total compensation. Source: BoardEx and ExecuComp
Peer / CEO total payout	A peer's total payout over CEO total payout. Source: BoardEx and ExecuComp
Post-SOX	Dummy variable equal to one for years > 2002. Source: Compustat.
PPM4WK	Premium of offer price to target trading price 4 weeks prior to the original announcement date. Source: SDC.
Price-to-earnings	Stock price over earnings per share. Source: Compustat
Private target	Dummy variable equal to one for private target. Source: SDC.
Public target	Dummy variable equal to one for public target. Source: SDC.
R&D	Research and development expense over sales (zero if missing). Source: Compustat.
Relative transaction value	Transaction value over acquirer market value of equity. Source: SDC.
ROA	Income before extraordinary items over total assets. Source: Compustat.
Salary	Salary ($ thousand). Source: ExecuComp.
Salary & Bonus	Salary + Bonus ($ thousand). Source: ExecuComp.
Sales	Firm's sales ($ million). Source: Compustat.

Sales growth	Growth rate in sales. Source: Compustat.
SEO Dummy	Dummy variable equal to one if the firm has any seasoned equity offerings in past three years. Source: SDC
Stock deal	Dummy variable equal to one for deals are paid for 100% by stock. Source: SDC.
Stock price volatility	Annualized standard deviation of monthly stock returns over the past 24 months. Source: CRSP.
Stock return	Firm's fiscal year raw return. Source: CRSP.
Stock return (industry)	Value-weighted Fama-French 49 industry fiscal year raw return. Source: CRSP.
Subsidiary target	Dummy variable equal to one for subsidiary target. Source: SDC.
Tender-offer	Dummy variable equal to one for tender offers. Source: SDC.
Toehold	Dummy variable equal to one if acquirer holds 5% or more of the target stock prior to the announcement. Source: SDC.
Total compensation	Salary + Bonus + Other Annual + Restricted Stock Grants + LTIP Payouts + All Other + Value of Option Grants ($ thousand). Source: ExecuComp.
Total payout	Salary + Bonus + Other Annual + Restricted Stock Grants + LTIP Payouts + All Other + Value of Options Exercised ($ thousand). Source: ExecuComp.
Transaction value	Deal value from SDC, adjusted to 2009 dollar. Source: SDC.

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