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ESSAYS IN EMPIRICAL CORPORATE FINANCE: ASSET SALES AND TAKEOVERS, CEO COMPENSATION, AND INVESTMENT UNDER UNCERTAINTY

by

Ting Ting Que

A thesis submitted in partial fulfillment of the requirements for the Doctor of Philosophy degree in Business Administration (Finance)
in the Graduate College of
The University of Iowa

August 2014

Thesis Supervisor: Professor Erik Lie

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Graduate College
The University of Iowa
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PH.D. THESIS

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Ting Ting Que

has been approved by the Examining Committee
for the thesis requirement for the Doctor of Philosophy
degree in Business Administration (Finance) at the
August 2014 graduation.

Thesis Committee: ___________________________
Erik Lie, Thesis Supervisor

____
David C. Mauer

____
Anand M. Vijh

____
Tong Yao

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Amrita Nain

To my family

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I dedicate this work to all of them.

ABSTRACT

This thesis consists of three essays and studies CEO compensation, asset sales and takeovers and investment under uncertainty in empirical corporate finance. The first essay is a joint work with Qianqian Huang, Feng Jiang and Erik lie, titled 'The effect of labor unions on CEO compensation'. The second essay '. Union Concessions following Asset Sales and Takeovers' is a joint work with Erik Lie. The third essay is titled 'The Effect of Systematic and Idiosyncratic Risk on Investment and R&D' and is sole-authored.

In the first essay, we document evidence that labor unions compel firms to curtail CEO compensation. First, we find that firms with strong unions pay their CEOs less. Further, firms curb CEO compensation, especially the part that is discretionary, prior to union contract negotiations. Finally, we report that curbing CEO compensation mitigates the chance of a labor strike, thus providing a rationale for firms to pay CEOs less when facing strong unions.

In the second essay, we document that the likelihood of asset sales increases with union wages. Furthermore, the acquiring firms gain significant concessions from the incumbent union following asset sales. Finally, the anticipation of union concessions helps explain the excess stock returns around asset sale announcements. We find no comparable effects for takeovers. We conclude that asset sales, but not takeovers, are partially motivated by the potential to extract concessions from unions.

Finally, in the third essay, in an attempt to shed some light on the puzzling positive sensitivity of investment to systematic volatility documented in Panousi and Papanikolaou (2012), we decompose systematic volatility into a firm's systematic risk exposure (beta) as well as the market and industry portfolio volatility. Surprisingly, we find a positive response of investment to a firm's systematic risk exposure. R&D expenditure is employed as an alternative form of investment. Our results show that

idiosyncratic risk actually encourages firms to engage in R&D spending, in contrast with its depressing effect on capital expenditure; whereas systematic volatility depresses R&D in contrast with the positive sensitivity of capital expenditure to systematic volatility.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER 1. THE EFFECT OF LABOR UNIONS ON CEO COMPENSATION

> 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

1.1 Introduction

There is a growing interest in understanding how labor relations, especially labor unions, affect firms' decisions and performance. Since the early study by Liberty and Zimmerman (1986) on firms' accounting choices around union contract negotiation, numerous studies have examined whether firms take strategic actions to improve their bargaining position against labor unions. The extant literature finds that firms with unionized workers tend to have higher leverage (Bronars and Deere, 1991; Hanka, 1998; Matsa 2010), hold less cash (Klasa et al., 2009), cut dividend payments (DeAngelo and DeAngelo, 1991), manage earnings downward (DeAngelo and DeAngelo, 1991; DeAngelo et al., 1994; Bova, 2013), and strategically choose accounting methods to shelter income from labor unions' demands (Cullinan and Knoblett, 1994; Bowen et al., 1995; D'Souza et al., 2000; Comprix and Muller, 2011). In this study, we examine whether such strategic considerations also affect executive compensation.

Jensen and Murphy (1990) argue that payoffs to executives and unionized workers are implicitly linked, because public information on executive compensation provides justification for union demands. For instance, unions might interpret the current level of executive compensation as executives' willingness to sacrifice for the good of the firm or as an indicator of the firm's expected future financial performance beyond what accounting figures convey.

Anecdotal evidence also highlights the important role of top executives' compensation in union negotiation. For example, in 2007, just as American Airlines' contract with its flight attendants' union was about to expire, the president of the flight attendants' union expressed dissatisfaction with the airline's intention to award millions in bonuses to executives.

We hypothesize that strategic considerations that arise in the bargaining between a firm and its unionized workers affect the firm's executive compensation. In particular, we argue that lower executive compensation improves firms' bargaining positions relative to labor unions, and that firms consider this when setting executive compensation. Prior studies have found evidence that labor unions curb excessive CEO pay by condemning pay disparity between managers and workers (e.g., Ertimur et al., 2011; Gomez and Tzioumis, 2006).[1] And, consistent with our argument, DeAngelo and DeAngelo (1991) report a significant CEO pay cut prior to union contract negotiations for a sample of seven distressed steel firms during the 1980s. We extend and generalize DeAngelo and DeAngelo both by using a broader sample (spanning multiple industries and including both distressed and non-distressed firms), by including option grants, and by conducting more extensive tests, some of which are designed to remedy identification concerns.[2]

[1] In fact, in 1997 the AFL-CIO launched *Executive Paywatch*, a website to track the disparity between CEO and worker pay. The website called itself then "a working families' guide to monitoring and curtailing the excessive salaries, bonuses and perks in CEO compensation packages." For the most recent version of *Executive Paywatch*, see http://www.aflcio.org/Corporate-Watch/CEO-Pay-and-You.

[2] DeAngelo and DeAngelo (1991) examine a unique set of negotiations that were opened early as a result of 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 strategic behavior leading up to negotiations that is not contaminated by financial difficulties. Moreover, 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.

We first document a significantly negative relation between industry-level unionization rate and the level of total CEO pay during 1993–2011. A one-standard-deviation increase in the industry unionization rate is associated with roughly 9.2% lower CEO compensation ($0.26 million) for the median CEO in our sample. These results are consistent with the notion that unions affect CEO compensation. We further take several approaches to address identification issues. First, we control for a group of industry-level characteristics to mitigate the concern that unionization acts as a proxy for some general industry characteristics that are also correlated with CEO compensation. Second, we conduct a time-series average analysis and year-by-year regressions to ensure that the documented negative relation is not attributable to time trends. Third, we manually collect firm-level unionization rates from firms' SEC filings to ensure that measurement errors in industry-level unionization do not significantly bias our estimates.

To alleviate identification concerns further, we also explore cross-sectional variations in the effects of (both industry- and firm-level) unionization on CEO pay. If firms strategically set CEO pay to bargain with unions, the negative relation between CEO pay and unionization should be more pronounced when unions have a strong bargaining position. Indeed, we find a stronger negative relation for firms located in states with no right-to-work laws, for firms located in states with lower unemployment rates, and for firms with more concentrated business operations. In addition, the impact of unionization on CEO pay is more pronounced for firms that are closer to financial distress, in which case lower CEO pay should be a more credible signal that a firm cannot concede to union demands.

Next, we examine whether firms' strategic actions in collective bargaining contribute to the negative relation between unionization and CEO pay. To do so, we investigate whether firms attempt to strengthen their bargaining power by curtailing CEO pay prior to scheduled labor negotiations. Using collective bargaining agreement information from the Bureau of National Affairs, we document a significantly negative relation between CEO pay in a given year and the negotiation ratio, defined as the percentage of employees involved in contract negotiations, in the following year. A one-standard-deviation increase in negotiation ratio is associated with an average 7.5% decline in total CEO compensation in the fiscal year prior to negotiation. This suggests that firms are more likely to curtail CEO pay when they face union contract negotiations. The empirical pattern is stronger when the unions are relatively strong (i.e., the firms have concentrated business operations and operate in states with no right-to-work laws and/or a low unemployment rate) and when the CEO pay level serves as credible evidence that a firm cannot comply with union demands. The pattern is also stronger for option grants, especially unscheduled grants, than for other components of CEO pay, presumably due to the ease with which firms can determine both how many options to grant and when to grant them.

Finally, we study how CEO compensation affects the decision of labor unions to initiate a strike. We find that both high CEO pay and recent increases in CEO pay raise the likelihood of a strike, thus providing a rationale for firms to use CEO pay as a strategic tool in their interactions with labor unions.

While our evidence suggests that firms' strategic actions prior to labor negotiations contribute to lower pay for CEOs, these strategic actions are unlikely to be the only reason for the lower CEO pay among unionized firms. Indeed, Ertimur et al. (2011) report evidence that union pension funds pressure firms to constrain CEO pay via shareholder activism. Furthermore, we find evidence that unionized firms pay CEOs less even in years other than those immediately prior to labor negotiations.

A final question is why union leaders are seemingly fooled by the temporary curtailment in CEO pay as they enter negotiations with firm managers. That is, after repeated negotiations, union leaders should have learned that CEO pay bounces back after the negotiations have concluded. In this regard, our study is particularly related to the studies that examine the use of accounting manipulation to temporarily deceive unions (Liberty and Zimmerman, 1986; DeAngelo and DeAngelo, 1991; DeAngelo et al., 1994; D'Souza et al., 2000; Bova, 2013; Comprix and Muller, 2011). There are several possible reasons for this apparent complacency. First, unions might be content that executive pay is curbed at least every few years, especially if they also recognize that executive pay is generally lower for unionized firms. Conversely, unions might be insulted by the failure of executives to show moderation prior to negotiations. Second, it is hard to detect compensation patterns, especially the slow recovery after negotiations, for only one firm across a small set of negotiations. To tease out the underlying patterns, our study makes use of thousands of firm-years. It is conceivable that when our results are widely disseminated and unions become more aware of the compensation patterns, these strategic actions of firms will diminish. Third, the agency problem between union leaders and workers could obscure what is rational from a union perspective.[3] For example, union leaders might recognize the strategic actions undertaken by the firm, but view them as necessary to save face with workers during negotiations.

[3] The agency problem between union leaders and workers is widely recognized, e.g., in the early studies of Ross (1948) and Martin (1984). The media periodically discusses the pay gap between union leaders and workers and corruption among union leaders. The *Detroit News* writes that "The pay disparity is taking a financial toll on many union halls" and "raises questions about whether labor leaders are sharing the economic struggles of their members" (August 14, 2007). The *Washington Times* writes that "The average union member has no idea how much the leaders make, said Stanley Oubre, a retired Boilermaker in Louisiana – and can hardly relate" (January 10, 2013). The *New York Times* writes that union leaders "helped corrupt contractors steal millions of dollars more from the union and its benefit funds" (August 5, 2009). The National Legal and Policy Center even creates an annual "Top Ten Union Corruption Stories of the Year."

1.2 Literature Review and Hypothesis Development

1.2.1. Literature review

1.2.1.1 Unions and CEO compensation

Several studies investigate the empirical relation between unions and executive compensation. For instance, DiNardo et al. (1997) find a negative relation between unionization and CEO pay for the periods 1971–74, 1975–78 and 1979–82. However, they caution against drawing strong inferences from this relation because the results are not robust to alternative specifications. Agarwal and Singh (2002) examine a cross-sectional sample of 86 firms in the Canadian metal-mining industry in 1996 and report that executives in unionized firms earn higher salaries than peers in nonunionized firms. In contrast, Banning and Chiles (2007) document a negative relation between union presence and CEO compensation using a sample of 170 US firms in 1997. Gomez and Tzioumis (2006) is the most comprehensive study so far examining unions and executive compensation. They estimate the relation between union presence and CEO compensation using a panel of publicly listed US firms for the period 1993–2001. They find that CEOs of union firms receive relatively lower total compensation, which they argue is due to the pressure that unions impose on firms to constrain CEO pay.

Our paper contributes to this literature in two major ways. First, we reexamine the relation between union presence and CEO compensation using triangulation tests to better establish causality. Second, we identify and empirically test a specific mechanism through which unions influence executive pay. In particular, we conjecture that firms facing strong unions strategically alter executive pay to enhance bargaining power.

1.2.1.2 Labor and other corporate policies

Labor unions have unique incentives and risk preferences and might seek to maximize their own interests at the expense of firm value. Studies find that firms facing stronger unions invest less in R&D (e.g., Hirsch, 1992), innovate less (Bradley et al., 2013), are less tax aggressive (Chyz et al., 2013), have weaker corporate governance (Atanassov and Kim, 2009), and take fewer risks (Chen et al., 2011, 2012). As a result, these firms have relatively lower profitability and shareholder value (Ruback and Zimmerman, 1984; Abowd, 1989; Hirsch, 1991; Lee and Mas, 2012).

Although private sector unionization has been declining since the mid-twentieth century, union bargaining still plays an important role in firms' operations (Hirsch, 2004). The extant literature suggests that firms can take strategic actions to lower their perceived ability to meet union demand.[4] For example, Dasgupta and Sengupta (1993) and Perotti and Spier (1993) show theoretically that firms can reduce future cash flows available to labor by strategically issuing more debt. Bronars and Deere (1991), Hanka (1998), and Matsa (2010) all find evidence consistent with this argument. Klasa et al. (2009) further show that unionized firms strategically hold small cash reserves so as to shelter corporate income from union demands. Additionally, firms can strategically choose accounting policies to gain an advantage in the collective bargaining. Cullinan and Knoblett (1994) find that unionization influences manufacturing firms' choices of inventory policy. Bowen et al. (1995) later report that unionized firms are more likely to adopt income-decreasing accounting methods for inventory and assets valuation. Further, D'Souza et al. (2001) find that when adopting SFAS 106, unionized firms are more inclined to

[4] Klasa, Maxwell, and Ortiz-Molina (2009) provides a review of bargaining between firms and labor unions.

use the immediate recognition method to reduce labor renegotiation costs. More recently, Mora and Sabater (2008) and Bova (2013) both show that firms manage earnings downward prior to labor negotiations to signal a negative outlook.[5]

The prior work most related to our study is that of DeAngelo and DeAngelo (1991), who use a sample of seven major steel firms from 1980 through 1988 to examine corporate decisions during a period in which managers sought concessions from union members. They show that firms reduce dividend payments and manage their earnings downward prior to union contract negotiations. More importantly, they document a significant CEO pay cut prior to union negotiations, suggesting that firms have incentives to publicize lower executive payment when seeking union concessions. 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 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 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 equity grants. This is critical because, in terms of both magnitude and timing, these grants are arguably the most discretionary component of CEO compensation in recent years. Third, our larger and broader sample allows us to conduct a more systematic and generalized study of the strategic role of CEO compensation in collective

[5] In a related paper on accounting decisions and labor costs, Comprix and Muller (2011) report evidence that managers opportunistically bias pension estimated to obtain labor concessions.

bargaining. A substantial part of this is designed to address identification concerns, which are prominent in the compensation literature.

1.2.1.3 CEO compensation

Executive compensation has been at the center of debate in the finance literature for at least the last couple of decades. The literature has identified various determinants of CEO pay and found evidence consistent with both optimal contracting and rent seeking (Smith and Watts, 1992; Core et al., 1999; Core and Guay, 1999; Ittner et al., 2003; Gabaix and Landier, 2008).[6] One documented factor that appears to affect executive compensation is politics and public perception. For instance, Joskow et al. (1996) find that political pressures constrain CEO pay levels in the electric utility industry, while Eldenburg and Krishnan (2003) show that public scrutiny leads to lower CEO pay in public hospitals. Murphy (1996) finds that managers adopt disclosure methodologies with reduced reported or perceived compensation, which suggests that managers bear non-pecuniary costs from reporting high levels of compensation. Dial and Murphy (1995) document how pressures on pay at General Dynamics lead the company to replace a controversial bonus plan with conventional stock options. Analyzing the impact of the $1 million "cap" on deductibility of non-performance pay, Rose and Wolfram (1997) and Perry and Zenner (2001) 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.

More recently, Core et al. (2008) use more than 11,000 press articles about CEO compensation to study the role of the press in monitoring and influencing executive

[6] See Frydman and Jenter (2010) for a review.

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.

1.2.2 Hypothesis development

Our main hypothesis is that firms' strategic considerations in collective bargaining with unionized workers affect CEO compensation. In particular, we argue that lower executive compensation strengthens firms' bargaining power and that firms consider this when determining CEO pay contracts. This leads to the following testable hypotheses on the effect of unionization on executive pay.

Hypothesis 1. Unionized firms maintain lower CEO pay.

Hypothesis 1a. The lower CEO pay for unionized firms is more pronounced when the unions have a strong bargaining position.

Hypothesis 1b. The lower CEO pay for unionized firms is more pronounced when lower CEO pay is more credible evidence that a firm cannot accede to union demands.

Hypothesis 1c. The lower CEO pay for unionized firms is more pronounced for the part of pay that is readily altered.

However, there are several possible ways that unions can affect CEO compensation. Ertimur et al. (2011) show that union pension funds use shareholder activism, including shareholder proposals, to curb CEO pay. We focus on the possibility that imminent labor negotiations compel firms to curb CEO pay. Unlike the shareholder activism effect, we expect that the labor negotiation effect manifests itself as a distinct time trend in CEO pay around the negotiations. In particular, we expect that firms curtail CEO compensation during the period before negotiations to strengthen their position relative to that of the unions. This leads us to a second set of hypotheses:

Hypothesis 2. Firms curtail CEO compensation prior to union contract negotiations.

Hypothesis 2a. The curtailment in CEO pay is more pronounced when the unions have a strong bargaining position.

Hypothesis 2b. The curtailment in CEO pay is more pronounced when lower CEO pay is more credible evidence that a firm cannot accede to union demands.

Hypothesis 2c. The curtailment in CEO pay is more pronounced for the part of pay that is readily altered.

Finally, we expect that a firm's failure to consider union pressure when setting executive pay could have adverse consequences for the firm and its executives. In the absence of adverse consequences, there would be no reason to curtail executive pay. Because the most visible and arguably costly consequence is unions' strike activity, we propose a third hypothesis:

Hypothesis 3. Firms that have higher CEO pay or that experience increases in CEO pay prior to union negotiations have a higher probability of union strikes.

1.3 Data

We study the population of firms covered by ExecuComp during the period 1993–2011 (excluding financial and utility firms). We require the sample firms to have available information on key variables used in our analysis, including CEO compensation, industry unionization rates, and financial information. Our data requirements yield an initial sample of 18,366 firm-year observations from 1993 to 2011.

1.3.1. 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. Because there is no publicly available database that provides systematic firm-level unionization information, most studies of labor unions assume industry unionization rates to be a reasonable proxy for the unionization rates of individual firms within an industry (Bronars and Deere, 1991; Matsa, 2010; Klasa et al., 2009; Chen et al., 2011). We therefore follow the literature and collect industry unionization rates from the Union Membership and Coverage Database, which reports the percentage of total workers in a three-digit Census Industry Classification (CIC) industry that are represented by unions in collective bargaining agreements.

To account for intra-industry variation in union coverage, we also manually collect firm-level unionization data from firms' SEC filings, whenever available. Specifically, we identify firms' 10-K filings that contain one or more of the following key words: *collective bargaining, collective-bargaining, union(s), labo(u)r agreement(s), labo(u)r contract(s), labo(u)r organization(s).* We then read through each of these filings to obtain union membership information.[7] Because firms are not required to provide union coverage information in their public filings, we are only able to collect firm-level rates for about half of our sample firms. Among these, some firms report their unionization rates directly, while others only disclose the number of employees represented by various unions. In the latter cases we calculate the

[7] Union membership information is normally reported in the "Employees" section of Item 1.

unionization rate by scaling by the total number of employees. Of the sample of 9,013 firm years with firm-level unionization rates, 4,862 firm years are not covered by any collective bargaining agreements, and 4,151 firm years have non-zero union coverage ranging from 0.003%to 96.4%.

1.3.2. Labor contract expirations

We obtain data on labor contract expirations from the BNA Labor Plus database maintained by the Bureau of National Affairs (BNA). Under the National Labor Relations Act, firms with labor union contracts are required to file notices of contract expiration with the Federal Mediation and Conciliation Service (FMCS) to help the FMCS prepare for potential strike mediation.[8] These filings contain information such as employer names, contract expiration dates, bargaining unit size and establishment size. The BNA has collected data on these contract expirations since 1990. In this study, 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, because firms could have 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.

We then merge the BNA database with our main sample using company names. Specifically, we compare firm names in the BNA database with those in COMPUSTAT. Because different abbreviations are used in the BNA and COMPUSTAT data sets, this

[8] 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 contract expires.

comparison could result in matching a contract negotiation to more than one company name. Therefore, we manually confirm each match using information on industry classification, negotiation date, and so forth. For firms that cannot be matched, we attempt to locate them using online resources, including Lexis/Nexis, Factiva, and Bloomberg Businessweek. This allows us to identify subsidiaries of publicly traded parent companies that are dropped in the previous step. We ultimately match 1,721 contract expirations from the BNA database to companies in COMPUSTAT during the 1993–2011 period.

1.3.3 Strike data

We collect strike data from the BNA Labor Plus database and the US Bureau of Labor Statistics (BLS). These databases—constructed with data from published sources, including BNA publications, newspapers, union publications, and government reports—contain information on employer name, strike beginning and ending dates, and number of workers involved in strikes since 1990. We follow the extant literature and focus on major strikes that involve at least 1,000 workers. After merging work stoppages with our main sample based on company names, we obtain 56 strikes during our sample period.

1.4 Empirical results

Table 1 reports summary statistics for the full sample and the subsample with data on firm-level unionization rates. All continuous variables except for unionization rates are winsorized at their 1st and 99th percentiles to reduce the influence of outliers, and all dollar values are adjusted to 2011 dollars. We construct two measures of CEO compensation: (1) cash pay, defined as salary plus bonus; and (2) total pay, defined as the sum of base salary, bonus, long-term incentive payouts, the value of restricted stock grants, and the value of option grants.[9]

For the full sample, the average cash pay and average total pay are $1.38 million and $5.36 million, respectively, suggesting that equity-related compensation accounts for the majority of total CEO pay. The mean (median) industry unionization rate is 0.12 (0.06), lower than the rate reported in Klasa et al. (2009). This is because we cover firms in all industries, whereas Klasa et al. focus on manufacturing firms only, whose unionization rates are generally higher. We also report firm-level unionization rates for the subsample for which this information is available. We find a very comparable mean of 0.13, but a smaller median of 0.00. Because the distribution of the unionization rate at the firm level is skewed to the right, the median for the distribution is expected to be lower than the median for a distribution of averages for subsamples based on industry classifications, for example.

1.4.1 The relation between unionization and CEO compensation

To test our hypothesis that firms facing labor unions maintain lower CEO pay, we examine whether CEOs of firms in more unionized industries receive relatively lower observable total pay than CEOs of firms in less unionized industries. We first conduct pooled OLS regressions of CEO pay on industry unionization rates and a group of control variables. CEO pay is measured as the natural logarithm of total 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 based on prior research in this area (e.g., Smith and

[9] 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.

Watts, 1992; Core et al., 1999; Murphy 1999, Core et al., 2008), including firm size, growth opportunities, stock performance, accounting performance, asset tangibility, and investment rates. To address the concern that unionization acts as a proxy for general industry characteristics that are also correlated with CEO compensation, we include several industry-level variables that proxy for the stage of the industry's life cycle, such as the industry's capital–labor ratio, industry R&D, and industry age. We also include fixed effects for years and two-digit SIC codes in the regressions.

Column 1 of Table 2 presents the results of our baseline model, with *p*-values based on standard errors adjusted for CIC industry clustering. The estimated coefficients on the control variables are generally in line with extant research and have the expected signs. For example, the level of total pay is positively related to firm size, growth opportunities, stock returns, cash flow volatility, and the dual CEO-chairman dummy. Turning to our variable of interest, we find that the coefficient on the industry unionization rate is negative and statistically different from zero at the 1% level, suggesting that CEOs of firms in more unionized industries receive lower total pay than those of firms in less unionized industries. The impact of unionization on CEO pay is also economically meaningful. *Ceteris paribus*, a one-standard-deviation increase in industry unionization is associated with a 9.2% reduction in total compensation. For comparison, we estimate that a one-standard-deviation increase in contemporaneous annual stock returns increases total compensation by 13.4%, and a one-standard-deviation increase in firm size (*log(assets)*) increases total pay by 71.5%. 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; Boschen and Smith, 1995). In column 2, we expand the baseline model by adding more governance variables and conduct the analysis on a subsample for which we have data available from RiskMetrics. The results are the same.

To further address the omitted variable concern, we estimate a change-on-change regression to remove unobserved time-invariant factors correlated with both unionization and CEO pay. In particular, given the limited time-series variation of industry unionization rates, we convert all variables into two-year changes. Unreported results show that changes in CEO pay are negatively related to changes in unionization rates, further suggesting that firms facing more powerful unions set a lower level of CEO compensation.

Another concern is that time trends drive our findings. In fact, private sector unionization has declined over time, while executive compensation has increased dramatically in the past few decades. This concern is mitigated by the year fixed effects in the regressions. However, because different industries could exhibit different time trends during our sample period, we adopt three approaches to strengthen our analysis. First, we conduct a time-series average regression where we convert all variables into time-series averages and estimate a pure cross-sectional regression. The results are presented in column 3. We find that the coefficient on unionization remains negative with even greater magnitude and statistical significance. Second, we estimate a Fama-MacBeth model, where we correct for serial correlation with a lag of one. The results in column 4 confirm a significantly negative relation between unionization and CEO pay. Finally, we repeat our OLS analysis for each year in the period 1993–2011. Untabulated results show that the coefficients on unionization rates are negative for all years and are statistically significant in 14 out of 19 years. These findings suggest that the documented negative relation is not a result of time trends.

Like other studies using industry unionization data, our analysis has a limitation in that it disregards intra-industry variation in union coverage. As a result, our findings might suffer from nontrivial measurement error. To ensure that the estimated coefficients are not significantly biased, we convert all firm-level variables into CIC industry averages and conduct an industry-level regression. This approach can also address the issue that industries with a greater number of firms receive larger weight in earlier tests. The results are reported in column 5, with *p*-values based on standard errors adjusted for CIC industry clustering. We find that the coefficient on unionization is again negative and statistically significant at the 1% level, and the effect of unionization on CEO pay is still economically large.

Finally, in column 6, we reestimate our baseline model using hand-collected firm-level unionization data and report *p*-values based on firm-clustered standard errors. We still find a significantly negative association between unionization and total CEO pay. All else being equal, a one-standard-deviation increase in firm-level unionization is associated with a decrease in total CEO pay of about 6.4 percentage points per year. This magnitude is comparable to the 9.2 percentage points obtained using industry unionization rates. In column 7, we add more governance controls, and our results remain the same.

1.4.1.1 Cross-sectional variation in the union effect

We now examine whether the relation between unionization and executive compensation depends on the bargaining environment. If the negative relation arises because firms consider union negotiations when setting CEO compensation, it should be more pronounced when unions have a relatively strong bargaining position. In addition, the relation should be related to the extent to which executive compensation is a credible signal of the firms' ability to comply with union demands.

First, we investigate how right-to-work laws, which are adopted at the state level, affect the relation between unionization and CEO pay. These laws state that workers should not be obligated to join or give support to a union as a condition of employment, thus weakening union power. We expect that when firms operate in states with right-to-work laws, they rely less on executive compensation to enhance their bargaining position. Using information from the US Department of Labor, we construct an indicator variable that equals one if the state in which a firm operates has right-to-work laws.[10] We then interact this indicator variable with the firm's industry unionization rate. Consistent with Hypothesis 1a, results in column 1 of Table 3 show that the coefficient on the interaction term is negative and statistically significant.

Our second proxy for the bargaining environment is local unemployment rate. Cramton and Tracy (1992) show that higher local unemployment lowers unions' bargaining power. Thus, we expect firms located in states with higher unemployment rates to be less likely to use executive compensation to strengthen their bargaining power. We collect state-level unemployment rates from the BLS and construct a dummy variable that takes a value of one if the unemployment rate in the firm's state is above the sample median. Column 2 shows that the coefficient on the variable that interacts industry unionization with the unemployment dummy is negative and statistically significant, consistent with Hypothesis 1a.

Rose (1991) argues that diversified firms have a better bargaining position relative to unions, because they can cross-subsidize costs associated with union activities, such as strikes. Therefore, following Chen et al. (2011), we construct a Herfindahl index measuring the

[10] As of December 2011, 22 states had passed right-to-work legislation. To determine in which state a firm has the majority of its operations, we use the Compustat variable "STATE".

concentration of a firm's sales across its business segments. A firm is classified as having concentrated business operations if its Herfindahl index is higher than the sample median. In column 3, we report that the negative relation between unionization and CEO pay is more pronounced for concentrated firms, providing even more evidence consistent with Hypothesis 1a.

Last, we examine whether the negative effect is stronger when lower CEO pay serves as more credible evidence that the firm cannot comply with union demands. In particular, we investigate whether firms closer to financial distress are more likely to use lower CEO pay to bargain with labor unions. A firm is defined as financially distressed if its Altman Z-score is below the sample median. The results in column 4 show a significantly negative coefficient on the interaction term between unionization and a distress dummy. This suggests that lower CEO pay provides more of a bargaining advantage for firms that face higher bankruptcy risk, consistent with Hypothesis 1b.

For completeness, column 5 presents a regression that includes all the aforementioned interaction terms. The results confirm our earlier findings. In Panel B of Table 3, we repeat all of these tests using unionization rates measured at the firm level and obtain the same results. Overall, the evidence in Table 3 suggests that strong unions compel firms to curtail executive compensation.

1.4.1.2 Unions' effect on different components of CEO pay

Unlike base salary and to some extent bonuses (which are generally tied to various pre-determined performance metrics), equity grants represent a key component of discretionary compensation to executives. We therefore expect that the effect of unions on CEO compensation to be more pronounced for equity grants. To test this, we decompose CEO compensation and repeat the earlier regressions for the different components. Table 4 reports the results.

In columns 1 and 2, the dependent variables are the logarithm of cash pay and the logarithm of equity pay, respectively. The independent variable of interest is the industry-level unionization rate. Unionization has no significant impact on the level of cash pay, but the effect on total equity-based pay is negative and statistically significant at the 1% level. *Ceteris paribus*, a one-standard-deviation increase in industry unionization is associated with a 14.7% reduction in total equity pay. Columns 3 and 4 show that the results are similar when we replace industry unionization rates with firm-level unionization rates. These results are consistent with Hypothesis 1c. Untabulated results also reveal that the cross-sectional variation in the union effects in Table 3 is primarily attributable to equity pay.

1.4.2 CEO compensation around contract negotiations

Prior studies have found some evidence that unions curb excessive pay by condemning pay disparity between managers and workers. To test more directly whether firms consider their bargaining position vis-à-vis unions when setting CEO pay, we examine patterns of CEO pay around the time of union negotiations.

Using labor contract expiration data from the BNA Labor Plus database, we calculate the percentage of employees involved in scheduled union contract negotiations in each year, denoted as *Negotiation ratio*. Panel A of Table 5 shows that 518 firm-years in our sample have at least one labor contract negotiation, with an average of 16.5% of the total labor force involved in a year.

We first employ univariate tests to examine whether unionized firms reign in CEO compensation prior to labor contract negotiations. More specifically, we examine median CEO compensation from year −2 to year +2 relative to the negotiation year. The compensation measure is either unadjusted total pay or abnormal pay, where the latter is defined as the residual of the baseline regression in column 1 of Table 2. Panel B of Table 5 reports the results of our analysis. We find an upward trend of total CEO compensation from year −2 to year +2, but the growth rate in total CEO compensation in year −1 is significantly lower than the growth rate in the other three years (years 0, +1, and +2). When we remove the effects of factors that influence CEO compensation (including general time trends and unionization rates), we find that the abnormal pay is positive in all years except in year −1. This is consistent with Hypothesis 2.

Next, we turn to multivariate tests in which we regress CEO pay in a fiscal year on *Negotiation ratio* in the subsequent fiscal year and various control variables for the same year as the CEO pay. By definition, firms with no union or with no union contract expiration during the year have a negotiation ratio of zero. The other control variables are the same as those used in Table 2, and their coefficients are suppressed for brevity. The full sample regression in column 1 shows that there is a significantly negative relation between CEO pay and the negotiation ratio in the following year, suggesting that firms are more likely to curtail CEO pay when they face union contract negotiations involving more workers. *Ceteris paribus*, a one-standard-deviation increase in negotiation ratio is associated with an average 7.5% decline in total CEO compensation in the fiscal year prior to negotiation. In column 2, we conduct the same analysis on a subsample for which we have corporate governance information available. The estimated coefficient on negotiation ratio is again negative and statistically significant at the 1% level. In columns 3 and 4, we replace the industry unionization rate with the firm-level unionization rate and repeat the analyses in columns 1 and 2. The results remain the same. In sum, the negative coefficients on the negotiation ratio suggest that firms curtail CEO compensation in the fiscal year preceding union contract negotiations. This is consistent with Hypothesis 2.

The coefficients on the unionization rates in Table 5 are negative and statistically significant from zero, irrespective of the unionization rates are estimated at the industry or firm level. Combined with the coefficients on the negotiation ratio, these results suggest that unionization is associated with lower CEO pay, especially in the year prior to labor negotiations, but also in other years. Thus, the lower CEO pay for unionized firms seems not only to be a temporary phenomenon as the firms are preparing for negotiations, but a persistent effect across all years. This is consistent with Ertimur et al. (2011), who show that union pension funds use shareholder proposals to constrain CEO pay. As such, union pressure arising from labor negotiations and shareholder activism combine to limit CEO pay, and are not mutually exclusive mechanisms in affecting firms' decisions.

1.4.2.1 The cross-sectional variation in the effect of labor negotiations

Analogous to our analysis of the relation between unionization and CEO pay, we explore the cross-sectional variation in the effects of union negotiation on CEO compensation. As stated in Hypothesis 2a, we expect the effect of negotiation to be more pronounced when the union is relatively strong. Moreover, as stated in Hypothesis 2b, we expect a stronger effect when lower CEO pay is more credible evidence that the firm cannot concede to union demands.

Table 6 presents our regression results. The choice of interaction variables and the underlying logic is the same as for Table 3. Consistent with Hypothesis 2a, we find that the negative relation between labor negotiation and CEO pay is more pronounced for firms located in states with no right-to-work laws, for firms located in states with lower unemployment rates, and for firms with more concentrated business operations. Consistent with Hypothesis 2b, the relation is more pronounced for firms that are closer to financial distress. These results corroborate the notion that executive pay trends around union negotiations are designed to improve the firms' bargaining situation vis-à-vis labor unions.

1.4.2.2 CEO option grants surrounding union contract negotiations

We next study the pattern of different compensation components around union negotiations. As stated in Hypothesis 2c, we expect the empirical pattern to be stronger for equity-based compensation. Option grants represent a key part of discretionary equity pay, as the board often has significant leeway in both how many options to grant and when to grant these options. Therefore, we examine individual option grants most closely.

Stock options can be issued to CEOs on a scheduled or an unscheduled basis. Lie (2005) and Heron and Lie (2007) find evidence that firms opportunistically "time" unscheduled grants. This suggests that it is easier for firms to manipulate CEO pay prior to negotiations using unscheduled option grants. We define option grants to be scheduled if they occur within two days of the one-year anniversary of the prior year's award date; if the grants do not occur within two days of this anniversary or if no options were awarded during the prior year, they are defined to be unscheduled. We then aggregate the value of scheduled or unscheduled options in each year using the value reported by the company.

Table 7 reports the results. In columns 1 and 2, the dependent variable is the logarithm of cash pay and the logarithm of total equity pay, respectively. We find that the negotiation ratio has no significant impact on the level of cash pay, but has a significantly negative impact on total equity-based pay. In columns 3 and 4, the dependent variable is the logarithm of scheduled grants and the logarithm of unscheduled grants, respectively. These regressions are based on the subsample of firm years with detailed option grant information.[11] We find that the value of scheduled option grants is significantly lower when there are more employees involved in labor contract negotiations in the subsequent year. The negative impact is even stronger for unscheduled option grants. *Ceteris paribus*, a one-standard-deviation increase in the negotiation ratio is associated with a 28.7% reduction in total unscheduled option grants. These results are consistent with Hypothesis 2c.

1.4.2.3 Additional robustness checks

DeAngelo and DeAngelo (1991) report executive pay cuts prior to union negotiations for a small set of distressed firms. To test whether the effects we document are attributable to labor negotiations of financially distressed firms, we redo our analysis while excluding such firms. We define firms to be financially distressed if their Altman Z-score is below 1.8 prior to labor contract negotiations. The results (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 times of distress.

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

[11] We focus the analysis after 2005 because the option grant date is not available in ExecuComp until 2006.

executives as a group experience a temporary curtailment in compensation prior to union contract negotiations.

1.4.3 Evidence from labor strikes

Our earlier analyses suggest that firms curtail CEO pay in anticipation of labor negotiations, ostensibly in an attempt to strengthen their bargaining power relative to that of unions. To rationalize this strategic pay curtailment, there must be some benefit for the firms. It is hard to measure the effect on the negotiation outcome because (i) we do not know what the contracts would have been in the absence of the preceding CEO compensation curtailment; (ii) 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; and (iii) we do not always have information about the outcome of the negotiations, which could limit the generalization and statistical power of the results. Instead, we focus on unions' decisions to initiate strikes in response to negotiation breakdowns, because strikes are both easy to identify and costly to the firm. As Hypothesis 3 states, we expect that firms that fail to curb CEO compensation prior to labor negotiations have a higher propensity for labor strikes.

To test our hypothesis, we follow Klasa et al. (2009) by matching our sample firms that experience strikes with firms in the same four-digit SIC industry but do not experience a strike in the same year. We then estimate a probit regression in which the dependent variable takes a value of one if a firm experiences a strike in the year and zero otherwise. The main independent variables of interest are the level of CEO pay and the change in CEO pay during the previous fiscal year.

We include a number of controls in the analysis. First, we use industry unionization rate to measure the ability of unions to organize the labor force. We also control for changes in the financial strength of the firm during the pre-strike year, including cash holdings, leverage, profitability, liquidity, and financial distress (proxied by Altman's Z-score). Finally, we control for firm size, growth opportunities, whether a firm is principally located in a state with right-to-work laws, and year and industry fixed effects.

Table 8 provides the results of our probit analysis. The results in column 1 show that there is a significantly positive association between the prior year CEO pay and the likelihood that a firm subsequently experiences a strike. The prior year change in CEO pay is also positively related to the likelihood of a strike. The marginal effect of CEO compensation is economically meaningful. A one-standard-deviation increase (around the mean) in the level of total pay increases the probability of a strike by approximately 1.4%, and a one-standard-deviation increase in the change in total pay increases the strike probability by 2.7%. Given that strikes are costly events to firms (Becker and Olson 1986), our results provide a rationale for curbing executive pay prior to negotiating with unions. That is, the failure to curb CEO pay increases the probability of a breakdown in labor negotiations and a costly labor strike.

Turning to our control variables, we find that the estimated coefficients are generally consistent with previous studies. Notably, firms experiencing increases in cash holdings, profitability, and liquidity are subsequently more likely to have a strike (DeAngelo and DeAngelo, 1991; Cramton and Tracy, 1992; Klasa et al., 2009). Finally, in the second model, we control for more variables, including stock performance, stock return volatility, inventory ratio, capital and labor intensity (Tracy, 1986). This does not affect the main results shown in the first model.

1.5 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, cut dividend payments, and even manipulate earnings in an attempt to dodge union demands. We extend this literature by examining whether firms also strategically set 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 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 negotiations with such unions.

We find a significantly negative relation between executive compensation and unionization. The negative association is more pronounced for firms located in states with no right-to-work laws, for firms located in states with lower unemployment rates, for firms with more concentrated business, and for firms that are closer to financial distress. These findings suggest that the negative relation between CEO pay and unionization rates is, at least partially, due to union pressure to constrain CEO pay.

We next examine CEO compensations around union contract expirations. We find that unionized firms curtail CEO compensation in the fiscal year preceding union contract negotiations. Moreover, the curtailment is greater when the unions are strong and when lower CEO pay is more credible evidence that the firm cannot concede to union demands. We also find that the decline of executive compensation in the fiscal year preceding union contract negotiations is most evident for option grants, particularly if the grants are unscheduled.

Finally, we find that firms with higher CEO pay or with recent increases in CEO pay prior to labor negotiations are more likely to experience a union strike. This shows that labor unions respond to the level of executive compensation, thereby providing a rationale for firms to use CEO pay as a strategic variable in their interactions with labor unions.

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 also use 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 1: Summary statistics

	Full sample (*N* = 18,366)			Subsample with firm unionization rate (*N* = 9,013)		
	Mean	Median	s.d	Mean	Median	s.d.
Cash pay (in millions)	1.38	0.97	1.79	1.29	0.92	1.62
Total pay (in millions)	5.36	2.85	2.54	5.01	2.68	9.60
Unionization rate	0.12	0.06	0.13	0.13	0.00	0.21
CEO tenure	7.92	5.67	7.47	8.08	5.92	7.35
Dummy (CEO is chair)	0.57	1.00	0.50	0.54	1.00	0.50
Total assets	6,842	1,585	20,582	5,047	1,322	13,129
Firm size	7.40	7.28	1.61	7.23	7.11	1.53
Book-to-market	0.63	0.63	0.26	0.64	0.64	0.27
Leverage	0.22	0.22	0.18	0.23	0.22	0.19
Annual return	0.16	0.09	0.53	0.17	0.09	0.56
Lag annual return	0.19	0.11	0.58	0.18	0.09	0.59
ROA	0.04	0.05	0.11	0.04	0.05	0.11
Lag ROA	0.04	0.05	0.11	0.04	0.05	0.11
Stock return volatility	0.12	0.11	0.07	0.13	0.11	0.07
Tangibility	0.32	0.25	0.25	0.32	0.25	0.25
Sales growth	0.07	0.08	0.22	0.07	0.08	0.23
Investment	0.06	0.05	0.06	0.06	0.04	0.06
R&D	0.06	0.00	0.14	0.06	0.00	0.15

Note: This table presents summary statistics for the full sample and the subsample with data on firm-level unionization rate. The full sample consists of 18,366 firm-years during the period 1993-2011. This corresponds to all firms in the ExecuComp database with no-missing data on the main control variables used in our later analyses. Unionization rate is measured at the industry level and calculated as the percentage of total workers in a 3-digit Census Industry Classification (CIC) industry that are represented by unions in collective bargaining agreements. The subsample consists of 9,013 firm-year observations, and unionization rate is calculated as the percentage of total workers represented by unions in a given firm. All continuous variables except for unionization rates are winsorized at the 1st and 99th percentiles and all dollar values are adjusted to 2011 dollars.

Table 2: CEO compensation and unionization

	Industry unionization rate					Firm unionization rate	
	Pooled OLS		Firm time-series means	Fama -MacBeth	Industry level	OLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Unionization rate	-0.737***	-0.788***	-1.008***	-0.754**	-0.643**	-0.303***	-0.342**
	(0.001)	(0.009)	(0.000)	(0.017)	(0.013)	(0.008)	(0.012)
Firm size	0.461***	0.444***	0.487***	0.456***	0.449***	0.469***	0.445***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Stock return	0.253***	0.282***	0.264***	0.247***	0.229***	0.252***	0.286***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lagged stock return	0.114***	0.117***	0.125***	0.106***	0.143***	0.121***	0.136***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ROA	0.108	0.159	0.171	0.148	0.192	0.131	0.193
	(0.646)	(0.327)	(0.467)	(0.413)	(0.348)	(0.661)	(0.324)
Lagged ROA	-0.178	-0.191	-0.228	-0.201	-0.111	-0.161	-0.201
	(0.518)	(0.453)	(0.386)	(0.252)	(0.792)	(0.621)	(0.523)
Lagged Leverage	-0.062	-0.057	-0.082	-0.099	-0.143	-0.061	-0.027
	(0.502)	(0.529)	(0.381)	(0.399)	(0.330)	(0.554)	(0.833)
Lagged Book-to-market	-0.719***	-0.738***	-0.689***	-0.672***	-0.548***	-0.635***	-0.763***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lagged Volatility	0.453***	0.532***	0.654***	0.687**	0.634***	0.437**	0.502***
	(0.001)	(0.000)	(0.000)	(0.036)	(0.002)	(0.011)	(0.000)
Lagged Investment	0.534*	0.444	0.404*	0.334	0.278	0.415	0.391
	(0.070)	(0.137)	(0.097)	(0.121)	(0.346)	(0.148)	(0.145)
Lagged Tangibility	-0.442***	-0.354***	-0.513***	-0.347***	-0.431**	-0.371***	-0.341**

Table 2 Continued

	(0.000)	(0.002)	(0.000)	(0.001)	(0.029)	(0.004)	(0.036)
Lagged Sale growth	0.054	0.034	0.043	0.032	0.077*	0.047	0.026
	(0.104)	(0.401)	(0.315)	(0.518)	(0.065)	(0.108)	(0.502)
Lagged R&D	0.003	0.002	0.002	0.007	0.005	0.002	0.002
	(0.316)	(0.279)	(0.665)	(0.159)	(0.149)	(0.289)	(0.159)
CEO tenure	-0.006**	-0.006**	-0.005**	-0.006**	-0.004*	-0.007**	-0.008
	(0.025)	(0.034)	(0.039)	(0.019)	(0.075)	(0.022)	(0.024)
Dummy (CEO is chair)	0.139***	0.153***	0.151***	0.138***	0.121***	0.142***	0.168***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Industry capital labor	0.001	0.000	0.001	0.001	0.001	0.000	0.000
	(0.211)	(0.247)	(0.172)	(0.312)	(0.166)	(0.231)	(0.291)
Industry R&D	0.001	-0.001	0.002	-0.002	0.001	0.0001	-0.002
	(0.611)	(0.845)	(0.745)	(0.644)	(0.709)	(0.749)	(0.556)
Ln (Industry age)	-0.018	-0.019	0.017	0.029	-0.051	-0.019	-0.021
	(0.533)	(0.639)	(0.698)	(0.424)	(0.187)	(0.556)	(0.573)
Board size		0.017					0.019
		(0.218)					(0.247)
Board independence		-0.039					-0.035
		(0.151)					(0.182)
G-index		0.012					0.008
		(0.121)					(0.381)
Year dummies	Yes	Yes	No	No	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	No	Yes	Yes
R-squared	0.509	0.479	0.629	0.445	0.556	0.454	0.431
# of observations	18,366	8,270	2,154	19	2,108	9,013	4,060

Table 2 Continued

Note: This table reports results of OLS regressions of CEO pay on unionization and control variables. The full sample consists of 18,366 firm years during the period 1993–2011, as described in Table 1. The dependent variable is the natural logarithm of CEO 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 independent variable of interest is unionization rate, measured at the 3-digit CIC industry level in models 1–5 and at the firm level in models 6–7. Models 1 and 2 are pooled OLS regressions. Model 3 uses firm-level time-series average of each variable. Model 4 is a Fama-MacBeth model. Model 5 uses annual means of variables for 3-digit CIC industries. Models 6 and 7 are OLS regressions on a subsample of 9,013 firm years with data available on firm-level unionization rate. Year fixed effects are included in all regressions expect for model 3. Fixed effects for 2-digit SIC codes are included in all regressions except model 5. The regression coefficients of these fixed effects are suppressed for brevity. *P*-values based on standard errors adjusted for clustering are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 3: The determinants of the relation between unionization and CEO pay

Panel A: Industry unionization rate	(1)	(2)	(3)	(4)	(5)
Unionization rate	-0.864***	-0.812***	-0.509**	-0.485**	-0.517*
	(0.000)	(0.000)	(0.024)	(0.018)	(0.086)
Unionization rate × Right-to-work dummy	0.533***				0.547***
	(0.002)				(0.008)
Right-to-work dummy	-0.061*				-0.066
	(0.079)				(0.243)
Unionization rate × Local unemployment dummy		0.283**			0.205*
		(0.026)			(0.063)
Local unemployment dummy		-0.038			-0.041
		(0.172)			(0.156)
Unionization rate × Business concentration dummy			-0.361**		-0.399**
			(0.031)		(0.038)
Business concentration dummy			0.029		0.038
			(0.568)		(0.451)
Unionization rate × Firm distress dummy				-0.384**	-0.395**
				(0.016)	(0.029)
Firm distress dummy				-0.021	-0.017
				(0.536)	(0.639)
Firm and industry controls	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes
R-squared	0.511	0.511	0.508	0.509	0.511
# of observations	18,366	18,366	18,366	18,366	18,366

Table 3 Continued

Panel B: Firm unionization rate	(1)	(2)	(3)	(4)	(5)
Unionization rate	-0.436***	-0.318***	-0.253**	-0.279**	-0.201
	(0.000)	(0.007)	(0.041)	(0.038)	(0.183)
Unionization rate × Right-to-work dummy	0.519***				0.526***
	(0.001)				(0.003)
Right-to-work dummy	-0.064*				-0.069
	(0.085)				(0.179)
Unionization rate × Local unemployment dummy		0.241**			0.219**
		(0.018)			(0.039)
Local unemployment dummy		-0.038			-0.041
		(0.233)			(0.185)
Unionization rate × Business concentration dummy			-0.209*		-0.206*
			(0.074)		(0.083)
Business concentration dummy			0.022		0.012
			(0.457)		(0.486)
Unionization rate × Firm distress dummy				-0.236*	-0.249*
				(0.061)	(0.072)
Firm distress dummy				-0.046	-0.031
				(0.238)	(0.396)
Firm and industry controls	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes
R-squared	0.461	0.445	0.459	0.454	0.461
# of observations	9,013	9,013	8,945	9,013	8,945

Note: This table reports results of OLS regressions of CEO pay on unionization, interaction terms, and control variables. The full sample consists of 18,366 firm years during the period 1993–2011, as described in Table 1. The dependent variable is the natural logarithm of CEO 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 independent variables of interest are unionization rate and its interactions with dummy variables indicating firms located in states with right-to-work laws, firms located in states with unemployment rate above the sample median, firms with business concentration index higher than the sample median, or firms with a Z-score below the sample median. Unionization rate is measured at the 3-digit CIC industry level in Panel A, and at the firm level in Panel B. As in model 1 of Table 2, all regressions control for firm and industry characteristics, year fixed effects and industry (2-digit SIC) fixed effects. The regression coefficients of these control variables are suppressed for brevity. *P*-values, which are reported in parentheses, are adjusted for industry-clustering in Panel A and for firm-clustering in Panel B. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 4: The effect of unionization on different components of CEO pay

	(1) Log (Cash + Bonus)	(2) Log (Equity pay)	(3) Log (Cash + Bonus)	(4) Log (Equity pay)
Industry unionization rate	-0.152	-1.172***		
	(0.302)	(0.001)		
Firm unionization rate			-0.077	-0.674***
			(0.357)	(0.004)
Firm and industry controls	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
R-squared	0.491	0.307	0.473	0.275
# of observations	18,366	18,366	9,013	9,013

Note: This table reports results of OLS regressions of different components of CEO pay on unionization and control variables. The full sample consists of 18,366 firm years during the period 1993–2011, as described in Table 1. The dependent variable in models 1 and 3 is the natural logarithm of total cash pay, defined as salary plus bonus. The dependent variable in models 2 and 4 is the natural logarithm of total equity-based pay, defined as CEO total compensation less total cash pay. The independent variables of interest are industry unionization rate and firm unionization rate. All other independent variables are the same as those in model 1 of Table 2, whose coefficients are suppressed for brevity. *P*-values, which are reported in parentheses, are adjusted for industry-clustering in models 1 and 2, and for firm-clustering in models 3 and 4. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 5: The effect of union contract negotiations on CEO pay

Panel A: Negotiation ratio in firm years with union contract negotiations

N	Mean	P25	Median	P75	Std
518	0.165	0.025	0.063	0.186	0.235

Panel B: Median CEO pay surrounding contract negotiations

Year relative to negotiation	-2	-1	0	1	2
Total pay ($ millions)	6.03	6.08	6.34	7.13	7.49
Abnormal pay ($ millions)	0.074	-0.083	0.031	0.061	0.064

Panel C: The relation between negotiation ratio and CEO pay

	(1)	(2)	(3)	(4)
Negotiation ratio	-0.322***	-0.441***	-0.414**	-0.493**
	(0.007)	(0.009)	(0.022)	(0.035)
Industry unionization rate	-0.721***	-0.794***		
	(0.001)	(0.004)		
Firm unionization rate			-0.293**	-0.351**
			(0.016)	(0.028)
Firm and industry controls	Yes	Yes	Yes	Yes
Governance controls	No	Yes	No	Yes
Year dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
R-squared	0.507	0.478	0.455	0.433
# of observations	18,366	8,270	9,013	4,060

Table 5 Continued

Note: This table reports the analysis of CEO compensation surrounding union contract negotiations. Panel A presents summary statistics for a sample of 518 firm years that experience at least one union contract negotiation. *Negotiation ratio* is calculated as the ratio of a firm's employees involved in scheduled union contract negotiations in a given year. Panel B reports median CEO compensation from year −2 to year +2, relative to the negotiation year, based on (1) unadjusted total pay and (2) abnormal pay, defined as the residual of the baseline regression in model 1 of Table 2. Panel C reports results of OLS regressions of CEO pay on negotiation ratio and control variables. The full sample consists of 18,366 firm years during the period 1993–2011, as described in Table 1. The dependent variable is the natural logarithm of CEO 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 independent variables of interest are negotiation ratio and unionization rate. All other independent variables are the same as those in model 1 or model 2 of Table 2, and their coefficients are suppressed for brevity. All independent variables are for the same year as the CEO compensation, except *Negotiation ratio*, which is for the following year. *P*-values based on standard errors adjusted for firm-clustering are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 6: The determinants of the relation between union negotiation and CEO pay

	(1)	(2)	(3)	(4)	(5)
Negotiation ratio	-0.504***	-0.422***	-0.283**	-0.298**	-0.767***
	(0.001)	(0.005)	(0.032)	(0.037)	(0.008)
Negotiation ratio × Right-to-work dummy	0.632***				0.723***
	(0.000)				(0.000)
Right-to-work dummy	-0.053*				-0.051*
	(0.089)				(0.085)
Negotiation ratio × Local unemployment dummy		0.494**			0.513**
		(0.018)			(0.021)
Local unemployment dummy		-0.027			-0.023
		(0.308)			(0.283)
Negotiation ratio × Business concentration dummy			-0.327**		-0.302**
			(0.034)		(0.041)
Business concentration dummy			0.025		0.022
			(0.538)		(0.569)
Negotiation ratio × Firm distress dummy				-0.272*	-0.116
				(0.081)	(0.666)
Firm distress dummy				-0.015	-0.018
				(0.653)	(0.580)
Firm and industry controls	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes
R-squared	0.503	0.502	0.502	0.504	0.503
# of observations	18,366	18,366	18,366	18,366	18,366

Note: This table reports results of OLS regressions of CEO pay on negotiation ratio, interaction terms, and control variables. The full sample consists of 18,366 firm years during the period 1993–2011, as described in Table 1. The dependent variable is the natural logarithm of CEO 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 independent variables of interest are negotiation ratio and its interactions with dummy variables indicating firms located in states with right-to-work laws, firms located in states with unemployment rates above the sample median, firms with a business concentration index higher than the sample median, or firms with a Z-score below the sample median. All other independent variables are the same as those in Table 3 Panel A, and their coefficients are suppressed for brevity. All independent variables are for the same year as the CEO compensation, except *Negotiation ratio*, which is for the following year. *P*-values based on standard errors adjusted for firm-clustering are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7: The effect of union contract negotiations on different components of CEO pay

	(1)	(2)	(3)	(4)
	Log (Cash + Bonus)	Log (Equity pay)	Log (Scheduled grant)	Log (Unscheduled grant)
Negotiation ratio	-0.053	-0.539**	-1.788**	-2.293***
	(0.415)	(0.026)	(0.046)	(0.001)
Firm and industry controls	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
R-squared	0.399	0.319	0.174	0.141
# of observations	18,366	18,366	5,223	5,223

Note: This table reports results of OLS regressions of different components of CEO pay on negotiation ratio and control variables. The full sample consists of 18,366 firm years during the period 1993–2011, as described in Table 1. The dependent variable in model 1 is the natural logarithm of total cash pay, defined as salary plus bonus. The dependent variable in model 2 is the natural logarithm of total equity-based pay, defined as CEO total compensation less total cash pay. The dependent variables in models 3 and 4 are the natural logarithm of total scheduled option grants and the natural logarithm of total scheduled option grants, respectively. The independent variable of interest is negotiation ratio. All other independent variables are the same as those in model 1 of Table 2, and their coefficients are suppressed for brevity. All independent variables are for the same year as the CEO compensation, except *Negotiation ratio*, which is for the following year. *P*-values based on standard errors adjusted for firm-clustering are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 8: The effect of CEO pay on the likelihood of strike

	(1)	(2)
Pre-strike year Log (pay)	0.014**	0.012*
	(0.043)	(0.061)
Pre-strike year change in Log (pay)	0.027**	0.025**
	(0.015)	(0.021)
Pre-strike year union coverage	0.016	0.017
	(0.331)	(0.293)
Pre-strike year change in cash holdings	0.024**	0.025**
	(0.023)	(0.026)
Pre-strike year change in total leverage	-0.002	-0.003
	(0.467)	(0.421)
Pre-strike year change in operating income/total assets	0.021*	0.019*
	(0.078)	(0.085)
Pre-strike year change in net working capital/total assets	0.025**	0.028**
	(0.032)	(0.026)
Pre-strike year change in Altman Z-score	-0.014	-0.013
	(0.231)	(0.272)
Pre-strike year in Book-to-Market	-0.003	-0.003
	(0.609)	(0.643)
Natural logarithm of real market value of assets	0.036***	0.034***
	(0.001)	(0.001)
Firm is primarily located in a state with right-to-work laws	-0.011	-0.012
	(0.167)	(0.145)
Pre-strike year stock return		-0.036*
		(0.066)
Pre-strike year stock volatility		-0.006
		(0.659)
Pre-strike year inventory/Sales		0.013
		(0.278)
Pre-strike year fixed assets/total assets		0.007
		(0.187)
Pre-strike year # of employees/total assets		0.021
		(0.229)
Year dummies	Yes	Yes
Industry dummies	Yes	Yes
# of observations	338	338

Table 8 Continued

Note: This table reports results of probit regressions of the probability that a firm experiences a strike in a given year. The sample consists of 56 firms that experience a strike over the period 1993–2011 and 282 matched firms that do not experience a strike for which we are able to collect necessary data for all variables that appear in the regression models. The dependent variable equals one if a firm experiences a strike and zero if a firm is a control firm that does not experience a strike. The independent variables of interest are Pre-strike year change in Log (pay) and Pre-strike year Log (pay). Year-fixed effects and the industry (2-digit SIC industry) fixed effects are included in both regressions, whose coefficients are suppressed for brevity. Estimated marginal effects are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

CHAPTER 2. UNION CONCESSIONS FOLLOWING ASSET SALES AND TAKEOVERS

2.1 Introduction

Courts, arbitrators, and the National Labor Relations Board (NLRB) have struggled for decades to define the rights and obligations of buyers and sellers of businesses that have unionized workers. The form of the transaction that alters corporate ownership, i.e., takeovers versus asset sales, largely dictates the rights and obligations of the buyer of the corporation (Wheeler and Murray (1991)). Union-related obligations typically survive the transfer of ownership following takeovers, and the surviving firm must recognize and bargain with the union and abide by the terms of the collective agreement as if no change occurred. This stands in contrast to asset sales, in which the buyer is generally not required to assume any existing collective agreement, and is free to extract whatever concessions it can from the employees of the seller.

Anecdotal evidence suggests that asset sales play an important role in gaining concessions from unions. For instance, Hostess Brands Inc. closed its factories in November, 2012, after failing to reach an agreement with its striking bakers' union on concessions. While Hostess aimed to sell Twinkies and other snack cake brands, the Teamsters, which was the company's largest union, objected the sale, arguing that "The sale process has only insured that the brands may live on – none of the buyers have made any comments to employ former Hostess workers let alone honor the terms of conditions of their employment with Hostess – in fact they specifically stipulated that none of the obligations carry forward as part of their bids." Despite the rejections of its unions, the sale of Twinkies to a pair of investment firms was completed in

June, 2013, with the buyers unlikely to rely as heavily on a unionized work force as the old Hostess did.

Atlantic Express Transportation Corp. offers another recent example. In late 2013, the company threatened in a news release that it would pursue an asset sale if it were "unable to reach agreement with [the union representing its employees] on a new collective-bargaining agreement and obtain additional financing." The company's president and CEO, David Carpenter, said in the release, "Quite simply, our current business model in our largest market, New York City, is not sustainable as union labor costs and operating expenses have severely hindered our ability to remain competitive."

A common feature of both examples is that asset sales, as opposed to takeovers, are pursued to obtain concessions from unions. In this paper, we examine the relation between union wealth concessions and asset sales versus takeovers. First, we examine the effect that union wages have on these two types of transactions. If unionized employees are paid more than their peers, the firm's managers have an incentive to remove these overpaid employees. But a prospective buyer would primarily be interested if it can set aside the unfavorable union-related obligations. Thus, we conjecture that a higher wage differential between a firm's unionized employees and average employees in the same industry increases the probability of an asset sale, but not the probability of takeovers. Results from logistic regressions support our conjecture.

To gauge the magnitude of union concessions, Rosett (1990) examines annual real wage growth following takeovers. He documents a weak decline in wage growth, consistent with the continuing employer in a takeover being obliged to adopt the substantive provisions of the collective bargaining agreement. In our sample, the takeovers have no detectable effect on wage growth. But we find a decline in wage growth following asset sales that is both statistically and economically significant. One year following the asset sale, the seller's union employee lost, on average, $3.25 million in 2009 dollars, representing about 3% of buyers' shareholder value. Over three years, we estimate that union members lost an average of $19.35 million, representing 18% of buyers' shareholder value. These results show that total union wealth concessions following asset sales are substantial compared with total shareholder wealth, and are likely to be a strong motivation behind these transactions.

We further examine whether union concessions explain the abnormal stock returns around announcements of assets sales and takeovers. To do so, we develop several measures designed to capture economic importance of the potential union concessions. These measures are based on the unionization rate, the union wage premium, and the relative transaction value. Our results show that the potential for union concessions explain a significant portion of the announcement returns around asset sales for both the buyer and seller, but there is no comparable effect for takeovers.

Prior studies indicate that right-to-work (RTW) laws, which prohibit unions from making membership or payment of union dues a condition of employment, weaken union bargaining power (Ellwood and Fine (1987), Holmes (1998), Klasa, Maxwell et al. (2009), Matsa (2010)). In our final analyses we therefore examine the effect of RTW laws. We find that our earlier results are primarily attributable to asset sales where the selling firms are located in states without RTW laws. In particular, union wage premiums increase the likelihood of asset sales in states without RTW laws, but not in states with RTW laws. Furthermore, unionization only affects asset sale announcement returns when the selling firms are located in states with RTW laws. These results corroborate our conclusion that firms use asset sales to extract concessions from powerful labor unions.

Our study contributes to the literature on whether the reduction in union rents can serve as a source of shareholder gains in takeovers. Prior studies find scant evidence of a relation between union rents and shareholder gains in takeovers (Becker (1995), Rosett (1990). We find that, because union-related obligations survive the transfer of ownership following a takeover, it is not surprising that acquiring firms fail to extract meaningful concessions from the incumbent unions. We further contribute to the literature that investigates the determinants of and sources of gains from asset sales. Some earlier studies on assets sales have emphasized the efficiency resulting from reallocation of assets to higher valued buyers as the primary determinant of gains in selloffs (Alexander, Benson et al. (1984), Hite, Owers et al. (1987), Maksimovic and Phillips (2001)). Our study is generally consistent with this literature, but points to high union wages as the particular source of efficiency gains. Finally, we contribute to the literature that examines the effect of labor laws on corporate restructuring. Atanassov and Kim (2009) show that highly protective union laws induce more asset sales when firms are in distress. The authors attribute these asset sales to joint efforts between managers and unions to avoid dismissals. Similarly, we find that protective union laws, in the form of RTW laws, induce asset sales, but primarily when firms have highly paid unionized workers. And, in contrast to Atanassov and Kim (2009), we conclude that asset sales in an environment of strong unions are designed to harm workers.

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

2.2 Motivation and related literature

2.2.1 The law on successorship following mergers and acquisitions

The law of successorship determines whether or which obligations of a predecessor employer will be imposed upon a successor or purchaser. The form and nature of the transaction that alters corporate ownership determines, to some extent, the rights and obligations of the purchaser or succeeding owner of the corporation (Wheeler and Murray (1991)).

2.2.1.1 Takeovers

In cases where there is a sale or transfer of stock and no change in corporate form, the continuing employer is obliged to adopt the substantive provisions of the collective bargaining agreement, as well as to recognize and bargain with the incumbent union. For example, in EPE, Inc. v NLRB (845 F2d 483, 4th Cir 1988), the court enforced the NLRB's order holding that EPE remained obligated to abide by the terms of its collective bargaining agreement with the Amalgamated Clothing and Textile Workers Union, AFL-CIO after 100% of EPE's stock was purchased by Echlin, Inc.

2.2.1.2 Asset sales

Generally, the buyer of assets is not bound to the substantive provisions of the collective bargaining agreement, but might incur other obligations as a successor employer.[12] The leading case that sets forth the legal requirements of a successor to honor the substantive provisions of a

[12] However, if the buyer is deemed to be the "alter ego" of the predecessor, the purchaser is bound by the substantive terms of the collective bargaining agreement between the predecessor employer and the union. Alter ego status is found where, subsequent to a change in corporate form, substantially identical management, business purpose, operations, equipment, customers, supervision, and ownership remain.

collective bargaining agreement between the acquired firm and its workers is the Supreme Court case NLRB. V. Burns International Security Services, Inc., 406 U.S.272 (1972). Lockheed Aircraft Company contracted for security at one of its plants with Wackenhut Corporation. Wackenhut had entered into a collective bargaining agreement with the United Plant Workers, a union certified by the NLRB. When Wackenhut’s service contract expired, Lockheed hired a new security firm, Burns Security. Burns retained 27 of the 42 original Wackenhut employees but refused to honor the terms of the previous agreement with Wackenhut and refused to bargain with the union. The NLRB found that Burns had violated the National Labor Relations Act by refusing to negotiate with the union and refusing to honor the collective bargaining agreement. The case reached the Supreme Court where Justice Byron White, writing for the majority, ruled that Burns was obligated to negotiate with the union, but that “it does not follow ... from Burns’ duty to bargain that it was bound to observe the substantive terms of the collective bargaining contract the union had negotiated with Wackenhut and to which Burns had in no way agreed” (NLRB v. Burns, 406 U.S. 281-82).

While NLRB v. Burns does not involve an asset sale, it is the guiding case in this area. An acquiring firm must bargain with the union following the sale, but it is not necessarily bound by the terms of any previous agreements. Thus, following the transaction, the acquirer is free to attempt to extract whatever concessions it can, but the union is under no greater obligation to make concessions to the acquiring management than it was to the original management. Whether acquiring firms generally win concessions is therefore an empirical question.

2.2.2 Labor and corporate control

Brown and Medoff (1988) is the first paper devoted to analyzing the impact of acquisition on labor. Contrary to the tenor of popular press coverage of acquisitions, which focuses on hostile takeovers of large firms, they find small (and sometimes positive) changes in wages and employment following acquisitions based on a sample of mostly small firms in the state of Michigan.

Rosett (1990) documents that union wealth changes in the six years following the acquisition account for 1% to 2% of target-firm share-price premium. Fallick and Hassett (1996) argue that when a union firm is "in play," management can threaten that the firm will merge with a nonunion firm unless the union grants concessions.

However, it is not obvious that acquisitions harm wages and employment. Kole and Lehn (2000) conclude that, in the case of USAir's acquisition of Piedmont Aviation, the major source of USAir's value destruction was the strategy it used to integrate the workforces of Piedmont and USAir. Attempting to buy labor peace, it brought Piedmont and PSA employees under the more generous pay scales and work rules of USAir's collective bargaining agreements. This raised labor costs substantially and lowered the productivity of the newly acquired airlines. In a competitive labor market, employee wages will reflect marginal productivity. Ouimet and Zarutskie (2010) find that wages at the target firms increase, especially when the acquisition is expected to generate greater productivity gains.

2.2.3 The sources of gains from asset sales

Past studies have proposed three hypotheses for the source of gains from asset sales. The corporate focus hypothesis postulates that divestitures that increase focus lead to improvements in investment policy for a couple of reasons. Scharfstein and Stein (2000) argue that when firms are comprised of several divisions, divisions with poor prospects will engage in rent-seeking behavior. This rent-seeking argument predicts that divestitures of divisions most likely to engage in rent seeking, such as those with low growth opportunities, should be associated with the greatest improvements in investment policy. Rajan, Servaes et al. (2000) argue that divisions that contribute to diversity in investment opportunities are likely candidates for rent seeking. Their diversity argument predicts that divestitures that reduce the diversity of investment opportunities are associated with the improvements in investment efficiency.

The financing hypothesis is based on Lang, Poulsen et al. (1995), who argue that asset sales are often an expedient financing mechanism when access to external capital is limited. That is, asset sales relax external financial constraints and allow firms to undertake valuable investments that would otherwise be forgone. This hypothesis predicts that divestitures are associated with increased investment for divisions that are unable to finance all their positive net present value projects. This hypothesis also predicts that divesting an overinvesting segment relaxes financial constraints for the firms' remaining segments, thereby improving the overall efficiency of investment policy.

We label the third hypothesis the efficiency hypothesis. Hite and Owers (1983) and Rosenfeld (1984) argue that redeployment of assets to higher valued users is an important source of gains from asset sales. In a related study, Maksimovic and Phillips (2001) find that asset sales tend to improve the allocation of resources.

2.2.4 The determinants of an asset sale

There has been extensive research on the determinants of asset sales. Ofek (1993) reports that the likelihood of asset sales by a sample of firms that substantially underperform the market increases with leverage. This is consistent with Jensen (1989), who argues that highly levered firms will respond more quickly to distress due to the greater likelihood of default and loss of going concern value. Other papers provide evidence that restructurings (including asset sales) are linked to various events that reduce managerial control, including takeover threats (Dann and DeAngelo (1988) and Bhagat, Shleifer et al. (1990)), managerial turnover (Denis and Denis (1995) and Weisbach (1995)), and shareholder activism (Del Guercio and Hawkins (1999)). Lastly, and perhaps most closely related to our study, Atanossov and Kim (2009) find that asset sales among firms in distress are more likely if investor protection is poor and union protections are strong, suggesting that unions endorse such asset sales in an effort to prevent layoffs.

2.3 Data

The sample of asset sales and takeovers is drawn from the universe of mergers and acquisitions proposed between January 1987 and December 2009 and included in the SDC mergers and acquisitions database. Our initial sample of 5,286 asset sales and 5,323 takeovers meet the following criteria: (i) the buyer and seller are both domestic firms, (ii) the reported value of the sale transaction is at least 10% of the market value of equity of the divesting firm one year prior to the sale, and (iii) the transaction is completed.

The data on contract settlements is derived from the BNA Labor Plus database maintained by the Bureau of National Affairs. BNA Labor 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.

The U.S. Department of Labor's web site on state RTW laws indicates whether the state in which a firm has its primary business has RTW laws. We collect this information on an annual basis over our sample period. RTW laws are statutes currently enforced in 22 states, and are allowed under provisions of the Taft-Hartley Act, which prohibit unions from making membership or payment of union dues or fees a condition of employment, either before or after an employee is hired. To determine in which state a target firm is located we use the SDC variable "TARGET STATE," which the SDC defines as the state of the target's primary business or division at the time of the transaction.

2.4 Empirical results

2.4.1 Summary statistics

The complete BNA data contains 14,759 contract settlements. We identify contracts signed by companies traded on the NYSE or AMEX, and match with information available on CRSP and Compustat. The resulting sample contains 4,603 contracts covering 516 companies from 1987 to 2009.

Figure 1 reveals that both the number of companies and the number of contracts signed by these companies exhibit a decreasing temporal trend. For example, 246 companies signed 362 contracts in 1987, while 93 companies settled 149 contracts in 2009. Interestingly, the average number of contracts signed by each company stays constant over time. Taken together, the result suggests a decline in union power with fewer companies left to deal with unions. But there is no apparent change for the companies that still negotiate with their unions. There is also anecdotal evidence that unions retain their stronghold in certain industries. For example, in April 2014, JetBlue Airways pilots voted overwhelmingly to be represented by Air Line Pilots Association, the largest pilots' union. As a result, all major US airlines are now unionized.

2.4.2 Union concessions and the likelihood of an asset sale/takeover

As noted earlier, the form of the transaction, i.e., takeover or asset sale, affects the rights and obligations of the acquiring firm. In a takeover, the continuing employer is obliged to adopt the substantive provisions of the collective bargaining agreement, as well as to recognize and bargain with the incumbent union. In contrast, the buyer of assets is not bound to the substantive provisions of the collective bargaining agreement, but might incur other obligations as a successor employer. Therefore, in an asset sale, the buyer is free to extract whatever concessions it can from the unionized employees of the seller. If unionized employees are paid at a high premium over average employees and a fraction of the value created by union concessions is passed on to the seller, the seller has an incentive to get rid of the overpaid union through asset sales. In a takeover this tactic does not work, because the buyer is obliged to abide by the terms of the collective agreement as though no change occurred. Hence, we hypothesize that a higher wage differential between unionized employees and average employees of the seller increases the probability of an asset sale, but not the probability of a takeover. We refer to this as Hypothesis 1.

Table 9 provides evidence supporting Hypothesis 1. The dependent variable in the first model equals one if the sample firm makes an asset sale in that year. The primary explanatory variable, union wage premium, is measured as the hourly wage difference between the unionized employees and average workers in the same industry (defined by two-digit SIC codes) scaled by the average hourly industry wage in the previous fiscal year. The coefficient is positive and statistically significant at the 0.01 level, suggesting that the union wage premium is associated with more incidences of asset sales. For a one percent increase in union wage differential, the log odds of an asset sale increase by 1.2%.

All of the target firms used in the logistic regressions are unionized, but the unionization rate naturally varies. Thus, we also control for the unionization rate in our regressions. The coefficient on unionization does not differ statistically from zero. Thus, the decision to sell assets depends on whether the unionized workers are paid better than their peers, but not on the number of unionized workers.

In the second model, we investigate whether union wage premium is related to the likelihood of takeovers. The dependent variable equals one if the sample firm is taken over in that year. The coefficient is not statically significant, indicating that union wage premium is unrelated to the occurrence of takeovers.

2.4.3 Do acquiring firms gain union concessions?

In the previous section, we present evidence that firms with high union wage premiums are likely to sell assets to get rid of overpaid union workers. Next, we examine whether acquiring firms win union concessions following asset sales.

Rosett (1990) measures union concessions by the average decline in annual real wage growth following a takeover, but reports trivial concessions. This should not come as a surprise, because in a takeover, the acquiring firm is obliged to adopt the substantive provisions of the collective bargaining agreement as if no change has occurred. We hypothesize that the acquiring firm is more likely to obtain concessions from the incumbent union following an asset sale than a takeover. We refer to this as Hypothesis 2.

2.4.3.1 Changes in the number of contracts settled following takeovers vs. asset sales

An implication of Hypothesis 2 is that the number of contracts settled should fall following asset sales and stay constant following takeovers. To test this implication, Figure 2 presents the number of contracts from both the targets and the acquirers from three years before through three years after asset sales and takeovers. Consistent with the implication, there is a drop in the number of contracts settled following asset sales of 3.5%, but there is no significant change in the number of contracts settled around takeovers.

2.4.3.2 Changes in union wage growth following takeovers vs. asset sales

Another implication of Hypothesis 2 is that union wage growth should fall following asset sales but remain constant following takeovers. To measure the changes in annual wage growth from before a transaction to after the transaction, we construct a ratio of the average wage growth rate in the post-sale n years to the average wage growth rates in the pre-sale n years, where n is either one, two, or three. (We refer to this later as the wage growth ratio.) The results are reported in Table 10.

We start by examining asset sales. Brown and Medoff (1988) distinguish among two different types of asset sales based upon the impact of transactions on employment: (i) firm A purchases the assets of firm B without absorbing its workers, and (ii) firm A purchases firm B and (at least initially) absorbs (most of) firm B's workers. To measure the changes in union wage growth associated with asset sales, we need to identify the contracts that are transferred to the buyers following an asset sale. We classify the contracts settled by buyers after the sale into two categories: new vs. renewed. New contracts are defined to be contracts that are absent in the buyers' contract history prior to the sale, whereas renewed contracts are renewals of the previous contracts settled prior to the sale. Panel A of Table 10 presents the comparison between new contracts and renewed contracts. The median differences indicate that union wage growth of the new contracts increases at a slower pace than that of the renewed contracts for all three horizons.

We then turn to takeovers. Because the legal entity remains intact following a takeover, we simply track the target firms' contracts around the transaction. To compare the targets both longitudinally with themselves as well as cross-sectionally with a control group, we define a set of control firms that share the same four-digit SIC with the target firms but are not involved in an acquisition. Panel B of Table 10 compares the average target firms' wage growth with that of the control firms around the takeover date. Both mean and median difference tests show that the average changes in annual wage growth of the target firms are not significantly different from the control firms over a period of one, two, or three years.

Next, we conduct a multivariate analysis. To estimate the relation between union real wage growth and takeovers/asset sales, we regress annual wage growth on indicator variables for takeovers/asset sales and a set of control variables. The regression is of the form

$$\begin{aligned} W_j = \alpha &+ \gamma_1 \text{wage growth after asset sales}_j \\ &+\gamma_2 \text{wage growth before or after asset sales}_j \\ &+\gamma_3 \text{wage growth after takeovers}_j \\ &+\gamma_4 \text{wage growth before or after takeovers}_j \\ &+X_j\beta + \varepsilon_j \end{aligned} \qquad (1)$$

where j denotes contracts and the vector X includes lagged values of firm size, leverage and ROA. The dependent variable is the annual rate of real wage growth over the contract, expressed as a percentage. The indicator variables wage growth before or after asset sales (takeovers) equals one if the contract is settled within three years before or after the asset sales (takeovers). The indicator variables wage growth after asset sales (takeovers) equals one if the contract is settled within three years after the asset sales (takeovers). Hence, γ_1 (γ_3) measures the average changes in annual wage growth from before to after asset sales (takeovers).

Table 11 summarizes the regression results for Eq. (1). The first specification, which excludes financial controls, shows that the coefficient on wage growth after asset sales, γ_1, is negative and statistically different from zero at the 0.01 level. This suggests that the wage growth declines following asset sales. On the other hand, the coefficient on wage growth after takeovers, γ_3, is of the opposite sign and not statistically different from zero. Thus, there is no evidence to suggest that the wage growth changes from before to after takeovers. In the second specification, we control for firm characteristics and year and two-digit SIC industry fixed effects to capture unobservable components. The coefficient on wage growth after asset sales, γ_1, remains mostly unaffected at −0.47%, whereas the coefficient on wage growth after takeovers, γ_3, is still positive and insignificant. Our estimates imply that the concessions are obtained from the incumbent union following an asset sale, but not following a takeover.

2.4.4 Union concessions and stock returns around announcements of asset sales and takeovers

In this section, we explore the sources of value in asset sales and takeovers. To do so, we examine the determinants of the abnormal stock returns around the announcements. Because asset sales, but not takeovers, can be used to extract concessions from unions, we hypothesize that union concessions affect announcement returns for asset sales but not for takeovers. We refer to this as Hypothesis 3.

Rosenfeld (1984) find that the economic gains to the shareholders of the selling and buying firms in asset sales are nearly identical. If part of the value created by union concessions is passed on to the seller via the selling price, union concessions should affect the announcement returns for both the acquirer and the target. Consequently, we examine the announcement returns for both parties in the transactions.

We estimate the cumulative abnormal returns (CARs) over the three days centered on the announcements using the market model and the CRSP equally weighted index returns as the proxy for the market returns. The parameters for the market model are estimated over the 200 trading days ending ten days before the announcements. Our results are similar if we calculate abnormal returns by simply subtracting the value-weighted CRSP market returns from the firms' returns.

2.4.4.1 The relation between unionization and acquirer announcement returns

Panel A of Table 12 displays CARs for acquirers in asset sales versus takeovers. In asset sales, the mean CAR for acquirers is 2.0%, which is similar to those reported by Rosenfeld (1984) and Slovin, Sushka et al. (2005). For the subsample of unionized targets, the acquirers' mean (median) CAR is 3.7% (1.6%), compared to a mean (median) CAR of 1.9% (0.7%) for the subsample of nonunionized targets.[13] The differences in both the means and medians are statistically significant at the one percent level. These results suggest that the unionization status of the target affects the acquirers' CAR.

For takeovers, the mean CAR for acquirers is 1.3%, irrespective of whether the target is unionized. The median CAR is also similar across the unionized and non-unionized targets. Thus, the unionization status of the target seems unrelated to the acquirers' CAR in takeovers.

[13] We note that, due to the limitation of the contract settlement database, unionized targets might not be identified in some cases.

Panel B of Table 12 presents results from OLS regressions of the acquirers' CARs in asset sales. In model 1, we use the proportion of union workers in the target firm for each announcement as a measure of targets' union status.[14] The coefficient on the target unionization rate is positive and significantly different from zero, suggesting that the gains of acquirers increase with the target unionization rate.[15]

All regression models controls for the relative transaction value, defined as the reported value of the sale transaction divided by the market value of equity of the buyer one year prior to the sale. Miles and Rosenfeld (1983) find that the relative transaction value is positively correlated to asset sale announcement return. Our results support their finding. In model 2, we also add an interaction variable between the unionization rate and the relative transaction value. The relative transaction value is the reported value of the sale transaction divided by the market value of equity of the buyer one year prior to the sale. The coefficient on the interaction variable between unionization rate and relative transaction value is positive and significant, indicating that the positive effect of unionization is magnified by the relative transaction value.

In model 3 and 4, we include union wage premium, calculated as the hourly wage difference between the unionized employees and average workers in the same industry defined by two-digit SIC code as a fraction of average hourly earnings at the two-digit SIC level in the previous fiscal year. As expected, the coefficient on this variable is positive, but it is statistically

[14] For robustness, we also use union presence at the target firm as the primary explanatory variable, and find that the acquirer abnormal returns are 2.6% higher for asset sales of which targets are union firms than for those of which targets are nonunion firms.

[15] We also test if the level of unionization has an incremental impact of on abnormal returns over the presence of union at the firm level; coefficients on both variables are positive and significant, implying that the level of unionization has additional power over union presence in influencing announcement returns.

insignificant. We then add an interaction term between union wage premium and relative transaction value in model 4. The coefficient on the interaction term is positive with a p-value less than 0.01, indicating that acquirers' gains are larger if the unionized employees at the target are paid a high premium and the transaction is relatively large.

In models 5 and 6, we examine the effect of the realized concessions. All observations are required to have available union concessions data, and, as a result, all acquirers are unionized in this subsample. The concession variable is calculated by subtracting the wage growth ratio, as defined in Section 4.3.2, from its mean. When considering concession alone, it does not have a significant impact on buyers' returns. But the coefficient on an interaction term between concession and relative transaction value is positive with a p-value of 0.03, indicating that union concessions, when amplified by the relative transaction value, positively affect acquirers' returns.

Because union-related obligations survive the transfer of ownership following a takeover, we predict that union concessions do not explain announcement period return for acquirers in takeovers. Untabulated results support our prediction. In particular, we run the same regressions as those in panel B of table 4 for the sample of takeovers, and find that none of coefficients are statistically significant at conventional levels.

2.4.4.1 The relation between unionization and target announcement return

Rosenfeld (1984) find that the economic gains to the shareholders of the selling and buying firms in asset sales are nearly identical. If part of the value created by union concessions is passed on to the target via the transaction price, union concessions should also affect the targets' abnormal returns. Table 13 presents evidence consistent with this conjecture. Panel A displays announcement period abnormal returns for targets in both asset sales and takeovers. We focus on asset sales first. The average CARs for the sample is 3.1% (p-value is less than 0.01) over a three-day period around the day of the sale announcement. In comparison, Klein (1986) finds an average abnormal return of 1.13% from day −2 to day 0, Jain (1985) finds an average abnormal return of 0.5% on the day preceding and the day of the announcement of the sale, Hite, Owers et al. (1987) find an average abnormal return of 1.66% from day −1 to day 0, and John and Ofek (1995) find an average abnormal return of 1.5% from day −2 to day 0. The abnormal returns vary significantly with the unionization status of the sellers; the average three-day CARs is 4.2% when the sellers are unionized, significantly higher than the average of 3.1% when the sellers are not unionized.

We then turn to examine announcement period CARs to targets in takeovers. The average three-day target firm abnormal return is 25.1%. Combined with Panel A of Table 12, the results suggest that the announcement period gains from takeovers primarily accrue to target firm shareholders, consistent with Jensen and Ruback (1983), Jarrell, Brickley et al. (1988) and Andrade, Mitchell et al. (2001). More importantly for the purpose of this study, we find no statistically significant difference in three-day CARs across unionized targets and nonunionized targets.

Panel B of Table 13 presents OLS regression results of the three-day CARs for the targets. Overall, the results are similar to those reported for acquirers' CARs in panel B of Table 4. In model 1, the coefficient on the unionization rate is positive and statistically significant (p-value is less than 0.01). In model 2, we include an interaction with relative transaction value, defined here as the reported value of the sale transaction divided by the market value of equity of the target one year prior to the sale. The coefficient on interaction between unionization rate and relative transaction value is positive with a p-value less than 0.01. In models 3 and 4, we include union wage premium. This variable is positively related to target returns, but only when interacted with the relative transaction value (p-value is 0.03).

Lastly, in model 5 and 6, we include our concession variable and an interaction between concession and relative transaction value. The coefficient on the interaction term is significantly positive (p-value is 0.02), indicating that union concessions, when amplified by the relative transaction value, have a positive effect on the targets' returns.

We also ran the same regressions using the announcement period return for targets in takeovers. Untabulated results reveal that none of coefficients are significant at conventional levels. Thus, there is no evidence that union concessions explain the announcement returns for targets in takeovers.

2.4.5 The effect of RTW laws

Our main results are presumably attributable to powerful unions that inflate labor costs. Without powerful unions, there would be no need to sell assets to mitigate high labor costs. RTW laws prohibit unions from making membership or payment of dues a condition of employment, thereby reducing unions' bargaining power. Consequently, we expect RTW laws to weaken our results, at least those that rely on expected concessions. (Results that rely on realized concessions already reflect the power of the unions.) To test this, we bifurcate our sample based on whether the target firms operating in states with RTW laws, and run our main tests separately for the two subsamples.

In Panel A of Table 14, we estimate the probability of asset sales for our subsamples based on RTW laws. The results show the coefficient on union wage premium is larger in magnitude and only statistically significant in the sample of firms operating without RTW laws. This suggests that firms that have primary business in a state without RTW laws in effect are more likely to use asset sales to obtain concessions from unions.

Next, we investigate how RTW laws affect the relation between our unionization measures and asset sale announcement returns. Panels B and C of Table 14 present model specifications identical to those in models 1 and 2 of of Panel B in Table 12 and Table 13. The results show that the relevant coefficients are only statistically significant in the non-RTW sample, suggesting that the majority of the values created by union concessions in asset sales come from those targets that have primary business in a state without RTW laws in effect. This supports the idea that when union power is constrained by RTW laws, there is less concession to be extracted from the incumbent union in the process of an asset sale.

2.4.6 The economic significance of union concessions from asset sales

In our final analysis, we evaluate the economic significance of union wealth concessions from asset sales based on the second model specification in Table 11. We first estimate the real value (in 2009 dollars) of annual contract costs per employee per year following the asset sale that would result if the presale wage growth rate were to continue. Then we estimate the contract costs based on the assumption that wage growth is reduced for *t* years, where *t* ranges from 1 to 3, following the asset sale. The union wealth concession is the divergence between the two costs. We can express the present value (in 2009 dollars) of the union wealth concession over three years following an asset sale for firm i as

$$\Delta U_i = H_i \times E_i \times W_i \times \left\{\sum_{t=1}^{3}\left[\left(\frac{1+w_i}{1+r}\right)^t - \left(\frac{1+w_i-\gamma}{1+r}\right)^t\right]\right\} \qquad (2)$$

where H_i is average hours worked (including 1.5 times overtime hours) per year for the two-digit SIC industry of the firm, E_i is the number of unionized employees for firm i, W_i is the average wage before the start of the contract for firm i, and w_i is the average annual wage growth rate for the company. The real interest rate, r, is the inflation defined as the rate of inflation of the CPI over the 12 months before the asset sale. Lastly, γ is the effect of the asset sales on wage growth estimated in the second specification of Table 11.

We scale the union wealth concession by market capitalization, where market capitalization is the value of outstanding shares two days before the announcement date of the asset sales. The nominal market capitalization is converted to constant (2009) dollars to be comparable with the union wealth concessions.

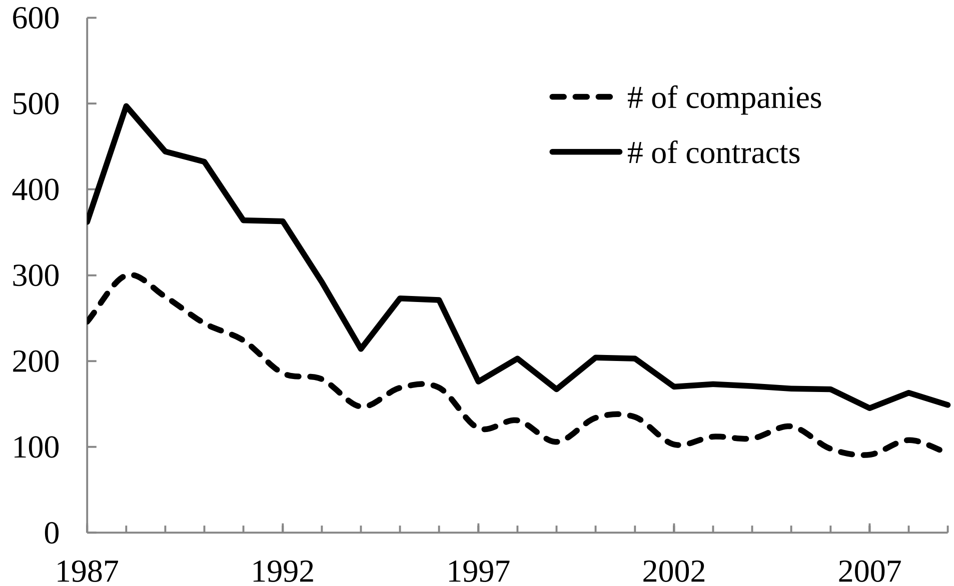
Table 15 presents average union wealth concessions for periods of one, two, and three years. The first column presents union wealth concessions for 88 asset sales for which all information necessary to calculate the change is available. One year following the asset sale, the seller union employees lost $3.3 million (2009 dollars) on average per asset sale, representing 3.2% of market capitalization. Over three years, the average loss is $19.4 million (2009 dollars), representing 18% of market capitalization.

The right panel of Table 15 compares union wealth concessions and target shareholder wealth. The sample size is larger, but the average concessions are similar to those in the left panel. Scaled by the market value of equity for the target, the larger sample yields more modest figures; over three years, the concession scaled by market value is about 3.6%.

2.5 Conclusion

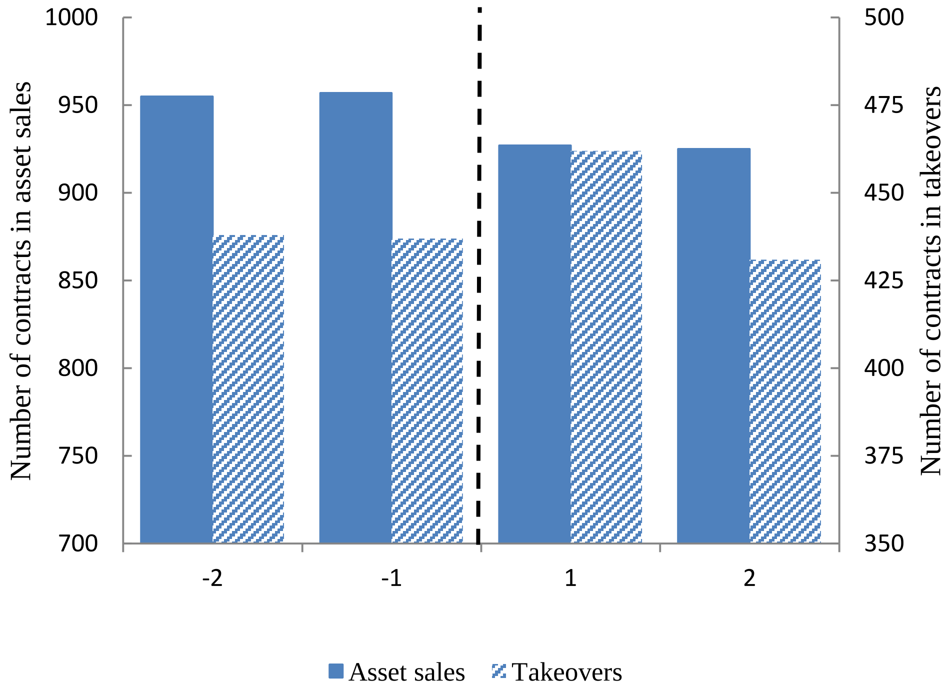
We examine the role of union wealth concessions in asset sales and takeovers. We conjecture that the potential for union concessions induces asset sales and explains much of the associated value creation. However, because of the law on successorship, we conjecture that union concessions play a trivial role in takeovers, in contrast to predictions of past studies. Our results support our conjectures. First, the union wage premium is associated with a higher incidence of asset sales, but is unrelated to the incidence of takeovers. Second, acquiring firms obtain substantial concessions from the incumbent union following an asset sale, but not following a takeover. Third, both anticipated and realized union concessions explain part of the excess stock returns around asset sale announcement, but are unrelated to takeover announcement returns. Finally, RTW laws, which weaken the unions that motivated the asset sales in the first place, also weaken our results.

Figure 1: The temporal trend in the contract sample



Note: This figure depicts the number of contracts and the number of firms that signed a contract by the settlement year. The sample has 4,603 contracts from the BNA Labor plus database between January 1987 and December 2009.

Figure 2: Number of contracts in the years around asset sales and takeovers



Note: The figure presents the number of contracts for both the acquiring and target firms during years relative to asset sales and takeovers. The sample consists of 4,603 contracts from the BNA Labor plus database between January 1987 and December 2009.

Table 9: Logistic regressions of the likelihood of asset sales and takeovers

	Asset Sales	Takeovers
Union wage premium	1.202	0.242
	(0.000)	(0.475)
Unionization rate	-0.259	0.321
	(0.294)	(0.409)
Financial controls	Yes	Yes
Observations	2,014	1,652
Likelihood ratio	513.97	63.01
Significance level (*p*-value)	0.000	0.000

Note: This table presents results of logistic regressions of the effects of unionization and labor cost on asset sale and takeover decisions. The dependent variable equals one if the sample firm sells assets or is taken over in a given year. Union wage premium is the hourly wage difference between the unionized employees and average workers in the same industry (defined by two-digit SIC codes) scaled by the average hourly industry wage in the previous fiscal year. Average hourly earnings were collected by year at the two-digit SIC level from the Bureau of Labor Statistics *Employment, Hours, and Earnings*. Unionization rate is the number of unionized workers scaled by total employment of the target one year prior to the transaction. The sample period is 1987–2009. *p*-values for the coefficients are provided in parentheses.

Table 10: The bargaining outcomes of union wage growth in asset sales and takeovers

	One-Year Window		Two-Year Window		Three-Year Window	
	Mean	Median	Mean	Median	Mean	Median
Panel A: Ratio of wage growth in new contracts relative to ratio of wage growth in renewed contracts around asset sales						
New	1.140	1.022	1.047	1.020	1.094	1.003
	(0.552)	(0.755)	(0.546)	(0.389)	(0.694)	(0.745)
Renewed	1.192	1.088	1.138	1.111	1.051	1.084
	(0.067)	(0.028)	(0.075)	(0.000)	(0.136)	(0.046)
New – Renewed	-0.051	-0.066	-0.091	-0.091	0.044	-0.081
	(0.114)	(0.036)	(0.136)	(0.000)	(0.296)	(0.000)
Panel B: Ratio of target firms' wage growth around takeovers relative to ratio of control firms' wage growth						
Targets	1.110	1.136	1.116	1.056	1.160	1.068
	(0.187)	(0.028)	(0.393)	(0.491)	(0.056)	(0.610)
Control	0.909	1.013	1.168	1.013	1.150	1.032
	(0.169)	(0.491)	(0.378)	(0.863)	(0.893)	(0.552)
Targets – Control	0.201	0.123	-0.052	0.043	0.009	0.036
	(0.148)	(0.645)	(0.878)	(0.645)	(0.800)	(0.261)

Note: This table shows changes in wage growth rates around asset sales and takeovers. The changes in wage growth are measured by the ratio of the average wage growth rate during the post-sale *n* years to the average wage growth rates during the pre-sale *n* years, where *n* equals either one, two, or three. Panel A compares new contracts to renewed contracts of acquiring firms in asset sales. New contracts are defined to be contracts that are absent from the acquirers' contract history prior to the transaction, and renewed contracts are the renewal of the previous contracts settled before the asset sales. Panel B compares the average targets' wage growth around the takeovers to that of control firms. The sample consists of 4,603 contracts from the BNA Labor plus database between January 1987 and December 2009. *p*-values for mean and median tests are provided in parentheses.

Table 11: The effects of asset sales and takeovers on annual wage growth

	Without financial controls	With financial controls
Wage growth after asset sales	-0.524	-0.469
	(0.000)	(0.000)
Wage growth after takeovers	0.048	0.078
	(0.412)	(0.168)
Wage growth before or after asset sales	0.048	0.071
	(0.204)	(0.051)
Wage growth before or after takeovers	-0.202	-0.170
	(0.000)	(0.000)
Financial controls	No	Yes
Year fixed effects	No	Yes
Industry fixed effects	Yes	Yes
Observations	4,603	4,603
R^2	0.181	0.238

Note: This table presents OLS regressions estimating the effects of either asset sales or takeovers on the level of annual wage growth using 4,603 contracts from the BNA Labor plus database between January 1987 and December 2009. The dependent variable is annual wage growth. Wage growth before or after asset sales (takeovers) is an indicator variable that equals one if the contract is settled within three years following an asset sale (takeover) or effective within three years before the asset sale (takeover). Wage growth after asset sales (takeovers) is an indicator variable that equals one if the contract is settled within 3 years following the asset sale (takeover). Financial control variables include lagged values of the log value of total assets, the ratio of total debt to total assets, and net income divided by total assets. p-values for the coefficients are provided in parentheses.

Table 12: The effect of unionization on announcement returns of acquirers

Panel A: Acquirers' CARs by transaction type and targets' unionization status

	Unionized targets	Nonunionized targets	Difference
Asset sales	n = 347	n = 4,939	
Mean	0.037	0.019	0.017
	(0.000)	(0.000)	(0.000)
Median	0.016	0.007	0.009
	(0.000)	(0.000)	(0.001)
Takeovers	n = 159	n = 5,164	
Mean	0.013	0.013	0.000
	(0.144)	(0.000)	(0.992)
Median	-0.002	-0.001	-0.001
	(0.543)	(0.485)	(0.938)

Table 12 Continued

Panel B: Regressions of acquirers' CARs around asset sales announcements						
	All asset sales		Asset sales with available union wage premium data		Asset sales with available union concessions data	
	1	2	3	4	5	6
Unionization rate	0.192	-0.482	-1.095	-1.060		
	(0.000)	(0.799)	(0.813)	(0.515)		
Unionization rate * RTV		1.022	2.610	2.694		
		(0.000)	(0.000)	(0.000)		
Union wage premium			-0.050	-0.554		
			(0.847)	(0.141)		
Union wage premium * RTV				1.421		
				(0.002)		
Concession					0.004	0.000
					(0.173)	(0.984)
Concession * RTV						0.053
						(0.032)
Rel. trans. value (RTV)	0.019	0.013	0.372	0.290	0.116	0.124
	(0.000)	(0.000)	(0.011)	(0.059)	(0.006)	(0.046)
Financial controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	No	No
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,121	5,121	195	195	154	154
Adjusted R^2	0.040	0.055	0.469	0.502	0.145	0.169

Table 12 Continued

Note: This table presents analyses of the cumulative announcement returns (CARs) around announcements of takeovers or asset sales for the acquiring firms. The sample comprises 5,286 asset sales and 5,323 takeovers completed between 1987 and 2009. Abnormal returns are calculated using a standard market model methodology, where the parameters are estimated over days −211 and −11 relative to the announcement dates. CARs are then estimated for days −1 to +1 relative to the announcement dates. In the takeover announcement of Parker & Parsley Petroleum, the three-day CAR is 108.13%, and this outlier has been excluded from the analyses. Panel A summarizes the mean and median CARs by transaction type (asset sale versus takeovers) and the unionization status of the targets. Panel B presents the results of OLS regressions of CARs around asset sale announcements. Unionization rate is the number of unionized workers divided by total employment of the target one year prior to the transaction. Relative transaction value (RTV) is the reported value of the transaction divided by the market value of equity of the seller one year prior to the sale. Union wage premium is the hourly wage difference between the unionized employees and average workers in the same industry defined by two-digit SIC code as a fraction of average hourly earnings at the two-digit SIC level in the previous fiscal year. Average hourly earnings were collected by year at the two-digit SIC level from the Bureau of Labor Statistics *Employment, Hours, and Earnings*. Concession is the difference between the ratio associated with the transaction, as defined in Table 10, and the mean of ratios. p-values for the coefficients are provided in parentheses.

Table 13: The effect of unionization on the targets' announcement returns

Panel A: Targets' CARs by transaction type and targets' unionization status			
	Unionized targets	Nonunionized targets	Difference
Asset sales	n = 312	n = 4,446	
Mean	0.042	0.031	0.011
	(0.000)	(0.000)	(0.303)
Median	0.019	0.014	0.005
	(0.000)	(0.000)	(0.048)
Takeovers	n = 140	n = 5,270	
Mean	0.224	0.252	-0.028
	(0.000)	(0.000)	(0.223)
Median	0.202	0.174	0.028
	(0.000)	(0.000)	(0.123)

Table 13 Continued

Panel B: Regressions of targets' CARs around asset sales announcements						
	All asset sales		Asset sales with available union wage premium data		Asset sales with available union concessions data	
	1	2	3	4	5	6
Unionization rate	0.164	-0.022	0.143	0.144		
	(0.000)	(0.668)	(0.251)	(0.271)		
Unionization rate * RTV		0.037	0.168	0.167		
		(0.008)	(0.287)	(0.347)		
Union wage premium			-0.025	-0.027		
			(0.749)	(0.793)		
Union wage premium * RTV				0.022		
				(0.030)		
Concession					0.008	-0.013
					(0.274)	(0.218)
Concession * RTV						0.083
						(0.019)
Rel. trans. value (RTV)	0.063	0.025	0.052	0.052	0.161	0.340
	(0.000)	(0.000)	(0.026)	(0.033)	(0.009)	(0.018)
Financial controls	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	No	No
Industry fixed effects	Yes	Yes	Yes	Yes	No	No
Observations	4,557	4,557	146	146	85	85
Adjusted R^2	0.008	0.004	0.067	0.095	0.329	0.563

Table 13 Continued

Note: This table presents analyses of the cumulative announcement returns (CARs) around announcements of takeovers or asset sales for the target firms. The sample comprises 1,412 asset sales and 5,410 takeovers completed between 1987 and 2009. Abnormal returns are calculated using a standard market model methodology, where the parameters are estimated over days −211 and −11 relative to the announcement dates. CARs are then estimated for days −1 to +1 relative to the announcement dates. Panel A summarizes the mean and median CARs by transaction type (asset sale versus takeovers) and the unionization status of the targets. Panel B presents the results of OLS regressions of CARs around asset sale announcements. Unionization rate is the number of unionized workers divided by total employment of the target one year prior to the transaction. Relative transaction value (RTV) is the reported value of the transaction divided by the market value of equity of the target one year prior to the sale. Union wage premium is the hourly wage difference between the unionized employees and average workers in the same industry defined by two-digit SIC code as a fraction of average hourly earnings at the two-digit SIC level in the previous fiscal year. Average hourly earnings were collected by year at the two-digit SIC level from the Bureau of Labor Statistics *Employment, Hours, and Earnings*. Concession is the difference between the ratio associated with the transaction, as defined in Table 10, and the mean of ratios. p-values for the coefficients are provided in parentheses.

Table 14: The effect of right-to-work laws

Panel A: The effect of right-to-work (RTW) law on the likelihood of asset sales		
	RTW	Non-RTW
Union wage premium	1.034	1.532
	(0.117)	(0.000)
Unionization rate	-0.122	-0.356
	(0.221)	(0.330)
Financial controls	Yes	Yes
Observations	408	1,596
Likelihood ratio	99.636	195.642
Significance level (p-value)	0.000	0.000

Table 14 Continued

Panel B: The effect of RTW laws on the relation between unionization and acquirers' CARs				
	1	2	3	4
	RTW	Non-RTW	RTW	Non-RTW
Unionization rate	-0.015	0.296	-0.079	-0.582
	(0.751)	(0.000)	(0.334)	(0.137)
Unionization rate * RTV			0.219	1.050
			(0.340)	(0.000)
Rel. trans. value (RTV)	0.026	0.019	0.020	0.013
	(0.000)	(0.000)	(0.000)	(0.000)
Financial controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Observations	1,606	3,324	1,660	3,246
Adjusted R^2	0.033	0.040	0.028	0.037

Panel C: The effect of RTW laws on the relation between unionization and targets' CARs				
	1	2	3	4
	RTW	Non-RTW	RTW	Non-RTW
Unionization rate	-0.131	0.218	-0.206	-0.022
	(0.415)	(0.012)	(0.557)	(0.697)
Unionization rate * RTV			0.433	0.040
			(0.833)	(0.019)
Rel. trans. value (RTV)	0.065	0.021	0.018	0.024
	(0.000)	(0.000)	(0.000)	(0.000)
Financial controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Observations	1,418	2,980	1,418	2,980
Adjusted R^2	0.040	0.058	0.049	0.052

Table 14 Continued

Note: This table presents the effect of right-to-work (RTW) laws on the probability of asset sales and the accompanying wealth effects. Panel A presents results of logistic regressions of the effects of unionization and labor cost on asset sale decisions. Subsamples are delineated based on whether the firm is located in a state with RTW laws. The dependent variable takes a value of one if the sample firm sells assets in that year. Union wage premium is the hourly wage difference between the unionized employees and average workers in the same industry defined by two-digit SIC code as a fraction of average hourly earnings at the two-digit SIC level in the previous fiscal year. Average hourly earnings were collected by year at the two-digit SIC level from the Bureau of Labor Statistics *Employment, Hours, and Earnings*. Unionization rate is the number of unionized workers divided by total employment of the target one year prior to the transaction. Panel B reports OLS regressions of CARs around announcements of asset sales for the acquiring firms. Panel C presents OLS regressions of CARs around announcements of asset sales for the target firms. Subsamples are delineated based on whether the target is located in a state with RTW laws. Abnormal returns are calculated using a standard market model methodology, where the parameters are estimated over days −211 and −11 relative to the announcement dates. CARs are then estimated for days −1 to +1 relative to the announcement dates. p-values for the coefficients are provided in parentheses.

Table 15: Economic significance of union wealth concessions

	Acquirers n = 88		Targets n = 271	
	Union wealth concession (2009 dollars)	Union wealth concession / market value	Union wealth concession (2009 dollars)	Union wealth concession / market value
Estimated over 1 year	3,256,147	0.032	4,228,629	0.006
Estimated over 2 years	9,722,360	0.093	12,602,673	0.018
Estimated over 3 years	19,353,802	0.184	25,041,645	0.036

Note: This table presents average wealth concession for unions and shareholders wealth associated with asset sales during the period from 1987 to 2009. Union wealth concessions are based on the divergence of wages from the level that would have prevailed without the asset sale using parameters in Table 3. The effect of asset sales on annual wage growth is estimated for one year, two years, or three years following the asset sales.

CHAPTER 3. THE EFFECT OF SYSTEMATIC AND IDIOSYNCRATIC RISK ON INVESTMENT AND R&D

3.1 Introduction

Theoretical work has much to say about the response of investment to uncertainty. Theories that look at the firm in relation to other firms emphasize covariances in the returns between investment projects as the channel through which uncertainty affects investment. The role of covariance is examined by Craine (1989) in the framework of capital asset pricing model (CAPM). An increase in the covariance should increase the riskiness of investment, increasing the required rate of return and thus lowering the desired level of the capital stock. The CAPM predicts that the greater the covariance in returns the less the incentive to invest. However, a recent empirical study of Panousi and Papanikolaou (2012) documents a positive sensitivity of investment to systematic volatility. The goal of this paper is to shed some light in solving the puzzling positive relationship between investment and systematic volatility.

We start by replicating the main results in Panousi and Papanikolaou (2012). Our results confirm the findings in Panousi and Papanikolaou (2012). An increase in idiosyncratic volatility depresses investment in contrast with that an increase in systematic volatility encourages investment.

Since the measure of systematic volatility depends on the firm's systematic risk exposure (beta) as well as the amount of market and industry risk, we decompose systematic volatility into its individual components to examine the role of covariance as well as other components. Surprisingly, we find a firm's exposure to systematic risk is positively correlated with investment. We also document weak negative relationship between the volatility of the market portfolio and investment.

Bar-Ilan and Strange (1996) show that in the presence of investment lags higher uncertainty encourages investment with long lags, i.e., R&D expenditure. While exploring R&D expenditure as an alternative form of investment, we find that idiosyncratic risk actually encourages firms to engage in R&D spending, in contrast with its depressing effect on capital expenditure; whereas systematic volatility depresses R&D in contrast with the positive sensitivity of capital expenditure to systematic volatility.

The remainder of the paper is organized as follows. Section 2 discusses related literature. Section 3 provides empirical results. Section 4 concludes.

3.2 Related Research

Economic theory has extensively examined the relationship between uncertainty and investment. Theories can be classified along two dimensions. First, we can distinguish between theories that emphasize the direct effect of uncertainty on investment and theories that emphasize the indirect effect of uncertainty on investment, i.e. uncertainty affects investment through the channel of covariance in the returns between investment projects. Craine (1989) explores the role of covariance in the CAPM. An increase in the covariance should increase the required rate of return and thus reduce the desired level of the capital stock. Second, we can distinguish between theories that predict that the marginal revenue of capital is convex and an increase in uncertainty will encourage investment, and theories that predict that the marginal revenue of capital is concave and an increase in uncertainty will discourage investment.

In the case of convexity, Hartman (1972) and Abel (1983) point out the flexibility of labor relative to capital produces the convex returns. Roberts and Weitzman (1981) show that if a firm has the option to abandon a project, then an increase in uncertainty increases the incentive to invest. Profit may also be a convex function when there is some lag before new investments become productive. Bar-Ilan and Strange (1996) show that with investment lags, the costs of deferring investment (i.e., forgone profits from having the *ex post* wrong input stock) are incurred in the future. As expected costs depend only on the possibility of "good news," higher uncertainty encourages investment in the presence of investment lags, and Bar-Ilan and Strange (1996) show numerically that these effects can dominate uncertainty's depressing effects due to irreversibility. Sarkar (2000) demonstrates that a higher level of uncertainty might have a positive effect on investment, particularly for low-growth and low-risk projects.

The main class of models that predict a concave marginal revenue product of capital are models with irreversible investment. McDonald and Siegel (1986) and Dixit and Pindyck (1994) emphasize that the interaction of capital irreversibility and price uncertainty generates option value of delay investment; and because the option value increases with price uncertainty, investment should decrease as uncertainty increases. Alternatively, the risk management literature proposes a dampening effect of uncertainty on investment due to financial friction that constrain firms' ability to raise external capital, see Smith and Stulz (1985) and Froot, Scharfstein et al. (1993).

Several empirical studies explore the relationship between investment and uncertainty and mostly find the sign to be negative with the exception of Stein and Stone (2012). Leahy and Whited (1996) find that uncertainty exerts a strong negative influence on investment. The results argue in favor of theories in which uncertainty affects investment directly rather than working through covariance and leaves capital irreversibility as the most likely explanation. Bulan (2005) and Panousi and Papanikolaou (2012) decompose total uncertainty into its systematic and idiosyncratic components and find that idiosyncratic risk discourages investment whereas systematic risk encourages investment. Stein and Stone (2012) find uncertainty depresses capital investment, hiring, and advertising, but encourages R&D spending due to the long investment lags between investment and project completion.

3.3 Empirical Results

3.3.1 Replication

In this section, we start by replicating Table 1 in Panousi and Papanikolaou (2012). Following Panousi and Papanikolaou (2012), the baseline measure of idiosyncratic volatility is constructed using weekly data on stock returns from CRSP. For every firm *i* and every year *t*, we regress the firm's return on the value-weighted market portfolio, R_{MKT} , and on the corresponding value-weighted industry portfolio, R_{IND}, based on the Fama and French (1997) 30-industry classification, across the 52 weekly observations.

$$R_{i,\tau} = \alpha_{1,i} + \beta_i^{mkt} R_{MKT,\tau} + \beta_i^{ind} R_{IND,\tau} + \varepsilon_{i,\tau} \,, \quad (1)$$

where τ indexes weeks. Then idiosyncratic risk is the log volatility of the regression residuals

$$\log(\sigma_{i,t-1}^{idio}) = \log \sqrt{\sum_{\tau \in t} \varepsilon_{i,\tau}^2} \,. \quad (2)$$

The response of investment to idiosyncratic risk is estimated using the following equation:

$$Capx_t / A_{t-1} = \gamma_0 + \beta \log(\sigma_{i,t-1}^{idio}) + \gamma_1 Z_{i,t-1} + \delta_i + \theta_t + \omega_{i,t} \,, \quad (3)$$

where the dependent variable is the firm's investment rate ($Capx_t / A_{t-1}$) and $Z_{i,t-1}$ is a vector of controls: (i) log Tobin's Q; (ii) the ratio of cash flows to assets (CF_{t-1}/A_{t-2}); (iii) log firm size; (iv) the firm's own stock return (R_{t-1}); and (v) log firm leverage, measured as the ratio of equity to assets ($\log(E_{t-1}/A_{t-1})$). Depending on the specification, we include firm dummies (δ_i) or year dummies (θ_t). Finally, the errors ($\omega_{i,t}$) are clustered at the firm level.

Our sample selection criterion is consistent with Panousi and Papanikolaou (2012). The sample includes all publicly traded firms in Compustat over the period 1970 to 2005, excluding firms in the financial (SIC code 6000−6999), utilities (SIC code 4900−4949), and government-regulated industries (SIC code > 9000). Firm-year observations with missing SIC codes, with missing values for investment, Tobin's Q, cash flows, size, leverage, stock returns, and with negative book values of capital are dropped. Firms with fewer than 40 weekly observations in that year are also excluded. The initial sample includes a total of 101,378 firm-year observations, which is reasonably close to the 104,646 observations of Panousi and Papanikolaou (2012). Finally, to remain consistent with Panousi and Papanikolaou (2012), data are winsorized by year at the 0.5% and 99.5% levels in all specifications.

The estimates of equation (3) are reported in Table 16. In the first column, only idiosyncratic volatility and firm fixed effects are included. The coefficient on idiosyncratic volatility is of -0.5% and statistically significant. The sign of the coefficient is the same as Panousi and Papanikolaou (2012), but the magnitude is smaller due to the reason that we use book assets instead of replacement value of capital (see, Salinger and Summers (1983)) in the dependent variable in equation (3). The second column presents the results of the benchmark estimation for equation (3). The coefficient on idiosyncratic volatility remains mostly unaffected at -0.8%. In sum, both specification imply that investment responds negatively to idiosyncratic risk, which confirms the result in Panousi and Papanikolaou (2012).

In the third column, we include lagged systematic volatility as an additional regressor. The coefficient on idiosyncratic volatility is still negative and significant (-1%), whereas the coefficient on systematic volatility is positive and significant (0.2%). The positive sensitivity of investment to systematic volatility remains puzzling.

3.3.2 Partition of Systematic Volatility

Since the measure of systematic volatility depends on the firm's systematic risk exposure (beta) as well as the amount of market and industry risk, we decide to decompose systematic volatility into individual components in an attempt to explain the positive response of investment to systematic volatility.

We estimate the response of investment to each component of systematic volatility as well as idiosyncratic risk using the following reduced-form equation:

$$Capx_t/A_{t-1} = \gamma_0 + \gamma_1 \log(\sigma_{i,t-1}^{idio}) + \gamma_2 \log(\sigma_{t-1}^{mkt}) + \gamma_3 \beta_{i,t-1}^{mkt} + \gamma_4 \log(\sigma_{t-1}^{ind}) + \gamma_5 \beta_{i,t-1}^{ind} + \gamma_6 Z_{i,t-1} + \delta_i + \theta_t + \omega_{i,t} , \quad (4)$$

where four regressors are included in addition to equation (3): market volatility ($\log(\sigma_{t-1}^{mkt})$) defined as the (log of the) square root of the variance of CRSP VW index; $\beta_{i,t-1}^{mkt}$ and $\beta_{i,t-1}^{ind}$ are coefficient estimates from the regression of weekly firm-level returns on the CRSP VW index and the corresponding industry portfolio in equation (1); industry volatility ($\log(\sigma_{t-1}^{ind})$) defined as the (log of the) square root of the variance of VW industry portfolio.

Table 17 presents estimates of equation (4). The first column shows that, when we include only the components of systematic volatility and firm fixed effects, the coefficients on systematic risk exposure ($\beta_{i,t-1}^{mkt}$ and $\beta_{i,t-1}^{ind}$) are positive and statistically significant whereas the coefficients on market and industry risk are negative (only the coefficient on industry risk is statistically significant). In the second column, we include lagged idiosyncratic volatility as an additional regressor. The coefficient on idiosyncratic volatility is still negative and significant (-0.2%), whereas the coefficient on each component of systematic volatility remains mostly unaffected. The last column presents the results of the benchmark estimation for equation (4). The coefficient on idiosyncratic volatility stays negative and significant (-0.2%). The coefficient on market risk is negative and statistically significant whereas the coefficient on industry risk is positive but insignificant. Invariably, the coefficients on systematic risk exposure ($\beta_{i,t-1}^{mkt}$ and $\beta_{i,t-1}^{ind}$) are positive and statistically significant.

Our estimates are consistent with Bloom (2009), who finds a negative relation between investment and the volatility of the market portfolio. Furthermore, a firm's exposure to systematic risk is positively correlated with investment, which is very puzzling.

3.3.3 R&D Expenditures vs. Capital Expenditures

Stein and Stone (2012) find that uncertainty depresses capital investment, but encourages R&D spending. This striking finding is consistent with R&D projects' high degree of technical uncertainty and long lags between investment and project completion. In this section, we introduce R&D rate ($R\&D_t/A_{t-1}$) in addition to investment rate ($Capx_t/A_{t-1}$). Descriptive statistics and correlations are given in Table 18. Note that the correlation of R&D rate with idiosyncratic risk is highly positive in contrast to the negative relationship between investment rate and idiosyncratic risk.

We substitute investment rate ($Capx_t/A_{t-1}$) with R&D rate ($R\&D_t/A_{t-1}$) to estimate the response of R&D spending to idiosyncratic risk, systematic volatility and its components using the following reduced-form equation:

$$R\&D_t/A_{t-1} = \gamma_0 + \gamma_1 \log(\sigma_{i,t-1}^{idio}) + \gamma_2 \log(\sigma_{t-1}^{mkt}) + \gamma_3 \beta_{i,t-1}^{mkt} + \gamma_4 \log(\sigma_{t-1}^{ind}) + \gamma_5 \beta_{i,t-1}^{ind} + \gamma_6 Z_{i,t-1} + \delta_i + \theta_t + \omega_{i,t} \,. \quad (5)$$

Our estimates of equation (5) are reported in Table 19. The first column shows that, when we include only idiosyncratic volatility and firm fixed effects, the coefficient on idiosyncratic volatility is 0.4% and statistically significant, implying that idiosyncratic risk encourages R&D spending in contrast with its depressing effect on capital expenditure. In column 2, we include lagged systematic volatility as an additional regressor. The coefficient on idiosyncratic volatility is still positive and significant (0.7%), whereas the coefficient on systematic volatility is negative and significant (-0.4). The puzzling positive sensitivity of investment to systematic volatility is gone. An increase in systematic volatility leads to a decrease in investment as expected. To explore further, we add the components of systematic volatility into the regression in the third column. The coefficient on idiosyncratic volatility remains mostly unaffected at 0.8%. The coefficient on market risk is negative and statistically significant whereas the coefficient on industry risk is positive but insignificant. Strikingly, the coefficients on systematic risk exposure ($\beta_{i,t-1}^{mkt}$ and $\beta_{i,t-1}^{ind}$) are negative and statistically significant as expected. The last column presents the results of the benchmark estimation of equation (5), in which case the coefficient on idiosyncratic volatility is still positive and significant, whereas the coefficients on all components of systematic volatility are negative and mostly significant (except for industry risk). In sum, R&D spending increases when idiosyncratic volatility is high, but decreases when systematic volatility as well as its components is high.

The most striking findings are that idiosyncratic risk actually encourages firms to engage in R&D spending, in contrast with its depressing effect on capital expenditure; whereas systematic volatility depresses R&D in contrast with the positive sensitivity of capital expenditure to systematic volatility. To ensure that these findings are not driven by sample differences, we repeat Table 16 and Table 17 using the same sample as of Table 19, i.e. only observations with available R&D spending information are included. The results are presented in Table 20 and Table 21 respectively. Comparing to Table 16, the coefficient in Table 20 on idiosyncratic risk remains negative and mostly unaffected whereas the coefficient on systematic volatility is still positive and significant. Similarly, the coefficients in Table 21 on components of systematic volatility are mostly consistent with the coefficients in Table 17. Despite the sample difference, the main results are very similar, implying that the striking difference between capital expenditure and R&D spending is not driven by sample difference.

3.4 Conclusion

Panousi and Papanikolaou (2012) documents a puzzling positive sensitivity of investment to systematic volatility. In an effort to shed some light on the puzzling sign, we partition systematic volatility into a firm's systematic risk exposure (beta) as well as the market and industry portfolio volatility. Surprisingly, we find a positive response of investment to a firm's systematic risk exposure. We also use R&D expenditure as an alternative form of investment. Strikingly we find that an increase in idiosyncratic risk lead firms to engage in R&D spending, in contrast with its depressing effect on capital expenditure; whereas systematic volatility discourages R&D in contrast with the positive sensitivity of capital expenditure to systematic volatility.

Table 16: Replication of Table 1 in Panousi and Papanikolaou (2012)

$Capx_t/A_{t-1}$	No Controls	Bench	Syst
$\log(\sigma_{i,t-1})$	-0.0051	-0.0086	-0.0100
	(-7.20)	(-11.14)	(-12.12)
$\log(\sigma_{t-1}^{syst})$			0.0020
			(4.77)
$\log(Q_{t-1})$		0.0205	0.0203
		(46.69)	(46.06)
CF_{t-1}/A_{t-2}		0.0559	0.0557
		(28.89)	(28.79)
$\log(A_{t-1})$		-0.0187	-0.0190
		(-41.85)	(-42.11)
R_{t-1}		0.0092	0.0092
		(20.41)	(20.52)
$\log(E_{t-1}/A_{t-1})$		0.0132	0.0130
		(21.47)	(21.24)
Observations	101,378	101,378	101,378
R^2	0.492	0.555	0.555
Fixed effects	F	F,T	F,T
Estimation method	OLS	OLS	OLS

Note: This table replicates Table 1 in Panousi and Papanikolaou (2012). The table reports estimation results of equation (3), where the dependent variable is the investment rate ($Capx_t/A_{t-1}$). Our measure of idiosyncratic risk, $\log(\sigma_{i,t-1}^{idio})$, is constructed from a regression of weekly firm-level returns on the CRSP VW index and the corresponding industry portfolio. Additional regressors include lagged values of: Tobin's $\log(Q_{t-1})$ defined as in Fazzari, Hubbard et al. (1988); operating cash flows (CF_{t-1}/A_{t-2}) defined as the ratio of operating income to book assets; the firm's size ($\log(A_{t-1})$) defined as the log value of book assets; the firm's stock return (R_{t-1}); leverage (E_{t-1}/A_{t-1}) defined as the ratio of book equity to book assets; and systematic volatility ($\log(\sigma_{t-1}^{syst})$) defined as the (log of the) square root of the difference between the firm's total variance and its idiosyncratic variance. The sample period is 1970 to 2005. Here, F denotes firm fixed effects, T denotes time fixed effects. The standard errors are clustered at the firm-level, and t-statistics are reported in parentheses.

Table 17: Partition of systematic risk

$Capx_t/A_{t-1}$	1	2	3
$\log(\sigma_{i,t-1}^{idio})$		-0.0021	-0.0090
		(-2.81)	(-11.58)
$\log(\sigma_{t-1}^{mkt})$	-0.0011	-0.0008	-0.0344
	(-0.79)	(-0.54)	(-3.76)
$\beta_{i,t-1}^{mkt}$	0.0024	0.0025	0.0012
	(7.56)	(7.83)	(3.80)
$\log(\sigma_{t-1}^{ind})$	-0.0112	-0.0108	0.0006
	(-7.52)	(-7.20)	(0.38)
$\beta_{i,t-1}^{ind}$	0.0019	0.0021	0.0012
	(4.53)	(4.80)	(2.90)
$\log(Q_{t-1})$			0.0204
			(46.16)
CF_{t-1}/A_{t-2}			0.0557
			(28.77)
$\log(A_{t-1})$			-0.0189
			(-41.87)
R_{t-1}			0.0092
			(20.51)
$\log(E_{t-1}/A_{t-1})$			0.0131
			(21.31)
Observations	101,382	101,378	101,378
R^2	0.494	0.494	0.555
Fixed effects	F	F	F,T
Est. method	OLS	OLS	OLS

Table 17 Continued

Note: The table reports estimation results of equation (4), where the dependent variable is the investment rate ($Capx_t/A_{t-1}$). Our measure of idiosyncratic risk, $\log(\sigma_{i,t-1}^{idio})$, is constructed from a regression of weekly firm-level returns on the CRSP VW index and the corresponding industry portfolio. Market volatility ($\log(\sigma_{t-1}^{mkt})$) is defined as the (log of the) square root of the variance of CRSP VW index. $\beta_{i,t-1}^{mkt}$ and $\beta_{i,t-1}^{ind}$ are coefficient estimates from the regression of weekly firm-level returns on the CRSP VW index and the corresponding industry portfolio. Industry volatility ($\log(\sigma_{t-1}^{ind})$) defined as the (log of the) square root of the variance of VW industry portfolio. Additional regressors include lagged values of: Tobin's $\log(Q_{t-1})$ defined as in Fazzari, Hubbard, and Petersen (1988); operating cash flows (CF_{t-1}/A_{t-2}) defined as the ratio of operating income to book assets; the firm's size ($\log(A_{t-1})$) defined as the log value of book assets; the firm's stock return (R_{t-1}); leverage (E_{t-1}/A_{t-1}) defined as the ratio of book equity to book assets. The sample period is 1970 to 2005. *F*, *T* denotes firm and time fixed effects, and *t*-statistics are reported in parentheses.

Table 18: Summary Statistics

Panel A: Descriptive Statistics (N=58,078)		
Variables	Mean	Std Dev
$Capx_t/A_{t-1}$	0.071	0.078
$R\&D_t/A_{t-1}$	0.074	0.118
$\log(\sigma^{idio}_{i,t-1})$	-0.959	0.545
$\log(\sigma^{syst}_{t-1})$	-1.949	0.735
$\log(\sigma^{mkt}_{t-1})$	-2.045	0.332
$\beta^{mkt}_{i,t-1}$	0.591	1.307
$\log(\sigma^{ind}_{t-1})$	-1.742	0.354
$\beta^{ind}_{i,t-1}$	0.374	0.998

Panel B: Sample Correlations (N=58,078)

Variables	$Capx_t/A_{t-1}$	$R\&D_t/A_{t-1}$	$\log(\sigma^{idio}_{i,t-1})$	$\log(\sigma^{syst}_{t-1})$	$\log(\sigma^{mkt}_{t-1})$	$\beta^{mkt}_{i,t-1}$	$\log(\sigma^{ind}_{t-1})$	$\beta^{ind}_{i,t-1}$
$Capx_t/A_{t-1}$	1							
$R\&D_t/A_{t-1}$	-0.034	1						
$\log(\sigma^{idio}_{i,t-1})$	-0.096	0.323	1					
$\log(\sigma^{syst}_{t-1})$	0.019	0.171	0.384	1				
$\log(\sigma^{mkt}_{t-1})$	-0.038	-0.014	0.186	0.342	1			
$\beta^{mkt}_{i,t-1}$	0.002	0.080	0.114	0.217	0.005	1		
$\log(\sigma^{ind}_{t-1})$	-0.055	0.067	0.274	0.389	0.776	0.008	1	
$\beta^{ind}_{i,t-1}$	0.036	-0.003	-0.051	0.214	-0.012	-0.767	0.016	1

Table 18 Continued

Note: Investment rate ($Capx_t/A_{t-1}$) is defined as the ratio of capital expenditure to book assets. R&D rate ($R\&D_t/A_{t-1}$) is defined as the ratio of R&D expenditures to book assets. Our measure of idiosyncratic risk, $\log(\sigma_{i,t-1}^{idio})$, is constructed from a regression of weekly firm-level returns on the CRSP VW index and the corresponding industry portfolio. Systematic volatility ($\log(\sigma_{t-1}^{syst})$) is defined as the (log of the) square root of the difference between the firm's total variance and its idiosyncratic variance. Market volatility ($\log(\sigma_{t-1}^{mkt})$) is defined as the (log of the) square root of the variance of CRSP VW index. $\beta_{i,t-1}^{mkt}$ and $\beta_{i,t-1}^{ind}$ are coefficient estimates from the regression of weekly firm-level returns on the CRSP VW index and the corresponding industry portfolio. Industry volatility ($\log(\sigma_{t-1}^{ind})$) defined as the (log of the) square root of the variance of VW industry portfolio. The sample period is 1970 to 2005.

Table 19: Risk and R&D Expenditures

$R\&D_t/A_{t-1}$	1	2	3	4
$\log(\sigma_{i,t-1}^{idio})$	0.0044	0.0073	0.0075	0.0059
	(5.23)	(8.04)	(8.41)	(6.22)
$\log(\sigma_{t-1}^{syst})$		-0.0039		
		(-8.18)		
$\log(\sigma_{t-1}^{mkt})$			-0.0092	-0.0369
			(-5.55)	(-3.59)
$\beta_{i,t-1}^{mkt}$			-0.0018	-0.0024
			(-4.95)	(-6.59)
$\log(\sigma_{t-1}^{ind})$			0.0010	-0.0012
			(0.58)	(-0.62)
$\beta_{i,t-1}^{ind}$			-0.0030	-0.0034
			(-6.05)	(-6.99)
$\log(Q_{t-1})$				0.0205
				(40.36)
CF_{t-1}/A_{t-2}				-0.0686
				(-32.79)
R_{t-1}				-0.0003
				(-0.64)
$\log(E_{t-1}/A_{t-1})$				0.0024
				(3.16)
Observations	58,078	58,078	58,078	58,078
R^2	0.765	0.766	0.766	0.779
Fixed effects	F	F	F	F,T
Est. method	OLS	OLS	OLS	OLS

Table 19 Continued

The table reports estimation results of equation (5), where the dependent variable is R&D rate ($R\&D_t/A_{t-1}$), defined as the ratio of R&D expenditures to book assets. Idiosyncratic risk, $\log(\sigma_{i,t-1}^{idio})$, is constructed from a regression of weekly firm-level returns on the CRSP VW index and the corresponding industry portfolio. Market volatility ($\log(\sigma_{t-1}^{mkt})$) is defined as the (log of the) square root of the variance of CRSP VW index. $\beta_{i,t-1}^{mkt}$ and $\beta_{i,t-1}^{ind}$ are coefficient estimates from the regression of weekly firm-level returns on the CRSP VW index and the corresponding industry portfolio. Industry volatility ($\log(\sigma_{t-1}^{ind})$) defined as the (log of the) square root of the variance of VW industry portfolio. Additional regressors include lagged values of: Tobin's $\log(Q_{t-1})$ defined as in Fazzari, Hubbard, and Petersen (1988); operating cash flows (CF_{t-1}/A_{t-2}) defined as the ratio of operating income to book assets; the firm's stock return (R_{t-1}); leverage (E_{t-1}/A_{t-1}) defined as the ratio of book equity to book assets. The sample period is 1970 to 2005. F, T denotes firm and time fixed effects, and t-statistics are reported in parentheses.

Table 20: Replication of Table 1 in Panousi and Papanikolaou (2012) Using Observations with Available R&D Data

$Capx_t/A_{t-1}$	No Controls	Bench	Syst
$\log(\sigma_{i,t-1})$	-0.0045	-0.0086	-0.0096
	(-5.54)	(-9.79)	(-10.21)
$\log(\sigma_{t-1}^{syst})$			0.0014
			(2.97)
$\log(Q_{t-1})$		0.0187	0.0186
		(39.88)	(39.39)
CF_{t-1}/A_{t-2}		0.0418	0.0417
		(21.42)	(21.35)
$\log(A_{t-1})$		-0.0178	-0.018
		(-34.98)	(-35.06)
R_{t-1}		0.0065	0.0065
		(13.61)	(13.67)
$\log(E_{t-1}/A_{t-1})$		0.0106	0.0105
		(15.13)	(14.99)
Observations	58,078	58,078	58,078
R^2	0.474	0.552	0.552
Fixed effects	F	F,T	F,T
Estimation method	OLS	OLS	OLS

Note: This table replicates Table 1 in Panousi and Papanikolaou (2012) using observations with available R&D data Only. The table reports estimation results of equation (3), where the dependent variable is the investment rate ($Capx_t/A_{t-1}$). Our measure of idiosyncratic risk, $\log(\sigma_{i,t-1}^{idio})$, is constructed from a regression of weekly firm-level returns on the CRSP VW index and the corresponding industry portfolio. Additional regressors include lagged values of: Tobin's $\log(Q_{t-1})$ defined as in Fazzari, Hubbard, and Petersen (1988); operating cash flows (CF_{t-1}/A_{t-2}) defined as the ratio of operating income to book assets; the firm's size ($\log(A_{t-1})$) defined as the log value of book assets; the firm's stock return (R_{t-1}); leverage (E_{t-1}/A_{t-1}) defined as the ratio of book equity to book assets; and systematic volatility ($\log(\sigma_{t-1}^{syst})$) defined as the (log of the) square root of the difference between the firm's total variance and its idiosyncratic variance. The sample period is 1970 to 2005. Here, *F* denotes firm fixed effects, *T* denotes time fixed effects. The standard errors are clustered at the firm-level, and *t*-statistics are reported in parentheses.

Table 21: Partition of Systematic Risk Using Observations with Available R&D Data

$Capx_t/A_{t-1}$	1	2	3
$\log(\sigma_{i,t-1}^{idio})$		-0.0014	-0.0090
		(-1.63)	(-10.08)
$\log(\sigma_{t-1}^{mkt})$	0.0057	0.0060	-0.0251
	(3.59)	(3.71)	(-2.63)
$\beta_{i,t-1}^{mkt}$	0.0023	0.0024	0.0011
	(6.64)	(6.76)	(3.19)
$\log(\sigma_{t-1}^{ind})$	-0.0170	-0.0167	-0.0028
	(-10.15)	(-9.90)	(-1.62)
$\beta_{i,t-1}^{ind}$	0.0019	0.0020	0.0015
	(3.99)	(4.13)	(3.29)
$\log(Q_{t-1})$			0.0186
			(39.44)
CF_{t-1}/A_{t-2}			0.0415
			(21.22)
$\log(A_{t-1})$			-0.0180
			(-35.08)
R_{t-1}			0.0065
			(13.62)
$\log(E_{t-1}/A_{t-1})$			0.0106
			(15.04)
Observations	58,081	58,078	58,078
R^2	0.476	0.476	0.552
Fixed effects	F	F	F,T
Est. method	OLS	OLS	OLS

Table 21 Continued

The table reports estimation results of equation (4), where the dependent variable is the investment rate ($Capx_t/A_{t-1}$). Our measure of idiosyncratic risk, $\log(\sigma_{i,t-1}^{idio})$, is constructed from a regression of weekly firm-level returns on the CRSP VW index and the corresponding industry portfolio. Market volatility ($\log(\sigma_{t-1}^{mkt})$) is defined as the (log of the) square root of the variance of CRSP VW index. $\beta_{i,t-1}^{mkt}$ and $\beta_{i,t-1}^{ind}$ are coefficient estimates from the regression of weekly firm-level returns on the CRSP VW index and the corresponding industry portfolio. Industry volatility ($\log(\sigma_{t-1}^{ind})$) defined as the (log of the) square root of the variance of VW industry portfolio. Additional regressors include lagged values of: Tobin's $\log(Q_{t-1})$ defined as in Fazzari, Hubbard, and Petersen (1988); operating cash flows (CF_{t-1}/A_{t-2}) defined as the ratio of operating income to book assets; the firm's size ($\log(A_{t-1})$) defined as the log value of book assets; the firm's stock return (R_{t-1}); leverage (E_{t-1}/A_{t-1}) defined as the ratio of book equity to book assets. The sample period is 1970 to 2005. *F*, *T* denotes firm and time fixed effects, and *t*-statistics are reported in parentheses.

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