TWO ESSAYS IN CORPORATE FINANCE

A Dissertation

by

KERSHEN HUANG

Submitted to the Office of Graduate Studies of
Texas A&M University
in partial fulfillment of the requirements for the degree of

DOCTOR OF PHILOSOPHY

May 2011

Major Subject: Finance

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ABSTRACT

Two Essays in Corporate Finance. (May 2011)

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In the first essay, "Why Won't You Forgive Me? Evidence of a Financial Misreporting Stigma in Bank Loan Pricing," we examine the relation between bank loan pricing and intentional financial misreporting. Firms that misreport financial information pay greater spreads on their bank loans for five years following their restatements, whether benchmarked against their pre-restatement loans or similar loans made to matched non-misreporting firms. Misreporting firms that promptly replace certain parties who are potentially related to the misreporting see their spreads fall to benchmark levels within three years following restatement. Large fractions of firms, however, do not promptly replace the potentially related parties and continue to pay premiums over benchmark spread levels for five years following restatement. The results suggest that misreporting creates a long-lasting and costly stigma, but that certain actions can reduce the duration of the stigma.

In the second essay, "Can Shareholder-Creditor Conflicts Explain Weak Governance? Evidence from the Value of Cash Holdings," we look into whether shareholder-creditor conflicts generate costs large enough to prevent improvements in governance. If firms choose to remain weakly governed, some cost must prevent improvements. We address our research question by estimating the value of cash as a function of governance, leverage, and the interaction of the two. We find that governance increases the value of cash, but that leverage reduces the gain from strong governance.

However, the magnitudes are far too small to explain why weak governance firms remain weakly governed. Our estimates suggest more than 80% of weakly governed firms would increase the value of their cash by improving governance. In fact, half of weakly governed firms would increase the value of their cash holdings by \$0.35 or more per dollar held by improving governance. Our focus on cash holdings does not seem to drive our results, nor do endogenous governance choices or nonlinearities reverse our conclusions.

To Peiling

ACKNOWLEDGMENTS

I am deeply indebted to my committee members Shane A. Johnson, Sudheer Chava, Neal E. Galpin, Michael F. Gallmeyer, and Mary Lea McAnally. Their guidance, mentoring, and support throughout my pursuit of the doctoral degree have built up the most academically fruitful days of my life. Over the course of my study, I have also benefited greatly from faculty members outside of my advisory committee, including Ekkehart Boehmer, L. Paige Fields, Donald R. Fraser, Wendy L. Galpin, D. Scott Lee, Arvind Mahajan, Shagun Pant, and Sorin M. Sorescu. Their scope of knowledge in all aspects of scholarship, teaching, and service has offered me invaluable lessons. I thank J. Amanda Adkisson, Richard K. Anderson, Donald R. Fraser, and Theodore L. Turocy for their encouragement and support during my decision stage in entering into a doctoral program in financial economics.

I am grateful to my colleagues in the Finance Ph.D. Program, especially T. Colin Campbell, Zhanhui Chen, Heungju Park, and Alexey Petkevich, for their friendship and peer support. I acknowledge the Department of Finance and the Dean's Office at Mays Business School for their financial support and caring for the doctoral student body. The ample opportunities that are offered to me at Texas A&M University have created numerous comparative advantages for me as a faculty candidate in the market.

This entire journey wouldn't have been successful without the continued, selfless love from my parents, Yen-Chu Lo and Chyi-Gang Huang. Finally and most importantly, I thank my fiancée, Peiling Cheng, for being the most beautiful thing that has ever happened to me.

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1. INTRODUCTION

This dissertation contains two essays, as presented in Sections 2 and 3, which are parts of larger research efforts of Chava, Huang, and Johnson (2011) and Galpin and Huang (2011), repectively.

In the first essay, “Why Won’t You Forgive Me? Evidence of a Financial Misreporting Stigma in Bank Loan Pricing,”[1] we study the stigma of the adverse effect of intentional financial misreporting on bank loan pricing and the mitigation of such effect associated with significant firm actions.

Firms that misreport financial information pay greater spreads on their bank loans for five years following their restatements, whether benchmarked against their pre-restatement loans or similar loans made to matched non-misreporting firms. Misreporting firms that promptly replace certain parties who are potentially related to the misreporting see their spreads fall to benchmark levels within three years following restatement. Large fractions of firms, however, do not promptly replace the potentially related parties and continue to pay premiums over benchmark spread levels for five years following restatement. The results suggest that misreporting creates a long-lasting and costly stigma, but that certain actions can reduce the duration of the stigma.

To the extent that banks should be able to revise estimates of value and cash flows relatively quickly, and thus quickly adjust loan spreads to the levels predicted by the restated financial figures, the long-lasting stigma evident in post-restatement loan spreads likely reflects higher information costs and uncertainty that banks face in

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This dissertation follows the style of Journal of Finance.

[1]We are grateful to seminar participants at Bentley University, Indiana University at South Bend, International University of Japan, Texas A&M University, and University of Kentucky for helpful comments. We thank Emmanuel Alanis for excellent research assistance. Any remaining errors or omissions are the authors’ alone.

making loans to firms that misreported long ago. Regaining credibility in reporting financial information appears to take a long time in the private loan market, but prompt replacements of certain parties appear to mitigate the duration of the stigma effect on a firm's cost of private debt capital. Overall, our results have implications for understanding the effects of misreporting on firm value and on the importance that banks place on the accuracy of reported financial information, despite that they may also use soft information in their credit screening and monitoring.

In the second essay, "Can Shareholder-creditor Conflicts Explain Weak Governance? Evidence from the Value of Cash Holdings,"[2] we ask whether shareholder-creditor conflicts generate costs that are large enough to prevent improvements in governance.

If firms choose to remain weakly governed, some cost must prevent improvements. Given that the conflict between shareholders and creditors is a widely proposed factor, we ask whether it is indeed enough to explain why weakly governed firms maintain weak governance. To do so, we estimate the value of cash as a function of governance, leverage, and the interaction of the two. We find that governance increases the value of cash, but that leverage reduces the gain from strong governance. However, the magnitudes are far too small to explain why weak governance firms remain weakly governed. Our estimates suggest more than 80% of weakly governed firms would increase the value of their cash by improving governance. In fact, half of weakly governed firms would increase the value of their cash holdings by $0.35 or more per dollar held by improving governance.

[2]Previous versions were circulated under the title "Creditor and Shareholder Conflicts: Evidence from the Value of Cash Holdings." We thank Sudheer Chava, Mike Gallmeyer, Shane Johnson, Scott Lee, Dave Mauer, Bill Maxwell, David Smith, Johan Sulaeman (discussant), Bill Willhelm and participants at the Florida State University, University of Melbourne, University of Virginia, and the 2009 Lone Star Symposium in Lubbock, TX for helpful comments. Any remaining errors or omissions are the authors' alone.

In sum, we show that the magnitudes of costs from shareholder-creditor conflicts do not appear large enough for shareholders to actually prefer weak governance to strong. In fact, the magnitudes of shareholder-creditor conflicts are too small to even make most shareholders reasonably indifferent between weak and strong governance. If shareholders find weak governance better than strong governance-and we know they must, since we observe weak governance-costs other than shareholder-creditor conflicts must prevent strengthening governance.

2. WHY WON'T YOU FORGIVE ME?

Revealing that financial statements were deliberately misreported conveys two things about the misreporting firm, both of which should have adverse consequences for firms' bank loan contracting terms. First, because most restatements by borrowers cause reported earnings to be revised downward, restatements should generally cause lenders to revise downward their estimates of future cash flows and firm value.[3] Ceteris paribus, these revisions should cause adverse changes to bank loan contract terms. Second, lenders learn that some factors inside the firm must have contributed to the misreporting. Bergstresser and Philippon (2006), Burns and Kedia (2006), and Johnson, Ryan, and Tian (2009) find that misreporting firms have greater stock and option-related managerial incentives to misreport. Other potential factors include weak internal controls or weak governance Farber (2005) and individual attributes or a company culture that encourages misreporting. If lenders are not credibly convinced that the factors that contributed to the misreporting have changed, they may question the credibility of future information provided by the firm, and more generally, question the veracity of the firm. The increased uncertainty about the reliability

[3]Most of the restatements in our sample are due to revenue/expense recognition. While there may be cases where cash flow items are misclassified, total net cash flows shouldn't change. One may raise the concern as to how an accrual event, such as most restatements, can have a cash flow effect. It is worth noting that here we are referring to expected future cash flows, from which debt payments will be serviced. The link between current earnings and future cash flows is suggested by the FASB Statement of Financial Accounting Concepts (No. 1, item 43), which clearly states that earnings information is helpful in the assessments of the amounts, timing, and uncertainty of future cash flows. Empirically, current earnings have been shown to be value-relevant and serve as a predictor for future cash flows (Dechow, 1994; Dechow, Kothari, and Watts, 1998; Finger, 1994). The cash flow predictability of earnings is also evident to be increasing over time (Kim and Kross, 2005). From our results in this paper, we indirectly provide support to the notion that financial statements are an important source of information about borrowers for banks. At the time of debt contracting, banks are pricing loans according to not only loan level characteristics, but also firm level fundamentals. They use information provided in financial statements to assess the risk and credit quality of the borrowing firms. As such, the quality of accounting information would affect the lenders' estimates of borrowers' future cash flows (Bharath, Sunder, and Sunder, 2008).

of information and veracity of the firm should increase screening and monitoring costs for banks, and may also reduce the effectiveness of covenants written on reported information. The increased costs and the reduced effectiveness of covenants should adversely affect loan contract terms.

Consistent with the hypothesis that misreporting should adversely affect firms' bank loan contracting terms, Graham, Li, and Qiu (2008) find that firms pay a misreporting premium in the form of significantly higher loan spreads for post-restatement loans. Loans to misreporting firms also have shorter maturities, a greater likelihood of being secured, and more restrictive covenants. We replicate Graham, Li, and Qiu's (2008) findings that misreporting firms pay significantly higher loan spreads in an expanded sample of restatements from 1997 to 2006. As in Graham, Li, and Qiu (2008), we also find that the effects of restatements on loan spreads are larger in cases that appear to have been deliberate misreporting (i.e., irregularities) as opposed to ones that stem from errors (Hennes, Leone, and Miller, 2008). The effects are large economically and suggest that banks view restatements by their borrowers as important events.

Given the misreporting premium evident in bank loan spreads, we test two hypotheses to attempt to shed additional light on the effects of misreporting on a borrower's contracting in debt markets. We first test the hypothesis that there is a long-lasting and costly misreporting stigma evidenced by higher loan spreads for misreporting firms. Once financial figures are restated, a bank should be able to revise estimates of value and cash flows relatively quickly, and thus quickly adjust loan spreads to levels appropriate for the true (restated) financial figures. If the effect of misreporting on loan spreads is limited to these short-term revisions, we should observe that misreporting firms pay spreads consistent with their restated financial figures soon after their restatements. In contrast, it may be difficult for a firm to credibly convince banks that it has eliminated the factors inside the firm that contributed to the misreporting. If so, firms could have a potentially long-lasting stigma that causes banks to question the veracity of the firm, and thus charge them greater loan spreads to cover the increased screening and monitoring costs. Given the nature of the research question, we focus our analysis on the subsample of cases in which misreporting was apparently deliberate (i.e., the *irregularities* in Hennes, Leone, and Miller, 2008).[4]

It is not obvious that firms should face a long-lasting misreporting stigma in bank loan markets. Many studies in the banking firm literature focus on the informational advantages banks have through their ability to observe debits and credits to borrowers' checking accounts. Norden and Weber (2010) provide an excellent review of this literature and new empirical results consistent with the usefulness of information derived from borrowers' checking accounts. If banks can observe sufficient information from the cash inflows and outflows from their borrowers' checking accounts at low cost, the credibility of a borrowing firm's financial statements and its veracity may matter less to banks than to other outside capital providers. Whether the information gleaned from borrowers' checking accounts can substitute for information reported by firms is an empirical question. Testing the hypothesis of a long-lasting misreporting stigma provides indirect evidence on this question.

We find strong support for the hypothesis that misreporting creates a long-lasting and costly stigma. Among misreporting firms, post-restatement loan spreads are significantly greater than pre-restatement spreads for at least five years following restatement. A misreporting premium in post-restatement loan spreads is also evident when we use a nearest neighbor matching estimator approach to compare misreporting firms' loans to similar loans obtained by non-misreporting firms with similar

[4]Consistent with Graham, Li, and Qiu (2008), the restatements in our sample are mostly due to revenue recognition (47.13%), cost of sales or operating expense (26.44%), and restructuring (assets/inventory, 16.09%). Comparing our sample to that in their paper, we see larger percentages of restatements relating to revenue recognition and cost or expenses. This is not surprising, given that we focus on irregularities (Hennes, Leone, and Miller, 2008).

characteristics. The misreporting premium exhibits a decline over time, but five years following a restatement misreporting firms still pay a loan spread that is 34% higher than comparable non-misreporting firms pay on comparable loans. The long-lasting misreporting premium is consistent with the view that banks face greater information costs and uncertainty when making loans to firms that have misreported even five years before the current pricing of a loan.

We next test the hypothesis that firms can avoid or reduce the misreporting stigma by promptly replacing certain parties who are potentially related to the misreporting. Prompt replacement of potentially related parties – CEOs, CFOs, audit committee chairs, and external auditors – could signal to banks that a firm takes the misreporting seriously and is willing to take significant steps to restore its credibility.[5] If banks view such replacements as credible signals by misreporting firms that they have taken all necessary steps to ensure veracity going forward, firms that make such replacements should have stigmas that are less severe and/or last for shorter time periods following restatement. In the nearest neighbor matching approach for these analyses, we require the matched firms to have similar firm and loan characteristics and also to have replaced the respective parties that we study. For example, when we examine the subsample of misreporting firms that promptly replaced their CEOs, we require matched firms to have also replaced their CEOs in the same year.

We find support for the hypothesis that prompt replacement of parties potentially related to the misreporting reduces the duration of the misreporting stigma, but the effects are limited to certain parties. Misreporting firms that promptly replace the chairs of their audit committees see reductions in their loan spreads to matched firm levels by the third post-restatement year. Misreporting firms that promptly

[5]Although some audit committee chairs and external auditors might have actively participated in misreporting, we include them in the set of *potentially related parties* primarily because outsiders may view the event of misreporting as a failure by these parties to prevent or detect misreporting before financial statements were released to outsiders.

replace their external auditors also see reductions in their loan spreads to matched firm levels by the third post-restatement year. Despite the apparent mitigating effects of promptly replacing audit committee chairs and external auditors, a majority of misreporting firms do not promptly replace these parties and continue to pay significant misreporting premiums for at least four years after their restatements.

The results for prompt replacements of CEOs and CFOs differ from those of the other parties. Misreporting firms that promptly replace their CEOs continue to pay misreporting premiums over matched firms even five years after their restatements. Misreporting firms that promptly replace their CFOs pay misreporting premiums over matched firms even in the fourth post-restatement year. The majority of misreporting firms do not promptly replace their CEO or their CFO, and pay a significantly greater spreads than matched firms for five years following their restatements.

A potential explanation of the greater post-restatement spreads for misreporting firms is that the pre-restatement banks discontinue relationships with the misreporting firms, which forces the firms to establish new bank relationships in the post-restatement period. The new bank relationships may translate into greater screening and monitoring costs that are then reflected in loan spreads. The observed premiums in post-restatement loan spreads are not driven by new bank relationships: the subset of misreporting firms that continue existing bank relationships into the post-restatement period pay higher spreads up to at least the fifth post-restatement year. We also address the concern that our findings are based on differences in unobservable firm fundamentals not captured by our loan spread models. Using a censored selection model that accounts for a firm's choice to misreport, our results remain statistically and economically significant.

Collectively, the results are consistent with the view that misreporting creates a long-lasting and costly stigma that causes banks to question the credibility of the firms' future reported financial information, and perhaps more generally, the veracity of firms. As a result, banks face greater screening and monitoring costs, and thus charge a significant misreporting premium in loan spreads. Prompt replacement of certain parties potentially related to the misreporting shortens the duration of the stigma, but a majority of firms do not make such prompt replacements and pay misreporting premiums for at least four to five years following their restatements. The long-lasting stigma evident in loan spreads suggests that banks place great importance on the truthfulness of information provided by firms. The results are broadly consistent with Karpoff, Lee, and Martin (2008b) that the reputation costs of misreporting (defined as the portion of total penalty in addition to that imposed by legal systems) are large economically.[6] Our research complements the findings of Graham, Li, and Qiu (2008) and extends those of Farber (2005) and Agrawal and Cooper (2009) to the bank loans market.

Section 2.1 describes our data. Sections 2.2 and 2.3 examine respectively the effects of misreporting on loan spreads among misreporting firms and compared to matched non-misreporting firms. Section 2.4 examines the effects of changes in various parties who are potentially related to the restatements. Section 2.5 provides robustness checks of the main results. Section 2.6 concludes.

2.1 Data and Descriptive Statistics

2.1.1 Data Sources

Financial Restatements

We obtain a sample of restatements from two reports released by the U.S. Government Accountability Office (GAO). The first report, released in October of 2002, covers 919 restatements that occurred between January 1, 1997 and June 30, 2002.

[6]According to Karpoff, Lee, and Martin (2008b), this expected loss due to reputation costs stems from the lower sales and higher contracting/financing costs associated with the restatement event.

The second report, released in July of 2006, covers 1390 restatements that occurred between July 1, 2002 and September 30, 2005. At the end of August 2006, the GAO published a supplementary file to the 2006 report with an additional 396 restatements from October 1, 2005 to June 30, 2006. There are a total of 2705 restatements in these reports.

Restatements in the GAO database represent cases in which the original misreporting stemmed from fraud (intentional) and cases that stem from apparently unintentional errors. Differential stock price reactions to the two types of restatement announcements suggest economic differences between them (Hennes, Leone, and Miller, 2008; Palmrose, Richardson, and Scholz, 2004). Given the nature of our research questions, we focus our analysis primarily on the subset of GAO restatements that are classified as "irregularities" by Hennes, Leone, and Miller (2008). Their classification procedure is based on a series of criteria. A restatement is classified as "irregular" if in the announcement or in any relevant filings in the four years surrounding the restatement, (1) the firm uses the words or any variants of the words "fraud" and "irregular" to refer to the event, or (2) the restatement itself is related to investigations conducted by the Securities and Exchange Commission (SEC) or the Department of Justice.[7] (3) For other non-SEC independent investigations documented in the public filings, Hennes, Leone, and Miller (2008) conduct a case-by-case analysis and classify as "irregular" where appropriate.[8]

Beginning with the 2705 restatements from the GAO reports, we follow Graham, Li, and Qiu (2008) in retaining only one restatement per firm in our sample to avoid issues created by the potential overlapping of pre- and post-restatement windows

[7]See Feroz, Park, and Pastena (1991) for a discussion of SEC enforcement of accounting and auditing-related infractions.

[8]We thank Hennes, Leone, and Miller (2008) for generously providing classifications of irregular restatements. The data are available on Andrew J. Leone's website at http://sbaleone.bus.miami.edu/.

when analyzing firms with multiple restatements. Instead of simply considering the first restatement as the restatement of interest for a single firm as in Graham, Li, and Qiu (2008), however, we consider the first irregular restatement when applicable as our restatement of interest for that firm. Next, we require that these restatement firms have the requisite data (discussed below) in Compustat and DealScan databases.

Approximately 37% of misreporting firms that have loans in the pre-restatement period have no loans in the post-restatement period (based on DealScan data). It is possible that these firms suffer such a misreporting stigma that they are effectively shut out of the credit market. If so, one can view our findings of a misreporting premium among the firms that do have post-restatement loans as a lower bound estimate of the effects of a misreporting stigma. Alternatively, the excluded firms may simply not need bank financing or any external financing because they have low investment opportunities or because they have sufficient internal cash flow to support investment. It is difficult to examine these questions because in the post-restatement period only about 7% (20% of the 37%) have Compustat data for the five post-restatement years we examine. Although analysis of these firms may shed light on the effect of misreporting on access to credit markets, we exclude them from our study because the analyses in this paper require that firms receive loans in the post-restatement period.

For our regression analysis, we require that all firms receive loans both prior to and after their restatement dates so that the comparison between pre- and post-restatement costs of debt is somewhat balanced. Thus, our final "restricted" sample of loan deals for regression analysis consists of 3882 loans made to 455 unique firms from 1989 to 2007.[9] Our primary focus in this part of the study is on the set of irregular restatements, which includes 1316 loans made to 140 firms.

For our matched analysis, we do not restrict firms to have both pre- and post-restatement loans because (1) we only conduct our matched analysis for post-event loans and (2) of sample size issues. Our final "unrestricted" sample for matched analysis consists of 701 post-restatement loans issued to 185 firms that are associated with irregular restatements, of which 626 loans issued to 180 firms take place within the first five post-restatement years.[10]

Bank Loans

Bank loan data are from the Loan Pricing Corporation's (LPC) DealScan database. The main source of data provided in DealScan is firms' SEC filings. The basic unit of observation in the database is a loan (also known as facility or tranche in the database) involving one borrower and one or more lenders.[11]

Our key variable of interest is the natural log of loan spread. Following Graham, Li, and Qiu (2008) and Chava, Livdan, and Purnanandam (2009), we obtain the all-in spread drawn from the DealScan database. This variable measures the amount the borrower pays in basis points over LIBOR (or LIBOR equivalent) for each dollar drawn down and includes any annual fees (or facility fees) paid to the bank or bank

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[9] When we restrict the sample to restatements in the first GAO report, the sample closely matches that in Graham, Li, and Qiu (2008). Our replication of the loan spread regressions in Table 3 of their paper is available upon request.

[10] A restricted version of this sample, where we require that all firms receive loans both prior to and after their restatement dates, consists of 533 post-restatement loans made to the 140 firms. In this restricted sample, there are 479 post-restatement loans issued to 138 firms during the first five post-restatement years. Our results do not change qualitatively when using this restricted sample to test our hypotheses.

[11] Chava and Roberts (2008) discuss details of firm level matching between DealScan and Compustat.

group. Not all loans are priced off of LIBOR; for those that are not, the variable is converted into LIBOR terms by the LPC using a periodically adjusted differential.

Our analyses control for various loan specific characteristics that are taken from the DealScan database. These characteristics include: the natural log of maturity; the natural log of loan size; whether the loan has a performance-pricing feature; the loan type (e.g., revolvers above and below one year, term loans, 364-day loans, etc.), and the deal purpose (e.g., corporate, repayment, takeover, and working capital). Panel A of Table 2.1 contains a list of loan-specific variables and their definitions.

Turnover Information

Our analysis of the effects of replacing various parties potentially related to the misreporting events requires turnover data on CEOs, CFOs, audit committee chairs, and external auditors. Following Hennes, Leone, and Miller (2008), we define our change determination window as the thirteen-month period centered on the restatement announcement month. A turnover for a particular party (e.g., a CEO) is recorded if, during this determination window, one or more turnovers of that party occurred. We extract turnover information for CEOs and CFOs from ExecuComp, for audit committee chairs from RiskMetrics, and for external auditors from AuditAnalytics.

The turnover analysis for CEOs and external auditors is straightforward, given the nature of the source databases. The annual position flag, start date, and end date for CEOs are relatively accurate and complete in the ExecuComp database. Similarly, for changes in external auditors, the exact change dates are given in the auditor change file from the AuditAnalytics database.[12]

[12]The auditor change file from the AuditAnalytics database, though offering accurate turnover dates, provides data only for 2000 and beyond.

Table 2.1

Variable Definitions for Section 2.

This table presents definitions of variables used in Section 2, "Why Won't You Forgive Me? Evidence of a Financial Misreporting Stigma in Bank Loan Pricing." The unit of observation within the sample is a loan, which we refer to as "the loan" in the definitions. The borrower of the loan is referred to as "the borrower" or "the borrowing firm."

Panel A: Loan Specific Variables	
lnspread	Natural log of the all-in drawn spread (measured in basis points) charged on the loan; all-in drawn spread is defined as the borrowing spread of the loan over LIBOR (or equivalent)
restatepost	Dummy variable indicating whether the facility start date of the loan is after the financial restatement announcement date for the borrower of the loan
restatepost(p)	Indicator variable stating whether the start date of the loan is during the pth year after the financial restatement announcement date for the borrower of the loan for $p = 1$ to 5, or after the 5th post-restatement year for $p = 6$; p is an integer from 1 to 6
fraud	Interaction term of the post-restatement indicator *restatepost* with another dummy variable indicating whether the borrower of the loan is associated with an irregular restatement; irregularity is classified using the method of Hennes, Leone, and Miller (2008)
lnt	Natural log of maturity (measured in months) for the loan
lnloansize	Natural log of the amount borrowed (measured in $millions) in the loan contract
perfprice	Binary variable indicating whether the loan employs performance pricing clause
Panel B: Firm Specific Variables	
lnassets	The natural log of total assets (measured in $millions) for the borrowing firm of the loan
mb	Market-to-book ratio of total assets for the borrower, calculated as the sum of market value of equity and book value of debt scaled by total assets
leverage	Leverage ratio of the borrower, defined as the sum of short-term and long-term debt divided by total assets
profitability	Profitability of the borrower, defined as EBITDA over total assets
tangibility	Tangibility of the borrower, defined as net PP&E scaled by total assets
cfvol	Cash flow volatility of the borrowing firm, measured by the standard deviation of quarterly net cash flow from operating activities, as derived from the cumulative account `oancfy` of the quarterly fundamentals file of Compustat, for the past four fiscal years before the start date of a loan, scaled by the sum of long-term debt and short-term debt
modzscore	Modified Altman z-score of the borrower
Panel C: Macroeconomic Variables	
crdtsprd	Credit spread, defined as the difference between AAA and BAA corporate bond yields
termsprd	Term spread, defined as the difference between 10-year and 2-year Treasury yields
econboom	Dummy variable indicating whether the loan starts during the economic boom period between 1996 and 2000, inclusive, as defined by Graham, Li, and Qiu (2008)

The identification of turnover for CFOs and audit committee chairs warrants additional discussion. In identifying CFO changes, we face two empirical limitations. First, the annual CFO flag field in ExecuComp is relatively incomplete. Second, unlike CEO information in the database, there are no exact dates of when the CFOs enter or leave office. We solve the first problem by using a string search method through executive annual titles, similar to the approach employed by Chava and Purnanandam (2009). Specifically, in addition to any available annual CFO flags, we search for specific strings such as "CFO," "FINANCE," "TREASURER," and "CONTROLLER" through executive annual titles. We assign scores to each string so that the individual who is most likely the CFO of a given year would have the highest score (e.g., "CFO" is assigned a score of 10). We also search for other words that might be related to these strings (e.g., "FINANCIAL"). In cases where more than one executive survive the string search and share the same score from the string search, we take the one with the highest annual compensation as the CFO of the firm. Having identified the set of CFOs and their links to firms, we attempt to solve the second problem by approximating the start (end) date of a CFO for a certain firm by the earliest (latest) fiscal year begin (end) date for any sequence of fiscal years in which the person is classified as the CFO of that firm.

The set of audit committee chairs are extracted from the directors file of RiskMetrics. We identify a board member as the chair of audit committee for her firm during a certain fiscal year if that person is in charge as of the board meeting date for the next fiscal year. As with our approach for CFOs, we approximate the start and end dates of an audit committee chair for a certain firm by the earliest and latest fiscal year begin and end dates, respectively, when the person is classified as the audit committee chair of that firm.[13]

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[13]The directors file of RiskMetrics provides coverage beginning in 1996, however, data for audit committee chair is not available until 1999 and beyond.

As we discuss later, we use a nearest neighbor matching estimator approach for much of our empirical analysis. Thus, we identify the replacement events of the potential related parties for the sample of misreporting firms and for all non-misreporting firms that compose the set of potential matches. Because we need all replacement events for all non-misreporting firms that compose the set of potential matches, hand collecting data items missing from the respective databases is infeasible.

Firm Level and Macroeconomic Controls

We extract firm level data from Compustat to control for firm specific characteristics that have been shown to be determinants of loan spreads. We include the natural log of total assets as a proxy for firm size because we expect larger firms to have easier access to external financing and have lower asymmetric information problems (and therefore lower monitoring costs). We use the market-to-book ratio of assets (sum of market value of equity and book value of debt scaled by book value of total assets) to indicate growth opportunities, which we expect to be negatively related to loan spread. We control for default risk with three variables: (i) book leverage ratio (sum of short-term and long-term debt scaled by total assets), (ii) profitability (EBITDA over total assets), and (iii) modified Altman's (1968) z-score.[14] We expect firms with higher default risk (i.e., higher leverage ratios, lower profitability, and/or lower z-scores) to pay higher loan spreads. We control for asset tangibility (net PP&E, scaled by total assets) and predict a negative relation with loan spread because tangible assets can likely be recovered at greater rates in the

[14] The modified z-score is defined as `(1.2 * wcap + 1.4 * re + 3.3 * pi + 0.999 * sale)/at` in terms of Compustat items names. We use the modified z-score instead of the traditional version of Altman (1968) in our analysis because we are also controlling for the market-to-book ratio of total assets. See Graham, Lemmon, and Schallheim (1998) and Graham, Li, and Qiu (2008).

event of default. We use cash flow volatility (standard deviation of quarterly net cash flow from operating activities for the prior four fiscal years before the start date of a loan, scaled by the sum of long-term and short-term debt) as a proxy for earnings risk and expect it to be positively related to loan spread. We also control for industry effects using indicator variables based on the two-digit SIC codes. Details regarding firm-level variable definitions and construction are in Panel B of Table 2.1.

We obtain macroeconomic control variables from the Board of Governors of the Federal Reserve System (http://www.federalreserve.gov/). We construct (1) credit spread as the difference between AAA and BAA corporate bond yields, and (2) term spread as the difference between 10-year and 2-year Treasury yields. These measures serve as proxies for macroeconomic conditions and have been shown to be related to bond market returns (Chen, Roll, and Ross, 1986; Fama and French, 1993). We also include an indicator for loans made during the 1996 to 2000 economic boom period as an additional macro-level control. A summary of macroeconomic variable definitions is in Panel C of Table 2.1.

Throughout our study, all nominal variables are adjusted to January 2006 dollars using the consumer price index (CPI) provided by the Bureau of Labor Statistics of the U.S. Department of Labor (http://www.bls.gov/). We winsorize loan and firm-specific variables at the 1% and 99% levels to reduce potential problems created by outliers.

2.1.2 Summary Statistics

Table 2.2 presents descriptive statistics for our sample before and after the restatements. Panel A contains descriptive statistics for the entire GAO restatements sample, while Panel B has statistics for only restatements identified as "irregular" according to Hennes, Leone, and Miller (2008). We focus discussion on statistics for the irregularities subsample. We also present the results of difference in mean and median tests, based on mean-per-firm level observations (i.e., pre-restatement loans for a given firm are averaged to form one observation, and likewise for post-restatement loans).

The mean (median) pre-restatement loan spread of 161 basis points (bps) (125bps) increases significantly to 261bps (250bps) in the post-restatement period (p-value $<$ 0.01). At the median, the increase is a doubling of the loan spread, which suggests that misreporting has a large effect economically. A smaller but still statistically significant increase in loan spread from pre-restatement to post-restatement is evident for the full GAO sample (in Panel A). The magnitudes of the increases in mean and median spreads confirm the importance of distinguishing irregular restatements from those categorized as errors based on Hennes, Leone, and Miller (2008).

As shown in Panel B, there are also statistically significant increases in mean and median loan maturity, and significant decreases in loan-to-asset ratio and the loan syndicate size. Post restatement loans are significantly more likely to be secured than are pre-restatement loans. Mean loan size and the proportion of loans with performance pricing features do not differ significantly across the pre and post-restatement periods.

Mean and median firm size (total assets) and leverage are significantly larger in the post-restatement period than in the pre-restatement period. The magnitude of the increase in firm size reflects in part the fact that larger firms have a relatively greater number of post-restatement loans than smaller firms do; untabulated results using mean-per-firm level observations show a smaller increase in firm size. Mean and median market-to-book assets, profitability, and tangibility show significant decreases from the pre to post-restatement periods. Mean cash flow volatility does not differ significantly across the pre and post-restatement periods.

In summary, there are significant differences in important loan and firm characteristics across the pre and post-restatement periods. Macroeconomic characteristics may also differ across a firm's pre and post-restatement periods. Given these differences, we next move to regressions and a matched analysis to examine the effects

Table 2.2
Descriptive Statistics for Section 2.

This table presents pre- and post-restatement descriptive statistics for loan and firm specific variables of the main sample. Panel A presents information for the all financial restatements within the GAO database, while Panel B presents that for the irregularities subset within the GAO restatements. Irregularity is defined according to Hennes, Leone, and Miller (2008). The difference in mean and median tests are computed using mean-per-firm level observations (loans for a given firm in each period are averaged into one observation).

	Before Restatement				After Restatement				Paired Diff p-val	
	N	Mean	Median	Std Dev	N	Mean	Median	Std Dev	Mean	Median
Panel A: Full GAO Sample										
Loan Specific Variables										
Loan Spread (Basis Points)	2330	172.21	150.00	137.26	1552	223.25	200.00	167.93	0.00	0.00
Maturity (Months)	2330	42.04	36.00	23.54	1552	45.25	51.00	22.90	0.00	0.00
Loan Size (Millions USD)	2330	326.89	154.20	485.30	1552	452.71	205.42	621.31	0.00	0.00
Loan-to-Asset Ratio	2330	0.24	0.15	0.27	1552	0.19	0.12	0.28	0.00	0.00
Performance Pricing?	2330	0.49	0.00	0.50	1552	0.49	0.00	0.50	0.17	0.18
Secured Loan?	1589	0.69	1.00	0.46	1166	0.76	1.00	0.43	0.00	0.00
Syndicate Size	2330	9.06	6.00	9.39	1552	8.70	6.00	8.19	0.37	0.33
Firm Specific Variables										
Total Assets (Millions USD)	2330	3749.60	885.46	6742.31	1552	5598.86	1569.98	8972.94	0.00	0.00
Market-to-Book Assets	2330	1.78	1.46	0.99	1552	1.66	1.38	0.87	0.00	0.00
Leverage Ratio	2330	0.32	0.29	0.21	1552	0.33	0.30	0.23	0.02	0.19
Profitability	2330	0.14	0.13	0.08	1552	0.11	0.11	0.08	0.00	0.00
Tangibility	2330	0.36	0.32	0.23	1552	0.32	0.28	0.22	0.00	0.00
Cash Flow Volatility	2330	0.73	0.09	2.83	1552	0.62	0.08	2.63	0.12	0.15
Panel B: Irregularities Sample										
Loan Specific Variables										
Loan Spread (Basis Points)	783	160.83	125.00	130.64	533	260.63	250.00	176.72	0.00	0.00
Maturity (Months)	783	40.89	36.00	23.31	533	44.58	48.00	23.59	0.01	0.00
Loan Size (Millions USD)	783	417.31	189.53	594.37	533	519.81	222.21	690.95	0.10	0.07
Loan-to-Asset Ratio	783	0.23	0.15	0.25	533	0.17	0.11	0.29	0.00	0.00
Performance Pricing?	783	0.47	0.00	0.50	533	0.42	0.00	0.49	0.22	0.33
Secured Loan?	518	0.68	1.00	0.47	412	0.81	1.00	0.40	0.00	0.00
Syndicate Size	783	10.89	7.00	10.85	533	8.86	6.00	8.97	0.01	0.03
Firm Specific Variables										
Total Assets (Millions USD)	783	4857.17	1164.45	7701.29	533	6762.24	1702.88	9773.85	0.00	0.00
Market-to-Book Assets	783	1.87	1.53	1.09	533	1.61	1.35	0.88	0.00	0.00
Leverage Ratio	783	0.31	0.28	0.20	533	0.36	0.32	0.23	0.00	0.00
Profitability	783	0.14	0.14	0.07	533	0.10	0.10	0.08	0.00	0.00
Tangibility	783	0.30	0.28	0.19	533	0.28	0.24	0.19	0.01	0.00
Cash Flow Volatility	783	0.55	0.09	2.27	533	0.42	0.07	2.02	0.24	0.13

of restatements on loan spreads while controlling for loan, firm, and macroeconomic characteristics.

2.2 Loan Spread Regressions

2.2.1 Full GAO Sample and Irregular Restatements

We first estimate regressions to examine the change in spreads following restatements, while controlling for other firm level and loan level characteristics that may have changed following the restatement. We begin with a replication of Graham, Li, and Qiu's (2008) findings on our expanded sample. Our regression model is specified as follows:

$$y_{in} = \alpha d_{in}^{\text{post}} + X_{in}'\hat{\mathbf{B}} + \epsilon_{in}, \tag{2.1}$$

where the dependent variable y_{in} is the natural log of loan spread for loan n made to firm i and d_{in}^{post} is the post-restatement indicator for that loan, taking the value of one for loans made after the announcements of financial restatements and zero for loans issued before the announcements. X_{in} denotes the vector of firm and loan level covariates as of the facility start date of the loan, along with a constant. The key explanatory variable of interest is the post-restatement indicator d_{in}^{post}. Given that the dependent variable is the natural log of the loan spread, the estimated coefficient on the post-restatement indicator variable, α, can be interpreted as the percentage increase in post-restatement loan spread over pre-restatement levels beyond that explained by firm and loan level controls specified in X_{in}. All standard errors are heteroscedasticity-consistent and clustered by firm.

We first estimate regressions for the full sample of GAO restatements from 1997 to 2006 and present results for regressions estimated over the subsample of the irregular-type restatements in Table 2.3.[15] The specifications are generally as employed by Graham, Li, and Qiu (2008), except for the sample period being extended and our industry fixed effects being defined at the two-digit SIC level, rather than the one-digit level in their base specifications. Other studies, such as Bradley and Roberts (2003) and Chava, Livdan, and Purnanandam (2009), use similar specifications.

Model 1 simply gives the effect of the post-restatement dummy itself alone on the natural log of loan spread. In models 2 through 4, we add incrementally firm characteristics, loan specific variables, and macro-economic controls. Model 5 adds variables to capture industry, loan type, and deal purpose fixed effects. In model 6 we include firm fixed effects instead of industry effects, and the results are similar to those in model 5.[16] Comparing our results to those presented in Graham, Li, and Qiu (2008), the adverse effect of financial misreporting on bank loan pricing are economically consistent and accord with the intuition that firms that intentionally misreport financial information should see more adverse effects on loan pricing than firms that have error-related accounting restatements. The results are also consistent with the differences in abnormal returns around the restatement announcement date documented by Hennes, Leone, and Miller (2008) and Palmrose, Richardson, and Scholz (2004) for irregular restatements versus error-related restatements.

Throughout the regressions, the signs and magnitudes of coefficient estimates on the firm, loan, and macro level control variables are generally as expected and in line with results from prior studies. Larger firms, firms with higher market-to-book ratio, and more profitable firms have lower loan spreads, whereas firms with higher leverage and cash flow volatility pay higher loan spreads. Also as expected, firms

[15]We do not tabulate replication results to conserve space. For the time period and sample considered in Graham, Li, and Qiu (2008), we are also able to closely replicate their findings.

[16]One might be concerned about firms switching industries throughout the sample period. In untabulated results, we estimated the same regression as models 5 and 6 of Table 2.3 with both industry and firm fixed effects included. The results are similar to those reported.

Table 2.3
Loan Spread Regressions.

This table presents replication results of loan spread OLS regressions from Graham, Li, and Qiu (2008) using the extended data, containing all loans made to restatement firms which are found in the two full GAO reports (recording restatements which occurred between January of 1997 and June of 2006). The regression models show the effect of financial restatements on cost of bank debt. The dependent variable in all models is the natural log of all-drawn loan spread. Models are estimated using only those made to firms associated with irregular restatements. The main explanatory variables is *restatepost*, a dummy variable indicating whether the loan facility start date is after the date for which the corresponding financial restatement occurred. Model 1 examines the effect of the *restatepost* dummy alone, while models 2 to 5 incrementally adds to model 1 firm specific controls, loan specific controls, macroeconomic level controls, and fixed effects, respectively. Industry fixed effects are according to the 2-digit SIC codes. Loan type (revolvers less or greater than 1 year, term loans, 364-day loans, etc.) and deal purpose (corporate, repayment, takeover, working capital, etc.) fixed effects control for additional loan characteristics. Model 6 replaces industry fixed effects in model 5 with firm fixed effects. All models are estimated with robust standard errors clustered by firm. Definitions and details for each variable can be found in Table 2.1.

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	Estimate	*t*-val	Estimate	*t*-val	Estimate	*t*-val	Estimate	*t*-val	Estimate	*t*-val	Estimate	*t*-val
restatepost	0.5721	(6.23)	0.4292	(5.93)	0.4222	(5.68)	0.3737	(4.76)	0.3432	(4.92)	0.3627	(4.82)
lnassets			-0.2248	(-10.97)	-0.1752	(-4.91)	-0.2042	(-5.89)	-0.1759	(-5.99)	-0.0699	(-1.36)
mb			-0.1170	(-3.77)	-0.1171	(-3.80)	-0.1048	(-3.21)	-0.0574	(-2.22)	-0.0496	(-1.46)
leverage			0.9532	(5.72)	0.9439	(5.60)	0.9141	(5.53)	0.6574	(5.36)	0.5744	(2.31)
profitability			-2.5033	(-5.46)	-2.2799	(-4.96)	-2.2921	(-5.16)	-2.5048	(-6.51)	-2.1385	(-4.05)
tangibility			0.2778	(1.31)	0.3038	(1.48)	0.3344	(1.68)	0.0829	(0.32)	0.1796	(0.51)
cfvol			0.0235	(2.47)	0.0216	(2.26)	0.0203	(2.20)	0.0157	(1.78)	0.0068	(0.86)
modzscore			-0.0768	(-2.69)	-0.0704	(-2.52)	-0.0630	(-2.21)	-0.0294	(-1.18)	0.0119	(0.23)
lnt					0.0611	(1.96)	0.0690	(2.20)	-0.1081	(-2.93)	-0.0983	(-2.42)
lnloansize					-0.0697	(-1.83)	-0.0400	(-1.10)	-0.0185	(-0.61)	-0.0397	(-1.37)
perfprice					-0.0814	(-1.55)	-0.0864	(-1.61)	-0.0380	(-0.79)	-0.0813	(-1.53)
crdtsprd							0.6232	(5.03)	0.6248	(5.52)	0.4732	(3.96)
termsprd							0.0662	(1.74)	0.0806	(2.80)	0.1128	(3.43)
econboom							-0.0030	(-0.04)	-0.0234	(-0.34)	-0.0396	(-0.53)
intercept	4.7281	(57.82)	6.6528	(42.47)	6.4330	(37.46)	5.8725	(26.47)	4.8862	(18.22)	5.1225	(12.44)
R2	0.096		0.504		0.513		0.541		0.661		0.746	
N	1316		1316		1316		1316		1316		1316	
Firms	140		140		140		140		140		140	
Fixed Effects												
Loan Type	No		No		No		No		Yes		Yes	
Deal Purpose	No		No		No		No		Yes		Yes	
Industry	No		No		No		No		Yes		No	
Firm	No		No		No		No		No		Yes	

with lower financial distress risk, as indicated by the modified Altman's (1968) z-score, pay lower spreads. For loan specific characteristics, loans of longer maturity and larger amount are associated with lower spreads.

In summary, we replicate on an expanded sample Graham, Li, and Qiu's (2008) findings that firms pay greater loan spreads following restatements, and that the effect is larger for irregular restatements (i.e., those that stem from apparently intentional misreporting based on the classification scheme of Hennes, Leone, and Miller, 2008). We next turn to tests of the hypothesis that misreporting creates a long-lasting and costly stigma evident in bank loan spreads. Given the nature of our research questions, the remainder of our analysis uses only the set of irregular restatements.

2.2.2 How Long Do Firms Pay a Misreporting Premium?

This section extends the analysis in the prior section to examine how long firms continue to pay higher spreads on their bank loans after intentionally misreporting their financial information. Instead of including one post-restatement indicator variable in the regression specification, we include separate indicator variables for each of the first five post-restatement years. The number of loans in years beyond +5 years is relatively small, so we combine loans that occur beyond the first five post-restatement years into one indicator variable. We also include a separate indicator variable for the year immediately preceding the restatement because Graham, Li, and Qiu (2008) find evidence suggesting some degree of information leakage in the period immediately prior to the restatement. We also include two-day abnormal returns measured on firms' restatement dates.[17] The abnormal return measured on

[17]We present results for the two-day cumulative abnormal stock returns of days 0 and -1 of the restatement announcement date. The results are qualitatively similar to those reported when we use the (-1,0) or (-1,+1) window abnormal returns.

a restatement date should capture the relative severity and magnitude of the misreporting, at least from the viewpoint of equity investors when they learn the new (true) accounting figures. We estimate market-model parameters during the one-year trading period ending 250 days prior to the restatement date.There is only one restatement announcement abnormal return per firm, so we can include the abnormal return measure only in the models without firm fixed effects.

The results are in Table 2.4. Similar to the layout in the previous table, we include in the first model only period indicators without any additional controls, and then incrementally add into the specification firm specific controls, loan specific controls, and macroeconomic level controls. Model 5 adds loan type, deal purpose, and industry fixed effects into the estimation, and Model 6 replaces industry fixed effects with firm fixed effects. We observe that the adverse effect of irregular restatement events on loan spread lasts for at least five years after the respective restatements. Using model 6 as an example, where we include all firm, loan, and macro level controls and firm, loan type, and deal purpose fixed effects, we observe percentage increases in post-restatement spreads compared to pre-restatement levels of approximately 51%, 50%, 38%, 45%, and 23% for the first five post-restatement years, respectively. All coefficients are significant at the 5% level or higher. Additionally, the coefficient on the indicator variable that combines loans in years six through ten is 35%, with a t-statistic of 2.22.

Thus, the premium that misreporting firms pay in their loan spreads is long lasting. As Graham, Li, and Qiu (2008) note, a restatement can convey new information that leads to downward revisions in cash flow and firm value estimates, and also convey new information that causes capital suppliers to question the credibility and veracity of a firm. It seems unlikely that restatements cause banks to continue revising downward estimates of cash flow and value five year after the restatement, so we interpret the long-lasting effect of restatements on loan spreads as consistent with the hypothesis that misreporting creates a long-lasting and costly stigma.

Table 2.4

Effect of Restatements on Loan Spread Over Time.

The table presents regression results showing the effect of financial restatements on cost of bank debt over time after the restatement announcement. All models are estimated using only loans that are made to firms associated with irregular restatements, as classified by Hennes, Leone, and Miller (2008). The dependent variable in all models is the natural log of all-drawn loan spread. The main explanatory variables are $restatepost(p)$, where p is an integer from 1 to 6, indicating whether the loan facility start date is during the pth post-restatement year for $p = 1$ to 5, or after the 5th post-restatement year for $p = 6$. All models are estimated with robust standard errors clustered by firm. Model 1 examines the effect of the $restatepost$ dummy alone, while models 2 to 5 incrementally adds to model 1 firm specific controls, loan specific controls, macroeconomic level controls, and fixed effects, respectively. Industry fixed effects are according to the 2-digit SIC codes. Loan type (revolvers less or greater than 1 year, term loans, 364-day loans, etc.) and deal purpose (corporate, repayment, takeover, working capital, etc.) fixed effects control for additional loan characteristics. Model 6 replaces industry fixed effects from model 5 with firm fixed effects. Definitions and details for each variable can be found in Table 2.1. The variable $car\_restate$ is the two-day cumulative abnormal stock returns of days 0 and -1 of the restatement announcement date. Benchmarks for abnormal returns are obtained by estimating the market model during the one-year trading period ending 950 days prior to the restatement date.

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	Estimate	t-val	Estimate	t-val	Estimate	t-val	Estimate	t-val	Estimate	t-val	Estimate	t-val
$restatepostn1$	0.4750	(4.05)	0.4851	(5.29)	0.4984	(5.69)	0.4231	(4.61)	0.3447	(4.92)	0.2958	(3.72)
$restatepost1$	0.7568	(6.19)	0.6760	(5.88)	0.6684	(5.71)	0.6145	(5.32)	0.5223	(4.69)	0.5078	(4.40)
$restatepost2$	0.8779	(8.08)	0.6370	(6.16)	0.6236	(6.00)	0.5157	(4.87)	0.4715	(5.50)	0.5000	(5.08)
$restatepost3$	0.6170	(5.02)	0.3906	(4.21)	0.3850	(4.12)	0.3216	(3.13)	0.3179	(3.33)	0.3764	(3.77)
$restatepost4$	0.5847	(4.42)	0.4758	(4.33)	0.4704	(4.39)	0.3868	(3.38)	0.3856	(3.91)	0.4513	(3.85)
$restatepost5$	0.3249	(2.22)	0.3271	(3.09)	0.3110	(2.86)	0.2149	(1.86)	0.2345	(2.12)	0.2338	(2.01)
$restatepost6$	0.2473	(1.08)	0.3401	(2.30)	0.3411	(2.44)	0.2641	(1.80)	0.2589	(2.09)	0.3544	(2.22)
$car\_restate$	-0.1464	(-0.32)	-0.1155	(-0.41)	-0.1272	(-0.48)	-0.1469	(-0.58)	-0.0067	(-0.03)		
$lnassets$			-0.2329	(-12.33)	-0.1847	(-5.74)	-0.2102	(-6.78)	-0.1848	(-6.83)	-0.1004	(-2.04)
mb			-0.1262	(-4.56)	-0.1243	(-4.50)	-0.1113	(-3.89)	-0.0670	(-2.84)	-0.0495	(-1.43)
$leverage$			0.9116	(5.49)	0.8935	(5.38)	0.8820	(5.43)	0.6271	(4.77)	0.5028	(1.95)
$profitability$			-2.0941	(-4.59)	-1.9007	(-4.18)	-1.9668	(-4.49)	-2.1913	(-5.64)	-1.9659	(-3.64)
$tangibility$			0.3059	(1.45)	0.3348	(1.63)	0.3564	(1.81)	0.1373	(0.54)	0.2317	(0.65)
$cfvol$			0.0232	(2.96)	0.0213	(2.74)	0.0202	(2.62)	0.0147	(1.80)	0.0066	(0.88)
$modzscore$			-0.0769	(-2.83)	-0.0711	(-2.68)	-0.0639	(-2.34)	-0.0318	(-1.33)	0.0077	(0.15)
lnt					0.0826	(2.79)	0.0860	(2.88)	-0.0875	(-2.54)	-0.0805	(-2.13)
$lnloansize$					-0.0662	(-1.84)	-0.0397	(-1.16)	-0.0186	(-0.64)	-0.0383	(-1.36)
$perfprice$					-0.0850	(-1.73)	-0.0861	(-1.71)	-0.0408	(-0.88)	-0.0877	(-1.68)
$crdtsprd$							0.5524	(4.34)	0.5643	(5.05)	0.4281	(3.89)
$termsprd$							0.0372	(1.03)	0.0578	(2.00)	0.0990	(3.00)
$econboom$							-0.0582	(-0.78)	-0.0572	(-0.82)	-0.0545	(-0.67)
$intercept$	4.6431	(47.80)	6.5921	(43.40)	6.2908	(38.18)	5.8580	(27.31)	4.9123	(19.05)	5.2956	(13.83)
$R2$	0.139		0.535		0.545		0.567		0.674		0.755	
N	1316		1316		1316		1316		1316		1316	
$Firms$	140		140		140		140		140		140	
Fixed Effects												
Loan Type	No		No		No		No		Yes		Yes	
Deal Purpose	No		No		No		No		Yes		Yes	
Industry	No		No		No		No		Yes		No	
Firm	No		No		No		No		No		Yes	

The effects are also large economically. For example, there is a 51% increase in spread for loans that are made during the one-year period after the restatement announcements compared to the ones that are made before. The mean pre-restatement loan spread is 161bps for firms associated with irregular restatements, so the 51% increase implies an additional 82bps cost of bank loans during the first post-restatement year over the cost of pre-restatement loans. This can be interpreted as a "misreporting premium" of 82bps higher spread for loans made during this year compared to those made prior to the event announcement. In dollar terms, with a mean post-restatement loan size of $520 million and an average maturity of 45 months, the increased cost to firms due to these spread increases is about $16 million per loan during this first post-restatement year. Following this same logic, we can see that the marginal dollar cost during the second year is also about $16 million per loan. For years 3 and 4 of the same model, the same calculations generate figures of $12 million and $14 million, respectively. For year 5 (beyond year 5), point estimates imply marginal dollar costs of $7 million ($11 million).[18]

Similar to results in Graham, Li, and Qiu (2008), we find evidence of information leakage in the period immediately prior to the restatement: the coefficient on the year -1 indicator variable is 0.2958 (p-value < 0.01). Although statistically and economically significant, the magnitude of the coefficient is smaller than for the years immediately following the restatement. This indicates that the restatement event itself has a material marginal effect on loan spreads.

Results for the control variables are generally as expected and in line with results from prior studies. Notably, however, the restatement announcement abnormal return variable that we include to capture the severity of the misreporting is not

[18] In untabulated results, we estimated the same model for our non-irregularities sample (firms whose restatements stem from unintentional errors). The estimated coefficients are all below 20%, indicating much smaller spread increases for misreporting that was apparently due to errors. These results are available from the authors upon request.

statistically significant in any of the regressions that include it. To the extent that the abnormal return variable does capture the severity of the misreporting, the lack of significance implies that banks are more concerned with the mere fact that a firm misreported.

In sum, using loan spread models similar to those in extant literature, we find that irregular restatements result in significant premiums in bank loan spreads over pre-restatement levels. The larger spreads are economically significant and continue for at least five years following the restatements. Given average loan sizes, the effects translate into millions of dollars in additional interest costs per loan.

2.3 Matched Analysis

2.3.1 Bias-corrected Nearest Neighbor Matching Estimator

In the previous section, we compare post-restatement loan spreads to pre-restatement loans spreads among the set of misreporting firms. The analysis shows a significant spread increase in the post-restatement period that lasts at least five years. In this section, we examine the effect of misreporting on loan spreads using a matched loan-matched firm approach. Specifically, for each post-restatement loan made by a misreporting firm (treatment loan), we find a non-misreporting control firm-loan during the same year that (1) is of the same loan type, same deal purpose, and within the same industry at the two-digit SIC level, if possible, and (2) has the minimum weighted distance to the treatment loan, based on firm and loan level covariates, out of all potential candidates. That is, for the vector of firm and loan characteristic covariates X_{inp} of loan n issued to misreporting firm i during $\bar{p}$ (where is $\bar{p}$ is defined relative to the firm's restatement date), we search for a loan m made to a non-misreporting firm j, with covariates X_{jmp} during that same year $\bar{p}$ that minimizes a distance function, denoted as $D(X_{inp}, X_{jmp}; p = \bar{p})$. Specifically,

$$X_{jmp} = \arg\min_{X_{jmp}} D(X_{inp}, X_{jmp}; p = \bar{p}), m \notin N, \tag{2.2}$$

where the distance function, defined as

$$D(X_{inp}, X_{jmp}; p = \bar{p}) \equiv \left[(X_{inp} - X_{jmp})' V_X (X_{inp} - X_{jmp})\right]^{\frac{1}{2}}, p = \bar{p}, \tag{2.3}$$

is essentially the Euclidean norm of vector $X_{inp} - X_{jmp}$ with p fixed to $\bar{p}$. V_X is a positive definite, diagonal weighting matrix consisting of inverse variances of each element in the characteristics vector (i.e., the matching dimensions). N denotes the set of treatment loans (i.e., loans made to misreporting firms). Note that in the above illustration, firm subscripts i and j can be dropped since loan subscripts n and m are already sufficient in describing the treatment and control samples of loans. We retain them, however, for ease of discussion. We perform a one-to-one match for each treatment *loan*, so a single misreporting firm may have more than one matched firms. Our matches are drawn from the pool of potential controls with replacement, so some treatment loans may share a matched control loan. Choosing matches with replacement ensures the closest possible matches across the sample observations.

Using simple matching estimators with inexact matches in a finite sample can produce bias in the estimated treatment minus control loan difference. Thus, we follow Rubin (1973) and Abadie and Imbens (2006) in adjusting the difference within matches for differences in the values of matching covariates. We first approximate control spreads on the matched observations $\hat{y}_{jmp}$, that is,

$$\hat{y}_{jmp} = X'_{jmp} \hat{\mathbf{B}}_p^{JM} \tag{2.4}$$

for some fixed period $p = \bar{p}$, where the coefficients vector $\hat{\mathbf{B}}_p$ satisfies the linear least squares function

$$\hat{\mathbf{B}}_p^{JM} = \arg\min_{\mathbf{B}_p^{JM}} \sum_{jmp} k_{jmp} \left(y_{jmp} - X'_{jmp} \mathbf{B}_p^{JM}\right)^2; m \in M, p = \bar{p}. \tag{2.5}$$

Control loan m belongs to the set M of matched control observations for treatment loans in N. k_{jmp} denotes the number of times that a certain loan $m \in M$ to firm j during year p relative to the restatement announcement date is being used as a match. The set of potential outcomes for the treatment sample can then be written as

$$\begin{cases} \tilde{y}^{\mathrm{C}}_{inp} &= y_{jmp} - \hat{y}_{jmp} + X'_{inp}\hat{\mathbf{B}}^{JM}_{p} \\ \tilde{y}^{\mathrm{T}}_{inp} &= y_{inp} \end{cases} \tag{2.6}$$

for our bias-corrected matching estimator, where $\hat{\mathbf{B}}^{JM}_{p}$ is defined as in Equation 2.5. Note that in the above set of potential outcomes, we implicitly set the number of matches for each treatment observation to one given our main setup, as mentioned earlier.[19] We tested the yearly average treatment effect using one to four matches per treatment observation, and for each test the results are qualitatively similar to those reported.

Finally, the average treatment effect τ of period p for our sample of loans made to misreporting firms can be written as

$$\tau_p = \frac{1}{N_p} \sum_{inp} \left[y_{inp} - \tilde{y}^{\mathrm{C}}_{inp} \right], \tag{2.7}$$

where N_p denotes the total number of observations in the misreporting firm sample of loans during event year p. Intuitively, spreads charged on loans issued to misreporting firms are captured by the observed y_{inp} in Equation 2.7, while spreads charged on loans issued to these firms had they not misreported are approximated by $\tilde{y}^{\mathrm{C}}_{inp}$.[20]

2.3.2 Year-by-year Spread Differences

Following the above procedure, we estimate the average treatment minus control difference in the natural log of spreads for our sample of loans made to misreporting

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[19] If we denote the number of matches for treatment loan n by $n^{\#}$ and some control loan for n as m_n, then the untreated outcome in Equation 2.6 becomes $\tilde{y}^{\mathrm{C}}_{inp} = \frac{1}{n^{\#}} \sum_{m_n} \left[y_{jmp} - \hat{y}_{jmp} + X'_{inp}\hat{\mathbf{B}}^{JM}_{p} \right]$.

[20] A recent paper by Almeida, Campello, Laranjeira, and Weisbenner (2009) also uses this matching estimator to test if firms in need of refinancing long-term debt during a credit crisis alter decisions related to real-side variables.

firms during each of the five post-restatement years following the announcement dates, and for years six through eight combined. There are 626 loans made to 180 firms that had irregular restatements in the five post-restatement years, and 66 loans made to 31 firms in years six through eight. The results of our matched analysis are in Panel A of Table 2.5. The abbreviation "SATT" in the table stands for "sample average treatment effect for the treated," which is simply τ_p for each period p as we define in Equation 2.7. As in the previous section, a year-one loan indicates that the loan was initiated within one year of the restatement date, a year-two loan was initiated between one and two years after the restatement, and so on for p out to five. For sample size reasons, we combine years six through eight into one group and report results for it.

As shown in Panel A of Table 2.5, misreporting firms pay loan spreads significantly greater than do matched firms in each of the first five post-restatement years. The spread differences over matched firms are 53%, 35%, 31%, 30%, and 34% for the first through the fifth post-restatement years, respectively. Each difference is statistically significant with a p-value < 0.01. Not surprisingly, the greatest spread difference occurs in the year immediately following the restatement announcement. Using the first year spread difference as an example, and recalling from Table 2.2 that spreads charged on loans during the post-restatement period for firms engaging in irregular restatements average 261bps (median 250bps), the implied spread without the 53% misreporting premium is 170bps (median 163bps). Given an average post-restatement loan size of $520 million and mean maturity of 45 months, the estimated treatment effect is also economically significant.[21]

[21]The estimated treatment effects are generally larger in magnitude than the coefficient estimates for the multivariate regression models shown in Table 2.4. The regression models are estimated within the restating firm sample and compare misreporting firms' spreads pre and post-restatement. Given Graham, Li, and Qiu's (2008) evidence of information leakage in bank loan spreads before the restatement itself, which we confirm, the smaller estimated differences of post-restatement minus

Table 2.5

Matched Analysis and Placebo Test.

This table presents results from loan-level matched analysis for the effects of irregular restatements on the cost of private debt. Panel A shows year-by-year estimations for the difference between the log of spreads charged on loans made to restatement firms and log of spreads charged on loans made to non-restatement firms. Irregularity is defined as in Hennes, Leone, and Miller (2008). During each of the five post-restatement years surrounding the restatement announcement, a bias-corrected matching technique based on the nearest neighbor estimator of Abadie and Imbens (2006) is employed at the loan level. Post-restatement loans are sorted according to the year relative to the restatement announcement date and categorized into one of the five years surrounding the announcement dates. For each loan in each year of the restatements sample, a matching loan, based on loan and firm level characteristics, is located with replacement. Panel B presents results of a placebo test, where the estimation method as demonstrated in Panel A is employed on an alternative set of loans made to 150 non-restatement firms. Non-restatement firms for the placebo test are randomly selected from the universe of non-restating firms with valid loan and firm level information. A date between the first and last loan start dates of these firms are assigned to these firms as their "virtual" restatement announcement dates.

Panel A: Matched Analysis				
Year (p)	Firms	Loans	SATT (τ_p)	z-stat
1	91	180	0.5272	8.62
2	87	159	0.3520	6.59
3	73	117	0.3118	4.66
4	58	108	0.2968	3.62
5	42	62	0.3436	3.63
6,7,8	31	66	-0.0105	-0.10
Panel B: Placebo Test				
Year (p)	Firms	Loans	SATT (τ_p)	z-stat
1	83	154	0.0488	0.71
2	57	89	0.0012	0.01
3	43	64	0.1675	1.00
4	29	46	0.0370	0.32
5	26	47	-0.0829	-0.62
6, 7, 8	29	62	0.0064	0.05

The results for the period combining the sixth through the eighth post-restatement year show no significant differences in loan spreads across misreporting firms and matched firms. One possible interpretation of these results is that six years or more is a sufficient amount of time for firms to rebuild their credibility in providing financial information to capital suppliers.

Panel B of Table 2.5 contains results of a "placebo test" to check the efficacy of our matching approach. In particular, we want to ensure that using the matching approach with the particular matching variables does not yield similar results for a random set of firms with random (pseudo) restatement dates. We randomly select 150 "pseudo treatment" firms from the universe of firms that have both loan and firm level information available. For each firm, we then randomly assign a pseudo restatement announcement date between its first and last loan dates. We then repeat the matching analysis used earlier in this section on this new group of pseudo-misreporting firms with pseudo-misreporting dates. The results are in Panel B of Table 2.5. For every pseudo-post-restatement period, the loan spread differences across treatment and control firms are small in magnitude and statistically insignificant. Thus, the matching approach does not find differences in spreads where there should be none. The results we find for the actual treatment firms are likely not driven by the nature of the matching approach itself. We conducted placebo tests several different ways; all produce results qualitatively similar to those reported.[22]

In summary, our results from employing matching estimators confirm the findings from the earlier within-misreporting-sample regression results: Firms that restate pay a significant premium in their loan spreads for five years. The differences in loan spreads between misreporting firms and matched firms become statistically insignif-

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pre-restatement spreads in the regressions likely stem from the fact that pre-restatement spreads reflect part of the negative news associated with the restatements.

[22]These results are available upon request.

icant in the period six to eight years following a restatement. The results support the hypothesis that misreporting creates a long-lasting and costly stigma. The next section uses the matching estimator approach to examine whether the turnover (replacement) of various parties that are potentially related to the misreporting shortens the duration that a firm pays a misreporting premium.

2.4 Effects of Replacements of Potentially-related Parties

We next test the hypothesis that the prompt replacement (or turnover) of parties who are potentially related to the misreporting lessens the severity or duration of the post-misreporting premium in loan spreads. Specifically, we identify changes in CEOs, CFOs, audit committee chairs, and external auditors of misreporting firms that occur during a 13-month time frame centered on the restatement announcement month. We use this 13-month window since it is the time period during which top executive turnovers are most likely to be related to the restatement event itself (Hennes, Leone, and Miller, 2008).[23] The 13-month window also allows us to differentiate firms that made replacements in potentially related parties *promptly*, rather than years after the restatement.

In many cases of apparently deliberate financial misreporting, there are no formal charges or sanctions brought against individuals by the Securities and Exchange Commission (SEC). In these cases, it might be difficult or impossible for capital

[23] Karpoff, Lee, and Martin (2008a) find a 93% turnover of the responsible parties during the SEC and Department of Justice enforcement periods for financial misrepresentations. The enforcement period in their study lasts for 57 months on average after the announcement date. In an earlier version of this paper, we employed a time-variant definition similar to that method (so that firms have different values in the turnover indicator throughout the post-restatement period) and found a high turnover rate for all four of our subjects of interest up to the fourth post-restatement year. Extending the change-determination window further beyond the restatement date, however, creates a trade-off between type I and type II errors and complicates interpretation of the results. Given our research questions, we follow the approach of Hennes, Leone, and Miller (2008) in focusing on a period closer to the restatement date.

suppliers to know exactly who at a firm was responsible for an event of misreporting. By examining the effects of replacing the four parties on which we focus, we hope to capture replacements of parties that are the most likely to have been involved in a misreporting or to have failed to detect it before financial figures were provided to outsiders. We refer to these parties collectively as *potentially-related parties*.

2.4.1 Differences in Turnover Ratios

We first compare the frequency of potentially-related party turnovers across misreporting firms and matched firms. The analysis in Table 2.6 compares turnover rates around the restatement announcement dates across the misreporting and the matched firms from the matched analysis in the previous section. We use the maximal number of observations in each case, so the sample size differs according to turnover data availability. There is CEO turnover information available for 115 out of the 180 firms associated with irregular restatements in our sample, 114 for CFOs, 87 for audit committee chairs, and 127 for external auditors.[24]

Panels A and B of Table 2.6 contain turnover counts and proportions for misreporting firms and matched non-misreporting firms, respectively. We note that there are more firms in the matched group than in the restating group. This is because we conduct a year-by-year matching method at the loan level. A restating firm can have loans in each of the five post-restatement years following the announcement date of the restatement. There can be different matches for each loan that a restating firm makes, so across the post-restatement periods, a restating firm can have multiple control firms matched to it.

[24] We present comparison on turnover rates for restating firms that receive loans during one or more of the first five post-restatement years. If we include firms that do not receive loans during the first five post-restatement years, but during years 6,7, or 8 to the comparison, our results do not change qualitatively.

Table 2.6
Turnovers Ratios.

This table presents a comparison in the turnover frequencies of CEOs, CFOs, audit committee chairs (ACC), and external auditors (AUD) between borrowing firms in the treatment and control groups in Panels A and B of Table 2.5, respectively, during the 13-month time frame surrounding restatement announcement dates. Turnover rates for the treatment group firms are presented in Panel A, while turnover rates for the control group firms are presented in Panel B. The treatment group firms are those which are associated with irregular financial restatements, as identified by Hennes, Leone, and Miller (2008). Panel C presents results from testing the differences in proportions of turnover ratios for each of the four parties between the two groups.

	CEO		CFO		ACC		AUD	
Panel A: Restated Firms								
Change	35	30.43%	35	30.70%	15	17.24%	27	21.26%
Total	115		114		87		127	
Panel B: Firms in Matched Loans Sample								
	CEO		CFO		ACC		AUD	
Change	37	11.97%	57	18.63%	44	17.81%	23	8.61%
Total	309		306		247		267	
Panel C: Difference in Proportions Tests								
	CEO		CFO		ACC		AUD	
Difference		18.46%		12.07%		-0.57%		12.65%
z-stat		3.95		2.48		-0.12		3.15

As shown in Table 2.6, 30% of the restatement firms experience CEO turnovers, which compares to 12% for the non-restating matched firms. For CFOs, the turnover proportion is 31% for misreporting firms versus 19% for matched non-misreporting firms. For external auditors, 21% of misreporting firms and 9% of matched non-misreporting firms experience a replacement. All three of these differences in turnover proportions are statistically significant. In contrast to the other three groups of parties, there is no significant difference in the replacement rates of audit committee chairs across misreporting and matched firms.

We cannot tell whether the higher percentages of turnover events for misreporting firms are independent decisions of the respective firms or stem from pressure created by shifts in control rights to banks. Studies such as Chava and Roberts (2008) and Nini, Smith, and Sufi (2009) stress the importance of creditor control rights. Roberts and Sufi (2009) further show that creditor rights are stronger when firms are bank-dependent because the borrower's alternative sources of finance are more costly. Our research questions do not depend on whether firms decided independently to initiate the turnovers or decided under pressure from banks. We are instead interested in whether post-restatement premiums in loan spreads differ across firms that do and do not replace potentially-related parties.

2.4.2 Effects of Replacements on Loan Spreads

We next examine the effects of replacing potentially-related parties on the duration of the misreporting premium in evident loan spreads. The analysis in the previous section showed that misreporting firms replace potentially-related parties at higher rates than matched non-misreporting firms. Because the replacement events themselves may have an effect on loan spreads, we change the matched firm sample to account for this. Specifically, we continue to use the nearest neighbor matching estimator approach of Abadie and Imbens (2006), but we add the restriction that a potential match must have a change in the respective party in the same time period.

For example, when we seek matches for a misreporting firm that replaced its CEO in the 13 months centered on its restatement year, we require the non-misreporting matched firm to have also replaced its CEO in that period. This approach should control for the direct effects of the replacements themselves on loan spreads, independent of whether a firm restated.

Results for how turnovers in CEOs, CFOs, audit committee chairs, and external auditors can affect the misreporting premium in loan spreads are presented in Panels A, B, C, and D of Table 2.7, respectively. As shown in Panel A, misreporting firms with prompt CEO replacements pay significant premiums in the loan spreads compared to matched firms in all five post-restatement years. Although not monotonic, there appears to be a downward trend in the magnitudes of the misreporting premium over time, but even in year five, misreporting firms that promptly replaced their CEO around the restatement date pay a 43% premium over matched firms' loan spreads (p-value = 0.01). Misreporting firms that do not replace their CEO promptly also pay significantly greater spreads than matched firms in all five post-restatement years, with a range from 33% to 57% greater. In contrast to the results for the subsample of misreporting firms that promptly replaced their CEOs, in the non-CEO-replacement subsample there is no clear trend in the magnitudes of the misreporting premium over the five post-restatement years.

Results for the sample sorted on CFO replacements are in Panel B of Table 2.7. Misreporting firms that promptly replace their CFO pay significant misreporting premiums over matched firms in post-restatement years 1, 2, and 4. By year five, the misreporting premium for CFO-replacement firms falls to approximately 15% and is statistically insignificant. We see no obvious explanation for the insignificantly negative premium in year three followed by the significantly positive premium in year four. As with the misreporting firms that do not promptly replace their CEOs, misreporting firms that do not promptly replace their CFO pay significant misreporting premiums over all five post-restatement years. Also similar to the non-

Table 2.7

Loan Spreads and Turnovers.

This table shows how the effect of irregular restatements on cost of debt capital can differ within the five post-restatement years when firms experience different turnover outcomes, whether forced or voluntary, in CEOs, CFOs, audit committee chairs, and external auditors. For each post-restatement year, the bias-corrected nearest neighbor matching estimator of Abadie and Imbens (2006) is employed separately for the turnover and non-turnover sub-samples of loans at the loan level. The results for CEO, CFO, audit committee chair, and external auditor are presented in Panels A, B, C, and D, respectively. The abbreviation "SATT" in the table stands for "sample average treatment effect for the treated." For the sub-panels labeled "Change," treatment loans issued to firms that experience a change in the particular party of that panel are compared control loans issued to firms that also experience the change. Likewise, for the sub-panels labeled "No Change," treatment loans issued to firms that do not experience a change in the particular party of that panel are compared control loans issued to firms that also do not experience the change.

	Panel A: CEO					Panel B: CFO				
	Year	Firms	Loans	SATT	z-stat	Year	Firms	Loans	SATT	z-stat
Change	1	28	58	1.1937	14.91	1	21	43	0.8049	6.64
	2	19	39	0.7774	5.99	2	18	34	0.3028	3.12
	3	16	23	1.2801	7.16	3	13	24	-0.2226	-1.19
	4	14	24	0.6166	3.48	4	12	21	0.7028	3.46
	5	10	15	0.4311	2.58	5	5	7	0.1496	0.32
No Change	1	35	67	0.4708	5.16	1	42	82	0.4527	5.41
	2	39	68	0.4331	5.26	2	39	70	0.5878	5.88
	3	32	49	0.3271	2.94	3	35	48	0.2225	2.05
	4	30	54	0.3532	2.91	4	32	57	0.4551	4.26
	5	18	27	0.5674	2.76	5	23	35	0.5953	4.51
	Panel C: Chair of Audit Committee					Panel D: External Auditor				
	Year	Firms	Loans	SATT	z-stat	Year	Firms	Loans	SATT	z-stat
Change	1	9	13	0.6160	2.53	1	12	29	1.3613	11.06
	2	7	16	0.2810	1.44	2	15	31	0.4353	3.78
	3	5	6	0.0994	0.19	3	13	26	0.0605	0.48
	4	3	5	0.1582	0.58	4	10	17	0.2240	0.60
	5	3	3	0.2971	0.51	5	6	9	-0.2912	-0.90
No Change	1	36	80	1.1065	16.67	1	50	91	0.5620	5.07
	2	39	75	0.5582	7.02	2	55	105	0.5368	8.32
	3	30	42	0.3603	3.05	3	41	59	0.2976	2.87
	4	29	50	0.4839	4.02	4	28	57	0.4585	3.84
	5	17	28	0.4718	3.44	5	13	24	0.1352	0.81

CEO-replacement firms, there is no clear trend in the magnitudes of the misreporting premium over the five post-restatement years.

The results for chair of audit committee replacements are in Panel C of Table 2.7. Misreporting firms with prompt replacements of audit committee chairs pay loan spreads insignificantly different from matched firms in years two through five following their restatements. We acknowledge that sample sizes for these analyses are relatively small, but the magnitudes of the points estimates are also relatively small compared to those in Panels A and B. The subsample of firms that do not promptly replace their audit committee chairs pay significant premiums over matched firms in all five post-restatement years, with a range from 36% to 111% greater. With the caveat about sample sizes, the results suggest that the prompt replacement of an audit committee chair is effective in reducing the duration of misreporting premiums.

Panel D of Table 2.7 reports the results for the sample sorted on external auditor replacements. Misreporting firms with external auditor changes pay a premium on average on their loans relative to benchmark loans during the first two years following the restatement announcements, but their spreads fall to benchmark firm levels for years three through five. Misreporting firms that do not replace their external auditor pay loan spreads significantly greater than matched firms (ranging from 30% to 56% greater) in post-restatement years one through four; by year five, the misreporting premium falls to approximately 14% and is statistically insignificant.

In sum, misreporting firms that promptly replace the chairs of their audit committees and/or their external auditors see their loan spreads fall to benchmark firm levels within three years of restating. Despite the apparent beneficial effects of replacing such parties, a majority of firms do not promptly replace their audit committee chairs or external auditors, and pay misreporting premiums in loan spreads for at least four to five years post restatement. Prompt replacement of CEOs has no clear mitigation effect, as misreporting firms pay significant misreporting premiums in all five post-restatement years whether they promptly replace their CEOs or not. The results support the hypothesis that firms can reduce the duration of the misreporting stigma by promptly replacing certain parties that are potentially related to the misreporting, but the mitigation effects occur only for the parties that are directly related to the auditing function, i.e., audit committee chairs and external auditors.

2.5 Robustness Checks and Discussions

2.5.1 Loan Pricing Premium of New Lenders

We interpret the long-lasting and economically large premiums that misreporting firms pay as a stigma effect stemming from harm to the credibility of a their future financial reporting and perhaps more generally their veracity. This stigma increases screening and monitoring cost for banks, which the bank passes along in the form of higher loan spreads. An alternative explanation of our findings may be the existence of new lenders. Suppose that a bank that made loans to a misreporting firm in the pre-restatement period discontinues its lending relationship with the firm after the bank learns of the misreporting. In seeking post-restatement bank loans, the misreporting firm is must seek out new lenders who do not have any informational advantages created by a prior relationship with the firm. If the lack of a prior relationship increases screening and monitoring costs, the higher post-restatement spreads that we observe may be a new-bank effect rather than a pure stigma effect. Although misreporting would still be the source of the higher post-restatement spreads (because it led to the discontinuation of the firm-bank relationship), the channel or mechanism at work would be quite different from what we discuss in the previous sections.

To examine this possibility, we identify the subset of post-restatement loans in which the bank and borrower had a lending relationship in at least one of the five years preceding the borrower's restatement. Because the firms in this subset continue an existing banking relationships, new-bank effects cannot drive any misreporting premiums we observe. More than 80% of the loans in our sample are syndicated. To more strictly address the possibility of a new bank pricing premium, we use only the lead of the syndicate as the "representative lender" for each loan. Thus, for a firm with syndicated loans to be in the subset of firms with continuing banking relationships, it must have had the same lead bank in the pre and post-restatement periods. We identify banks using information in the lender-level file of DealScan. Note that because banks can co-lead a syndicate, there can still be multiple lenders for a loan even after singling out representative lenders.

Results from the loan spread regression model and matching estimator approach for the subset of firms with continuing banking relationships are presented as Models 1 and 2 of Table 2.8, respectively. For both approaches, we still observe statistically and economically significant misreporting premiums out to at least the fifth post-restatement year. Thus, information effects stemming from new lending relationships do not drive the results.

2.5.2 Choice to Misreport

By definition, deliberate misreporting is a choice that a firm makes. Although we confirm in a placebo test in Panel B of Table 2.5 that the *observable* dimensions in our models are adequate to capture spread differences in general, we might still be missing some unobservable difference between misreporting firms and non-misreporting firms that is unrelated to the fact that one group misreported. As much as possible, we want to ensure that our findings of long-lasting and economically large misreporting premiums are not driven by such a factor. We attempt to do so by capturing potential unobservable factors in bank loan pricing using a censored selection model (Heckman, 1979). The estimation employs full information maximum likelihood for a misreporting selection model and then second-stage loan spread model. We model the choice of misreporting by borrowing firms based on firm characteristics observed just prior to the intentionally misreported accounting periods. The inverse Mills

Table 2.8
Robustness Checks.

This table presents results from robustness checks of the base findings. Panel A addresses new lender bias. Models 1 and 2 present re-estimations of post-restatement spreads of loans issued through existing lending relationships using regression and matching estimator approaches, respectively. Both models are estimated on a subsample of loans made to restatement firms that have lending relationships existing prior to event announcements. New banking relationship is defined as one where the lender (or lead if the loan is syndicated) and the borrower of a loan have not contracted with each other during the five-year time frame prior to the restatement announcement date. Panel B addresses selection bias and presents results from a censored selection model estimated through full maximum likelihood. Misreporting propensity is modeled using borrowing firm characteristics just prior to the intentionally misreported accounting periods. The upper right portion of Panel B shows bias statistics and the result from a likelihood-ratio test between the unrestricted and restricted (where ρ is assumed to be zero) versions of the model. Regression models are estimated with robust standard errors clustered by firm. Definitions and details for each variable can be found in Table 2.1.

	Panel A: New Lender Bias						Panel B: Selection Bias				
	Model 1		Model 2				Model 3				
	Spread OLS Reg		Matching Estimator				Spread		Bias Statistics		
	Estimate	t-val	Sample				Estimate	t-val		Estimate	t-val
restatepostn1	0.3443	3.99	Firms	Loans	SATT	z-stat	0.3679	5.12	atanh(ρ)	-0.1252	-0.54
restatepost1	0.6835	4.43	40	77	0.7360	6.51	0.5445	4.82	ln σ	-0.6552	-16.05
restatepost2	0.3673	2.76	27	40	0.6075	4.94	0.4910	5.63			
restatepost3	0.3521	1.97	21	31	0.4930	3.65	0.3155	3.25	ρ	-0.1246	
restatepost4	0.5224	3.30	13	25	1.4824	16.15	0.3513	3.53	σ	0.5193	
restatepost5	0.4490	2.80	5	6	0.8487	3.31	0.2266	2.13	λ	-0.0647	
restatepost6	0.0202	0.11					0.2615	2.25			
lnassets	-0.2034	-6.01					-0.2009	-5.61	LR Test		
mb	-0.0727	-2.27					-0.0681	-3.07	χ^2	0.29	
leverage	0.5446	3.16					0.6001	4.72	p-val	0.59	
profitabilility	-2.1695	-3.40					-2.1796	-5.65			
tangibility	0.2968	0.75					0.1467	0.56	Misreport Propensity		
cfvol	0.0156	1.85					0.0125	0.95	Pre-Misreport	Estimate	t-val
modzscore	-0.0534	-1.32					-0.0219	-0.93	lnassets	0.2858	9.67
lnt	-0.0980	-1.89					-0.0791	-2.36	mb	-0.0030	-0.13
lnloansize	-0.0185	-0.42					-0.0230	-0.77	leverage	0.0178	0.08
perfprice	0.0011	0.02					-0.0225	-0.48	profitabilility	1.1397	2.78
crdtsprd	0.7520	4.70					0.6040	5.23	tangibility	-0.4036	-1.30
termsprd	0.0393	0.78					0.0463	1.55	cfvol	-0.1154	-2.31
econboom	-0.0116	-0.11					-0.0738	-1.06	modzscore	-0.0066	-0.69
Loan Type FE	Yes						Yes				
Deal Purpose FE	Yes						Yes		Year FE	Yes	
Industry FE	Yes						Yes		Industry FE	Yes	

ratios (λ) obtained from the probit selection models by all firms should capture the unobservable aspects that might contribute to the choice of engaging in intentional misreporting by firms.

In Panel B of Table 2.8, we provide results from the estimations of both the misreporting propensity equation and the loan spread equation. We also provide estimates of ρ, the correlation between the error terms of the two equations, and σ, the standard deviation of the error term of the loan spread model,. Note that ρ is estimated to be -0.1246, which is statistically insignificant based on the asymptotic t-statistic of -0.54.[25] The magnitude and significance of coefficients from the loan spread model do not differ qualitatively from our regression results that ignore selection. We further conduct a likelihood-ratio (LR) test between the the unrestricted and restricted (where ρ is assumed to be zero) versions of the model. As reported in the same table, the LR test yields a χ^2 value of 0.29, therefore the null hypothesis of $\rho = 0$ cannot be rejected.Given the results of these tests, unsurprisingly, we continue to observe larger spreads being charged on loans issued during at least the five post-restatement years following the announcements of irregular restatements.

2.5.3 Demand Side Considerations

Our stigma results may also be due to the demand side of private debt. Pungaliya (2010), for instance, finds that fraud announcements are followed by credit rating downgrades. If firms become more bank-dependent once deliberate financial misreporting activities are revealed, they may lose bargaining power in the process of bank loan contracting. We make sure that our findings of the financial misreporting stigma is not due to such demand side considerations.

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[25]Here ρ is estimated indirectly by its inverse hyperbolic tangent atanh(ρ). Testing the null hypothesis of atanh(ρ) = 0 is equivalent to testing the null hypothesis of $\rho = 0$.

Following earlier studies such as Kashyap, Lamont, and Stein (1994) and Chava and Purnanandam (2011), we use the existence of public-debt rating for a firm as a proxy for its bank-dependence. For the 140 firms in our restricted sample, where firms have both pre- and post-restatement loans, we see that 65 do not have a public debt rating before the announcements, and 67 do not have a public debt rating after the announcements. The difference is due to 7 firms losing their ratings and 5 firms gaining ratings after announcing their restatements. To ensure that our results are not due to demand effects, we re-conduct our previous nearest neighbor matching analyses excluding the 7 firms that lost their rating. In untabulated results, we continue to observe larger loan spreads being charged on post-restatement loans for firms that are associated with irregular financial restatements.[26]

We employ an additional check by examining whether the results in our previous analysis are due to changes in investment grade status. The threshold between investment-grade and speculative-grade ratings (junk-rated) often has important market implications for borrowing costs of the debt issuer. Investment grade firms are deemed to have better access to capital in the public-debt market. We see that, for the 140 firms in our restricted sample, 103 is junk rated before the restatement announcements, while 117 is junk rated afterwards. The difference of 14 firms is consisted of 16 falling from above investment grade to below around the announcement dates, and 2 moving the opposite direction. Once again, to enure that our results are not due to the demand side of private bank debt, we re-estimate our model excluding the 16 firms that fall below investment grade after their restatement announcement dates.[27] Our results remain qualitatively unchanged.

[26]The results in this subsection are similar to those presented in Panel A of Table 2.5. This is unsurprising, given that the firms excluded take up only a small portion of the original sample. Results for our demand side analyses are available from the authors upon request.

[27]Note that excluding only firms experiencing an adverse effect on the demand side biases towards demand side considerations, as firms that gain public debt ratings or improve in investment grade

2.5.4 Other Issues

Our measurement for the adverse effect of irregular restatements on firms in the private debt market is the all-in drawn spreads that are being charged on the loans issued to those firms. We analyze either (1) a restricted sample, where firms receive loans both prior to and after their restatement dates, or (2) an unrestricted sample, where only post-restatement loans are required. Doing so results in the 140 and 185 firms in the final irregular restatements samples, respectively. In each of these two samples, every firm has at least one loan contracted after announcing its restatement.

One group of firms that hasn't been examined thus far would be those which only received loans prior to their restatement dates. 85 firms associated with irregular restatements fall into this category. That is, for a total of 225 irregular restatement firms that have loan data reported in DealScan prior to their restatement announcements, nearly 40% of them do not have loan records for their post-event periods.[28]

We do not include these firm in our main analysis for two reasons. First, we cannot empirically observe loans made to these firms. We therefore do not have a direct way to quantify and measure the differences in costs of private debt for these firms, as compared either to their own pre-restatement levels or to their appropriately matched non-restatement benchmarks. Second, although our findings are subject to survivorship bias, such bias likely does not work against our results. There can be several reasons as to why there are firms that have only pre-restatement loans.

statuses are likely to have more bargaining power in contracting private loans than before. We also re-conduct the tests in this section by excluding all 12 (18) firms that has a change in their statuses of public debt rating (investment grade), our results are qualitatively unchanged when doing so.

[28]The 85 newly mentioned firms in this section, along with the 140 firms that are already in our restricted sample, make up a total of 225 firms that have loan data reported in DealScan prior to their restatement announcements. If we look into the full GAO sample, there are a total of 752 firms which have loans specified in the DealScan database prior to their restatement announcements, meaning that 297 firms do not have loan records during the post-restatement period. This is almost the same percentage as for the irregularities sample.

As we discuss, however, such firms are likely to undergo more serious consequences than simply being charged more on loans when engaging in irregular restatements (e.g., not able to receive loans or cease to exist). Our larger loan spreads are therefore effectively capturing a lower bound of the misreporting stigma for firms in the private debt market.[29]

While we cannot empirically observe the costs of private debt for these firms, we are still interested in why they actually no longer receive loans after the event announcements. By looking into the Compustat universe, we find that 69 of these 85 firms disappear from the data sometime during our sample period. Notably, 56 of the 69 disappearances happen within their first 5 post-restatement years. For the 56 cases, we look into their CRSP delisting codes. We find that 19 are due to merger-related activities, while 37 are dropped due to mainly not meeting financial guidelines for continued listing, delinquent in filing or non-payment of fees, or bankruptcy.[30]

[29]One possible counter-argument to the our survivorship explanation is the possibility that firms foresee bad situations in the near future, and lump their loans during the years prior to announcing the restatements. If firms accumulate enough cash by doing so, then they might not need to borrow cash in during the first five post-restatement years. If this is the case, then we should observe firms without post-restatement loans to have a larger amount of total loan size during the years just prior to the restatement announcement than do firms with post-restatement loans. We find exactly the opposite. During the five years just prior to the restatement announcement, the mean total loan size for firms without post-restatement loans is $700 million, while that for firms with post-restatement loans is $1.56 billion. During this five-year time period, firms without post-restatement loans receive on average 3 loans, while firms with post-restatement loans receive on average 3.6 loans. We also analyze the four-year, three-year, two-year, and one-year pre-restatement period and find similar trends for each of those time lengths. Firms without post-restatement loans borrow less in terms of total loan size than firms with post-restatement loans. Although the total number of loans in those pre-restatement time periods are never significantly different between the two groups, firms without post-restatement loans were never able to borrow more in terms of loan quantity based on point estimates. These results are not tabulated, but are available from the authors upon request.

[30]For firms that do not stay in the sample after restatement announcements due to data availability, most are associated with non-merger-related reasons. This means that firms do not simply become more attractive targets after being identified of deliberate reporting misbehavior. Consistent with our evidence, Amel-Zadeh and Zhang (2010) find that restating firms are significantly less likely to become takeover targets than matched counterparts. These results indicate that low reporting quality of firms introduces frictions to the market for corporate control.

We also provide evidence that our results are not merely a consequence of certain firm and loan characteristics specific to our sample. Through a comparison of both the restricted and unrestricted irregularities sample with the universe of DealScan-Compustat intersection at the loan level, other than having larger firm and loan sizes for our samples, we see that they are very similar with the universe in terms of all other characteristics, including loan-to-asset ratios.

Moreover, consistent with extant literature, our loan spread regression models yield R^2 values of around 70%. Along with evidence provided by our validation of the nearest neighbor matching technique in Panel B of Table 2.5, the observable dimensions that we employ in our models are adequate to capture loan spread differences in general. We therefore assert that our final samples are representative of the original DealScan-Compustat intersected universe where they came from.[31]

2.6 Concluding Remarks

Graham, Li, and Qiu (2008) find that firms that misreport their financial statements pay greater spreads on their bank loans following restatement of the misreported figures. We extend work on the effects of misreporting on bank loan spreads by testing two hypotheses about misreporting stigma effects. We first test the hypothesis that misreporting creates a long-lasting and costly stigma that leads to misreporting premiums in loan spreads. We find strong support this hypothesis: misreporting firms pay significant premiums in loan spreads for five years after restating, whether we use misreporting firms' pre-restatement loans as benchmarks or similar loans made to matched firms that did not misreport. We next test the hypothesis that the prompt replacement of parties potentially related to the mis-

[31]These numbers are not tabulated for space concerns, but are available from the authors upon request.

reporting reduces the duration of the stigma. Misreporting firms that promptly replace the chairs of their audit committees and/or their external auditors see their loan spreads fall to benchmark firm levels within three years of restating. Despite the mitigation effects of prompt replacements of audit committee chairs and external auditors, a majority of misreporting firms do not promptly replace these parties and pay significantly greater spreads than matched firms in the five years following their restatements. Whether or not firms promptly replace their CEOs, they continue to pay misreporting premiums for at least five years post restatement. Prompt replacements of CFOs also appear to have weak or non-existent mitigation effects. Neither the possibility that firms have to find new banks for post-restatement loans nor self-selection issues related to the decision to misreport appear to explain our findings.

Our results document a long-lasting and costly stigma of financial misreporting. To the extent that banks should be able to revise estimates of value and cash flows relatively quickly, and thus quickly adjust loan spreads to the levels predicted by the true (restated) financial figures, the long-lasting stigma evident in post-restatement loan spreads likely reflects higher information costs and uncertainty that banks face in making loans to firms that misreported long ago. Regaining credibility in reporting financial information appears to take a long time in the private loan market, but prompt replacements of certain parties appear to mitigate the duration of the stigma effect on a firm's cost of private debt capital. The results have implications for understanding the effects of misreporting on firm value and on the importance that banks place on the accuracy of reported (hard) financial information even though they may also use soft information in their credit screening and monitoring (Petersen, 2004).

3. CAN SHAREHOLDER-CREDITOR CONFLICTS EXPLAIN WEAK GOVERNANCE?

In order to explain why shareholders allow weak governance to persist, substantial costs to improving governance must exist. But what makes improving governance so costly? In this paper we ask whether one proposed cost – the shareholder-creditor agency conflict – is large enough to offset the benefits of governance in controlling uses of cash. To do so, we examine how values of cash holdings react to strong governance, leverage, and the interaction of the two. As in prior work, we find governance increases the value of cash holdings to shareholders. We also find that higher leverage decreases the benefit of strong governance. This is as expected if shareholder-creditor conflicts make strong governance costly for shareholders. However, the vast majority of weak governance firms do not use enough leverage to eliminate shareholder gains from strong governance. Thus, shareholder-creditor conflicts can provide only a small part of the explanation for why shareholders allow their firms to remain weakly governed.

Extant research suggests shareholder-creditor conflicts as a plausible barrier to stronger governance. The residual nature of equitys claim gives shareholders a preference for risky projects and bondholders a preference for safe projects. John, Litov, and Yeung (2008) and Acharya, Amihud, and Litov (2010) show that shareholder vs creditor focus affects the riskiness of firm investment policies, while John and John (1993) show that when such conflicts exist, it is optimal for firms to weaken alignment between shareholders and managers. Moreover, evidence suggests that creditors do not benefit from strong governance systems. Chava, Livdan, and Purnanandam (2009) show that bank loans are costlier when the firm has strong governance. The same result appears in yields on public debt, especially in firms without covenants (Cremers, Nair, and Wei, 2005; John and Litov, 2009; Klock, Mansi, and Maxwell, 2005). Finally, Francis, Hasana, John, and Waisman (2010) show that state antitakeover laws appear to protect bondholders, and that bond issues lead to negative stock returns in takeover-friendly states. These papers imply that solving agency problems between managers and shareholders deepens the agency problem between creditors and shareholders, and that these agency conflicts are costly to shareholders. Our paper adds to this work by examining not only the direction, but also the magnitudes of these effects.

We look for evidence of shareholder-creditor conflicts in the value of cash holdings. Cash is a natural place to begin looking at tradeoffs in governance because governance is intended to control uses of free cash flow. However, as work on shareholder-creditor conflicts make clear, shareholders and creditors disagree on what makes for a good use of cash. Moreover, other choices over which shareholders and managers might disagree, such as dividends or leverage, are controlled by covenants. Managers can decide on uses of cash with relative freedom, however. Therefore, if shareholder-creditor conflicts arise, the effects are likely largest in uses of cash.

We apply the methodology of Faulkender and Wang (2006) and Dittmar and Mahrt-Smith (2007) to estimate the value of cash to shareholders. The basic model of Faulkender and Wang (2006) regresses stock returns on changes in cash. Variables that change the value of cash show up as interactions with changes in cash. For example, Dittmar and Mahrt-Smith (2007) and Masulis, Wang, and Xie (2009) interact governance variables with changes in cash holdings to show that governance increases the value of cash holdings on average. We add an additional three-way interaction between governance and leverage to capture the fact that shareholder-creditor conflicts reduce the benefits of governance as leverage increases.

We confirm the strong benefits of governance in controlling uses of cash found by Dittmar and Mahrt-Smith (2007) using several measures. For example, in an unlevered firm with five or six of the provisions identified by Bebchuk, Cohen, and Ferrell (2009) as indicative of bad governance, \$1 of cash is worth approximately \$1.50. In an unlevered firm with zero of those provisions, \$1 of cash is worth an additional \$1.38. As market leverage increases from zero to one hundred percent, the value of cash stays relatively constant in a firm with five or six entrenching provisions. As market leverage increases from zero to one hundred percent, the value of cash falls by \$2.08 for a firm with zero entrenching provisions. Thus, the net benefit of governance as a function of leverage is $1.38 - 2.08 \times \mathit{Leverage}$. This is consistent with the existence of shareholder-creditor conflicts, as leverage reduces the net benefit of stronger governance.[32] However, when we calculate this net benefit for those firm-years with five or six entrenching provisions, fewer than ten percent see a zero or negative net benefit of governance. In fact, almost eighty-five percent of these firm-years would gain more than \$0.25 of value for each dollar of cash held. Even after accounting for shareholder-creditor conflicts, strong governance improves value in the vast majority of firms.

Other governance measures paint a picture similar to that of the Bebchuk, Cohen, and Ferrell (2009) index. Moving from the bottom quintile of pension ownership to the top quintile increases the value of cash in nearly 70% of firm-years. Somewhat surprisingly, moving from the bottom quintile to the second highest quintile of pension ownership increases the value of cash in almost 85% of firms, with nearly 65% gaining \$0.25 per dollar of cash held. Moving from the bottom quintile of total institutional ownership to the top quintile increases the value of cash in 80% of firm-years. Increasing from low institutional ownership increases the value of cash by \$0.25 per dollar held for more that 65% of low institutional ownership firm years. Once again, it appears that even in the presence of shareholder-creditor conflicts, shareholders gain substantial value from strong governance.

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[32]Of course, this is also consistent with the idea that leverage acts as a substitute mechanism for controlling free cash flows. Our estimate therefore provides an upper bound on the severity of shareholder-creditor conflicts.

We check for robustness of results in several ways. First, we show that non-linearity in leverage does not affect our conclusions. Using different transformations of leverage or different measures of leverage produces very similar estimates. Second, we show that endogenous choices of governance do not drive the results. In fact, the results using a two-stage least-squares show greater governance benefits than our simple OLS results. Finally, we show that broadening our analysis to non-cash assets does not substantively change our conclusions.

Finally, we extend our analysis to separate out movements toward optimal cash targets and movements away from optimal cash targets. The net benefits of governance in controlling excess cash appear similar in magnitude to the net benefits using total cash. The net benefits of governance in controlling optimal cash are much smaller. This suggests our basic tests are picking up values of cash that managers could choose to waste. Moreover, the negative impact of leverage on the benefit of governance also appears most concentrated in excess changes in cash. This is important, as it is the cash over which managers have discretion that should generate conflicts between shareholders and creditors, not the cash required for simply running the firm.

Overall, we show the magnitudes of these costs do not appear large enough for shareholders to actually prefer weak governance to strong. In fact, the magnitudes of shareholder-creditor conflicts are too small to even make most shareholders reasonably indifferent between weak and strong governance. If shareholders find weak governance better than strong governance–and we know they must, since we observe weak governance–costs other than shareholder-creditor conflicts must prevent strengthening governance.

One additional note is in order. We do not compare shareholder gains to creditor losses, so we cannot take a side in the debate about whether weak governance is firm value-maximizing. Shareholder-creditor conflicts are clearly important for understanding firms' use of covenants (see, for example, Chava, Kumar, and Warga, 2009, and banks and bondholders clearly dislike stronger governance and the increased risk it implies. Instead, our point is that shareholders control the strength of governance, and shareholders appear to gain from strong governance. Thus, the fact that weak governance destroys value even after accounting for leverage policy implies some force unrelated to leverage prevents shareholders from improving governance. While our results argue against shareholder-creditor conflicts as a major source of costs of strong governance systems, what costs prevent shareholders from fixing weak governance remains an important question for future research.

Section 3.1 discusses our additions to the empirical work of Faulkender and Wang (2006) and Dittmar and Mahrt-Smith (2007). Section 3.2 provides the results of our main tests. Section 3.3 presents evidence in excess and optimal cash changes. Section 3.4 comments on the economic significance of our findings. Section 3.5 shows results of several auxiliary tests to support the robustness of our findings. Section 3.6 concludes.

3.1 Research Design

3.1.1 Empirical Model

We use a specification in the spirit of Faulkender and Wang (2006) and Dittmar and Mahrt-Smith (2007) to estimate the value of cash.[33] We augment the Dittmar and Mahrt-Smith (2007) specification in one of the following two ways for each test:

$$AR_{it} = \beta_{it}\frac{\Delta C_{it}}{M_{it-1}} + \mathbf{Z}_{it}\mathbf{B} + \varepsilon_{it} \tag{3.1}$$

$$\beta_{it} = v + v_G G_{it} + v_L L_{it-1} + v_{GL} G_{it} L_{it-1} \tag{3.2}$$

[33]They interpret their methodology as a long-run event study, and as such focus on surprises in cash. However, papers utilizing q-theory to explain returns, for example Liu, Whited, and Zhang (2009), show that assets show up in returns when managers invest optimally. Papers such as Gomes, Yaron, and Zhang (2006) and Whited and Wu (2006) further show that characteristics that relax financial constraints, as cash flows must, matter for returns, as well.

$$\beta_{it} = v + \sum_j v_{G|G=j} I[G_{it} \in j] + v_L L_{it-1} + \sum_j v_{GL|G=j} I[G_{it} \in j] L_{it-1}, \quad (3.3)$$

where the subscript indicates firm i at time t. We group firm years according to the strength of their governance. We use these groups in two ways. First, in equation 3.2 we treat the group number as a continuous measure of governance.[34] One advantage of 3.2 is that it is relatively easy to understand. The coefficient captures the average benefit of improving governance by increasing governance into the next group. More importantly, however, we cannot use the indicator variable specification in our later two-stage least-squares tests as we do not have enough instruments to identify all of the indicator variables. In equation 3.3, we use indicator variables, denoted $I[\cdot]$, to assign firms to groups based on the strength of their governance. The advantage of 3.3 is that it captures non-linearity, and even non-monotonicity, in the governance variables.

The value of cash, β, depends on several firm characteristics in our specification. The first parameter, v measures the average value of cash in an unlevered, weak-governance firm. The second set of parameters, v_G or $v_{G|g=j}$ provide estimates of the difference between unlevered, stronger governance firms and unlevered, weak-governance firms. We proxy for stronger governance using a series of indicator variables, in the spirit of Dittmar and Mahrt-Smith (2007). The third parameter, v_L, estimates the effect of leverage on the value of cash in weak-governance firms, as in Faulkender and Wang (2006). The fourth set of parameters, v_{GL} or $v_{GL|g=j}$, represents our addition. If shareholder-creditor conflicts increase with leverage, then the net benefit of governance will fall as leverage increases. This implies $v_{GL} < 0$ is a necessary condition for the existence of costs to shareholder-creditor conflicts.

[34] For the Bebchuk, Cohen, and Ferrell (2009) entrenchment index, the group number is effectively the raw variable. Raw institutional ownership measures are measured with error, especially at extreme institutional ownership levels. Our use of group number pulls extreme observations together, mitigating, but not eliminating, this problem. See Dlugosz, Fahlenbrach, Gompers, and Metrick (2006) for more analysis on ownership, and especially high ownership.

Notice that 3.2 also provides a way to estimate the value of governance for a given firm. The base value of governance were the firm unlevered is v_G. Leverage reduces the benefit of governance at a rate v_{GL}. Thus, $v_G +_{GL} \times L_{it-1}$ estimates the net benefit of strong governance for a given firm year. If that number is positive, strong governance firms have higher values of cash even accounting for leverage. If that number is negative, strong governance firms have lower values of cash at the firm's leverage ratio. Our tests focus mainly on the distribution of this estimated net benefit of governance.

By attributing the entire effect of leverage to shareholder-creditor conflicts, we can estimate a "best-case scenario" for the ability of shareholder-creditor conflicts to explain weak governance choices. Even if the value of governance in controlling uses of cash falls for innocuous reasons–for example, if leverage acts as a substitute for strong governance in controlling free cash flows–we will count it as evidence of a shareholder-creditor conflict. Thus, our calculations likely overestimate the true costs of shareholder-creditor conflicts.

The dependent variable in 3.1, AR_{it}, is the firm's stock return over the fiscal year adjusted by the return on a matched portfolio. Our main question is whether shareholder-creditor conflicts can explain why weakly governed firms choose to remain weakly governed. Because shareholders determine the strength of governance, the shareholder focus underlying our choice of stock returns is the correct one. Notice that we do not claim strong governance is optimal in a broad sense. Shareholder-creditor conflicts might mean that weak governance maximizes firm value. However, if strong governance increases shareholder value, shareholders prefer strong governance to weak. Our main analysis uses the Daniel, Grinblatt, Titman, and Wermers (1997) and Wermers (2004) 125 size, book-to-market, and momentum characteristic-based benchmarks.[35] We also follow Faulkender and Wang (2006) and Dittmar and Mahrt-Smith (2007), who use size and book to market portfolios as benchmarks, Masulis, Wang, and Xie (2009), who use Fama-French industries as benchmarks, as well as raw simple raw returns.[36] Our results are similar to those reported here using any of those return specifications.

Aside from our governance measures, the main explanatory variables of interest are ΔC_{it}, the change in cash, and L_{it-1}, the lagged market leverage ratio. We find consistent results if we replace the lagged market leverage ratio with the contemporaneous leverage ratio, as in Faulkender and Wang (2006) and Dittmar and Mahrt-Smith (2007), or if we use the Welch (2004) and Iliev and Welch (2010) active financing approach to calculate the contemporaneous leverage ratio not attributable to current returns. The matrix $\mathbf{Z}_{it}$ includes the other explanatory variables used by Faulkender and Wang (2006) and Dittmar and Mahrt-Smith (2007): the change in cash interacted with a financial constraint measure, lagged cash holdings, lagged cash holdings interacted with the change in cash, the change in earnings, change in non-cash assets, change in R&D, the change in interest expense, change in dividends, net financing, leverage, and the governance indicators. We also include year and firm fixed effects, and cluster standard errors by firm. We scale all explanatory variables by the lagged market value of equity with the exception of leverage and the governance variables. Thus, the coefficient on each is the value of a $1 change to shareholders. Please see Table 3.1 for more detailed variable descriptions. All variables are measured in Year 2005 dollars using the CPI.

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[35] Please see those papers for details on the construction of the portfolios. The DGTW benchmarks and portfolio assignments are available via
http://www.smith.umd.edu/faculty/rwermers/ftpsite/Dgtw/coverpage.htm.

[36] Size and book-to-market portfolio breakpoints and returns, as well as industry definitions and returns, come from Ken French's website at
http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.

Table 3.1

Variable Definitions for Section 3.

This table presents definitions of variables used in Section 3, "Can Shareholder-creditor Conflicts Explain Weak Governance? Evidence from the Value of Cash Holdings."

Dependent Variables	
AR_{it}	DGTW-125 Benchmark-adjusted return over fiscal year t (CRSP and Russ Wermers) http://www.smith.umd.edu/faculty/rwermers/ftpsite/Dgtw/coverpage.htm
Governance Variables	
BCF	Bebchuk, Cohen, and Ferrell (2009) entrenchment index (RiskMetrics)
BCF_0	Initial value of the Bebchuk, Cohen, and Ferrell (2009) entrenchment index for each firm (RiskMetrics)
Pension	Total percent ownership by pensions, quarter prior to fiscal-year end (Thomson-Reuters 13-F)
Block	Total percent ownership by institutions with 5% of shares, quarter prior to fiscal-year end (Thomson-Reuters 13-F)
Institutions	Total percent ownership by institutions, quarter prior to fiscal-year end (Thomson-Reuters 13-F)
Other Controls	
M_{it}	$PRCC\_F_{it} \times CSHO_{it}$ (Compustat)
ΔC_{it}	Change in CHE from year $t-1$ to t (Compustat)
L_{it} Leverage	$\frac{DLTT_{it}+DLC_{it}}{DLTT_{it}+DLC_{it}+M_{it}}$ (Compustat)
ML_{it} Mechanical Leverage	$\frac{DLTT_{it-1}+DLC_{it-1}}{DLTT_{it-1}+DLC_{it-1}+M_{it-1}(1+R_{it})}$ (Compustat and CRSP)
NML_{it} Non-Mechanical Leverage	$L_{it-1} + [(L_{it} - L_{it-1}) - (ML_{it} - L_{it-1})]$
ΔE_{it}	Change in $IB + XINT + TXDI + ITCI$ from year $t-1$ to t (Compustat)
ΔNA_{it}	Change in $AT - CHE$ from year $t-1$ to t (Compustat)
ΔRD_{it}	Change in $\max\{XRD, 0\}$ from year $t-1$ to t (Compustat)
$\Delta XINT_{it}$	Change in $XINT$ from year $t-1$ to t (Compustat)
ΔDVC_{it}	Change in DVC from year $t-1$ to t (Compustat)
$Payer_{it}$	Indicator variable for $DVC > 0$ in year t (Compustat)
C_{it-1}	CHE at year $t-1$ (Compustat)
NF_{it}	$SSTK - PRSTKC + DLTIS - DLTR$ (Compustat)

3.1.2 Governance Measures

We require governance systems that help control waste of free cash flows. Any mechanism, internal or external, that prevents waste of cash is a candidate. We measure external governance using the Bebchuk, Cohen, and Ferrell (2009) index (BCF).[37] Following Bebchuk, Cohen, and Ferrell (2009), we treat this index as missing for dual-class firms. Similar to Dittmar and Mahrt-Smith (2007), we also use the initial value of the BCF measure for each firm as an additional variable. These measures come from RiskMetrics (formerly IRRC). We also use measures of internal governance. We use the percent ownership by activist public pensions (Pension) as our first internal governance measure. Our list of public pensions follows Dittmar and Mahrt-Smith (2007).[38] Pensions are a particularly interesting form of governance, as prior studies show that pensions affect firms charter provisions and thus can improve governance systems in the future (Gillan and Starks, 2000; Guercio and Hawkins, 1999; Smith, 1996). We also use the percent of shares held by institutional blockholders (Block) following Cremers and Nair (2005) and Dittmar and Mahrt-Smith (2007). Anticipating results, we find strong evidence of non-monotonicity in the pension measure, as well as extremely small effects of blockholdings on the value of cash. Based on the results of Morck, Shleifer, and Vishny (1988), who show that even outside directors appear to become entrenched when ownership passes the 5% level, and McConnell and Servaes (1990), who show that total institutional ownership increases firm value while blockholdings do not, we also consider total institutional ownership (Institutions) as an alternative measure of institutional oversight.

[37]For completeness, we have also conducted our tests using the O-Index, which is the set of provisions from Gompers, Ishii, and Metrick (2003) not included in the Bebchuk, Cohen, and Ferrell (2009) index. We find no evidence that the O-Index increases the value of cash holdings, nor does it seem to affect shareholder-creditor conflicts. This non-result is consistent with those reported in Bebchuk, Cohen, and Ferrell (2009). Thus, we do not report the results of that measure.

[38]See Table 1 on page 604 of Dittmar and Mahrt-Smith (2007).

We use the same breakpoints for the BCF index as Bebchuk, Cohen, and Ferrell (2009): $\{0, 1, 2, 3, 4, 5-6\}$. We use annual quintiles for the institutional ownership variables because those variables are continuous but notoriously error-ridden. Though continuous, approximately 25% of our observations have zero pension ownership, while approximately 30% of our observations have zero blockholdings. Thus, very few observations account for the second quintile of these measures. We have explored alternative breakpoints, such as quartiles or terciles, and find that our inferences do not depend on the breakpoints chosen for these measures.

3.1.3 Sample

Our sample firms are those in the intersection of CRSP and Compustat. We augment that data with information from the Thomson-Reuters 13-F database and RiskMetrics (formerly IRRC). Our main sample begins in 1990 and ends in 2007 because those are the start and end years of the RiskMetrics data.[39] As in prior work, we remove financial firms and utilities, as these firms face regulatory burdens that distort leverage policies. We work with share codes 10 and 11 only, which restricts the sample to U.S. common stocks. Finally, we remove any firms with a fiscal year not covering 12 months and firms for which the full fiscal year of monthly returns are not available in CRSP. In all analysis, we trim the dependent variable and the non-governance independent variables in equation 3.1 at the top and bottom percentiles by year. Because most of these variables represent changes in an accounting item relative to the market value of the firm, large positive and negative changes likely represent coding errors.

[39]Beginning with its 2007 file, RiskMetrics has revised its data collection. Several of the variables necessary for the Gompers, Ishii, and Metrick (2003) are no longer collected. In addition, the state laws required for the Bebchuk, Cohen, and Ferrell (2009) are also no longer available through that source.

3.2 Main Results

We report estimates of the value of cash in Tables 3.2 and 3.3. For our reported results, we focus on the lagged market leverage ratio. We see very similar results using several alternative measures, including (1) contemporaneous market leverage, (2) non-mechanical leverage ratio from Welch (2004), and (3) interest expense scaled by lagged market equity.[40] We include five governance variables: The contemporaneous Bebchuk, Cohen, and Ferrell (2009) entrenchment index (BCF), each firm's initial entrenchment index value (BCF_0), public pension ownership (pension), institutional blockholder ownership (block), and total institutional ownership (institutions). Panel A in Table 3.2 and the entire Table 3.3 show the main coefficients of interest. In Table 3.2, the coefficients labeled v_G provide the value of increasing one governance group regardless of starting point, while the coefficients labeled v_{GL} estimate the rate at which leverage reduces the value of stronger governance. In Table 3.3, the coefficients labeled $v_{G|G=j}$ provide the value of moving from the weakest governance group to stronger governance group j for an unlevered firm. The coefficients labeled $v_{GL|G=j}$ show how leverage reduces the value of stronger governance.

[40]The contemporaneous market leverage is arguably a better measure for our tests than lagged market leverage because leverage going forward should be more important to shareholders valuing cash holdings than past leverage. However, this measure suffers from the fact that Welch (2004) shows substantial variation in market leverage ratios follow mechanically from stock returns. The non-mechanical leverage ratio from Welch (2004) is also likely a better measure than lagged leverage for our purposes. While it does not get the complete picture of a firm's leverage ratio, it does provide a measure closer to current leverage ratios by incorporating active financing choices. In the regression of Fama and French (1998)–which is closely related to the Faulkender and Wang (2006) regression–interest expense is used as a leverage measure. We therefore use interest expense scaled by lagged market equity as a third alternative measure of leverage ratio. This measure once again allows some current financing decisions into leverage without mechanically relating to returns. The measure also captures elements of leverage that can be missing from the stated book value of long-term debt. In our main tests, although we choose the conservative, likely noisy, measure of lagged market leverage, inferences about the fraction of firms with potential gains from governance, and the value of those gains, are consistent across all leverage measures. These results are available upon request.

Table 3.2
Leverage Reduces the Net Benefits of Governance.

We run the following regression for groups of firms based on one of five measures of governance:

$$\begin{aligned} AR_{it} &= \beta_{it}\frac{\Delta C_{it}}{M_{it-1}} + \mathbf{Z}_{it}\mathbf{B} + \varepsilon_{it} \\ \beta_{it} &= v + v_G G_{it} + v_L L_{it-1} + v_{GL} G_{it} L_{it-1}, \end{aligned}$$

where G_{it} represents a measure of governance. We use group numbers to measure governance rather than raw scores because ownership measures are measured with significant error. For the BCF and BCF_0 groups, we follow the breakpoints of Bebchuk, Cohen, and Ferrell (2009); we define ownership groups according to annual quintiles. The dependent variable is a size, book-to-market, and momentum adjusted fiscal-year return (Daniel, Grinblatt, Titman, and Wermers, 1997; Wermers, 2004). Panel A presents estimates of coefficients used to estimate the net benefits of governance. The coefficient v_G provides the average estimated benefit of improving by one governance group for an unlevered firm. The coefficient v_{GL} provides an average rate at which leverage reduces the benefits of improved governance across all governance groups. Panel B provides estimates of control variables, including the base value of cash and the interaction between cash and leverage in weakly governed firms. All regressions include firm and year fixed effects, which we suppress for space. p-values clustered by firm appear in brackets. Please see Table 3.1 for variable definitions.

Panel A: Coefficients used in calculating the net benefits of improved governance					
Coefficient	BCF	BCF_0	Pension	Block	Institutions
v_G	0.2331	0.1687	0.1484	0.0213	0.1580
	[0.007]	[0.089]	[0.000]	[0.536]	[0.000]
v_{GL}	-0.2744	-0.2185	-0.3480	-0.1118	-0.3571
	[0.173]	[0.322]	[0.002]	[0.259]	[0.002]
Panel B: Control variable coefficient estimates					
Variable	BCF	BCF_0	Pension	Block	Institutions
$\Delta C_{it}/M_{it-1}$	1.5604	1.7667	2.0121	2.1886	1.9266
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\Delta C_{it}/M_{it-1} \times L_{it-1}$	-0.5530	-0.8415	-0.8611	-1.1131	-0.7592
	[0.379]	[0.263]	[0.000]	[0.000]	[0.002]
$\Delta C_{it}/M_{it-1} \times Payer_{it}$	-0.5191	-0.5366	-0.4934	-0.4728	-0.4717
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
L_{it-1}	0.5207	0.5241	0.5645	0.5676	0.6073
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$Payer_{it}$	-0.0941	-0.0898	-0.0949	-0.0942	-0.0956
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\Delta NA_{it}/M_{it-1}$	0.3336	0.3469	0.4232	0.4212	0.4152
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\Delta E_{it}/M_{it-1}$	0.4805	0.4906	0.5340	0.5318	0.5348
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\Delta RD_{it}/M_{it-1}$	0.6166	0.6416	0.4038	0.4136	0.3588
	[0.221]	[0.232]	[0.152]	[0.142]	[0.201]
$\Delta XINT_{it}/M_{it-1}$	-2.6274	-2.8182	-3.0160	-2.9845	-2.9851
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\Delta DVC_{it}/M_{it-1}$	5.1381	4.5512	4.8925	4.7202	4.9858
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
NF_{it}/M_{it-1}	-0.2616	-0.2564	-0.1329	-0.1311	-0.1335
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
C_{it-1}/M_{it-1}	0.8831	0.8728	0.9016	0.9062	0.9138
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
$\Delta C_{it}/M_{it-1} \times C_{it-1}/M_{it-1}$	-0.8075	-0.7106	-0.8073	-0.8567	-0.7599
	[0.003]	[0.021]	[0.000]	[0.000]	[0.000]
Observations	2,103	2,023	6,069	6,069	6,069
Firms	15,859	14,893	45,989	45,989	45,989

Table 3.3

Net Benefits of Governance: Governance Quintile Indicators.

We run the following regression for groups of firms based on one of five measures of governance:

$$\begin{aligned} AR_{it} &= \beta_{it}\frac{\Delta C_{it}}{M_{it-1}} + \mathbf{Z}_{it}\mathbf{B} + \varepsilon_{it} \\ \beta_{it} &= v + \sum_j v_{G|G=j} I[G_{it} \in j] + v_L L_{it-1} + \sum_j v_{GL|G=j} I[G_{it} \in j] L_{it-1}, \end{aligned}$$

where $I[\cdot]$ takes a value of one when its argument is true, and G_{it} represents a measure of governance. BCF and BCF_0 groups are defined according to the breakpoints in Bebchuk, Cohen, and Ferrell (2009); ownership groups are defined according to annual quintiles. The dependent variable is a size, book-to-market, and momentum adjusted fiscal-year return (Daniel, Grinblatt, Titman, and Wermers, 1997; Wermers, 2004). The table presents estimates of coefficients used to estimate the net benefits of governance. The coefficient $v_{G|G=j}$ provides the estimated benefit of improving from the weakest governance group to the j^{th} governance group for an unlevered firm. The coefficient $v_{GL|G=j}$ provides an estimate of the rate at which leverage reduces the net benefits of governance for the j^{th} governance group. Estimates of control variables, including the base value of cash and the interaction between cash and leverage in weakly governed firms, are similar to those reported in Table 3.2 and are therefore omitted. All regressions include firm and year fixed effects, which we suppress for space. p-values clustered by firm appear in brackets. Please see Table 3.1 for variable definitions.

Coefficient	BCF	BCF_0	Pension	Block	Institutions
$v_{G\|G=2}$	0.1805	0.5249	0.0659	0.1922	-0.1522
	[0.745]	[0.465]	[0.669]	[0.374]	[0.304]
$vG\|G=3$	0.6224	0.5315	0.5561	0.1972	0.1745
	[0.240]	[0.430]	[0.001]	[0.233]	[0.270]
$v_{G\|G=4}$	0.8835	0.7629	0.5923	0.1126	0.2525
	[0.095]	[0.220]	[0.001]	[0.445]	[0.181]
$v_{G\|G=5}$	0.7222	0.8156	0.3879	0.0676	0.6450
	[0.197]	[0.195]	[0.019]	[0.653]	[0.001]
$v_{G\|G=6}$	1.3766	1.1333	-	-	-
	[0.020]	[0.092]	-	-	-
$v_{GL\|G=2}$	-1.1918	-0.5310	0.2850	-0.0143	0.2998
	[0.290]	[0.728]	[0.517]	[0.982]	[0.471]
$v_{GL\|G=3}$	-1.5979	-1.2814	-1.0101	-0.1646	-0.3965
	[0.103]	[0.332]	[0.023]	[0.733]	[0.408]
$v_{GL\|G=4}$	-2.1997	-0.6582	-1.1246	-0.3888	-0.5537
	[0.026]	[0.607]	[0.025]	[0.339]	[0.279]
$v_{GL\|G=5}$	-1.6035	-1.7458	-1.1436	-0.3705	-1.5446
	[0.111]	[0.161]	[0.022]	[0.392]	[0.003]
$v_{GL\|G=6}$	-2.0779	-1.4157	-	-	-
	[0.080]	[0.298]	-	-	-

In Panel A of Table 3.2, we see little evidence that blockholders have any effect on values of cash, a pattern repeated throughout our tests.[41] The results for the other four measures, however, suggest that improving from a starting governance group to the next group is worth between \$0.15 and \$0.23. The gain to stronger governance is not uniform, however, as leverage generally decreases the gains from governance.[42] Because the benefit falls, there is some leverage ratio at which the difference between the value of cash in strong and weak governance firms is zero. For example, the BCF measure suggests that at a leverage ratio of 85%, the value of governance falls to zero. This is an inordinately high leverage ratio. The pension holdings and total institutional holdings measures suggest that the value of governance falls to zero at a leverage ratio of 42% to 44%. Though much more reasonable, these leverage ratios are still unusually high, as we now show.

In Panel B of Table 3.2, we present results for the control variables. One important difference between our results and those seen in Faulkender and Wang (2006) is the sign of leverage. In our tests, leverage has a positive and statistically significant effect on returns, whereas Faulkender and Wang (2006) see a negative and statistically significant effect. The difference is one of timing. When we use con-

[41]McConnell and Servaes (1990) provide consistent results, showing that blockholders do not have an effect on firm value while total institutional ownership does.

[42]The BCF measure has a negative but insignificant coefficient in the levels regression. There are three important things to note about this result. First, the lack of significant result implies that leverage does not decrease the value of governance. This means that leverage cannot possibly explain why firms choose weak governance, and, therefore, implies that shareholder-creditor conflicts cannot explain why firms choose weak governance. By ignoring the insignificance of this coefficient, we are biasing in favor of shareholder-creditor conflicts. Second, when we estimate the value of governance using a dummy variable for zero leverage firms, the value of strong governance using the BCF measure appears negative for unlevered firms. Note that this evidence is also strongly inconsistent with shareholder-creditor conflicts driving the value of governance, as zero leverage firms should have no shareholder-creditor conflicts. Third, the dummy variable results do suggest that, at the highest levels of the BCF index, there is a negative relationship between the leverage and the value of governance. For these three reasons, we continue with the analysis as though the coefficient were statistically significant.

temporaneous leverage ratios, as in Faulkender and Wang (2006), we see a negative relationship. As Welch (2004) argues, leverage and returns have a mechanical relationship (firms with high returns will tend to have low leverage).

For ease of interpretation, we present results graphically. For each measure, we use the estimates of Table 3.2 to estimate the difference between the value of cash in stronger and weaker governance firms as

$$\widehat{\Delta V}_G = v_G + v_{GL} \times L_{it-1}, \tag{3.4}$$

which measures the net benefit of strong governance in controlling uses of cash.[43] We estimate this value using firm years in the weakest governance group.[44]

We present the empirical cumulative densities of the net benefit of governance in Figure 3.1. The blockholder measure, as noted before, shows no effect either on its own or interacted with leverage. We start with this measure to illustrate features of our figures. First, stronger blockholder firms do not see higher values of cash even when unlevered. This lack of value for low leverage firms means that nearly one hundred percent of firm years have a zero or negative net benefit of governance. Second, blockholders and leverage do not interact significantly. The lack of interaction results in a curve that is extremely steep, indicating that leverage does not explain differences in the net benefits of governance. If shareholder-creditor conflicts explain weak governance choices, we expect to see two features in the figure. First we expect the majority of weakly governed firm years to exhibit a negative net benefit of governance, as seen in the blockholder measure. Second, we expect a sloped curve, indicating that leverage affects the net benefit of governance. It is in

[43]This is akin to a treatment effect estimate where the treatment effect of moving from weak to strong governance depends on leverage.

[44]The estimates suggest that the benefit of strong governance falls with leverage. Because weak governance firms have higher leverage than strong governance firms (Francis, Hasana, John, and Waisman, 2010), using strong governance firms and their lower leverage ratios will tend to overstate the value of governance. Therefore, we use only weak governance firms.

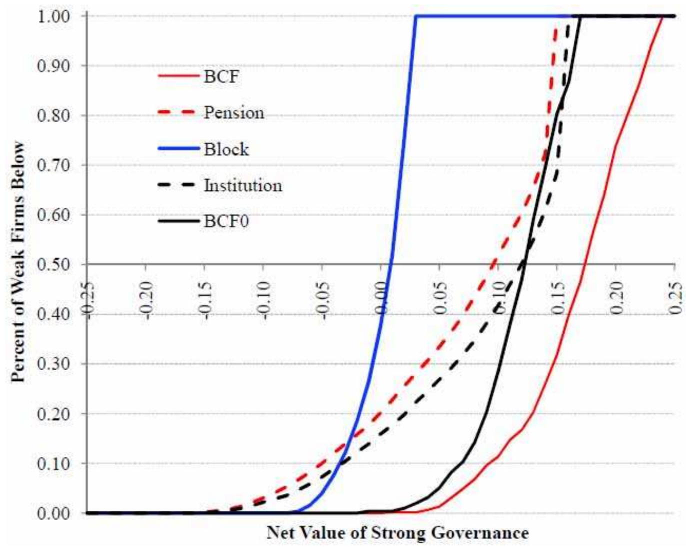

Fig. 3.1. Raw Governance Levels.

For each firm year in the weakest governance group, we calculate the net benefit of increasing governance from a lower group to a higher group as $v_G + v_{GL} \times L_{it-1}$. Estimates of v_G and v_{GL} appear in Panel A of Table 3.2. The x-axis shows different net benefits of governance, while the y-axis represents the cumulative percent of weakly governed at or below that net benefit. For the BCF measure, decreases indicate stronger governance, while for the ownership measures, increases generally indicate stronger governance.

this second requirement that our blockholder measure fails to support shareholder-creditor conflicts as a major determinant of weak governance choices.

Other measures of governance also do not provide much evidence for shareholder-creditor conflicts as a major explanation of weak governance choices. The BCF measure, for example, shows a relatively flat curve, indicating that variability in the net benefits of governance. However, no firms in our weak governance sample have a leverage ratio high enough to cause a negative value of governance. This result suggests that all of our weak governance firms could gain by improving governance. The magnitudes are also very large. Half of our weak governance firms stand to gain over \$0.17 per dollar of cash held by reducing their entrenching provisions to four instead of five or six. Pensions and total institutional holdings tell a similar story. The net benefit of governance appears positive for slightly more than 80% of our weak pension and institutional holdings firms, while net benefits of strong governance exceed \$0.10 to \$0.12 per dollar held for half of our weak pension and institutional holdings firms. These results do not support shareholder-creditor conflicts–or any forces related to leverage, for that matter–as a major explanation for why firms choose weak governance.

In Table 3.3, we report results of increasing from the weakest governance group to each of the stronger governance groups.[45] As before, the relationship between the BCF index and the value of cash holdings is very strong. For unlevered firms, the difference between the value of cash in strong and weak governance firms exceeds \$1.30 per dollar held. This number exceeds that in Table 3.2 by a large amount. The reason is that Table 3.2 estimates a one group increase, while Table 3.3 estimates a five group improvement in governance. Moreover, the dummy variable specification

[45]Estimates of control variables, including the base value of cash and the interaction between cash and leverage in weakly governed firms, are similar to those reported in Table 3.2 and are therefore omitted.

shows extreme nonlinearity in the BCF measure, which sheds some light on why the BCF measure does not show strong evidence of a negative relationship between leverage and net benefits of strong governance. The pension and institutions measures both show higher value in the highest ownership quintile than in the lowest ownership quintile. Note, however, that the maximum value of cash occurs in the second highest quintile for pension holdings. One possible explanation is that strong institutions can harm minority shareholders.[46] Romano (1993) suggests that public pension fund managers may succumb to political pressures. At low levels of ownership, only activities that create value for all shareholders can pass. As ownership increases, however, public pensions and other institutions may be able to influence firms in ways that help the fund, but hurt shareholders in general.[47] Because of the theoretical arguments and empirical evidence that the highest public pension ownership does not always lead to higher values of cash, we take the second highest quintile as a measure of strong governance for the pension ownership measure. Finally, the value of blockholdings is once again extremely small and not statistically significant.

To evaluate magnitudes, we again turn to figures. In Figure 3.2, we use the estimates from in Table 3.3 to estimate the net benefit of strong governance for those firm years with the weakest governance group according to each measure. The x-axis provides a range of net benefits of governance, while the y-axis shows how many weakly governed firms have net benefits below that point. The results provide little support for shareholder-creditor conflicts as a major determinant of why firms choose weak governance. Consider Panel A of Figure 3.2. The solid red

[46] Pound (1988) shows that banks and insurance companies often side with management in proxy contests, so increasing their shares may partially drive the results. However, pension holdings also exhibit an inverted-U shaped pattern, albeit at higher levels, so this does not completely explain the inverted-U.

[47] Romano (1993) notes recommendations by a New York State task force that public pensions account for local concerns (state economy and employment) during hostile takeovers. As Romano notes, this creates a wedge between the interests of pensions and the interests of other shareholders.

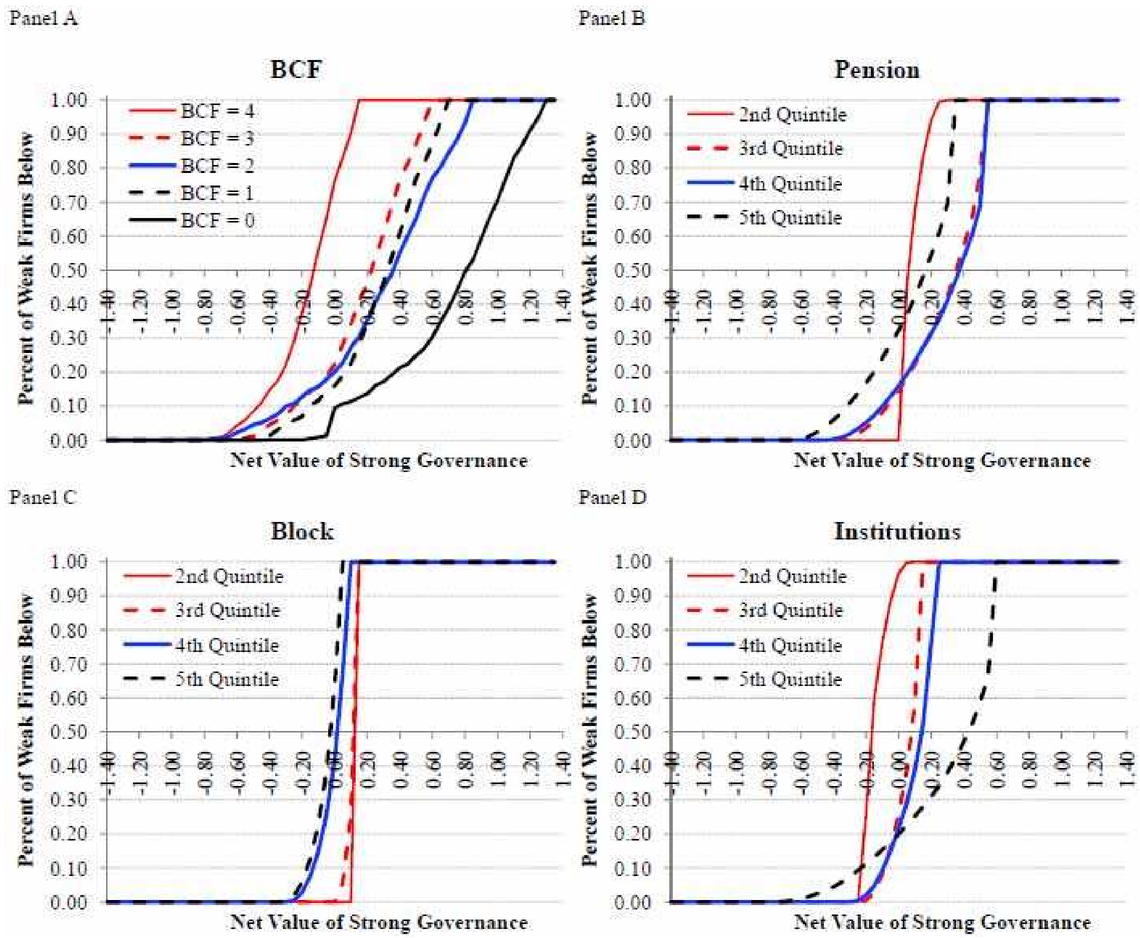

Fig. 3.2. Governance Level Indicators.

We calculate the net benefit of improving governance from the weakest group to group j as $v_{G|G=j} + v_{GL|G=j} \times L_{it-1}$ for each weakly governed firm year. Estimates of $v_{G|G=j}$ and $v_{GL|G=j}$ appear in Panel A of Table 3.3. The x-axis shows different net benefits of governance, while the y-axis represents the cumulative percent of weakly governed at or below that net benefit. For the BCF measure, decreases indicate stronger governance, while for the ownership measures, increases generally indicate stronger governance.

line shows that almost no firms have a positive value of moving from the weakest governance group to the second weakest governance group. The figure also suggests that leverage affects the net benefit of strong governance. Thus, one can conclude that shareholder-creditor conflicts explain why weak governance firms do not improve governance into the second weakest group. However, the three intermediate values of the BCF measure tell a very different story. Only 20% of weak governance firms have leverage ratios high enough that our net benefit of strong governance falls below zero. The remaining 80% of weak governance firms have positive net benefits of strong governance. Almost 90% of weakly governed firms have positive net benefits of strong governance. The magnitudes are large, as well. Our estimates imply an \$0.80 per dollar of cash difference between strong and weak BCF firms for the median weak governance firm.

The pension and total institutional holdings measures tell similar stories in Figure 3.2. For 65% of low pension ownership leverage ratios, the value of cash under strong governance exceeds the value of cash under weak. As noted before, the value of pension holdings is highest for the intermediate holding measures. More than 80% of low pension ownership leverage ratios see a higher value of cash under strong governance than weak. For the median leverage ratio, the difference between the value of cash in strong and weak governance firms is \$0.40 per dollar of cash held. The total institutional ownership measure, for which only the highest levels of ownership increase the value of cash, tells a similar story. For 80% low ownership leverage ratios, the value of cash is higher under strong governance than weak. At the median leverage ratio for low institutional ownership firms, the difference between the value of cash in strong governance firms and weak governance firms is \$0.40 per dollar of cash. The blockholder measure, which shows no value of governance nor any interactive relationship between governance and leverage, also shows most firms' net benefit of governance near zero. The basic results tell a consistent story; higher leverage reduces the net benefit of strong governance, but leverage ratios are not high enough for shareholder-creditor conflicts to explain weak governance choices.

Overall, the main results suggest the following: Leverage does appear to decrease the net benefits of governance. This is consistent with shareholder-creditor conflicts, which provide one rationale for the negative effect of leverage. However, the leverage ratios of weak governance firms are not nearly high enough to offset the value of governance to shareholders completely. Therefore, we can conclude two important things. First, there must be some cost that prevents shareholders from improving governance. Second, that cost is unrelated to leverage, so things like shareholder-creditor conflicts cannot explain why shareholders allow weak governance to persist. This is not to say that shareholder-creditor conflicts are not important for other reasons, but rather that these conflicts are too small quantitatively to explain why firms choose weak governance.

3.3 Excess and Optimal Changes in Cash

Citing concern that it is excess cash that managers waste, Dittmar and Mahrt-Smith (2007) examine firms with positive excess cash holdings separately. We follow their lead, applying the model of Opler, Pinkowitz, Stulz, and Williamson (1999) to estimate a firm's optimal cash holding. We do not simply replace changes in cash with changes in excess cash however. As Dittmar and Mahrt-Smith (2007) note, changes in excess cash represent both active changes in cash holdings and changes in target.

A firm's optimal change in cash should bring the cash holding in line with the target, so that $\Delta C_{it} = C^*_{it} - C_{it-1}$ if the change in cash is optimal. If the change in cash is sub-optimal, then the excess cash raised is $C_{it} - C^*_{it} = (C_{it} - C_{it-1}) - (C^*_{it} - C_{it-1})$. While it is tempting to think about using changes in excess cash to replace the total change in cash, the level of excess cash provides a natural decomposition of cash holdings. We therefore decompose change in cash into two parts. The first is the optimal change in cash measured as the difference between the current target and lagged cash. The second is the excess change in cash measured as the level of excess cash. This provides us separate estimates of the relationship between governance, leverage, and the value of cash depending on whether the firm has raised "too much" cash or "not enough" cash.[48]

In order to make the excess cash predictions comparable to the change in cash in our return regression, we modify the model of Opler, Pinkowitz, Stulz, and Williamson (1999) so that dependent variable is scaled by market values of equity instead of book asset values. This makes the predicted cash and excess cash comparable to the change in cash used in the return regressions. Specifically, our regression to predict excess cash is

$$\begin{aligned}\ln\left(\frac{C_{it}}{M_{it-1}}\right) &= \alpha_0 + \alpha_1 \ln M_{it-1} + \alpha_2 \ln NA_{it} + \alpha_3 \frac{FCF_{it}}{NA_{it}} + \alpha_4 \frac{NWC_{it}}{NA_{it}} \\ &\quad + \alpha_5 \sigma_{industry} + \alpha_6 \frac{RD_{it}}{NA_{it}} + \alpha_7 \frac{MV_{it}}{NA_{it}} + u_{it}. \end{aligned} \tag{3.5}$$

The regression follows that in Dittmar and Mahrt-Smith (2007) closely. Because the original specification is in natural logs, we add the log book value of non-cash assets to both sides and subtract the log market value of equity from both sides. As in Dittmar and Mahrt-Smith (2007), we use firm and year fixed effects in the regression.[49]

Given our predicted value of cash, we estimate the following regression:

$$AR_{it} = \beta_{it}\left(\frac{C_{it}}{M_{it-1}} - \frac{\widehat{C}_{it}}{M_{it-1}}\right) + \beta'_{it}\left(\frac{\widehat{C}_{it}}{M_{it-1}} - \frac{C_{it-1}}{M_{it-1}}\right) + \mathbf{Z}_{it}\mathbf{B} + \varepsilon_{it} \tag{3.6}$$

[48] We have also fit the model ignoring changes in the target. The results are qualitatively and quantitatively similar.

[49] Results for our optimal cash estimation are omitted for space concerns, but are available upon request. We have also fit the model replacing the market-to-book asset ratio with trailing 3-year average sales growth. The predicted values of cash from the two models share a correlation of 0.966. Not surprisingly, therefore, results are very similar using either model.

$$\beta_{it} = v + \sum_j v_{G|G=j} I[G_{it} \in j] + v_L L_{it-1} + \sum_j v_{GL|G=j} I[G_{it} \in j] L_{it-1} \quad (3.7)$$

$$\beta'_{it} = v' + \sum_j v'_{G|G=j} I[G_{it} \in j] + v'_L L_{it-1} + \sum_j v'_{GL|G=j} I[G_{it} \in j] L_{it-1}, \quad (3.8)$$

where $\frac{\widehat{C}_{it}}{M_{it-1}}$ is the predicted optimal cash using equation 3.5.

We present results in Table 3.4. From Panel A, the BCF measures work in excess cash much the same as they did in total changes in cash from Table 3.3. The value of governance in an unlevered firm increases slightly from \$1.38 to \$1.49 for contemporaneous BCF and from \$1.13 to \$1.34 for initial BCF. The ownership measures also yield similar results. Moving from the bottom quintile of pension holdings to the fourth quintile increases the value of each dollar of excess cash by \$0.63 as opposed to the \$0.59 in total changes in cash. Moving from the bottom quintile of institutional holdings increases the value of each dollar of cash held by \$0.75 instead of \$0.65. We also see similar evidence of the conflict between shareholders and creditors, as leverage reduces the net benefits of governance at approximately the same rates as reported in Table 3.3.

Panel B provides more surprise. When making optimal adjustments in cash, the net benefit of strong governance is zero according to the BCF measures. The pension and total institutional ownership suggest a small, though still statistically significant, net benefit of strong governance in optimal changes in cash. Since we must estimate the optimal level of cash, it is not surprising that the model cannot split cash changes into truly optimal and truly sub-optimal changes. However, the fact that the coefficients drop so far relative to the excess changes in cash reported in Panel A is comforting. The value of increasing to the fourth quintile of pension holdings drops by half, as does the value of increasing to the top quintile of institutional holdings. Thus, it appears that shareholder-manager and shareholder-creditor conflicts both exist mainly in unusual cash decisions.

We plot cumulative distributions of the net benefits of governance in Figure 3.3. We do not plot the blockholder, which as before shows no value of governance or

Table 3.4

Value of Optimal and Excess Cash

We augment the main specification to the following regression for groups of firms based on one of five measures of governance:

$$
\begin{aligned}
AR_{it} &= \beta_{it}\left(\frac{C_{it}}{M_{it-1}} - \frac{\widehat{C}_{it}}{M_{it-1}}\right) + \beta'_{it}\left(\frac{\widehat{C}_{it}}{M_{it-1}} - \frac{C_{it-1}}{M_{it-1}}\right) + \mathbf{Z}_{it}\mathbf{B} + \varepsilon_{it} \\
\beta_{it} &= v + \sum_j v_{G|G=j} I[G_{it} \in j] + v_L L_{it-1} + \sum_j v_{GL|G=j} I[G_{it} \in j] L_{it-1} \\
\beta'_{it} &= v' + \sum_j v'_{G|G=j} I[G_{it} \in j] + v'_L L_{it-1} + \sum_j v'_{GL|G=j} I[G_{it} \in j] L_{it-1}
\end{aligned}
$$

where $\frac{\widehat{C}_{it}}{M_{it-1}}$ is the predicted optimal cash using equation 3.5. Please see Table 3.1 for variable definitions.

	BCF		BCF0		Pension		Block		Institutions	
	vG—G=j	vGL—G=j	vG—G=j	vGL—G=j	vG—G=j	vGL—G=j	vG—G=j	vGL—G=j	vG—G=j	vGL—G=j
Panel A: Value of Excess Cash Changes										
G=2	0.2404	-0.7685	0.8768	-1.3380	0.1952	0.2008	0.0449	0.2689	-0.1617	0.4436
	[0.697]	[0.514]	[0.250]	[0.404]	[0.218]	[0.670]	[0.836]	[0.683]	[0.332]	[0.347]
G=3	0.5744	-1.4851	0.8763	-2.0035	0.5396	-0.8339	-0.0007	0.2936	0.1431	-0.2461
	[0.307]	[0.149]	[0.218]	[0.162]	[0.002]	[0.066]	[0.997]	[0.555]	[0.422]	[0.648]
G=4	0.8638	-1.8944	0.7743	-0.7815	0.6331	-1.0750	0.0855	-0.0945	0.2467	-0.1218
	[0.127]	[0.078]	[0.231]	[0.561]	[0.002]	[0.041]	[0.626]	[0.839]	[0.214]	[0.826]
G=5	0.8868	-1.6456	0.7892	1.8784	0.5571	-1.3173	0.0838	-0.2586	0.7482	-1.2062
	[0.144]	[0.135]	[0.238]	[0.158]	[0.004]	[0.013]	[0.625]	[0.582]	[0.000]	[0.035]
G=6	1.4933	-2.4274	1.3413	-1.5355	-	-	-	-	-	-
	[0.020]	[0.068]	[0.056]	[0.296]	-	-	-	-	-	-
Panel B: Value of Optimal Cash Changes										
G=2	-0.4665	0.5747	-0.2509	0.5265	-0.0064	0.4989	0.0223	0.6170	-0.1716	0.4923
	[0.515]	[0.721]	[0.759]	[0.774]	[0.973]	[0.316]	[0.924]	[0.356]	[0.331]	[0.295]
G=3	-0.1044	0.5041	-0.4721	0.4150	0.3789	-0.4363	0.0167	0.2609	-0.0924	0.1231
	[0.878]	[0.733]	[0.551]	[0.813]	[0.030]	[0.381]	[0.926]	[0.622]	[0.601]	[0.810]
G=4	-0.1096	0.4555	0.0568	0.4641	0.3712	-0.6117	0.0356	-0.1515	-0.1082	0.1252
	[0.871]	[0.754]	[0.941]	[0.789]	[0.050]	[0.226]	[0.839]	[0.747]	[0.603]	[0.832]
G=5	-0.2934	1.4095	0.0614	0.1925	0.2542	-0.8333	0.0406	-0.3885	0.3793	-1.0051
	[0.675]	[0.352]	[0.935]	[0.908]	[0.191]	[0.152]	[0.823]	[0.441]	[0.073]	[0.078]
G=6	0.4196	0.4244	0.2537	-0.5761	-	-	-	-	-	-
	[0.566]	[0.782]	[0.748]	[0.757]	-	-	-	-	-	-

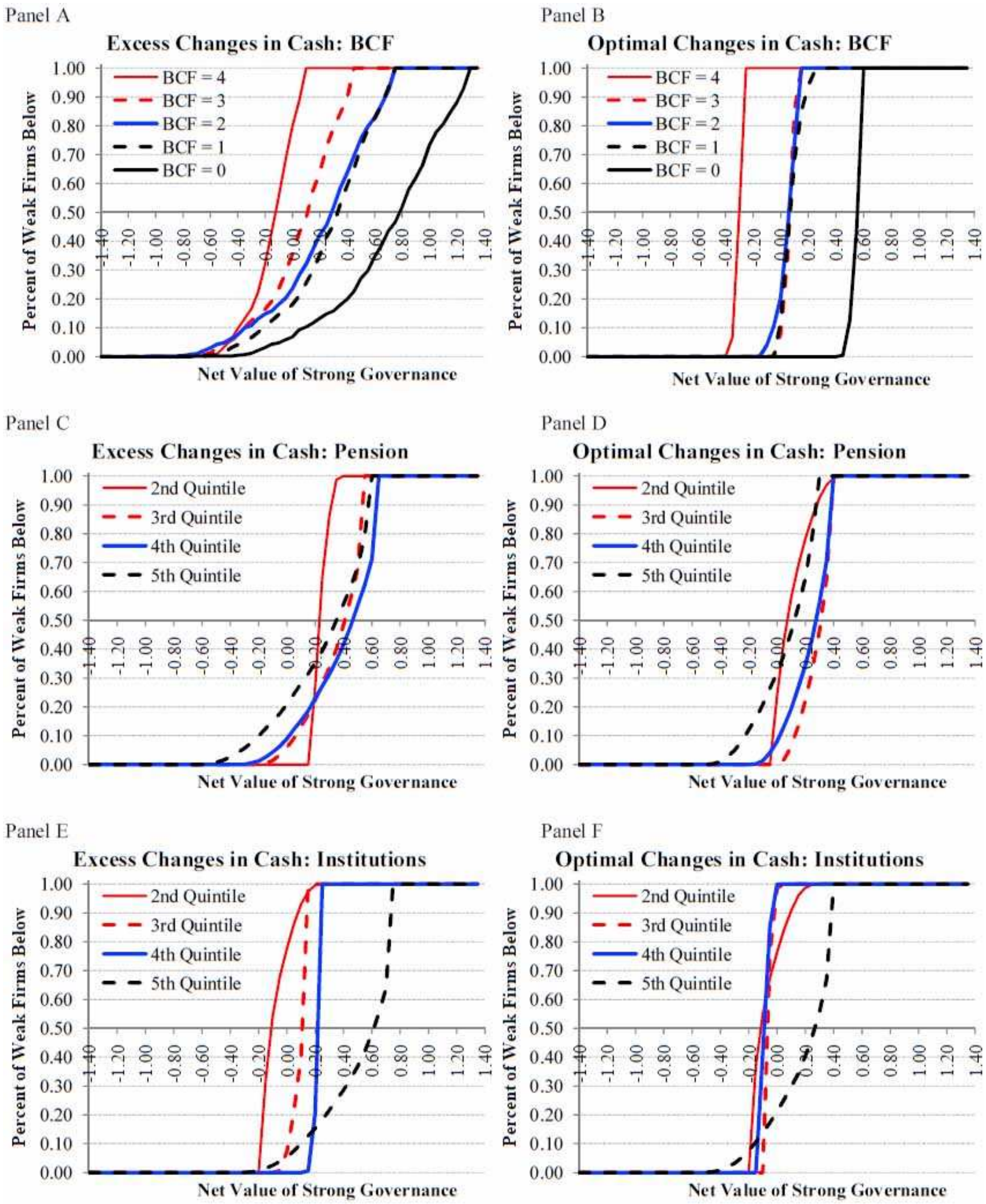

Fig. 3.3. Excess and Optimal Changes in Cash.

The x-axis shows different net benefits of governance, while the y-axis represents the cumulative percent of weakly governed at or below that net benefit. For the BCF measure, decreases indicate stronger governance, while for the ownership measures, increases generally indicate stronger governance.

relationship between governance and leverage, or the initial BCF measure, which looks very similar to the contemporaneous BCF measure. The figures tell an interesting story. As discussed previously, the benefits in excess cash mirror those of the total change in cash. Thus, the benefits of governance in controlling uses of cash reflect uses of excess cash. More importantly, firms are not losing anything in optimal changes in cash. For all three measures, most firms have a small gain from governance even in optimal cash. This implies that firms gain significantly from governance in controlling excess cash. Moveover, firms do not lose significantly from governance in terms of optimal cash policy. On net, therefore, the benefits of strong governance appear positive.

3.4 Economic Significance of Cash Holdings

Of course, if the value of $1 cash is twice as big under strong governance as weak, but firms hold only a few dollars cash, the effects we've identified are trivial. For each weakly governed firm year, we estimate the net benefits of governance as before. We then multiply that estimated benefit by the ratio of cash to market equity. This transforms the gains from per dollar of cash held to a percent of market equity. We report summary statistics in Table 3.5. Our average gains to shareholders are of similar magnitude to Nikolov and Whited (2010), who build and estimate a structural model that includes agency conflicts between shareholders and managers. While our estimation technique does not overlap with theirs, nor do they work with costs of conflicts between shareholders and creditors, we view their average costs as an important benchmark for the average value of governance in controlling shareholder-manager conflicts (Nikolov and Whited, 2010, Table 5). Nikolov and Whited (2010) report losses of 5.1% on average, whereas our estimated losses are 7.2% for a weak

BCF firm, 7.7% using initial BCF, and 7.2% for institutions.[50] We find it comforting that two completely different approaches provide such similar estimates.[51]

Also as before, the twenty-fifth percentile of our net benefit measure is positive. This implies that only at the top twenty-fifth percentile of leverage ratios sees the value of cash in strong governance firms below that in weak governance firms. The median gains, which are smaller than the mean gain because cash holdings are skewed, also tend to suggest 3% to 4% increases in shareholder value associated with strong governance. For the bottom twenty-fifth percentile of leverage ratios, the difference in values of cash holdings between strong and weak governance firms is on the order of 10% of equity value. The gains to strong governance are large. The losses associated with leverage seem very small. This makes it hard to believe that shareholders optimally choose weak governance to protect themselves against costly shareholder-creditor conflicts.

One intriguing alternative is that firms choose weak governance so that they can adjust leverage easily. We cannot rule out this alternative entirely, but we can provide some evidence against it. If firms correctly judge their leverage needs on

[50] The fact that these numbers mesh so well is all the more surprising given that Nikolov and Whited (2010) do not attempt to match the value of cash holdings directly. The value of cash holdings is implied after matching other moments of financing and investment policies. Moreover, in their model, ownership and bonuses are the mechanisms built in to align shareholders and managers. As long as the Bebchuk, Cohen, and Ferrell (2009) index and 13-F institutional holdings measures either (1) act as substitutes for compensation or (2) help determine compensation policies, the estimates are roughly comparable.

[51] Also, Nikolov and Whited (2010) report gains to governance where firms are grouped by the BCF, blockholder, and total institutional holdings measures. Our estimates are not directly comparable to those estimates because they represent cross effects of managerial compensation with other governance measures. For example, the difference between the high institutional ownership and low institutional ownership losses in Nikolov and Whited (2010) are approximately 13%. However, this implies that strong institutional ownership decreases the effectiveness of managerial compensation by 13%, not that the gain to institutional holdings is 13%. A more correct comparison is the following: For firms with weak institutional ownership, Nikolov and Whited (2010) estimate that improving incentive alignment through compensation increases shareholder value by 4.99%. For firms with weak institutional ownership, we estimate that increasing institutional ownership increases shareholder value by 7.2%.

Table 3.5
Weighted Net Shareholder Gains from Improved Governance.

This table provides estimates of the net benefits of governance in controlling cash as a percent of market value of equity. We use the estimates from Table 3.3 to calculate the net benefit of governance for all firms in the weakest governance group. We then multiply those benefits by the ratio of cash held to market value of equity to estimate wealth losses to shareholders.

		Mean	Std Dev	25th Pct	Med	75th Pct
BCF	G = 2	-0.014	0.055	-0.011	-0.002	0.001
	G = 3	0.016	0.062	0.001	0.006	0.026
	G = 4	0.024	0.086	0.001	0.010	0.037
	G = 5	0.025	0.066	0.002	0.010	0.033
	G = 6	0.072	0.117	0.012	0.038	0.087
BCF_0	G = 2	0.039	0.057	0.006	0.020	0.048
	G = 3	0.019	0.054	0.001	0.007	0.024
	G = 4	0.060	0.086	0.010	0.030	0.073
	G = 5	0.035	0.078	0.003	0.013	0.043
	G = 6	0.077	0.115	0.012	0.039	0.096
Pension	G = 2	0.024	0.040	0.004	0.011	0.027
	G = 3	0.074	0.137	0.004	0.026	0.092
	G = 4	0.077	0.147	0.004	0.026	0.096
	G = 5	0.035	0.110	-0.001	0.010	0.053
Block	G = 2	0.030	0.044	0.005	0.015	0.037
	G = 3	0.026	0.040	0.004	0.013	0.032
	G = 4	0.007	0.027	0.000	0.003	0.012
	G = 5	0.001	0.024	-0.001	0.001	0.006
Institutions	G = 2	-0.019	0.035	-0.024	-0.007	-0.001
	G = 3	0.020	0.041	0.001	0.007	0.027
	G = 4	0.030	0.059	0.002	0.011	0.039
	G = 5	0.072	0.153	0.003	0.026	0.097

average, the net benefit of strong governance in their highest leverage year should be negative, offsetting any positive net benefit of strong governance in their lowest leverage year. We calculate the net benefit of governance at each firm's minimum and maximum leverage ratios, then add the two net benefit measures together. If firms gain from governance in their lowest leverage years but lose from governance in their highest leverage years, we expect a sum at or below zero.

For the BCF, BCF_0, and total institutional ownership measures, we consider the difference between the value of cash in the top and bottom governance groups. For the pension holdings measure, we consider the difference between the second-highest quintile and the lowest quintile of pension holdings. We add the net benefit of strong governance at the firm's minimum leverage ratio to the net benefit at the firm's maximum leverage ratio. We plot histograms for the four measures and provide results in Figure 3.4. The black bars show the (not cumulative) histogram, while the red bars show the cumulative distribution. Based on the Figure, the major clustering occurs at a minimum of \$1.00 to \$1.25 (Pension) and a maximum of \$1.75 to \$2.00 (BCF). This means that at the firm level, most firms gain much more from strong governance in their lowest leverage year than they lose from governance in their highest leverage year. The Figure shows no evidence of symmetry around \$0.00, either. At most, 15% of firms lose more from strong governance in their highest leverage years than the gain from governance in their lowest leverage years. Either firms drastically underestimated the amount of leverage they needed over the sample period, or dynamic concerns in leverage do not drive these firms weak governance choices.

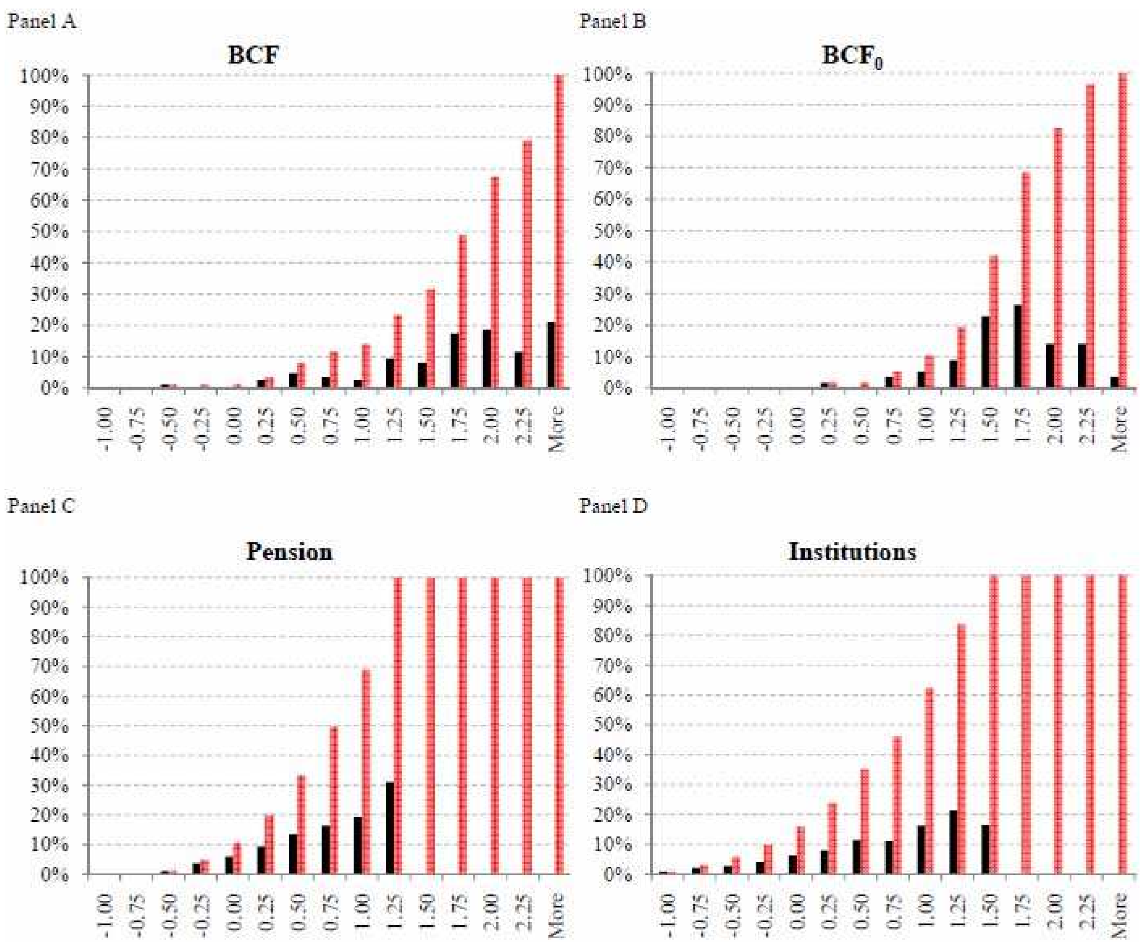

Fig. 3.4. Minimum and Maximum Leverage Ratios.

We use estimates from Table 3.3 to calculate the net benefit of governance at each firms' minimum and maximum leverage ratios. We add these values together for firms in the weakest governance group. This figure shows the empirical distribution of that sum (black bars) and cumulative distribution of the sum (red bars).

3.5 Robustness Checks

3.5.1 Non-linear Effects of Leverage

If the effects of leverage on the net benefits of governance are non-linear, do our interpretations change? In Table 3.6, we provide estimates using power transformations of our leverage ratio. We replace the variable L_{it-1} in Equation 3.1 with L_{t-1}^{γ} for different values of γ. Because the blockholder measure shows no significant effects, we exclude it here. Two important results emerge. First, we see that the point estimates for the benefits of governance are not changed much by nonlinear specifications for the BCF measure. For the ownership measures, there is a bit more variation. Second, note that the R^2 measures suggest that a linear (or near-linear) specification in leverage fits best. The coefficients v_{GL} are harder to interpret in the non-linear specifications. Though their magnitude changes markedly across different specifications, so does the number that they multiply. To ease interpretation, we once again provide graphical results for the distribution of losses. The curves line up closely with our original estimates, confirming that the vast majority of firms gain from strong governance even when the relationship between leverage and the benefit of strong governance is not assumed linear.

3.5.2 Endogeneity

A second important question is whether endogeneity can overturn our conclusions. We must be careful here in what we view as possible endogeneity problems. Our tests are not intended to uncover jointly optimal choices of governance, leverage, and cash. Instead, we are asking whether strong governance helps or hurts shareholders at observed leverage ratios. Though endogeneity could change the interpretation of our estimates, we can still shed some light on this question even if firms set cash holdings and leverage anticipating returns.

Table 3.6

Nonlinear Leverage Specifications.

We replace L_{t-1} in Equation 3.1 with L_{t-1}^{γ} for $\gamma = \{0.5, 0.75, 1, 1.5, 20\}$. We provide the difference between strong and weak governance firms based on the governance grouping with the highest value of cash from Table 3.3. p-values clustered by firm appear in brackets. We also report partial R^2, which gives the R^2 of the regressions net of firm fixed-effects.

	BCF			BCF_0			Pension			Institutions		
γ	$v_{G\|G=6}$	$v_{GL\|G=6}$	R^2	$v_{G\|G=6}$	$v_{GL\|G=6}$	R^2	$v_{G\|G=6}$	$v_{GL\|G=6}$	R^2	$v_{G\|G=6}$	$v_{GL\|G=6}$	R^2
0.5	1.3803	-0.6757	0.1597	1.2360	-0.5349	0.1657	0.7447	-0.5054	0.1821	0.8110	-0.6169	0.1853
	[0.129]	[0.327]		[0.213]	[0.472]		[0.001]	[0.017]		[0.001]	[0.006]	
0.75	1.4102	-1.3372	0.1624	1.1710	-0.9280	0.1682	0.6594	-0.8105	0.1832	0.7197	-1.0463	0.1865
	[0.043]	[0.147]		[0.139]	[0.375]		[0.001]	[0.019]		[0.000]	[0.003]	
1	1.3766	-2.0779	0.1636	1.1333	-1.4157	0.1692	0.5923	-1.1246	0.1831	0.6450	-1.5446	0.1865
	[0.020]	[0.080]		[0.092]	[0.298]		[0.001]	[0.025]		[0.001]	[0.003]	
1.5	1.2858	-3.7731	0.1636	1.0768	-2.6267	0.1687	0.4978	-1.7457	0.1819	0.5400	-2.7771	0.1854
	[0.009]	[0.038]		[0.046]	[0.197]		[0.003]	[0.052]		[0.001]	[0.002]	
2	1.2109	-5.7920	0.1623	1.0321	-4.1373	0.1671	0.4381	-2.3326	0.1804	0.4737	-4.3758	0.1838
	[0.007]	[0.027]		[0.029]	[0.136]		[0.005]	[0.102]		[0.003]	[0.002]	

To overturn our conclusions about the small economic magnitudes of shareholder-creditor conflicts in choosing governance, alternative stories must pass several hurdles. The most important hurdle is that any endogeneity must create value for shareholders, destroy value for creditors, and vary with governance. If, for example, managers respond to unexpected returns by hoarding cash, and this effect differs by governance, it might explain why our estimates differ. However, this type of story will not change our conclusions for two reasons. First, if cash does not increase shareholder value, then shareholder interests cannot differ from those of creditors. If shareholder interests do not differ from those of creditors, then there is no shareholder-creditor conflict to drive governance choices. Second, if governance has a causal impact, then our results still imply that some behavior differs between managers facing strong governance and managers facing weak governance. That behavior converges as leverage increases, but only for extremely high leverage ratios. If strong governance aligns managers with shareholders and weak governance aligns managers with creditorsVan assumption supported by extant researchVthis still means that strong governance alters managerial behavior for the vast majority of firms.

The prior arguments require a causal impact of governance. If governance improves when the value of cash is highest, we could still see what appears to be a net benefit of governance where there is none. Therefore, endogenous governance choices might reverse our conclusions. Because the initial BCF measure is set before the valuation of cash in a given year, Dittmar and Mahrt-Smith (2007) use that variable to help establish causality. According to the prior results, the exogenous part of that measure supports the hypothesis that strong governance creates value for shareholders, but does not strongly support the hypothesis that leverage reduces the net benefits of governance. This is prima facie evidence against shareholder-creditor conflicts as an important cost of strong governance because it implies that shareholders prefer strong governance regardless of leverage.

The institutional ownership measures are not as persistent, and as such, initial holdings do not provide much information about current holdings (as Dittmar and Mahrt-Smith, 2007 also note). We turn to an IV estimator to establish the causality of institutional ownership measures. We use the natural log of lagged shares outstanding as an instrument. This instrument satisfies two important criteria. First, shares outstanding changes ownership structure. For example, for a given market capitalization, greater shares outstanding reduces price and therefore encourages individual ownership. Second, the natural log of lagged shares outstanding should not convey any additional information to the market concerning returns. Several arguments support this claim. First, we are using levels of shares outstanding, not differences. Therefore, the fact that financing events predict low returns is mitigated here.[52] Second, we include additional controls for the natural log of lagged market cap and lagged abnormal returns. This means we rely on lagged shares outstanding's explanatory power above and beyond available return and size information to identify plausibly exogenous changes in institutional ownership. Unless shares outstanding changes the composition of ownership, it is hard to see why splitting the same firm into more or fewer pieces could affect returns.

Unfortunately, we do not have enough instrumental variables to identify each governance dummy variables used in Table 3.3. However, we can use the quintile number as a governance variable as in Table 3.2. Rather than estimating the marginal impact of each different governance level, these models once again provide the average treatment effect of increasing ownership from one quintile to the next for the ownership measures.

We report results in Table 3.7. Because blockholdings do not show results in the prior tests, we again exclude that measure here. Both the effect of governance

[52]See Pontiff and Woodgate (2008) and McLean, Pontiff, and Watanabe (2009) for U.S. and international evidence that share issuance and repurchases predict returns.

Table 3.7

Two-stage Least-squares Evidence.

We fit equation 3.2 using two-stage least-squares:

$$\begin{aligned} AR_{it} &= \beta_{it}\frac{\Delta C_{it}}{M_{it-1}} + \mathbf{Z}_{it}\mathbf{B} + \varepsilon_{it} \\ \beta_{it} &= v + v_G G_{it} + v_L L_{it-1} + v_{GL} G_{it} L_{it-1} \end{aligned}$$

Because each quintile dummy variables requires an additional instrument, we use the quintile number of institutional ownership as a continuous variable. We use the natural log of lagged shares outstanding as an instrument for institutional ownership quintile number. The results are therefore average effects of improving one governance quintile, comparable to those in Table 3.2. We also include the natural log of lagged market cap and the lagged abnormal return as additional controls for size effects. p-values clustered by firm appear in brackets.

	Pension Quintile	Institutions Quintile
v_G	0.2989	0.4040
	[0.0003]	[0.0002]
v_{GL}	-0.4858	-0.6561
	[0.0258]	[0.0159]
v	1.6092	1.3658
	[0.0000]	[0.0000]
v_L	-0.6579	-0.3290
	[0.0371]	[0.4425]

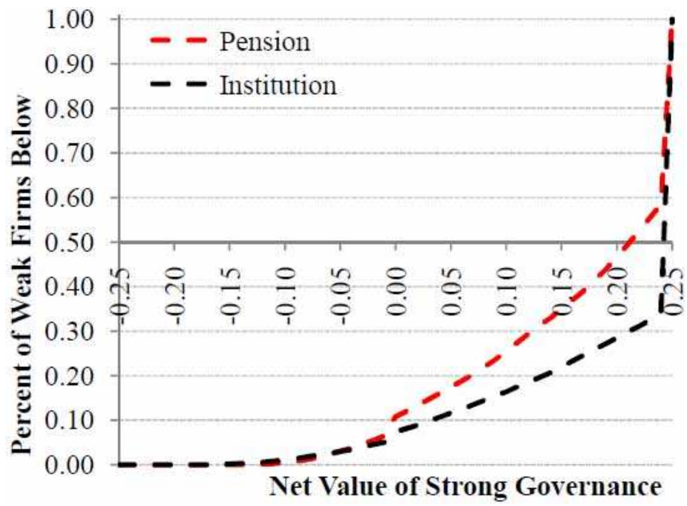

Fig. 3.5. Two-stage Least-squares.

For each firm year in the weakest governance group, we calculate the net benefit of increasing governance from a lower group to a higher group as $v_G + v_{GL} \times L_{it-1}$. Two-stage least-squares estimates of v_G and v_{GL} appear in Table 3.7. The x-axis shows different net benefits of governance, while the y-axis represents the cumulative percent of weakly governed at or below that net benefit.

and the interaction between governance and leverage increase in magnitude. For the pension measure, the benefit of stronger governance in an unlevered firm increases from \$0.15 to \$0.30. The rate at which leverage reduces the value of governance moves from \$0.35 to \$0.49. We see similar increases in the institutional holdings measure. This suggests that when institutional investors select firms, they select those firms where the value of cash is relatively low. By focusing on exogenous institutional ownership, we get a better idea about the value they create. We plot the values of governance that these estimates imply in Figure 3.5. Here the results are even more striking. More than 85% of weakly governed firms would increase the value of their cash holdings by increasing the exogenous portion of pension and institutional holdings into the next highest group. Of low pension holdings firms, half would gain \$0.20 or more per dollar of cash held by increasing pension presence, and 65% of low total institutional ownership firms could gain more \$0.25 or more per dollar by increasing institutional ownership. These amounts are large. Whatever costs keep shareholders from improving governance must be at least that big. However, the major costs of governance appear unrelated to leverage based on our results. Thus, shareholder-creditor conflicts do not appear major contributors to firms' choice of weak governance.

3.5.3 Non-cash Assets

The third important question is whether the benefits of governance in increasing cash are offset by costs in other policies. While we could never refute this argument fully, we can consider the value of cash, which represents future actions, against other assets, which represent assets currently in place. We re-estimate our models with additional parameters that allow the value of non-cash assets to depend on governance and leverage. We present estimates of the effects of governance and leverage on cash and non-cash assets in Table 3.8. The ownership measures and the initial BCF measure are not affected to any notable degree by the inclusion of values of non-cash assets. The value of large institutional ownership falls by approximately \$0.04 per dollar held, for example. The contemporaneous BCF measure shows a much larger value of strong governance when we account for other assets separately. In fact, the point estimate increases by more than \$0.30 per dollar of cash held. The interaction between leverage and governance also becomes much stronger. From Panel B, the ownership and initial BCF measures also show no effects on the value of non-cash assets. There is a nearly significant coefficient on the interaction between leverage and total institutional holdings. We treat this as significant, which biases us toward finding shareholder-creditor conflicts. However, for the other measures, we fail to find even nearly significant governance or governance and leverage interaction effects. The BCF measure, on the other hand, shows a relationship with the value of cash holdings as well as a strong governance and leverage interaction.

To evaluate the magnitudes of the non-cash asset effects for the BCF and total institutional holdings measures, we once again plot the distribution of net values of governance for weak governance firms. We face an interpretation issue however. Non-cash assets make up much more of a firm's assets than cash does. Therefore, we multiply our cash estimates from Panel A by the ratio of cash to market value of equity, we multiply our non-cash assets from Panel B by the ratio of non-cash assets to the market value of equity, and sum the resulting gains.[53] We plot the distributions in Figure 3.6.

[53]For non-cash assets, especially the institutional holdings measure, we face an important choice. If we treat both the governance term and the governance and leverage interaction term as significant in non-cash assets, we will find tremendous costs of governance for all firms because both point estimates are negative. Since non-cash assets are a large part of firms, this automatically implies the value of governance is negative for most firms. This is incorrect for two reasons. First, the negative coefficient for governance is small economically and has a very high p-value. Second, by starting with a negative value of governance, we give credit to shareholder-creditor conflicts as explaining weak governance firms even though the results literally imply that the value of governance is simply low for all firms regardless of leverage. We therefore only use the interaction between leverage and governance to estimate the net benefit of governance in non-cash assets for our total institutional holdings measure.

Table 3.8

Leverage and Governance Effects on the Value of Non-cash Assets.

We augment equation 3.1 as the following regression for groups of firms based on one of five measures of governance:

$$\begin{aligned}
AR_{it} &= \beta_{it}\frac{\Delta C_{it}}{M_{it-1}} + \Theta_{it}\frac{\Delta NA_{it}}{M_{it-1}} + \mathbf{Z}_{it}\mathbf{B} + \varepsilon_{it} \\
\beta_{it} &= v + \sum_{j} v_{G|G=j} I[G_{it} \in j] + v_L L_{it-1} + \sum_{j} v_{GL|G=j} I[G_{it} \in j] L_{it-1} \\
\Theta_{it} &= \theta + \sum_{j} \theta_{G|G=j} I[G_{it} \in j] + \theta_L L_{it-1} + \sum_{j} \theta_{GL|G=j} I[G_{it} \in j] L_{it-1}.
\end{aligned}$$

Panel A: Value of Cash

	BCF		BCF_0		Pension		Block		Institutions	
	$v_{G\|G=j}$	$v_{GL\|G=j}$	$v_{G\|G=j}$	$v_{GL\|G=j}$	$v_{G\|G=j}$	$v_{GL\|G=j}$	$v_{G\|G=j}$	$v_{GL\|G=j}$	$v_{G\|G=j}$	$v_{GL\|G=j}$
G=2	0.4395	-2.0168	0.5348	-0.5928	0.0404	0.3213	0.2357	-0.2829	-0.1190	0.0912
	[0.477]	[0.107]	[0.500]	[0.721]	[0.798]	[0.471]	[0.278]	[0.664]	[0.430]	[0.830]
G=3	0.9591	-2.4410	0.5586	-1.1828	0.5583	-1.1049	0.1856	-0.2165	0.1404	-0.4094
	[0.092]	[0.029]	[0.457]	[0.430]	[0.001]	[0.014]	[0.272]	[0.659]	[0.393]	[0.407]
G=4	1.1491	-2.8046	0.7576	-0.5110	0.5390	-0.9942	0.1303	-0.6574	0.2669	-0.8133
	[0.045]	[0.014]	[0.285]	[0.730]	[0.004]	[0.053]	[0.383]	[0.011]	[0.165]	[0.128]
G=5	1.0001	-2.3857	0.8110	-1.4396	0.4012	-1.3976	0.0177	-0.3893	0.6041	-1.6514
	[0.096]	[0.036]	[0.255]	[0.308]	[0.019]	[0.006]	[0.911]	[0.376]	[0.002]	[0.001]
G=6	1.7006	-3.2235	1.2016	-1.6787	-	-	-	-	-	-
	[0.007]	[0.013]	[0.110]	[0.281]	-	-	-	-	-	-

Panel B: Value of Non-cash Assets

	BCF		BCF_0		Pension		Block		Institutions	
	$\Theta_{G\|G=j}$	$\Theta_{GL\|G=j}$	$\Theta_{G\|G=j}$	$\Theta_{GL\|G=j}$	$\Theta_{G\|G=j}$	$\Theta_{GL\|G=j}$	$\Theta_{G\|G=j}$	$\Theta_{GL\|G=j}$	$\Theta_{G\|G=j}$	$\Theta_{GL\|G=j}$
G=2	0.0492	-0.6168	-0.1580	0.3635	-0.1383	0.3844	0.0896	-0.2744	0.0604	-0.0611
	[0.736]	[0.083]	[0.440]	[0.488]	[0.332]	[0.106]	[0.338]	[0.171]	[0.374]	[0.654]
G=3	0.2808	-0.8425	-0.0601	0.2508	0.0622	-0.1321	-0.0177	0.0183	-0.0853	0.0704
	[0.056]	[0.013]	[0.766]	[0.622]	[0.383]	[0.373]	[0.804]	[0.903]	[0.500]	[0.750]
G=4	0.2314	-0.7614	-0.0219	0.1034	-0.0009	-0.0977	-0.0436	-0.0200	0.0732	-0.2676
	[0.184]	[0.061]	[0.909]	[0.837]	[0.990]	[0.543]	[0.483]	[0.879]	[0.360]	[0.108]
G=5	0.1947	-0.6277	0.0847	0.1737	-0.0651	0.0605	-0.0760	-0.0495	-0.0185	-0.3409
	[0.289]	[0.103]	[0.733]	[0.758]	[0.467]	[0.747]	[0.564]	[0.831]	[0.839]	[0.102]
G=6	0.3529	-1.0355	0.1070	-0.1510	-	-	-	-	-	-
	[0.043]	[0.009]	[0.617]	[0.776]	-	-	-	-	-	-

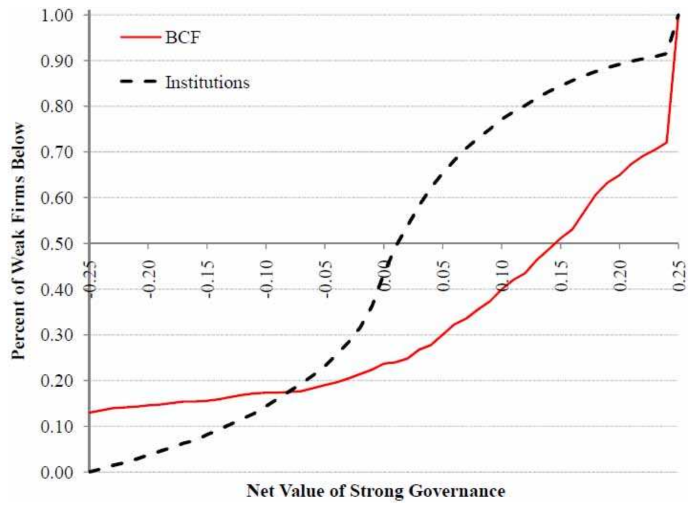

Fig. 3.6. Cash and Non-cash Assets.

We calculate the net benefit of improving governance on cash values from the weakest group to group j as $\left(v_{G|G=j} + v_{GL|G=j} \times L_{it-1}\right) \times \frac{C_{it}}{M_{it}}$ for each weakly governed firm year. Estimates of $v_{G|G=j}$ and $v_{GL|G=j}$ appear in Panel A of Table 3.8. We calculate the net benefit of improving governance on values of other assets from the weakest group to group j as $\left(\Theta_{G|G=j} + \Theta_{GL|G=j} \times L_{it-1}\right) \times \frac{NA_{it}}{M_{it}}$ for each weakly governed firm year. Estimates of $\Theta_{G|G=j}$ and $\Theta_{GL|G=j}$ appear in Panel B of Table 3.8. For each firm year, we then sum these values to calculate the overall net benefit of improved governance on cash and non-cash assets. The x-axis shows different net benefits of governance, while the y-axis represents the cumulative percent of weakly governed at or below that net benefit. We plot the overall benefit of moving from five or six to zero entrenching provisions for the BCF measure, as that measure shows statistically significant governance and leverage effects in Panel B of Table 3.8. We also plot the overall benefit of moving from the smallest institutional holding quintile to the largest institutional holding quintile as that measure shows a nearly significant leverage and governance interaction in Table 3.8.

The results for the BCF model are as before, although the scaling has undergone an important change here. As in Table 3.5, all costs and benefits are expressed as a percent of equity values instead of per dollar of cash held. Approximately 75% of weak governance firms show a positive difference between the equity value in strong and weak governance firms. Moreover, for the median leverage ratio of weak BCF firms, the difference in value exceeds 15%. These are tremendous gains. However, we also see that governance is extremely costly for some firms. Approximately 25% of weak governance firm leverage ratios imply negative net benefits of strong governance. Almost 20% of leverage ratios suggests losses from strong governance greater than 5% of equity value. This is consistent with extreme costs of shareholder-creditor conflicts for those firms and suggests that for those firms, shareholder-creditor conflicts provide a plausible explanation for their choice to remain weakly governed. For the 75% of firms with gains to governance, however, shareholder-creditor conflicts are too small to explain why they've chosen weak governance.

The institutional holdings results are a bit weaker. However, more than 55% of weak governance firms could increase their market value of equity by improving governance. Moreover, approximately 25% of weak institutional holdings firms could increase their equity value by 5% or more by improving governance. It is important to remember that we calculate these gains assuming that leverage hurts the value of governance in other assets even though (1) governance does not show any impact alone on the value of other assets and (2) the coefficient is not statistically significant at traditional levels. We view this result as an overstatement of the effects of leverage and yet, even here, leverage ratios are typically too low to explain weak governance choices in more than half of weak governance firm years.

3.6 Concluding Remarks

We find that leverage reduces the net value of governance in controlling uses of cash. Even if one ascribes the entire reduction to conflicts between shareholders and creditor, however, leverage ratios are too small to explain why two-thirds of weakly governed firms maintain weak governance. Our conclusion is not driven by endogenous changes in governance or nonlinearity, and it is suboptimal cash holdings rather than optimal cash holdings that generate our main results.

Because many of the choices we make in this study bias in favor of finding strong conflicts between shareholders and creditors, we view our results as a reasonable upper bound for the number of firms that can benefit from weak governance. While our estimates suggest some firms gain very little or even lose value in cash holdings from switching to strong governance, these firms represent a small minority. The estimates suggest that the losses from weak governance decreases equity values by 2% to 3%, with averages closer to 7%.

An alternative explanation for our results is that leverage acts as a substitute mechanism for controlling free cash flow. As leverage increases, governance becomes relatively less effective as there are fewer instances of managerial malfeasance to correct. Our tests cannot distinguish between this hypothesis and the shareholder-creditor hypothesis. However, this alternative force should make finding evidence for shareholder-creditor conflicts even easier. Even with alternatives, strong governance appears beneficial for all but the most highly levered firms. We leave further analysis of how the substitution effect and conflict effect interact to future research.

Although our focus was on whether the conflicts between shareholders and creditors could explain governance choices, our results have some implications for the under-leverage debate, as well. Graham (2000) shows that firms do not exhaust the tax benefits of debt leading to the debate on whether firms are under-levered. Parrino and Weisbach (1999) argue via simulation that the costs of conflicts between shareholders and creditors are too small to explain why firms choose such low leverage. Although our focus is on governance choices, rather than leverage choices, we find strong support that the costs of shareholder-creditor conflicts at observed leverage ratios seems too small relative to the governance choice, as well. On the other hand, Ju, Parrino, Poteshman, and Weisbach (2003) show that the value of the firm is quite flat with respect to tradeoffs between tax shields and distress costs, which implies that even a small conflict effect could help explain variation in leverage ratios. This argument suggests strong governance firms should choose lower leverage, as shown by John and Litov (2009). Sorting out the leverage effects of tax shields, distress costs, and apparently small costs of shareholder-creditor conflicts provides an interesting challenge for future work.

4. CONCLUSIONS

In this dissertation, we address two research questions. First, we study the stigma of the adverse effect of intentional financial misreporting on bank loan pricing and the mitigation of such effect associated with significant firm actions. Second, we ask whether shareholder-creditor conflicts generate costs that are large enough to prevent improvements in governance.

From the findings presented in the first essay, “Why Won’t You Forgive Me? Evidence of a Financial Misreporting Stigma in Bank Loan Pricing,” we find consistency with the view that misreporting creates a long-lasting and costly stigma that causes banks to question the credibility of the firms’ future reported financial information, and perhaps more generally, the veracity of firms. Banks therefore face greater screening and monitoring costs and charge a significant misreporting premium in loan spreads. Prompt replacement of certain parties potentially related to the misreporting shortens the duration of the stigma, but a majority of firms do not make such prompt replacements and pay misreporting premiums for at least four to five years following their restatements. The long-lasting stigma evident in loan spreads suggests that banks place great importance on the truthfulness of information provided by firms.

From the second essay, “Can Shareholder-creditor Conflicts Explain Weak Governance? Evidence from the Value of Cash Holdings,” consistent with shareholder-creditor conflicts, we find that leverage reduces the net value of governance in controlling uses of cash. However, even if one ascribes the entire reduction to conflicts between shareholders and creditor, leverage ratios are still too small to explain why two-thirds of weakly governed firms maintain weak governance. Given our results, we assert that there must be some cost that prevents shareholders from improving governance, but that cost is unrelated to leverage. Therefore, the widely proposed potential explanation–shareholder-creditor conflicts–cannot explain why shareholders allow weak governance to persist.

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