ESSAYS ON CORPORATE FINANCE

by

Mingming Qiu

A dissertation submitted to the faculty of
The University of Utah
in partial fulfillment of the requirements for the degree of

Doctor of Philosophy

in

Business Administration

David Eccles School of Business

The University of Utah

August 2013

The University of Utah Graduate School

STATEMENT OF DISSERTATION APPROVAL

The dissertation of **Mingming Qiu**

has been approved by the following supervisory committee members:

Michael Cooper	, Chair	**5/7/2013** Date Approved
Rachel Hayes	, Member	**5/7/2013** Date Approved
James Schallheim	, Member	**5/7/2013** Date Approved
Christopher Stanton	, Member	**5/7/2013** Date Approved
Yihui Pan	, Member	**5/7/2013** Date Approved

and by **William Hesterly** , Associate

Dean of the **David Eccles School of Business**

and by Donna M. White, Interim Dean of The Graduate School.

ABSTRACT

This thesis consists of three chapters. In Chapter 1, we use a unique dataset of natural disasters, including earthquakes, floods, storms, volcanic eruptions, and wildfires, to test whether investors suffer from behavioral bias such as underreacting to news and investor sentiment. In Chapter 2, we study the rationale for firms' use of inside debt (pension and deferred compensation) by exploiting the relation between firms' default risk and inside debt. In Chapter 3, we research the role of inside debt in the optimal structure of chief executive officer pay by performing a simulation analysis of investment distortions.

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ACKNOWLEDGMENTS

I am especially grateful for my dissertation chair, Professor Michael J. Cooper, for his generous help and guidance in the last few years. I would also like to express my gratitude to the members of my dissertation committee for their helpful advice and comments. I also wish to express my love and gratitude to my beloved parents and sister for their unconditional love and support.

CHAPTER 1

NATURAL DISASTERS

This chapter uses a unique dataset of natural disasters, including earthquakes, floods, storms, volcanic eruptions, and wildfires, to test whether investors suffer from behavioral bias such as underreacting to news and investor sentiment. Natural disasters represent a natural experiment setup as they are orthogonal to macroeconomic factors and managerial decisions. We start by assessing natural disasters' effects on stock prices and firm characteristics of U.S. firms. The value-weighted average four-factor-model abnormal return from the first to 30 days after the event is -3.87% or $63 million loss per natural disaster. Abnormal returns continue to be negative for up to 360 days following the disasters. Correspondingly, firm profitability and investment decrease. The negative capital market response together with decreased firm performance and more restricted investment policies support either slow information diffusion, or investor underreaction. We find some evidence consistent with each hypothesis. Finally, using natural disasters as a negative shock to investor sentiment, we provide direct evidence that investor sentiment affects stock prices in a setup with arguably little endogeneity concern.

1.1 Introduction

In this chapter, we investigate the effects of natural disasters on publicly traded firms to shed light on behavioral theories about the capital market, including investor underreaction and investor sentiment. Natural disasters represent a natural experiment setup as they are orthogonal to macroeconomic factors and managerial decisions. Specifically, we test whether investors suffer from behavioral bias such as underreacting to news and whether investors are subject to sentiment.

We use a unique dataset of natural disasters from 1970 to 2010 in the United States, including earthquakes, floods, storms, volcanoes and wildfires. This dataset includes a total of 47 natural disasters, which affect 241 unique counties and cause an average of $11 billion worth of damage per event. We identify affected firms by selecting those firms headquartered within the counties that are reported to be affected by our sample of natural disasters. Despite the obvious loss of life and often immense physical damage to property, plant, and equipment, little is known about how natural disasters affect firms operating in the disaster areas. In this chapter, we directly address the effects of natural disasters on shareholder wealth, accounting performance, and firm operating policies. This is important because it provides insight into firm preparedness for rare-event risk and allows for a truly exogenous environment to evaluate investor reaction to these rare events and test behavioral theories such as investor underreaction and sentiment.

We start by assessing natural disasters' effects on shareholder wealth of U.S. firms by implementing a standard event study. Intuitively, we might expect that many publically traded firms are geographically well diversified and/or adequately insured against natural disasters. In this sense, when their headquarters are affected by natural disasters, there should be no direct effect on the shareholder wealth, despite the significant damage associated with natural disasters. However, we find results directly opposite to a no-effect hypothesis. Specifically, we present new evidence of statistically significant negative abnormal return for firms affected by natural disasters in our sample. The value-weighted average four-factor-model abnormal returns from event day -1 to +1 is -0.22%, growing to a highly statistically significant -3.87% over the day 1 to day 30 window following the disaster. Further, it decreases up to -6.1% over the day 1 to day 120 window, and then increases to -1.39% over the day 1 to day 360 window. Thus, this result indicates that natural disasters are associated with large negative shareholder wealth effects and that the event returns are, on first glance, not entirely consistent with an efficient market reaction to the event (i.e., we find evidence of continued negative returns well after the event date). This result is robust to different models for estimating abnormal returns, and is robust to value-weighting, square-root-of-market-capitalization weighting, and to equal weighting of the abnormal returns.

To investigate the negative capital market response, we then look for evidence that natural disasters affect firm characteristics, besides returns, in a way consistent with natural disasters causing physical damages to the firm. We measure firms' accounting performance and policies from pre to post-disasters. We find that the accounting performance for affected firms decreases on average post-disasters. At the same time, we find that firms decrease their capital expenditure and tangible assets, and increase their leverage ratios. We interpret this as evidence that natural disasters have a negative effect on both firms' accounting performance and investment policies. This is consistent with the possibility that firms are not geographically diversified and/or adequately insured against losses from natural disasters in the United States. This is also at least partially consistent with a rational reaction in the equity markets given that the overall negative return effect is accompanied by deteriorating firm performance and more restricted investment policies.

Then we investigate two hypotheses that may potentially explain the negative capital market response coupled with decreased firm performance and more restricted investment policies (slow information diffusion or investor underreaction).

The first hypothesis, a rational behavior hypothesis, is based on Hong and Stein (1999) which provides a rational explanation for momentum trading strategies. In their model, there are two groups of rational investors, "news-watchers" and "momentum traders". If information diffuses gradually across the population, prices underreact in the short run. This rational slow diffusion of information coupled with decreases in operating performance would then explain the intermediate to long-term negative returns after the event. The second hypothesis, an irrational underreaction hypothesis, which is not necessarily exclusive of the first hypothesis, is that investors irrationally underreact to the disasters, causing intermediate to long-term positive autocorrelation in returns.

Despite the difficulty to potentially differentiate between the two hypotheses, we try to shed some light on which hypothesis is more consistent with our empirical results.

We first test the rational behavior hypothesis. If the negative effect on shareholder wealth is directly related to the potential damages that natural disasters can cause to the firm, we would expect a stronger effect for more severe disasters. Not surprisingly, we find that firms affected by worse disasters exhibit statistically significant lower post-event abnormal returns.[1] We take this as evidence supporting the information diffusion hypothesis as there is a direct link between the news of natural disasters and subsequent negative effect on shareholder wealth. However, we are aware that we are making an assumption that an irrational investor reaction is independent of the severity of the news.

To explore the rational behavior hypothesis further, we investigate a necessary condition for this hypothesis. To the extent that the post-disaster negative capital market effect is associated with rational behavior coupled with slow information diffusion, then at a minimum we should observe the firm's accounting performance and operating policies change in a way consistent with the revealed investor response to the capital market. Specifically, if the post-disaster negative effect is partially driven by slow diffusion of

[1] We use the reported insured loss as a measure for the severity of disasters, despite the fact that these numbers will not be revealed until later.

information, then we would expect worse accounting performance for firms experiencing worse negative abnormal returns. We measure how changes in firm performance and policies vary in the cross section. Indeed, we find that firms experiencing more negative abnormal returns are associated with worse accounting performance. This result provides further support for the rational behavior hypothesis. However, we find that firms experiencing more negative abnormal returns are associated with less restricted investment policies. This result supports the irrational behavior hypothesis.

We then move to test the irrational behavior hypothesis. If the negative return drifts post-disasters can be partially attributed to irrational investor underreaction, then we would expect that the firms where investors are more likely to underreact experience lower negative returns in the long run. We find some marginal evidence that firms with lower negative returns during the day -1 to 1 event window experience less negative subsequent return. If firms that exhibit lower negative returns around the event date are also those that investors may underreact in a longer term, then this evidence is contrary to the irrational underreaction hypothesis. However, this result is subject to measurement errors and omitted variables bias.

To explore the irrational behavior hypothesis further, we also investigate a necessary condition for this hypothesis. To the extent that the post-disaster negative capital market effect is associated with irrational behavior, then at a minimum we should observe the post-disaster negative capital market effect is independent of the potential loss natural disasters cause to a firm. Specifically, if the post-disaster negative effect is partially driven by irrational underreaction, then we would expect firms with different degrees of exposure to natural disasters experience similar negative abnormal returns. Recall that we identify affected firms by selecting those firms headquartered within the counties that are reported to be affected by our sample of natural disasters. We directly measure firms' geographical dispersion by counting the number of states mentioned in the operation section of firms' 10-k annual report.[2] We then use this geographical dispersion measure as a proxy for the degree of the firm's exposure to natural disasters. Interestingly, firms that are more geographically diversified exhibit similar negative abnormal returns following natural disasters to less geographically-diversified firms. This result provides further support for the irrational underreaction hypothesis.

To test the two hypotheses in a unified framework, we investigate to what extent the

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[2]We thank Garcia Diego and Norlis Oyvind for sharing these data. In our sample, 2344 firms out of 2844 firms have this measure.

variation in cumulative abnormal returns can be explained by changes in firms' accounting performance and operating policies. If the negative return drifts post-disasters can be partially attributed to slow diffusion of information, then we would expect that the changes in key accounting performance and operating policies, and how they affect returns, are consistent with economic intuition. More importantly, we would expect these variables have sufficient explanatory power for the post-disaster negative return. We find that firms that become less profitable in face of natural disasters experience lower negative returns. However, the explanatory power of this regression analysis is below 10%. This indicates that our results are subject to omitted variables bias. In other words, 90% of the negative returns cannot be explained by observable firm characteristics. We take this as evidence supporting both the rational behavior and the irrational underreaction hypothesis.

Finally we use natural disasters as a negative shock to investor sentiment and test the hypothesis of whether investors are subject to sentiment. Specifically, we hypothesize that investors of those firms affected by disasters will experience a negative shock in terms of sentiment. In the cross section, those firms that are potentially more prone to sentiment bias (such as growth firms and past winners) before the event should experience more negative returns.

We start by characterizing the types of firms that experience the worst returns. In addition to lower returns for higher disaster firms, we also find that growth firms (low book-to-market ratios) and past winners (high lagged 12-month returns) experience the worst effects. This evidence is consistent with investor overreaction to growth firms and past winners (Lakonishok, Shleifer, & Vishny, 1994); growth firms and past winners tend to be firms that have been affected by investor sentiment and overvalued. Natural disasters introduce a negative shock to investor sentiment and serve as a catalyst for the start of "correction phase" for such firms. This provides direct evidence that investor sentiment affects stock prices in a setup with arguably little endogeneity concern..

In this chapter, we document that natural disasters are followed by a statistically significant negative effect on shareholder wealth and firms' accounting performance. More importantly, we find some evidence consistent with rational behavior coupled with slow information diffusion of the events and irrational underreaction hypothesis. However, we do not have strong evidence to rule out either hypothesis due to the limitations of our experiments. First, the accounting information we use only becomes publicly available long after our event window. Second, we can only explain 10% of the negative returns using observable firm characteristics.

In summary, we find evidence that natural disasters have negative impacts on affected firms: firms appear to have restricted access to capital, and suffer from worse accounting performance post-disasters. However, these impacts appear to be insufficient to explain the negative reaction in the capital market. We also use natural disasters as a negative shock to investor sentiment and find some evidence consistent with the hypothesis that investor sentiment affects stock prices.

This chapter is related to both a relatively new literature of natural disasters and the literature of market efficiency.[3] As to the literature of natural disasters, a related paper is Worthington and Valadkhani (2004). They use a unique dataset of Australian natural disasters and find that bushfires, cyclones and earthquakes, unlike severe storms and floods, have a major effect on the Australian capital markets. Interestingly, they find that the effects can be either positive or negative and most effects appear on the day of the event with only minor adjustment in the following days. As to the literature of market efficiency, we contribute to the literature by providing new evidence on the effect of natural disasters on shareholder wealth. We provide supporting evidence for the rational behavior hypothesis, the irrational underreaction hypothesis, and the hypothesis that investor sentiment affects stock prices.

1.2 Natural disasters' effects on stock prices and firm characteristics

1.2.1 Natural disaster sample construction

The sample of natural disasters is from the Centre for Research on the Epidemiology of Disasters database "EM-DAT: The OFDA/CRED International Disaster Database" for the period January 1971 to December 2010. The original dataset covers 671 natural disasters in the U.S. including earthquakes, epidemics, floods, storms, droughts, extreme temperatures, volcanoes, wildfires, and mass wet movements of land. Because our goal is to identify the capital market's response to the most significant natural disasters, we exclude 333 observations with missing data on total caused damage as well as the remaining six droughts, and six extreme temperature events. From the remaining observations, we then select the most significant natural disasters, defined as those ranked in the top 10% of the total

[3] Another strand of literature uses natural disasters as exogenous events to research economic questions. For example, Barro (2006) uses rare economic disasters like World War I to explain asset-pricing puzzles such as the high equity premium, low risk-free rate, and volatile stock returns. S. Baker and Bloom (2011) use natural disasters, terrorist attacks and unexpected political shocks as instruments to research the causal relation between uncertainty and growth. Morse (2011) uses natural disasters as an exogenous shock to identify a causal relation between welfare and access to credit.

caused damage. Finally, we identify the counties affected by natural disasters by manually matching each natural disaster to the list of historical Presidential disaster declaration areas obtained from the FEMA GIS data feeds (http://gis.fema.gov/DataFeeds.html). The final sample consists of 49 natural disasters affecting 1,562 unique counties.

We use the firm headquarters address data from Compustat to identify firms affected by natural disasters. We first geocode firm addresses and plot them on the Census county map for the entire U.S for each decennial Census from 1970 to 2010. For each natural disaster, we then select all firms headquartered in the affected counties during the Census year proceeding the year of the natural disaster. The sample of firms affected by natural disasters consists of 8,507 firm-disasters and covers 4,478 unique firms.

1.2.2 Summary statistics for natural disasters

First, we describe summary statistics for our final disaster sample. We start by collecting firms' stock return data from CRSP (The Center for Research in Security Prices). We require that firms that are affected by our disaster sample satisfy the following two restrictions to facilitate our empirical tests. First, we require that the firm's stock price 61 days before the date that natural disasters happen (disaster event date) is greater than three dollars.[4] This restriction helps exclude relatively small firms. Second, we require that firms have at least 120 observations of stock return data between 360 and 61 days before the disaster event date. This restriction helps guarantee that we have reasonable four-factor model alpha and betas estimates to calculate the four-factor model expected return. These restrictions result in a final disaster dataset of 5,151 firm-disasters.

First in Table 1.1, we present the descriptive statistics for our final natural disaster sample. We start by presenting the number of unique natural disasters, total number of affected unique counties, total number of affected unique firms, total number of affected firm-disasters, average damage per unique disaster and average damage per unique firm-disaster for our final natural disaster sample. We have 47 unique natural disasters, including earthquakes, floods, storms, volcanoes and wildfires. These disasters affect 241 unique counties, 2844 unique firms, 5151 unique firm-disaster years, and result in an average damage of $10,748 million per unique disaster, and an average damage of $98 million per firm-disaster.

Next, we present relevant summary statistics for each type of natural disaster. We can

[4]If there is no trading activity for that particular day, then the price is calculated as the average of the bid-ask spread.

see that storms are the most frequent natural disaster in our sample. Thirty out of the total 47 natural disasters are storms. The storms in our sample affect 216 unique counties and result in an average damage of $13,684 million per disaster, and an average damage of $120 million per firm-disaster. Floods are the second most frequent natural disaster. Seven out of the total 47 natural disasters are floods. They affect 62 unique counties and result in an average damage of $2,876 million per disaster, and an average damage of $30 million per firm-disaster. However, it is worth noting that the natural disasters that results in the highest average dollar amount of damage are earthquakes. Only 3 out of our total 47 natural disasters are earthquakes. They affect 12 unique counties but result in an average damage of $18,707 million per disaster, and an average damage of $165 million per firm-disaster. Earthquakes and storms are the only two types of natural disasters that result in above average dollar amounts of damage. Not surprisingly, storms affect the most unique counties and firm-disaster years.

Second, we describe summary statistics for firm characteristics. We start by presenting the size deciles of affected firms by our final disaster sample using the decile statistics of all firms traded in New York Stock Exchange (NYSE). Then we present summary statistics of some characteristics for affected firms.

In Table 1.2 we use the firm's market capitalization 61 days before the disaster event date and report the dispersion of market capitalization for our sample using the NYSE decile breakpoints calculated as of the disaster event date. The sample clearly tilts toward small capitalization firms. This may be due to the fact that larger market capitalization firms in the United States cluster on the coasts, and natural disasters in our sample randomly spread out across the United States. There are 4,148 total firm-disaster years that lie below decile 5, representing 81% of our total firm-disaster observations. This is the decile distribution if we use the decile break points of all NYSE firms for the same year and month as the 61 days before the disaster happened. If we use the median market capitalization of all NYSE firms during years 1970 to 2010, which is $393 million, 65% of all 5,151 firm disaster observations are below the median value of $393 million.

Third, we provide summary statistics for firm characteristics that we use in our regression analysis presented in the empirical test section. We further require that firms in our final disaster sample satisfy the following two restrictions. In our regression analysis, we analyze firms' day 1 to day 30 abnormal return. Hence the first requirement is that firms have non-missing abnormal return data for each day in the day 1 to day 30 event window. Second, we require that the firm has non-missing data for all chosen explanatory firm characteristics.

These restrictions result in a final sample of 4868 firm-disasters.

In Table 1.3, we provide summary statistics of related firm characteristics used in our empirical analysis. The affected firms in our sample have an average research and development expense of 0.05 and an average of capital expenditure of 0.07. Tangibility is 0.28 on average. Book-to-market equity is 0.60. Log (sales) is 5.21 on average. Dividend payout ratio is 0.63 on average. Book leverage is 0.2 on average. The last 12 months' cumulative holding return is 26%. Return on assets is on average 0.002, while profitability is 0.095 on average. Net income is 0.001 on average.

1.2.3 Event study of natural disasters

We start by performing a standard event study following natural disasters and present cumulative abnormal return for various event windows. Then we characterize the type of firms that get affected the most by natural disasters. Finally, we research whether the reaction to natural disasters can be rationalized with corresponding changes in firm performance and policies.

In the event study, we calculate two types of abnormal returns at firm level. First, we calculate the four-factor-model abnormal return. For each firm, we use firms' stock return from 360 to 61 days preceding the disaster event date to estimate the four-factor model. The four pricing factors are market, HML, SMB and momentum. This gives us the alpha (the intercept) and four beta estimates for the four-factor model. To guarantee estimate accuracy, we require that firms in our final sample have at least 120 observations to calculate alpha and betas. We then use the estimated alpha and betas to calculate the four-factor model expected return for each day, from day 1 to day 360 following the disaster event date. And the four-factor-model abnormal return is calculated by subtracting expected return from raw return. Second, we calculate the CAPM abnormal return. The procedure to calculate CAPM abnormal return is similar to the four-factor-model abnormal return, except that we only have one marketing pricing factor instead of four factors.

In Table 1.4 we report abnormal returns for various event windows and associated t-statistics, using three different ways to weight individual firms' return data.[5] Our weighting methodologies include 1) using -61 day market capitalization to value weight return; 2) using square root of -61 day market capitalization to value weight return; 3) equal weight return. Refer to Table 1.4, we report the four-factor-model abnormal return using three weighting

[5] In Appendix B, we provide a detailed description of how we calculate cumulative abnormal return and associated t-statistics.

methodologies. For each event window, we simply add the individual day abnormal return. For example, following natural disasters day 2 to 15, day 1 to day 30, day 1 to day 90, and day 1 to day 120, respectively, earn a statistically significant value-weighted four-factor-model abnormal return of -1.58%, -3.87%, -5% and -6.1%. Using square root of value and equal weighting methodologies, the results are stronger in the sense that we get statistically significant return for all selected windows.

Refer to Table 1.4, we report the CAPM abnormal return using three weighting methodologies. Similarly, for each event window, we simply add the individual day abnormal return. For example, following natural disasters day 2 to 15, day 1 to day 30, day 1 to day 90, day 1 to day 120, and day 1 to day 360, respectively, earn a statistically significant value-weighted CAPM abnormal return of -2.1%, -4.39%, -6.41%, -10.28% and -15.92%. Using square root of value and equal weighting methodologies, the results are similar.[6]

We also graph the cumulative abnormal return, both the four-factor-model abnormal return and CAPM abnormal return using all three weighting methodologies in Figure 1.1 to 1.3. Consistent with Table 1.4, Figure 1.1 also shows that the cumulative four-factor-model abnormal returns earned around the declaration date of natural disasters are negative and the results are stronger when we use square root of value and equal weighting methodology.

1.2.4 Changes in firm accounting performance and operating policies

To investigate the negative capital market response, we then look for evidence that natural disasters affect firm characteristics, besides returns, in a way consistent with natural disasters causing physical damages to the firm. We measure firms' accounting performance and policies from pre to post-disasters. Refer to Table 1.5, we find that the accounting performance for affected firms decreases on average post-disasters. At the same time, we find that firms decrease their capital expenditure and tangible assets, and increase their leverage ratios. We interpret this as evidence that natural disasters have a negative effect on both firms' accounting performance and investment policies. This is consistent with the possibility that firms are not geographically diversified and/or adequately insured against losses from natural disasters in the United States. This is also at least partially consistent with a rational reaction in the equity markets given that the overall negative return effect is accompanied by deteriorating firm performance and more restricted investment policies.

[6] It is worth noting that using CAPM model gives us larger negative abnormal returns. This is consistent with the fact that our sample tilts toward small capitalization firms. Hence, it is important to control for SMB pricing factor when we calculate abnormal return in our sample.

1.3 Hypothesis development

In Section 1.2, we document that natural disasters are followed by a statistically significant negative reaction in the capital market and firm profitability and investment decrease. In this section, we present two hypotheses that may potentially explain our results.

The first hypothesis, a rational behavior hypothesis, is based on Hong and Stein (1999) which provides a rational explanation for momentum trading strategies. In their model, there are two groups of rational investors, "news-watchers" and "momentum traders". If information diffuses gradually across the population, prices underreact in the short run. This rational slow diffusion of information coupled with decreases in operating performance would then explain the negative returns.

The second hypothesis, an irrational underreaction hypothesis, which is not necessarily exclusive of the first hypothesis, is that investors irrationally underreact to the disasters, causing intermediate to long-term positive autocorrelation in returns.

1.4 Empirical tests of the two hypotheses

Natural disasters represent an exogenous event that is in nature uncorrelated with firm and investor characteristics. In this sense, natural disasters serve as an ideal testing ground for various models of investor behavior. We exploit natural disasters to differentiate the two hypotheses for investor behavior in response to news related to disasters.

1.4.1 Subsample analysis on the basis of natural disaster severity

In Section 1.3.1, we show that natural disasters are followed by a statistically significant negative abnormal return. If the negative effect on the capital market is directly linked to the news of natural disasters, we would expect a stronger effect for more severe disasters. In this section, we directly test this intuition by performing standard event studies in subsamples.

We first sort our disaster sample, a total of 47 disasters, into two groups based on the total damage. Then we assign firms into high and low disaster firm group based on whether the firm-disaster year is affected by a high or low damage disaster. In our final sample, 44% (5151 observations) is assigned to the low disaster group. This gives us a relatively balanced dispersion between the low and high disaster group. Refer to Table 1.6 Panel A, the negative effect of natural disasters is statistically stronger for the high disaster damage group for all windows except the day 1 to day 360 window. This evidence reinforces the link between the news of natural disasters and subsequent negative reaction in the capital market.

1.4.2 Changes in firm performance and policies post-disasters

In this section, we explore a necessary condition for the rational behavior hypothesis. To the extent that the post-disaster negative effect is associated with rational behavior coupled with slow information diffusion, then at a minimum we should observe the firm's accounting performance and operating policies change in a way consistent with the revealed investor response to the capital market. Specifically, if the post-disaster negative effect is primarily driven by slow diffusion of information, then we would expect worse firm performance for firms experiencing worse negative abnormal returns. We directly measure how changes in firm performance and policies vary in the cross section. Indeed, refer to Table 1.7, we find that firms experiencing more negative abnormal returns are associated with worse accounting performance. This result provides further support for the rational behavior hypothesis.

1.4.3 Analysis using firms' exposure to natural disaster

To explore the irrational behavior hypothesis further, we also investigate a necessary condition for this hypothesis. To the extent that the post-disaster negative capital market effect is associated with irrational behavior, then at a minimum we should observe the post-disaster negative capital market effect is independent of the potential loss natural disasters cause to a firm. Specifically, if the post-disaster negative effect is partially driven by irrational underreaction, then we would expect firms with different degrees of exposure to natural disasters experience similar negative abnormal returns. Recall that we identify affected firms by selecting those firms headquartered within the counties that are reported to be affected by our sample of natural disasters. We directly measure firms' geographical dispersion by counting the number of states mentioned in the operation section of firms' 10-k annual report. We then use this geographical dispersion measure as a proxy for the degree of the firm's exposure to natural disasters. Interestingly, firms that are more geographically diversified exhibit similar negative abnormal returns following natural disasters to less geographically-diversified firms. This result in Table 1.6 Panel B provides further support for the irrational underreaction hypothesis.

1.4.4 A unified test of the two hypotheses

To test the two hypotheses in a unified framework, we investigate to what extent the variation in cumulative abnormal returns can be explained by changes in firms' accounting performance and operating policies. If the negative return drifts post-disasters can be partially attributed to slow diffusion of information, then we would expect that the changes in key accounting performance and operating policies, and how they affect returns, are consistent with economic intuition. More importantly, we would expect these variables have sufficient explanatory power for the post-disaster negative return.

Refer to Table 1.8, we regress four-factor-model abnormal return from day 1 to day 30 following natural disasters on changes in some key firm performance and policy variables, including tangibility, profitability, dividend payout ratio, and book leverage. We also include some key firm characteristics that have been shown to be correlated with post-disaster abnormal return, including market capitalization, book-to-market ratio, past 12 months' return and a disaster indicator that takes disaster damage into consideration. To construct the disaster rank variable, we first sort our final disaster sample, a total of 47 disasters, into two groups based on the total damage with Group 1 as the low disaster damage group and Group 2 as the high disaster damage group. Then we assign firms into high and low disaster firm group based on whether the firm-disaster year is affected by a high or low damage disaster. The disaster rank variable is equal to zero if the firm belongs to Group 1, and one if the firm belongs to Group 2.

Focusing on the coefficients of variables describing changes in firms' performance and policies, we find that firms that become less profitable in face of natural disasters experience lower abnormal returns. We take this as evidence supporting the rational behavior hypothesis. However, the explanatory power of this regression analysis is below 10%. This indicates that our results are subject to omitted variables bias. In other words, 90% of the negative returns cannot be explained by observable firm characteristics. We take this as evidence supporting both the rational behavior and the irrational underreaction hypothesis.

1.5 Empirical test of investor sentiment

In this section, we use natural disasters as a negative shock to investor sentiment and test the hypothesis of whether investors are subject to sentiment. Specifically, we hypothesize that investors of those firms affected by disasters will experience a negative shock in terms of sentiment. In the cross section, those firms that are potentially more prone to sentiment bias (such as growth firms and past winners) before the event should experience more negative returns.

Specifically, we investigate what type of firms get affected the most from natural disasters in a regression format. We regress firms' abnormal return (four-factor-model abnormal return and CAPM abnormal return) on firm characteristics that may correlate with the degree that the firm suffers from natural disasters according to economic intuition and theory. More importantly, we test whether those firms that are potentially more prone to sentiment bias (such as growth firms and past winners) before the event actually experience more negative returns after investors are subject to negative sentiment shock. Namely, we use natural disasters as a shock to investor sentiment to test whether the capital market is subject to investor sentiment.

M. Baker and Wurgler (2006) show that investor sentiment affects the cross-section of stock returns. They hypothesize that investor sentiment has larger effects on securities that are hard to value and difficult to arbitrage, for example growth stocks. Then they show that this type of stocks experience subsequent lower (higher) returns if sentiment is high (low) at the beginning period. We have a setup where natural disasters introduce a shock to investor sentiment. This helps us test investor sentiment with arguably little endogeneity problems. Following the argument in M. Baker and Wurgler (2006), we hypothesize that following a negative shock to investor sentiment, the type of stocks that are potentially affected more by sentiment will experience lower returns.

In Table 1.9, we provide empirical results when we regress abnormal return on firm characteristics. We select day 1 to day 30 abnormal return as the dependent variable because this event window has shown consistent statistically significant negative abnormal return when we use various weighting methodologies. We have performed the same regression specifications using day 2 to day 90 abnormal return, and get similar results. All regressions include industry and year fixed effects to control for any unobserved time-invariant heterogeneity across industries and calendar years.[7] The inclusion of fixed effects thus identifies the average within-industry and within-year changes in the dependent variables as a function of the independent variables in the regressions.

In addition to lower returns for higher disaster firms (we document this in Section 1.4.1), we also find that growth firms (low book-to-market ratios) and past winners (high lagged 12-month returns) experience the worst effects. This evidence is consistent with investor overreaction to growth firms and past winners (Lakonishok et al., 1994). Growth firms and past winners tend to be firms that are affected by sentiment and overvalued. Natural disasters introduce a negative shock to investor sentiment and serve as a catalyst for the start of "correction phase" for such firms. This provides direct evidence that investor sentiment affects stock prices.

In summary, we test the hypothesis of whether investors are subject to sentiment. Specifically, we hypothesize that the investors of the firms affected by disasters will experience a

[7]Industry definitions can be found at the following link. We use 30 industries to guarantee enough number of firms in each industry. http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html

negative shock in terms of sentiment. In the cross section, those firms that are potentially more prone to sentiment bias (such as growth firms and past winners) before the event should experience more negative returns. We find evidence consistent with this prediction.

1.6 Summary

This chapter provides new evidence of the effect of natural disasters on the capital markets and contemporaneous changes in firm performance and firm policies. We use a unique dataset of various natural disasters such as earthquakes, floods, storms, volcanoes and wildfires from 1970 to 2010 in the United States. These disasters result in an average of $11 billion in infrastructure damage per event. We perform an event study and find that the value-weighted average four-factor-model abnormal returns 30 days following natural disasters is approximately -3.87%, which translates into $63 million loss in shareholder wealth per firm-event. This negative effect continues for up to 360 days following the disasters. At the same time, firm profitability and investment decrease. This negative effect coupled with decreased firm performance and more restricted investment policies can be attributed to either rational behavior together with slow information diffusion, or irrational investor underreaction.

We find some evidence consistent with both hypotheses. First, we show that firms affected by worse disasters experience more negative post-disaster returns. Further, these firms also exhibit worse firm profitability. We take these as evidence supporting the rational behavior hypothesis. However, firms affected by worse disasters experience less restricted investment policies. Also, firms with less exposure to natural disasters show similar negative post-disaster returns to those with more exposure. We take these as evidence supporting the irrational underreaction hypothesis. Finally, changes in observable firm characteristics can only explain less than 10% of the negative post-disaster returns. In a nutshell, we do not have strong evidence to rule out either hypothesis due to the limitations of our experiments.

Finally, using natural disasters as a negative shock to investor sentiment, we test the hypothesis of whether investors are subject to sentiment. Specifically, we hypothesize that investors of those firms affected by disasters will experience a negative shock in terms of sentiment. In the cross section, those firms that are potentially more prone to sentiment bias (such as growth firms and past winners) before the event should experience more negative returns. Indeed, we find that growth firms (low book-to-market ratios) and past winners (high lagged 12-month returns) experience the worst effects. This evidence is consistent with investor overreaction to growth firms and past winners (Lakonishok et al., 1994). Growth firms and past winners tend to be firms that are affected by sentiment and overvalued. Natural disasters introduce a negative shock to investor sentiment and serve as a catalyst for the start of correction phase for such firms. This provides direct evidence that investor sentiment affects stock prices.

In summary, we find evidence that natural disasters have negative impacts on affected firms: firms appear to have restricted access to capital, and suffer from worse accounting performance post-disasters. However, these impacts appear to be insufficient to explain the negative reaction in the capital market. We provide supporting evidence for investor underreaction to natural disasters. Finally, we use natural disasters as a shock to investor sentiment and find some evidence consistent with the hypothesis that investor sentiment affects stock prices.

We contribute to the literature by exploiting exogenous events of various natural disasters and provide new evidence for investor underreaction and investor sentiment. Natural disasters, by definition exogenous events, serve as an ideal testing ground for behavioral models. We provide supporting evidence for both theories in a setting with arguably little or no endogeneity concerns.

Table 1.1: Summary statistics of the natural disaster sample.
This table reports summary statistics for our final natural disaster sample. The final natural disaster sample includes 47 natural disasters that happened from 1970 to 2010 in the United States, including earthquakes, floods, storms, volcanoes and wildfires. For all natural disasters and each type of natural disaster in our sample, we report the number of unique natural disasters, total number of affected unique counties, total number of affected unique firms, total number of affected firm-disasters, average damage per unique disaster and average damage per unique firm-disaster.

Event	Number of unique natural disasters	Total number of affected unique counties	Total number of affected unique firms	Total number of affected firm-disasters	Average damage per unique disasters ($000)	Average damage per unique firm-disasters ($000)
All	47	241	2844	5151	10,747,920	98,069
Earthquakes	3	12	326	341	18,706,516	164,573
Floods	7	62	656	662	2,876,154	30,413
Storms	30	216	2306	3435	13,683,836	119,509
Volcanoes	1	6	12	12	2,224,495	185,375
Wildfires	6	9	436	701	2,693,338	23,053

Table 1.2: Size deciles of affected firm-disasters.

This table reports 10 size deciles of our final sample of firm-disasters. The final sample of firm-disasters should satisfy the following two restrictions: first, we require that the firm' stock price 61 days preceding the disaster event date is greater than three dollars; second, we require that the firm has a minimum of 120 non-missing stock return data between 360 and 61 days preceding the disaster event date. The second restriction helps guarantee that we have reasonable four-factor model betas and alpha estimates to calculate the four-factor model expected return. These restrictions result in a final dataset of 5,151 firm-disasters. The breakpoints for the size (market capitalization, i.e., price times shares outstanding) are determined at the same time as the disaster event date using all New York Stock Exchange (NYSE) stocks on CRSP (The Center for Research in Security Prices). We use the firm's market capitalization 61 days preceding the natural disaster event date. And we categorize the firm-disasters into 10 deciles.

NYSE Decile	Number of affected firm-disasters
Decile 1	1841
Decile 2	964
Decile 3	574
Decile 4	412
Decile 5	357
Decile 6	267
Decile 7	217
Decile 8	225
Decile 9	131
Decile 10	163
Total	5151

Table 1.3: Summary statistics of affected firm-disasters.

This table reports summary statistics of some firm characteristics that we use in our regression analysis, presented in Table 1.8. We require that firm-disasters affected by our disaster sample satisfy the following four restrictions. First, we require that the firm' stock price 61 days preceding the disaster event date is greater than three dollars. Second, we require that the firm has a minimum of 120 non-missing stock return data between 360 and 61 days preceding the disaster event date. The second restriction helps guarantee that we have reasonable four-factor model alpha and beta estimates to calculate the four-factor model expected return. Third, in Table 1.8, we implement a regression framework to analyze firms' day 1 to day 30 abnormal return. Hence we require firms to have non-missing abnormal return data for each day in the event window. Fourth, we require that the firm-disaster has non-missing data for all chosen explanatory firm characteristics. These restrictions result in a final sample of 4868 firm-disasters. Variables are defined in Appendix A.

	N	Standard Deviation	Mean	25 Percentile	50 Percentile	75 Percentile
Return on assets	4868	0.200	0.002	0.003	0.044	0.085
Profitability	4868	0.188	0.095	0.064	0.122	0.185
Net income	4868	0.205	0.001	0.000	0.044	0.086
Research and development expense	4868	0.103	0.052	0.000	0.000	0.065
Capital expenditure	4868	0.070	0.067	0.022	0.044	0.083
Log(market capitalization)	4868	1.780	12.335	11.029	12.202	13.477
Tangibility	4868	0.225	0.276	0.096	0.212	0.397
Book-to-market	4868	0.580	0.601	0.281	0.494	0.800
Log(sales)	4868	1.962	5.211	3.984	5.194	6.479
Dividend payout ratio	4868	0.462	0.628	0.138	1.000	1.000
Book leverage	4868	0.197	0.203	0.022	0.166	0.319
Last 12 months' return	4868	0.700	0.262	-0.150	0.117	0.469

Table 1.4: Event study of the full sample.

This table reports the results of our event study that we implement to study natural disasters' effect on shareholder wealth. We use standard event study methodology and report four-factor-model abnormal return and CAPM abnormal return (all expressed in percentage) for various event windows using three weighting methods, including the Value-weighted method, the Square-root-of-value-weighted method and the Equal-weighted method. The reported return for each window is the cumulative return. T-statistics are reported in parentheses. The methodology to calculate cumulative return and corresponding t-statistic is outlined in Appendix B.

Event Window (in days)	Four-factor-model abnormal return	T-statistic	CAPM abnormal return	(T-statistic)
Value-weighted method				
-15 to -2	-1.61	(-2.15)	-1.71	(-1.55)
-1 to 1	-0.22	(-0.78)	-0.04	(-0.08)
-2 to 2	-0.66	(-1.40)	-0.11	(-0.18)
2 to 15	-1.58	(-2.12)	-2.10	(-2.05)
2 to 30	-3.87	(-3.53)	-4.39	(-2.99)
1 to 90	-5.00	(-3.22)	-6.41	(-2.95)
1 to 120	-6.10	(-2.78)	-10.28	(-3.35)
1 to 360	-1.39	(-0.37)	-15.92	(-2.99)
Square-root-of-value-weighted method				
-15 to -2	-1.56	(-3.65)	-1.46	(-2.35)
-1 to 1	-0.61	(-3.09)	-0.35	(-1.22)
-2 to 2	-0.76	(-2.87)	-0.44	(-1.17)
2 to 15	-1.27	(-2.86)	-1.59	(-2.63)
2 to 30	-2.95	(-4.61)	-3.74	(-4.30)
1 to 90	-4.53	(-4.92)	-5.55	(-4.25)
1 to 120	-6.94	(-5.34)	-7.92	(-4.29)
1 to 360	-10.58	(-4.70)	-16.78	(-5.24)
Equal-weighted method				
-15 to -2	-1.06	(-4.41)	-0.78	(-2.30)
-1 to 1	-0.44	(-3.44)	-0.29	(-1.55)
-2 to 2	-0.42	(-2.74)	-0.32	(-1.32)
2 to 15	-0.97	(-3.29)	-1.03	(-2.46)
2 to 30	-2.38	(-5.49)	-3.09	(-5.15)
1 to 90	-3.57	(-5.89)	-4.16	(-4.74)
1 to 120	-5.37	(-6.26)	-3.97	(-3.20)
1 to 360	-11.91	(-8.01)	-12.53	(-5.83)

Table 1.5: Summary statistics of changes in firm characteristics.
This table reports summary statistics of changes in firm characteristics, including firm performance and policies. We calculate all available changes of these variables for our final sample of 4868 firm-disasters in Table 1.3. The change in each firm characteristic is calculated as the most recent available firm characteristic post- disaster minus the last available firm characteristic pre- disaster. We adjust the variable pre- and post- disaster by both industry and firm size. Variables are defined in Appendix A. For the change variable, * indicates that the mean or median is statistically significantly different from zero at 5% significance level.

	N	Standard Deviation	Mean	25 Percentile	50 Percentile	75 Percentile
Change in						
Return on assets	4820	0.166	-0.006*	-0.033	0.001	0.028
Profitability	4815	0.120	-0.003	-0.033	0.002	0.032
Net income	4820	0.175	-0.007*	-0.037	0.000*	0.029
Research and development expense	4825	0.052	0.000	-0.001	0.000	0.001
Capital expenditure	4798	0.051	-0.003*	-0.015	-0.001*	0.013
Tangibility	4825	0.060	-0.004*	-0.026	-0.005*	0.018
Book-to-market	4820	0.515	0.000	-0.132	0.004	0.146
Log (sales)	4811	0.411	0.020*	-0.132	-0.006	0.139
Dividend payout ratio	4815	0.434	0.010	-0.033	0.000*	0.072
Book leverage	4825	0.106	0.004*	-0.035	-0.002*	0.029

Table 1.6: Event study of subsamples.
This table reports the results of our event study for subsamples on the basis of disaster damage amount and the firm's geographical dispersion. In Panel A, we first sort our final disaster sample, a total of 47 disasters, into two groups based on the total damage with Group 1 as the low-disaster-damage group and Group 2 as the high-disaster-damage group. Then we assign firms into high and low disaster firm group based on whether the firm-disaster year is affected by a high or low damage disaster. In Panel B, we first select firms that we have an available geographical dispersion rank, with Group 1 as the low-geographical-dispersion group and Group 2 as the high-geographical-dispersion group. The geographical-dispersion-measure is the number of unique state counts mentioned in firms' 10-k. In our sample, 2344 firms out of 2844 firms have this measure. The reported return for each window is the cumulative equal-weighted four-factor-model abnormal return (all expressed in percentage). T-statistics are reported in parentheses. The methodology to calculate cumulative return and corresponding t-statistic is outlined in Appendix B. In the final rows, we report p-values for tests of the null hypothesis of equality of mean and median across the two groups using an F-test and Kruskal and Wallis χ^2 test, respectively.

Panel A

			Event	period	(in days)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-15 to -2	-1 to 1	-2 to 2	2 to 15	2 to 30	1 to 90	1 to 120	1 to 360
All	-1.06	-0.44	-0.42	-0.97	-2.38	-3.57	-5.37	-11.91
	(-4.41)	(-3.44)	(-2.74)	(-3.29)	(-5.49)	(-5.89)	(-6.26)	(-8.01)
Group 1	-0.68	-0.21	0.13	-0.10	-1.05	-1.55	-3.37	-10.17
	(-2.24)	(-0.17)	(0.60)	(-0.24)	(-1.67)	(-1.75)	(-2.69)	(-4.69)
Group 2	-1.36	-0.62	-0.87	-1.67	-3.45	-5.19	-6.98	-13.30
	(-3.09)	(-3.22)	(-3.32)	(-3.63)	(-5.21)	(-5.53)	(-5.25)	(-5.78)
F-test	0.0449	0.0374	$< .0001$	$< .0001$	$< .0001$	0.0252	0.0052	0.2213
χ^2 test	0.0298	0.296	0.0002	$< .0001$	0.0004	0.0455	0.0127	0.5759

Panel B

			Event	period	(in days)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-15 to -2	-1 to 1	-2 to 2	2 to 15	2 to 30	1 to 90	1 to 120	1 to 360
All	-1.00	-0.43	-0.37	-0.95	-2.39	-3.41	-5.24	-11.97
	(-4.10)	(-3.52)	(-2.56)	(-3.36)	(-5.88)	(-5.90)	(-6.41)	(-8.46)
Group 1	-1.03	-0.47	-0.33	-1.02	-2.74	-3.52	-5.43	-13.79
	(-2.97)	(-2.78)	(-1.65)	(-2.68)	(-4.98)	(-4.51)	(-4.92)	(-7.21)
Group 2	-0.98	-0.39	-0.40	-0.88	-2.08	-3.31	-5.06	-10.35
	(-3.51)	(-2.91)	(-2.28)	(-3.01)	(-4.94)	(-5.43)	(-5.88)	(-6.94)
F-test	0.1560	0.0341	0.0016	0.1179	0.2346	0.1419	0.0658	0.5191
χ^2 test	0.3265	0.3486	0.0669	0.2693	0.9572	0.2607	0.2004	0.3333

Table 1.7: Industry fixed-effect regressions describing firm performance and policies.
This table provides industry fixed-effect regressions that explore the relation between changes of individual firm level firm performance and policies from pre- to post- disasters and the negative effect associated with natural disasters. We use annual accounting data and the change in each firm characteristic is calculated as the most recent available firm characteristic post- disaster minus the last available firm characteristic pre- disaster. We adjust the variable pre- and post- disaster by both industry and firm size. For independent variables, we include the changes in log (total assets), a post-disaster abnormal return rank and a constant. To construct the post-disaster abnormal return rank variable, we sort our final firm-disaster sample, a total of 4868 firm-disasters, into five groups based on the post-disaster day 1 to day 30 window four-factor-model abnormal return. Then the post-disaster abnormal return rank variable is set to zero for the most negative return group and set to four for the most positive return group. The table reports t-statistics based on robust standard errors clustered at the industry level in parentheses. Variables are defined in Appendix A. Significance at the 1, 5, and 10% levels are signified with ***, **, and *, respectively.

	Change in ROA	Change in profitability	Change in net income	Change in research and development expense	Change in capital expenditure	Change in tangibility	Change in book leverage	Change in dividend payout ratio	Change in Zscore
Change in Log(total -assets)	0.179***	0.112***	0.187***	-0.0562**	-0.00112	-0.0360***	0.0425***	-0.108***	2.151**
	(5.25)	(3.84)	(5.58)	(-2.58)	(-0.21)	(-6.37)	(4.22)	(-3.75)	(2.08)
Post-disaster abnormal return rank	0.00417**	0.00326**	0.00437**	-0.000674	-0.00206***	-0.00147**	-0.00335*	-0.00213	0.351***
	(2.18)	(2.44)	(2.14)	(-1.19)	(-3.21)	(-2.11)	(-2.03)	(-0.47)	(5.48)
N	4820	4815	4820	4825	4798	4825	4825	4815	4612
Adjusted R^2	0.103	0.076	0.100	0.104	0.002	0.044	0.022	0.015	0.022

Table 1.8: Industry and year fixed-effect regressions describing individual firm abnormal return following disasters.
This table provides results for our regression analysis that explore the relation between individual firm level abnormal return and the changes of firm characteristics from pre- to post- disasters. We use annual accounting data and the change in each firm characteristic is calculated as the most recent available firm characteristic post- disaster minus the last available firm characteristic pre- disaster. We adjust the variable pre- and post- disaster by both industry and firm size. For independent variables, we include the changes in tangibility, profitability, dividend payout ratio, book leverage, log (market capitalization), and the levels of book-to-market, last 12 months return, the disaster rank, day -1 to day 1 four-factor-model abnormal return and a constant. To construct the disaster rank variable, we first sort our final disaster sample, a total of 47 disasters, into two groups based on the total damage with Group 1 as the low-disaster-damage group and Group 2 as the high-disaster-damage group. Then we assign firms into high and low disaster firm group based on whether the firm-disaster year is affected by a high or low damage disaster. The disaster rank variable is equal to zero if the firm belongs to Group 1, and one if the firm belongs to Group 2. The table reports t-statistics based on robust standard errors clustered at the industry level in parentheses. Variables are defined in Appendix A. Significance at the 1, 5, and 10% levels are signified with ***, **, and *, respectively.

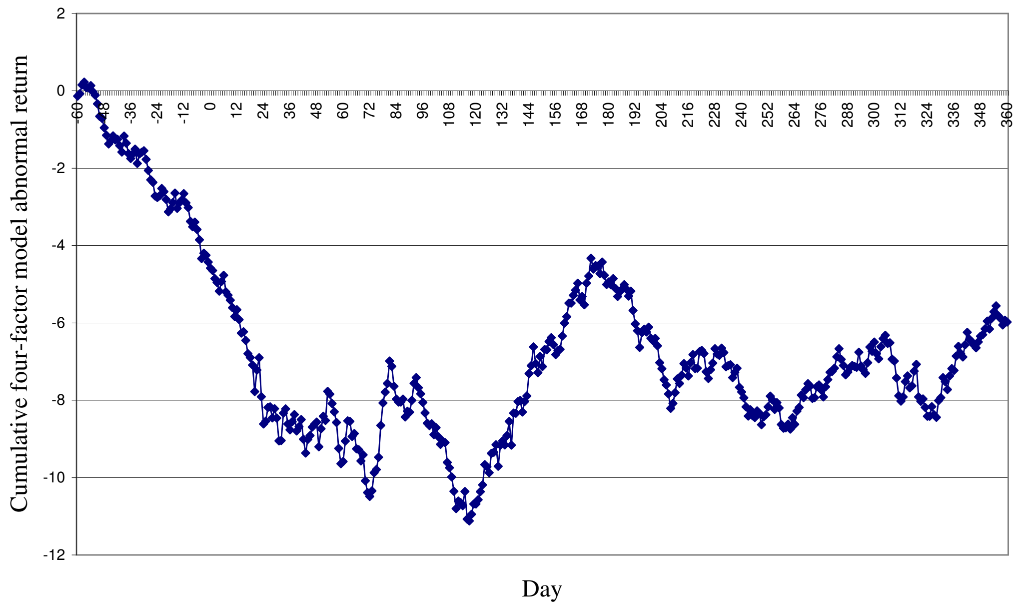
	Day 1 to day 30 Four-factor-model abnormal return	Day 1 to day 30 CAPM abnormal return
Change in tangibility	-7.108	-7.377
	(-1.14)	(-1.24)
Change in profitability	8.329***	7.501***
	(3.71)	(3.51)
Change in dividend payout ratio	-0.481	-0.254
	(-0.84)	(-0.54)
Change in book leverage	-5.547*	-5.468*
	(-1.79)	(-1.96)
Log (market capitalization)	0.0743	0.120
	(0.47)	(0.72)
Book-to-market	3.126***	3.356***
	(5.95)	(6.47)
Last 12 months' return	-5.227***	-5.291***
	(-9.20)	(-9.56)
Disaster indicator	-3.143**	-4.862***
	(-2.59)	(-4.16)
Day -1 to day 1	-0.0784*	-0.0566
four-factor-model abnormal return	(-1.96)	(-1.51)
N	4815	4815
Adjusted R^2	0.077	0.098

Table 1.9: Industry and year fixed-effect regressions describing individual firm abnormal return following disasters.

This table provides industry and year fixed-effect regressions that explore the cross-sectional variation of firm-level abnormal returns following natural disasters. The dependent variable is the firm's day 1 to day 30 cumulative abnormal return. We calculate cumulative abnormal return for the event windows for each individual firm and require firms to have non-missing abnormal return data for each day in the event window. Then we require that the firm-disaster has non-missing data for all chosen explanatory firm characteristics. These restrictions result in a final sample of 4868 firm-disasters for the day 1 to day 30 window. We implement industry and year fixed-effect regression for each type of abnormal return and report the coefficients on each chosen firm characteristic. We adjust all independent variables by both industry and firm size except the disaster indicator, log (market capitalization), last 12 months' return and day -1 to day 1 four-factor-model abnormal return. We first sort all firms into 30 industries and for each industry we sort the firms into two groups based on the firm's market capitalization 61 day before the disaster event date. Then we subtract the median of each group from each variable for the adjustment. Variables are defined in Appendix A. The methodology to calculate cumulative return and corresponding t-statistic is outlined in Appendix B. The table reports t-statistics based on robust standard errors clustered at the industry level in parentheses. Significance at the 1, 5, and 10% levels are signified with ***, **, and *, respectively.

	Day 1 to day 30 Four-factor-model abnormal return	Day 1 to day 30 CAPM abnormal return
Log (market capitalization)	0.0510	0.100
	(0.31)	(0.59)
Tangibility	-0.673	-0.833
	(-0.31)	(-0.41)
Profitability	2.310	2.327
	(1.46)	(1.30)
Book-to-market	3.110***	3.371***
	(5.35)	(5.85)
Dividend payout ratio	-0.289	-0.260
	(-0.44)	(-0.41)
Last 12 months' return	-4.846***	-4.952***
	(-8.96)	(-9.59)
Book leverage	0.465	0.248
	(0.23)	(0.13)
Disaster indicator	-3.260**	-4.942***
	(-2.41)	(-3.83)
Day -1 to day 1	-0.0611	-0.0422
four-factor-model abnormal return	(-1.56)	(-1.15)
N	4868	4868
Adjusted R^2	0.070	0.092

Panel A



Panel B

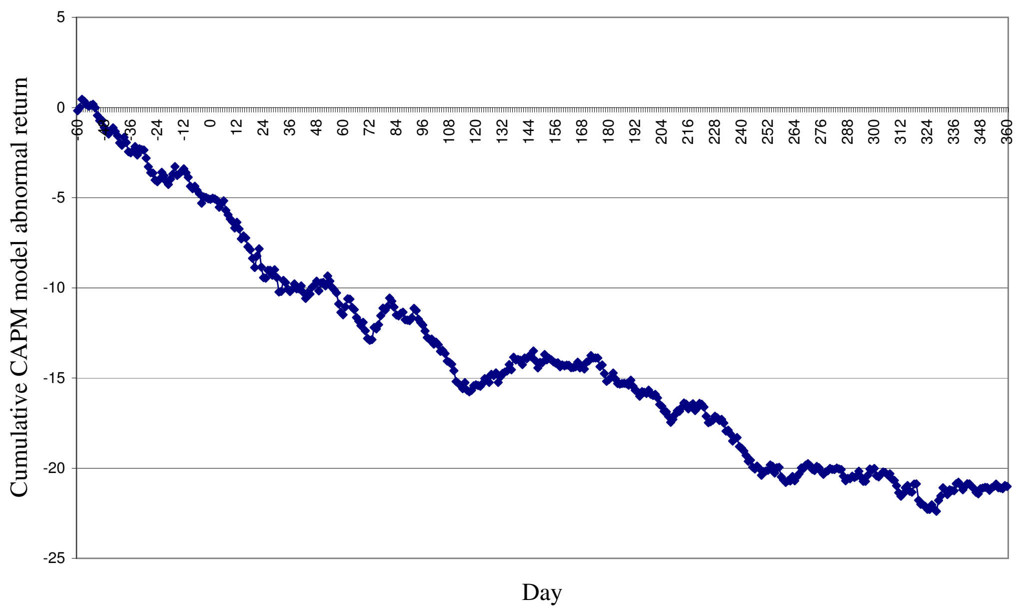
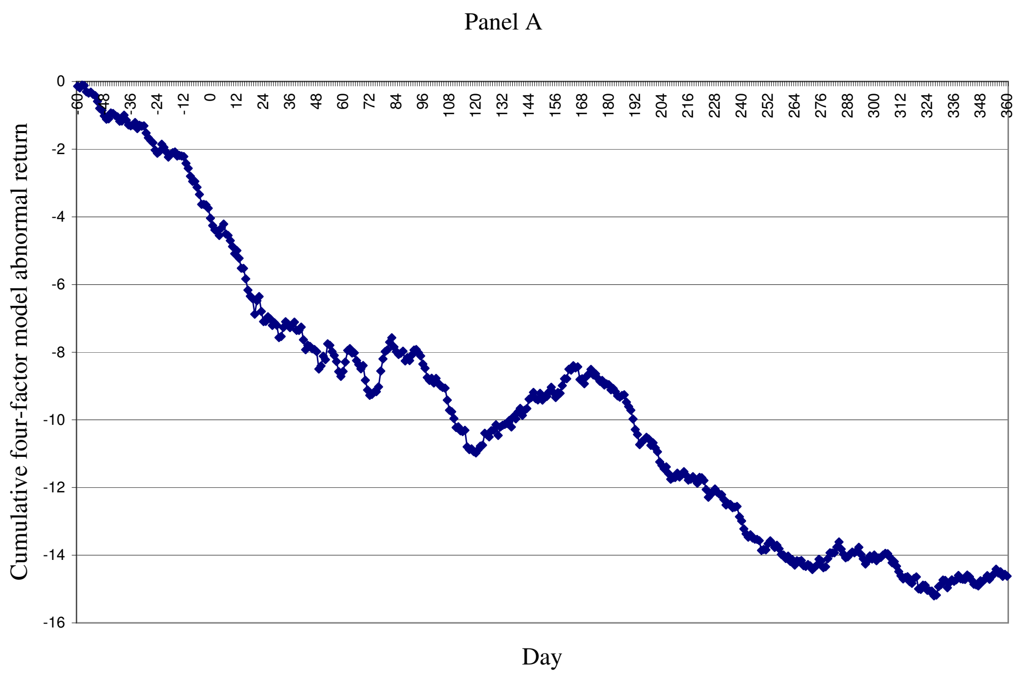


Figure 1.1: Cumulative value-weighted abnormal return.
These figures show the cumulative value-weighted abnormal return using both the four-factor-model and the CAPM model to calculate expected return. For each model, we show the cumulative value-weighted abnormal return from 60 days before to 360 days after the natural disaster event date. Variables are defined in Appendix A. The methodology to calculate cumulative return is outlined in Appendix B.



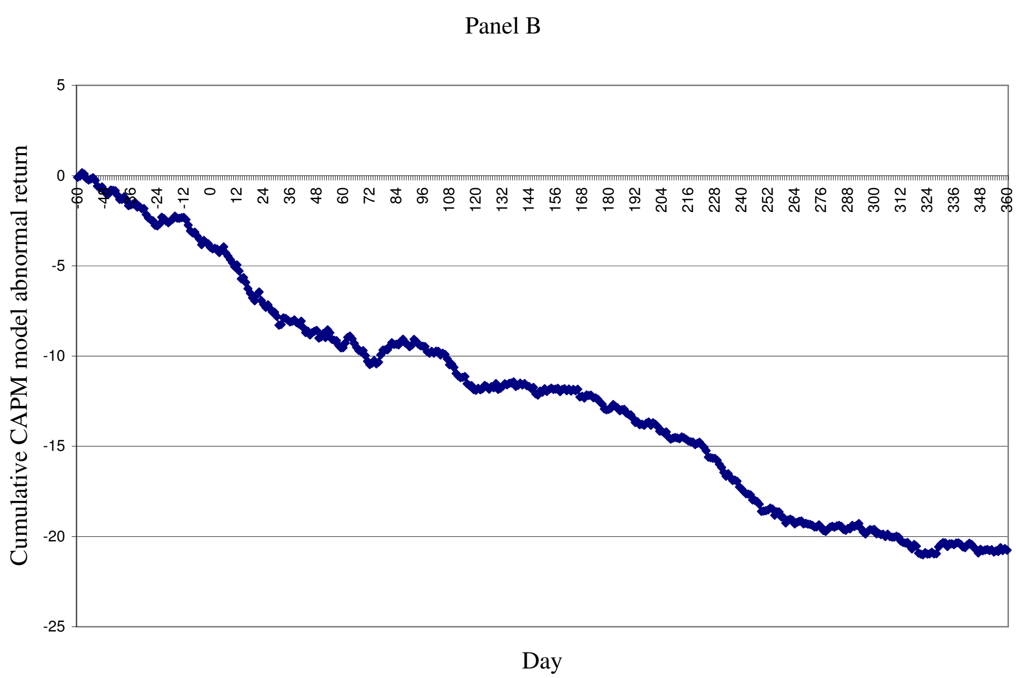
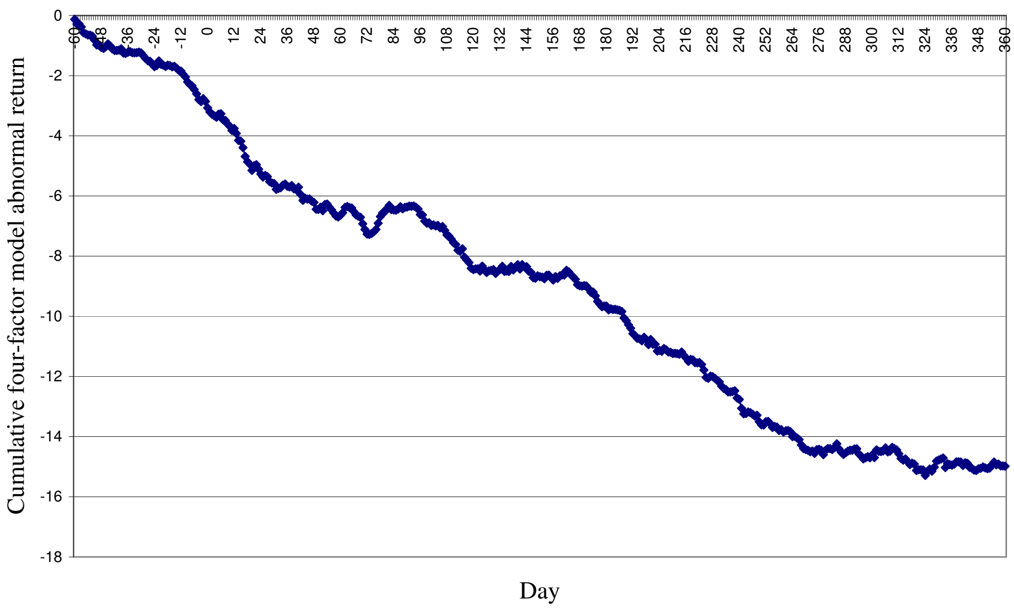


Figure 1.2: Cumulative square-root-of-value-weighted abnormal return.
These figures show the cumulative square-root-of-value-weighted abnormal return using both the four-factor-model (Panel A) and the CAPM model (Panel B) to calculate expected return. For each model, we show the cumulative square-root-of-value-weighted abnormal return from 60 days before to 360 days after the natural disaster event date. Variables are defined in Appendix A. The methodology to calculate cumulative return is outlined in Appendix B.

Panel A



Panel B

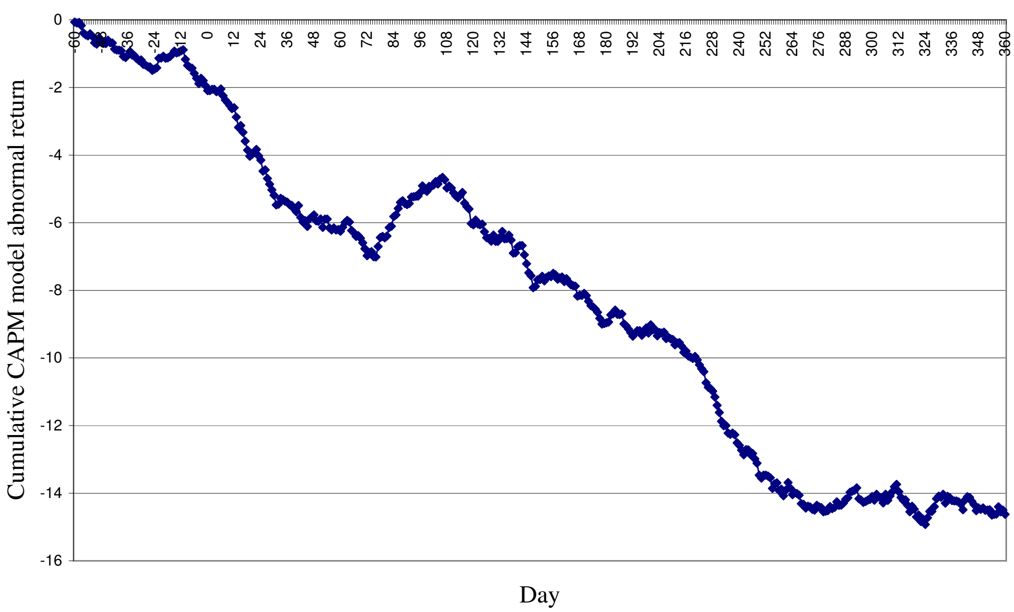


Figure 1.3: Cumulative equal-weighted abnormal return.
These figures show the cumulative equal-weighted abnormal return using both the four-factor-model (Panel A) and the CAPM model (Panel B) to calculate expected return. For each model, we show the cumulative equal-weighted abnormal return from 60 days before to 360 days after the natural disaster event date. Variables are defined in Appendix A. The methodology to calculate cumulative return is outlined in Appendix B.

CHAPTER 2

INSIDE DEBT AND DEBT INCENTIVES

We study the rationale for firms' use of inside debt by exploiting the relation between firms' default risk and inside debt (pension and deferred compensation). The classical principal-agent theory indicates that the agency costs of debt are higher when the firm's debt is riskier. We test whether firms that are likely to face more severe agency problems of debt provide more debt incentives. We provide evidence that less distressed firms use more inside debt. The 2008 financial crisis significantly increases firms' default risk. This exogenous shift in firms' default risk offers an opportunity to provide evidence on the potential causal relation between default risk and inside debt. Based on a difference-in-differences approach, we find that firms with increased default risk during the crisis period increase their debt incentives significantly less than those firms with decreased default risk. Overall, we find little evidence consistent with the conventional wisdom that more distressed firms use more debt incentives to alleviate the agency costs of debt. Anecdotal evidence suggests that personal tax concerns appear to be an important determinant for inside debt usage.

2.1 Introduction

Inside debt refers to the part of executive compensation with payoffs similar to those of debt securities held by external investors. It usually includes both pension and deferred compensation. As inside debt represents unsecured and unfunded liabilities of the firm, it exposes chief executive officers (CEOs) to default risk. In this sense, Jensen and Meckling (1976) argue that inside debt aligns CEO incentives with those of external debt holders and induces less risky firm policies. Sundaram and Yermack (2007) hand collect pension data for 237 large capitalization firms. They show that CEOs receive significant amounts of pension and interpret this result as evidence that CEO compensation exhibits a balance between debt incentives and equity incentives. In 2007, U.S. Securities and Exchange Commission (SEC) disclosure reform required firms to disclose CEO's inside debt positions. This provides publicly-available inside debt data from calendar year 2007 onward. Wei and Yermack (2011) show that for firms with sizeable inside debt, bond prices rise and equity prices fall in response to the disclosure in 2007. They interpret their results as market reaction evidence that inside debt is used to align managerial incentives with those of outside creditors. Cassell, Huang, Manuel, and Stuart (2012) further show that inside debt is correlated with less risky investment and financial policies. Specifically, they show that CEO inside debt holdings are positively correlated with asset liquidity and negatively correlated with research and development expenditures and financial leverage. They interpret their results as evidence that large inside debt holdings induce lower levels of risk-seeking behavior.

Overall the empirical evidence is consistent with the theoretical argument that inside debt helps alleviate agency costs of debt and align managerial incentives with those of debt holders. However, the existing evidence has largely been indirect tests of the agency theory. The agency problem between the manager and debt holders (project selection problems such as, debt overhang or asset substitution) indicates that for firms with risky debt, managers may forgo positive net-present-value (NPV) projects or invest in negative NPV projects at the expense of debt holders. Firms with riskier debt are likely to face more severe agency problems of debt. Thus, firms with riskier debt are precisely those that need to provide more debt incentives all else equal. Edmans and Liu (2011) formalize this insight in an equilibrium model. In this paper, we provide direct evidence by empirically exploring this largely ignored insight. We accomplish this task by researching the causal effect of firms' default risk, which proxies for the riskiness of debt, on inside debt.

We start by testing whether inside debt is correlated with firms' default risk. Following Jensen and Meckling (1976), we measure inside debt by the ratio of debt pay-performance sensitivity to equity pay-performance sensitivity. The intuition is that this measure weighs the relative benefits from aligning incentives related to project selection and effort problems. Edmans and Liu (2011) show that when firms are financially healthy, they usually have an equity bias (i.e., debt to equity pay-performance sensitivity ratio smaller than one) to induce effort. When bankruptcy is likely, firms tend to have a debt bias (i.e., debt to equity pay-performance sensitivity ratio greater than one) to manage the project selection problem. Empirically, the debt to equity pay-performance sensitivity ratio is proxied by CEO to firm leverage ratio, which is defined as the CEO inside debt to firm debt ratio scaled by the inside equity to firm equity ratio. Contrary to the prediction of agency theory, we show that there is a negative correlation between various default risk measures and CEO to firm leverage ratio. Next we show that this relation is robust to controlling for other factors (firm size, leverage, research and development expense, and CEO age) that have been shown to be correlated with inside debt. We interpret this as the first piece of evidence indicating that incentive alignment does not appear to be a first-order determinant of inside debt usage.

The negative correlation between firms' default risk and inside debt is plagued by the universal endogeneity issue in empirical corporate finance both default risk and inside debt are simultaneously determined in equilibrium and the causation can be in either direction. For example, Sundaram and Yermack (2007) show that firms with higher CEO pension to equity ratios scaled by firm leverage have lower default risk. They interpret this result as evidence that pension helps reduce the riskiness of the firm's external debt. We show that for firms that need to provide more debt incentives (i.e., firms with higher default risk), CEO to firm leverage ratios are lower. We interpret this result as evidence contradicting the empirical prediction of an equilibrium model where inside debt is used to provide debt incentives. To shed light on the causal relation between default risk and inside debt, we use the 2008 financial crisis as an exogenous shift in firms' default risk.

The 2008 financial crisis originates from the 2007 credit crisis and leads to an increase in average firm-level default risk. Specifically, one of our default risk measures, Altman's Z-score, decreases from 3.97 in 2007 to 3.15 in 2008, a 26% decrease. At the same time, the financial crisis is not likely to be associated with significant changes in firm or CEO fundamentals related to debt incentive alignment other than default risk (e.g., firm size or CEO age). This helps mitigate the confounding effects that might bias our experiment. To the extent that the shift in default risk is exogenous, a difference-in-differences approach can potentially identify the causal relation between default risk and inside debt. We show that during the crisis period firms with increased default risk increase their inside debt significantly less than those firms with decreased default risk. This result is robust to various proxies for firms' default risk. Further, this main result remains after controlling for cross-sectional heterogeneity. The results suggest that default risk has a causal effect on inside debt. However, this negative relation indicates that when firms become more distressed, they do not increase their debt incentives in a manner consistent with the theoretical prediction. Overall, we find little evidence consistent with the conventional wisdom that more distressed firms use more debt incentives to alleviate the agency costs of debt.

It is puzzling why less distressed firms use more inside debt. A possible reason is related to personal tax concerns. Shivdasani and Stefanescu (2010) show that tax benefits play an important role in all-employee pension usage. Specifically, the firm-level tax shields from pension contributions are about a third of those from interest payments. However, the firm-level tax benefit generated from CEO pension and deferred compensation is relatively small compared to the all-employee tax benefit. We provide some anecdotal evidence that personal tax concerns appear to be an important determinant of CEO inside debt. For example, Alberto-Culver's proxy statement notes that pension and deferred compensation help executives "accumulate funds on a tax favored basis." Specifically, CEOs can defer the tax payments on pension and deferred compensation until withdrawal. This deferral feature benefits the CEO in two dimensions: 1) if the CEO's tax rate does not change in the future, it lowers the present value of his/her tax on the deferred amount; 2) if the CEO's tax rate is lower in the future, then the tax on the deferred amount is lower in addition to the time value of money mentioned in part one.

In a nutshell, the rationale of debt incentives is straightforward and appears to be supported by prior empirical evidence. However, there is no formal analysis that directly tests whether firms that need to provide more debt incentives use more inside debt. We fill this gap by linking firms' default risk to inside debt. We show that less distressed firms use more inside debt. This result contradicts the traditional wisdom that firms with risky debt are precisely those who will benefit from debt incentive alignment. Although it is beyond this paper's scope to provide first-order determinants of inside debt holdings, we conjecture that personal tax concerns may be an important determinant.

2.2 Data and summary statistics

2.2.1 Summary statistics for the full sample

We start by collecting different components of CEO compensation (including salary, bonus, stock options, stock holdings, pension, and deferred compensation) from the ExecuComp Database. Because ExecuComp starts to report complete information on inside debt holdings (including pension and deferred compensation) from 2006 onward, we restrict our sample period to fiscal years 2006 through 2010. In addition, we remove firms in the financial (firms with SIC code between 6000 and 6999) and utility (firms with SIC code between 4900 and 4999) industries. As part of our empirical analysis is to evaluate how firms react to the 2008 financial crisis, we further restrict the sample to those firms with non-missing inside debt data for both fiscal years 2007 and 2008. Finally, we require all firm-year observations have non-missing data for our main default risk measure (Z-score) and key determinants of inside debt, such as CEO age, firm size, firm leverage, and research and development expense. This results in a final sample of 4,608 firm-year observations.

To measure inside debt usage, we focus on two variables: total dollar value of inside debt (CEO inside debt holdings), total dollar value of inside debt scaled by cash compensation (CEO inside debt to cash compensation ratio). To measure debt incentives, we focus on two variables: CEO inside debt to firm debt ratio scaled by the inside equity to firm equity ratio (CEO to firm leverage ratio) and CEO relative incentive ratio defined in Wei and Yermack (2011). As mentioned in the introduction, we use CEO to firm leverage ratio as our main debt incentive measure. Under the assumption that CEO inside debt and inside equity are a fixed proportion of firm debt and firm equity, CEO to firm leverage ratio is the ratio of debt pay-performance sensitivity to equity pay-performance sensitivity. Edmans and Liu (2011) note that intuitively CEO to firm leverage ratio < 1 indicates an equity incentive bias and CEO to firm leverage ratio > 1 indicates a debt incentive bias. All four variables are winsorized at the upper 1 percentile to avoid data errors and outliers. We do not winsorize these four variables at the lower 1 percentile as they are truncated at zero.

In Table 2.1, we present summary statistics for our full sample including 4,608 firm-year observations. We start by presenting different components of debt incentives and equity incentives in CEO compensation. Inside debt refers to pension and differed compensation. These types of compensation are considered “debt like” as their payoffs are similar to those of the debt securities held by external investors and they represent unsecured and unfunded liabilities of the firm. The average total outstanding CEO pension is \$2.8 million and it is worthwhile noting that the median is zero. The average total outstanding CEO deferred compensation is \$2.3 million. We report the average annual CEO cash compensation (salary plus bonus), which is \$1 million, for easy interpretation of the magnitude of inside debt. The total outstanding equity incentives appear larger in magnitude compared to debt incentives. The average total outstanding CEO options are \$14 million. And the average total outstanding CEO stock holdings are \$41 million. This set of summary statistics confirms the result in Sundaram and Yermack (2007) that CEO compensation exhibits a balance between debt and equity incentives, and inside debt is an important part of compensation. Next, we present various measures of inside debt. The dollar value of total outstanding inside debt (CEO Inside debt holdings) is \$5.2 million on average. Note that this variable is highly right skewed. There are 1,420 firm-year observations that have zero CEO Inside debt holdings, while 290 firms have zero inside debt for the full sample period. The dollar value of total outstanding inside debt scaled by the current year’s cash compensation (CEO Inside debt to cash compensation ratio) is 4.6 on average. This indicates that the total dollar value of inside debt is approximately 4.6 times annual cash compensation. Further, CEO inside debt to firm debt ratio scaled by the inside equity to firm equity ratio (CEO to firm leverage ratio) is on average 2.97. CEO relative incentive ratio is on average 2.23.

The classical agency theory indicates that debt incentives are only relevant in firms with risky debt. Further, firms with riskier debt are more likely to face more severe project selection problems. Hence we need a proxy for the riskiness of the firm's debt. In this chapter, we use four default risk measures to proxy the riskiness of debt. The first measure is Z-score introduced in Altman (1968). Altman's Z-score is a comprehensive measure for the financial health of a company. It is a weighted average of various accounting and market data typically associated with a firm's financial health. Higher Z-scores indicate that the firm is less likely to become distressed. As in Altman (1968), a Z-score of less than 1.81 signifies that bankruptcy is likely. A Z-score between 2.99 and 1.81 is a "gray area" that signifies distress, but not necessarily severe enough to warrant bankruptcy. The average Z-score in our sample is 3.59. ExecuComp database includes only firms in the S&P 1500 index, so it is not surprising that our sample firms are in relatively good financial health. Our second default risk measure is Ohlson's O-score developed in Ohlson (1980). This measure is a weighted average of several accounting variables related to the financial health of the firm. Using the estimated coefficients from the probit regression in Ohlson (1980) we calculate O-score for each firm-year in our sample. Lower O-scores indicate that the firm is less likely to become distressed. Further, we include the nave distance-to-default (DD) measure and associated expected probability of default (EDF) developed in Bharath and Shumway (2008). The nave distance-to-default measures the number of standard deviations that the firm's asset value can fall before the firm defaults. The expected probability of default translates the distance-to-default to a probability measure of firm default using a standard normal distribution.

The final rows of Table 2.1 present various firm and CEO characteristics that are shown to be correlated with inside debt by prior literature. We define firm size as log of total assets and firm leverage as the ratio of book value of debt to market value of equity. CEO leverage is defined as the ratio of CEO inside debt to inside equity and is included for comparison purposes. R&D is defined as research and development expense scaled by book value of total assets. The firms in our sample have an average firm size of 7.72 (the total book assets are $7 billion on average), average firm leverage of 0.5, average CEO leverage of 0.27, average R&D of 0.02, and average CEO age of 55 years old.

2.2.2 Summary statistics of subsamples

General principle-agent models with respect to the agency problem between the manager and debt holders (e.g., Jensen and Meckling (1976)) suggest that optimal project choice results in equal proportion of debt and equity incentives (i.e., CEO to firm leverage ratio equals one). However, the summary statistics indicate that CEO to firm leverage ratio varies in a much larger range than the theory's prediction. Edmans and Liu (2011) indicate that an equity bias (CEO to firm leverage ratio smaller than one) is usually preferred to induce effort and a debt bias (CEO to firm leverage ratio bigger than one) is preferred when default risk is higher. We now further investigate the relation between inside debt and firm characteristics.

Inside debt (CEO to firm leverage ratio) and firms' default risk (Z-score) are positively correlated. This indicates that less distressed firms use more inside debt. Further, CEO to firm leverage ratio is highly skewed. Approximately one quarter of our firm-year observations have zero CEO to firm leverage ratio and one quarter of our firm-year observations have zero CEO to firm leverage ratio > 1. To analyze this matter further, we provide summary statistics for firms with a large debt bias, i.e., firms with CEO to firm leverage ratio > 5 and firms with CEO to firm leverage ratio ≤ 5. We compare firm and CEO characteristics across the two groups in Table 2.2. The high CEO to firm leverage ratio group has significantly lower leverage, more research and development expense, and older CEO on average. Most importantly for our purposes, the high CEO to firm leverage ratio group is significantly less likely to become financially distressed based on all four default risk measures. These results are confirmed in a probit regression framework in Table 2.3. Similar to the univariate evidence, the high CEO to firm leverage ratio group tends to have more assets, older CEOs and lower leverage, and most importantly has less default risk.

2.3 Hypothesis development

The traditional wisdom from Jensen and Meckling (1976) indicates that CEO compensation should include not only equity incentives that alleviate the agency problem between managers and equity holders, but also debt incentives that alleviate the agency problem between the manager and debt holders. They indicate that CEO to firm leverage ratio should be one when considering the project selection problem. Edmans and Liu (2011) take one step further and indicate that CEO to firm leverage ratio of one may not always be optimal. An equity bias or a debt bias might be needed to address different concerns. The empirical research has confirmed that in real contracts CEO compensation exhibits a balance between debt and equity incentives and inside debt is correlated with less risky firm policies. Further there is market reaction evidence that disclosure of sizable inside debt positions leads to increased debt prices and decreased equity prices. At the very least, CEO to firm leverage ratio varies from 0 to 104. This motivates us to further analyze the characteristics of the firms with a debt or an equity bias, and whether it is consistent with the traditional wisdom.

In this chapter, we research whether firms that are likely to face more severe agency problems of debt actually provide more debt incentives. The project selection problem suggests that debt incentives are only relevant in firms with risky debt. We follow this insight and note that inside debt should be driven by the riskiness of debt according to the agency problem between the manager and debt holders. Further, in the equilibrium model of Edmans and Liu (2011) in which inside debt is used to provide debt incentives, an empirical prediction is that firms that are more likely to go bankrupt will provide more debt incentives. We use the firms' default risk to proxy for the riskiness of debt and to test its relation with various measures of inside debt. This leads to our testable hypotheses.

Hypothesis 1: According to the general principal-agent theory, inside debt should be positively correlated with default risk after controlling for other variables that affect inside debt.

Hypothesis 2: An exogenous increase in firms' default risk should lead to an increase in inside debt usage after controlling for confounding effects.

2.4 Empirical analysis

In this section, we present the empirical analysis testing the relation between inside debt and the riskiness of debt. We start with analyzing the cross-sectional variation in inside debt usage. We then use the 2008 financial crisis as an exogenous shift in firm default risk and shed light on the causal relation between default risk and inside debt.

2.4.1 Cross-sectional variation of inside debt

We analyze the cross-sectional variation of inside debt by conducting panel data regressions of our four variables describing inside debt, on control variables that include default risk, firm size, firm leverage, research and development expense, and CEO age using all 4,608 firm-year observations in our sample. We include dummy variables for each year to account for time-fixed effects and our standard errors are clustered by firm to account for firm-fixed effects.

For presentation purposes, we only report results using Z-score as the default risk measure. In Table 2.4, we confirm that firms with more inside debt are likely to be bigger in size, have less leverage, and have older CEOs, consistent with the prior literature. Refer to the coefficient on our default risk measure (Z-score), we see that less distressed firms are more likely to have higher inside debt holdings (CEO Inside debt to cash compensation ratio), and higher debt incentives (CEO to firm leverage ratio and CEO relative incentive ratio). This result suggests that firms that face more severe project selection problems and that could potentially benefit from higher debt incentives, do not use more inside debt or show a debt bias in CEO incentives.

2.4.2 Changes in inside debt around the 2008 financial crisis

Our strategy to identify the causal relation of default risk on inside debt relies on an exogenous change in default risk. We use the ongoing global financial crisis as an exogenous event (i.e., an event beyond the firm's control) which leads to an exogenous change in default risk. "The 2008 global financial crisis came to the forefront of the world in September 2008, with the failure and merging of a number of American financial companies." We review the timeline of the crisis to determine the crisis period. On 7 September 2008, two firms, Fannie Mae and Freddie Mac, were declared to be nationalized to ensure their financial stability. On 14 September 2008, Lehman Brothers filed for bankruptcy and Bank of America announced merger plans with Merrill Lynch. On 16 September 2008, the Federal Reserve bailed out American International Group (AIG) $85 billion. This is followed by more failed banks and the stock market crash in both the US and Europe. Given that the start of the crisis is in the fourth quarter of 2008, for simplicity we refer fiscal year 2008 as the crisis period. There is a relatively large increase in overall firm default risk measured by Z-score around fiscal year 2008 that resembles the timeline of the crisis. The financial crisis originated from the sub-prime mortgage crisis that raised concerns about the financial stability of the American and European banking industries. The American economy is built on credit with firms and consumers borrowing money from banks. The shock to the credit supply around the 2008 financial crisis led to an increase in default risk on average. This increase in default risk is exogenous to the extent that the other economic fundamentals (e.g., firm size, research and development expense and CEO age) that have been shown to be correlated with inside debt usage are largely left intact. We admit that our experiment still falls short of an ideal experiment because the crisis may also change other unobservable factors that affect the firm's need to provide debt incentives. Nevertheless, this change in firms' default risk offers an opportunity to provide evidence on the causal relation between default risk and inside debt. We apply this exogenous change in default risk to test our second hypothesis, i.e. whether the increase in default risk leads to an increase in inside debt. We use a difference-in-differences approach to analyze how CEOs' inside debt holdings and debt incentives have changed in response to the 2008 financial crisis.

An ideal experiment to establish a causal relation between default risk and inside debt is to identify control and treatment groups that are identical in all aspects except that the treatment group receives the treatment (i.e., change in default risk) while the control group does not. Despite the overall increase in default risk, we are able to split our full sample into the control group, 217 firms with decreased default risk from 2007 to 2008, and the treatment group, 816 firms with increased default risk from 2007 to 2008. We assume that the control group is not affected by the financial crisis in the sense that they do not become more distressed.

Before applying the difference-in-differences approach, we test the underlying assumptions in this approach. First, we test whether the control group is statistically identical to the treatment group both in inside debt usage, and in various dimensions that previous literature has identified as key determinants of inside debt. In Table 2.5 Panel A, we show various firm and CEO characteristics, and inside debt variables for both the control and treatment group in the pre-crisis period (i.e., fiscal year 2006 and 2007). With respect to the level of size, leverage, R&D, CEO age, CEO inside debt to cash compensation ratio, CEO to firm leverage ratio, and CEO relative incentive ratio, the mean of the control group is statistically identical to that of the treatment group at 1% significance level. Mean CEO inside debt holdings of the control group are statistically different from those of the treatment group at 10% significance level. Second, we do a parallel-trends test of various inside debt variables in the pre-crisis period. This helps guarantee that our results in the difference-in-differences test are not driven by trends in the pre-crisis period. As seen in Table 2.5 Panel B, the changes for all four inside debt variables in the pre-crisis period are statistically identical across the control and treatment groups. Finally in Table 2.6, our probit analysis also indicates that the firms in the treatment group are statistically identical to those in the control group, consistent with the univariate analysis.

After establishing the validity of the control and treatment groups, we apply the difference-in-differences approach to evaluate how the treatment group responds to the financial crisis compared to the control group. Firms with higher default risk should have greater needs to provide debt incentives, all else equal. Hence, the difference in the change of inside debt between the treatment and control groups should be positive. However, the empirical results suggest otherwise. In Table 2.7, we present the raw changes of each inside debt variable. With respect to CEO to firm leverage ratio, firms in the control group increase their CEO to firm leverage ratio by 2.39, while firms in the treatment group increase their CEO to firm leverage ratio by only 0.12. A significant test indicates that the treatment group increased their CEO to firm leverage ratio significantly less than the control group. The results for CEO inside debt holdings, CEO inside debt to cash compensation ratio, and CEO relative incentive ratio are similar, although the difference for CEO inside debt to cash compensation ratio is not statistically significant.

Last, we do the difference-in-differences test in a regression framework to take firm heterogeneity into consideration. After including an indicator for the crisis period and an indicator for the treatment group, the interaction term of the two indicators will capture the difference-in-differences estimate between the treatment and control group from the pre-crisis to crisis period. Refer to Table 2.8 column 3, crisis is significantly positive, indicating an increase of CEO to firm leverage ratio for the control group from the pre-crisis to the crisis period. It is also worth noting that the coefficient on treatment is statistically insignificant, i.e., the difference between the treatment and control group in the pre-crisis period is statistically identical. Most importantly, crisis*treatment is significantly negative when we research the changes in CEO to firm leverage ratio between the two groups around the crisis. This indicates that the incentive measure that weights the relative debt to equity incentives (CEO to firm leverage ratio) for the treatment group increases significantly less than that of the control group. Intuitively, the treatment group includes those firms that experience increased agency costs of debt in the crisis period and will benefit from debt incentive alignment. However, the incentives that the treatment group provides through inside debt increase significantly less than those of the control group.

In sum, the analyses in Table 2.7 and 2.8 do not generally support the conventional wisdom that more distressed firms use more debt incentives to alleviate the agency costs of debt. The mechanism indicates that inside debt helps align managerial incentives with those of debt holders. However, the empirical evidence of less distress firms using more debt incentives suggests that providing debt incentives does not appear to be first order determinant for inside debt usage. In the next section, we decompose the incentive measure and show that firms appear to let this ratio drift without actively managing them in the direction of providing incentives.

2.5 Decomposing the incentive measure

The two debt incentive measures contain a lot of moving parts. To understand which components contribute to the results in Section 2.4.2, we decompose the incentive measures into their underlying parts. First, we briefly describe the institutional details of both pension and deferred compensation using information from the firm's proxy statement. Second, we examine the real data in light of these institutional details. This exercise helps to uncover the contributing factors for the results presented in Section 2.4.2.

2.5.1 Institutional details for pension and deferred compensation

In our sample, 43% of the firm year observations have non-zero outstanding pension liabilities. CEO pension plans are typically defined benefit plans, in which CEOs will receive a pre-determined amount per year after leaving the company. The outstanding pension liability is usually calculated as the present value of a surviving annuity starting from the firm's designated retirement age. The annual amount for the annuity usually depends on both the CEO's recent cash compensation level (including both salary and bonus, usually the past three years) and the number of years served in the company. This amount is usually disclosed in the firm's proxy statement. The outstanding pension balance is usually available to CEOs either in a lump sum or an annuity. If CEOs leave the company before this age, there will typically be adjustment made to the outstanding pension balance. For example, usually the terms of the pension agreement will attach a penalty to the outstanding pension balance if the CEO departs from the company before the specified retirement age. Sundaram and Yermack (2007) among others introduce the formula that firms use to calculate the outstanding pension balance. They note that the annual amount is around 60% of the average pay received in their final years in office. In 2007 the SEC required all firms to disclose the present value of pension liabilities. The expected annual annuity payments are typically calculated by multiplying the annual annuity payment each year by the probability that CEO will be alive in that year. The expected annuity payments are then discounted at the firm's at a rate commensurate with the risk of the cash flows to arrive at a present value of pension liabilities. For example, Sundaram and Yermack (2007) use the rate on the firm's long-term debt.

Several factors may contribute to the year-over-year changes to pension liabilities. 1) The annuity payments will increase as CEO cash compensation and years served in the company increase. Usually changes to CEO cash compensation are positive, thus these two components typically result in increases in annuity payments. 2) Firms may change the discount rate used to value pension liabilities according to changes in the riskiness of the annuity cash flows. 3) CEOs may withdraw funds from their pension accounts. Withdrawals typically occur when CEOs retire or reach the defined retirement age. 4) From time to time, firms may restructure their pension plans resulting in withdrawals. 5) CEO turnover will result in the settlement of the old CEO's pension liabilities.

In our sample, approximately 10% of our sample firms have negative changes to pension liabilities from 2007 to 2008. For the control (treatment) group approximately 7% (11%) firms have negative changes to pension liabilities, 47% (43%) of the negative changes are due to CEO turnover.

In our sample, 33% of the CEOs have deferred compensation plans. Firms usually allow CEOs to defer part of their cash compensation (cash or cash bonus) or equity incentive compensation for a pre-determined period of time. Earlier withdrawal will result in a penalty. Usually firms will match the amount that CEO chooses to defer and there is a cap over the deferred amount. Deferred compensation is typically indexed to a mutual fund or a specific rate. In this sense, their market value fluctuates every year along with the overall market condition. Each year, there might be deferred compensation from early years that becomes available and can be withdrawn. This leads to the fact that deferred compensation can decrease significantly from year to year despite the fact that CEOs might accumulate new deferred compensation each year. In our data, the decrease in inside debt appears to be driven by the decrease in deferred compensation.

2.5.2 Decomposing the incentive measure around the crisis period

In this section, we decompose the incentive measure into its underlying components and try to unravel the mechanisms that drive the results around the crisis period using the institutional details in Section 2.5.1. As our incentive measures CEO to firm leverage ratio and CEO relative incentive ratio are correlated with coefficient of 0.99, we focus only on CEO to firm leverage ratio. CEO to firm leverage ratio can be interpreted as debt to equity pay performance sensitivity. Empirically, debt pay performance sensitivity is calculated as the ratio of inside debt value to face value of debt. Equity pay performance sensitivity is calculated as the ratio of inside equity value to market value of equity. Debt pay performance sensitivity is on average 0.03 and equity pay performance sensitivity is on average 0.02 in our full sample. This indicates that CEO wealth will increase by 0.03(0.02) dollars if he increases firm debt (equity) value by one dollar.

Refer to Table 2.9, we decompose the variable CEO to firm leverage ratio into its underlying components, and show the changes for each individual variable around the crisis period. Overall, CEO to firm leverage ratio increases by 2.67 (0.07) for the control (treatment) group. First, we investigate whether the difference in changes for the control and treatment group comes from the difference in changes for debt or equity pay performance sensitivity, or both. For the control (treatment) group, debt pay performance sensitivity changes by 0.0142 (-0.0043) and equity pay performance sensitivity changes by -0.0033 (-0.0028) on average. This results in an overall larger increase of CEO to firm leverage ratio for the control group than the treatment group. It appears that the main difference between the changes in CEO to firm leverage ratio for the control and treatment groups is attributed to the changes in debt pay performance sensitivity. Next, we investigate which underlying variables drive the changes in debt pay performance sensitivity. We tabulate both the inside debt and firm debt in Table 2.8 Panel B. For the control (treatment) group, inside debt changes by \$-0.05 (\$-0.69) million and firm debt changes by \$-59.3 (\$219.1) million. This indicates that the main difference between the control and treatment groups with respect to the debt pay performance sensitivity comes from both a numerator and denominator effect.

Inside debt (the numerator) decreases for the treatment group but increases for the control group. Firm debt (the denominator) increases more for the treatment group than for the control group. These two effects around the crisis period together contribute to the findings in Section 2.4.2. This exercise also helps to point out that firms do not appear to actively manage the incentive ratio. Based on results in Section 2.4 and 2.5, we conjecture that providing debt incentives may not be first order determinant for firms' use of inside debt.

2.6 Conclusion

The classical principal-agent theory indicates that CEO compensation should include both equity incentives to induce effort and debt incentives to alleviate the agency costs of debt. The empirical evidence has been mostly indirect tests of this insight. Prior research shows that CEO compensation exhibits a balance between debt incentives and equity incentives. Inside debt has been shown to be correlated with less risky firm policies. Further there is market reaction evidence that disclosure of sizable inside debt leads to an increase in debt value.

In this chapter, we directly test whether the observed contracts are consistent with the theoretical prediction. The classical principal-agent theory indicates that the agency problem between the manager and debt holders is linked to the riskiness of debt. Specifically, the project selection problem is only an issue when the firm's debt is risky. The severity of the problem should be directly related to the riskiness of debt. We provide direct evidence on firms' rationale of inside debt by exploiting this largely ignored insight.

Contrary to the above conjecture, we show that firms with less default risk use more inside debt. This means that more distressed firms (i.e., those firms with severe agency problems between debt holders and equity holders) are less likely to use inside debt. On the contrary, healthier firms, where the project selection problem is less of a problem, tend to use more inside debt. Further, we analyze whether this relation is causal by exploiting the 2008 financial crisisan exogenous shift in default risk. Using a difference-in-differences approach, we find that firms with increased default risk over the crisis period increase their inside debt significantly less than those firms with increased default risk. In summary, we provide evidence that firms' default risk has a causal effect on inside debt. However, the relation is inconsistent with the agency theory's prediction.

This chapter builds on both the theoretical framework of agency theory and related empirical research of inside debt. Our results suggest that traditional contracting theories do not appear to be first-order determinants of inside debt. Although it is beyond this chapter's scope to explore why less distressed firms use more inside debt, we conjecture that personal tax considerations appear to be an important driver of inside debt. Further research is required to fully reveal the mystery of inside debt.

Table 2.1: Summary statistics of the inside debt sample.
This table contains summary statistics for the variables used in the analysis over the full sample period. Our sample consists of 4,607 firm-year observations from fiscal years 2006 to 2010.

variable	N	Mean	sd	q1	median	q3
Compensation components						
Pension	4607	2.96	7.01	0.00	0.00	2.57
Deferred compensation	4607	2.54	8.10	0.00	0.20	1.72
Cash compensation	4607	1.11	2.22	0.63	0.86	1.11
Stock option	4607	15.52	33.44	1.64	6.14	16.53
Stock holding	4607	92.79	1022	2.38	7.30	20.55
Inside debt variables						
CEO inside debt holding	4607	5.50	12.31	0.00	0.95	5.57
CEO inside debt/cashcomp	4607	4.67	8.58	0.00	1.13	5.82
CEO to firm leverage ratio	4607	3.24	15.86	0.00	0.24	1.32
market relative debt ratio	2350	1.36	2.79	0.01	0.34	1.35
CEO incentive ratio	4607	2.37	11.12	0.00	0.19	0.97
CEO equity incentives	4607	0.02	0.05	0.00	0.01	0.02
CEO debt incentives	4607	0.03	0.16	0.00	0.00	0.01
Default risk measures						
Z-score	4607	3.54	3.30	2.11	3.14	4.61
O-score	4604	-1.82	1.87	-2.91	-1.88	-0.85
DD	4456	6.31	5.43	2.70	5.35	8.52
EDF	4456	0.07	0.19	0.00	0.00	0.00
Firm and CEO characteristics						
Firm assets	4607	7603	20559	784.5	2024	5876
Firm size	4607	7.72	1.49	6.67	7.61	8.68
Debt-to-equity	4607	0.69	4.51	0.09	0.23	0.49
Market Debt-to-equity	2350	0.65	2.52	0.16	0.30	0.56
Market value of debt	2350	3165	6331	452.3	1207	3165
Book leverage	4607	0.25	0.17	0.12	0.23	0.34
Market leverage	4607	0.23	0.20	0.08	0.19	0.33
Profitability	4602	0.14	0.08	0.09	0.13	0.18
Tangibility	4602	0.28	0.23	0.10	0.20	0.40
Market-to-book	4607	1.41	0.88	0.83	1.17	1.71
CEO leverage	4607	0.28	0.70	0.00	0.06	0.30
R&D	4607	0.03	0.05	0.00	0.00	0.03
CEO age	4607	55.44	7.01	51.00	55.00	60.00

Table 2.2: Summary statistics across sub-samples: the group of firms with CEO to firm leverage ratio > 5 and the group of firms with CEO to firm leverage ratio <= 5.
This table splits the full sample into two groups. The mean and standard deviation (in parentheses) are reported. Significance at the 1, 5, and 10% levels are signified with ***, **, and *, respectively.

Variable	CEO to firm leverage ratio <= 5	CEO to firm leverage ratio > 5	p-value
Inside debt variables			
CEO inside debt holding	4.61	14.47	0.00***
	(10.40)	(22.39)	
CEO inside debt/cashcomp	3.97	11.70	0.00***
	(7.62)	(13.29)	
CEO to firm leverage ratio	0.65	29.39	0.00***
	(1.02)	(45.12)	
Market relative leverage	0.74	9.64	0.00***
	(1.04)	(4.94)	
CEO incentive ratio	0.48	21.49	0.00***
	(0.75)	(31.08)	
Default risk measures			
Z-score	3.35	5.45	0.00***
	(3.25)	(3.15)	
O-score	-1.71	-3.02	0.00***
	(1.84)	(1.82)	
DD	5.80	11.56	0.00***
	(4.84)	(7.76)	
EDF	0.07	0.01	0.00***
	(0.20)	(0.08)	
Firm and CEO characteristics			
Firm size	7.70	7.86	0.07*
	(1.46)	(1.69)	
Debt-to-equity	0.75	0.11	0.00***
	(4.72)	(0.32)	
Market Debt-to-equity	0.68	0.15	0.00***
	(2.61)	(0.13)	
Market debt	3053	4659	0.05*
	(5909)	(10378)	
R&D	0.03	0.03	0.10
	(0.05)	(0.05)	
CEO age	55.27	57.21	0.00***
	(7.05)	(6.34)	
Book leverage	0.26	0.10	0.00***
	(0.17)	(0.11)	
Market leverage	0.25	0.08	0.00***
	(0.20)	(0.10)	
Profitability	0.14	0.17	0.00***
	(0.08)	(0.09)	
Tangibility	0.28	0.25	0.00***
	(0.23)	(0.18)	
Market-to-book	1.38	1.71	0.00***
	(0.86)	(1.01)	
No. obs.	4192	415	
No. obs. (o-score)	4189	415	
No. obs. (dd)	4058	398	
No. obs. (edf)	4058	398	

Table 2.3: Probit analysis of the group of firms with greater than five CEO to firm leverage ratio.

This table presents a probit regression across all firm-years where the dependent variable is equal to one if the CEO to firm leverage ratio for the firm-year is greater than five; otherwise, the dependent variable is equal to zero. All independent variables are defined in Appendix. Z-statistics are reported in parentheses. Significance at the 1, 5, and 10% levels are signified with ***, **, and *, respectively. Due to extremely high correlation (0.8) between Firm leverage and EDF, we do not include Firm leverage as an independent variable in model (4).

	(1)	(2)	(3)	(4)
	CEO to firm leverage ratio > 5			
Firm size	0.08**	0.04	0.04	0.03
	(2.35)	(1.08)	(1.24)	(0.79)
Firm leverage	-1.39	-1.39	-0.86	
	(-1.60)	(-1.50)	(-1.46)	
R&D	-0.02	0.13	-0.08	1.02
	(-0.02)	(0.15)	(-0.10)	(1.48)
CEO age	0.02***	0.02***	0.02***	0.02***
	(4.58)	(4.18)	(4.48)	(4.22)
Z-score	0.06**			
	(2.42)			
O-score		-0.11*		
		(-1.88)		
DD			0.06***	
			(4.96)	
EDF				-1.71***
				(-4.13)
Constant	-3.30***	-2.86***	-3.27***	-2.82***
	(-6.84)	(-7.00)	(-7.96)	(-6.78)
No. Obs	4607	4604	4456	4456
pseudo R^2	0.14	0.14	0.16	0.04
p-value	0.00	0.00	0.00	0.00

Table 2.4: Panel regressions describing cross sectional variation of inside debt and firms' default risk.

This table contains the results from panel regressions on the full sample of 4,607 firm-year observations. The dependent variable is one of our measures of inside debt (CEO Inside debt holdings, CEO Inside debt to cash compensation ratio, CEO to firm leverage ratio, or CEO relative incentive ratio). Dummy variables for each fiscal year in the sample are also included in the regression. Firm leverage is excluded from regression (3) and (4) as it is the denominator of CEO to firm leverage ratio and highly correlated with the denominator of CEO relative incentive ratio. The table reports t-statistics based on standard errors clustered at the firm level in parentheses. Significance at the 1, 5, and 10% levels are signified with ***, **, and *, respectively.

	(1) CEO inside debt holdings	(2) CEO inside debt/ cash compensation	(3) CEO to firm leverage ratio	(4) CEO relative incentive ratio
Z-score	0.16**	0.14**	0.62***	0.45***
	(2.12)	(2.48)	(3.48)	(3.49)
Firm size	3.79***	2.14***	-0.25	-0.32*
	(9.91)	(10.25)	(-0.91)	(-1.65)
Firm leverage	-0.06***	-0.05***		
	(-2.99)	(-2.89)		
R&D	8.19*	0.91	11.68*	6.53
	(1.89)	(0.33)	(1.78)	(1.36)
CEO age	0.22***	0.15***	0.12**	0.09**
	(6.24)	(5.62)	(2.21)	(2.45)
Constant	-36.71***	-20.84***	-3.83	-2.19
	(-9.52)	(-9.10)	(-0.95)	(-0.77)
No. Obs.	4607	4607	4607	4607
Adj. R^2	0.23	0.16	0.02	0.02
p-value	0.00	0.00	0.00	0.00
Year dummies	Y	Y	Y	Y

Table 2.5: Tests of the control and treatment group in the pre-crisis period.
The table presents the results that compare the control and treatment groups in the pre-crisis period. We have 2066 firm-year observations for fiscal years 2006 and 2007. The control group is composed of those firms with increased default risk during the 2008 financial crisis. The treatment group is composed of those firms with decreased default risk during the 2008 financial crisis. The change in firms' default risk during the 2008 financial crisis is measured by the change in the firm's Z-score from fiscal year 2007 to 2008. Panel A presents the mean and standard deviation, in parentheses, of each group's CEO and inside debt variables in the pre-crisis period. Panel B presents the results of changes in each inside debt variable, i.e., the parallel trends test. P-values associated with t-tests for different means between groups are also reported. Significance at the 1, 5, and 10% levels are signified with ***, **, and *, respectively.

Panel A: Pre-Crisis (2006 and 2007)	Control group	Treatment group	p-value
Firm size	7.55	7.61	0.46
	(1.52)	(1.48)	
Debt-to-equity	0.38	0.34	0.13
	(0.57)	(0.67)	
R&D	0.02	0.02	0.07*
	(0.04)	(0.05)	
CEO age	55.32	54.91	0.29
	(7.12)	(7.13)	
CEO inside debt holdings	4.29	5.23	0.09*
	(9.67)	(12.16)	
CEO inside debt/cash comp.	4.11	4.26	0.70
	(7.08)	(7.52)	
CEO to firm leverage ratio	3.01	5.21	0.35
	(15.84)	(90.08)	
CEO incentive ratio	1.89	1.87	0.95
	(7.40)	(6.45)	
No. obs.	434	1632	
Panel B: growth rate (2006 – 2007)			
CEO inside debt holdings	0.23	0.28	0.87
	(2.50)	(6.70)	
CEO inside debt/cash comp.	0.10	0.20	0.60
	(2.10)	(3.46)	
CEO to firm leverage ratio	-1.25	-5.19	0.38
	(14.40)	(124.0)	
CEO incentive ratio	-0.26	0.09	0.22
	(3.75)	(3.55)	
No. obs.	217	816	

Table 2.6: Tests of the control and treatment group in the pre-crisis period.
The table presents the results that compare the control and treatment groups in the pre-crisis period. We have 2066 firm-year observations for fiscal years 2006 and 2007. The control group is composed of those firms with increased default risk during the 2008 financial crisis. The treatment group is composed of those firms with decreased default risk during the 2008 financial crisis. The change in firms' default risk during the 2008 financial crisis is measured by the change in the firm's Z-score from fiscal year 2007 to 2008. We present the results of probit regressions using data in the pre-crisis period. The dependent variable is a dummy variable treatment that equals one if the firm belongs to the treatment group and zero otherwise. Z-statistics are reported in parentheses. Significance at the 1, 5, and 10% levels are signified with ***, **, and *, respectively.

	(1) Treatment	(2) Treatment	(3) Treatment	(4) Treatment
Firm size	0.01	0.02	0.02	0.02
	(0.27)	(0.90)	(1.06)	(1.06)
Firm leverage	-0.05	-0.06	-0.06	-0.06
	(-1.18)	(-1.31)	(-1.31)	(-1.33)
R&D	1.17	1.17	1.17	1.16
	(1.57)	(1.57)	(1.58)	(1.56)
CEO age	-0.01	-0.01	-0.00	-0.00
	(-1.34)	(-1.14)	(-1.12)	(-1.12)
CEO inside debt holdings	0.00			
	(1.41)			
CEO inside debt to cash compensation ratio		0.00 (0.19)		
CEO to firm leverage ratio			0.00	
			(0.48)	
CEO relative incentive ratio				-0.00
				(-0.08)
Constant	1.06***	0.92***	0.90***	0.91***
	(3.51)	(3.13)	(3.21)	(3.22)
No. Obs.	2066	2066	2066	2066
pseudo R^2	0.00	0.00	0.00	0.00
p-value	0.13	0.26	0.23	0.26

Table 2.7: Difference-in-differences mean tests of inside debt across the control and treatment groups.
Difference-in-differences mean tests of inside debt across the control and treatment groups. This table presents the results of difference-in-differences tests of the means of various inside debt variables surrounding the 2008 financial crisis. The control group is composed of those firms with increased default risk during the 2008 financial crisis. The treatment group is composed of those firms with decreased default risk during the 2008 financial crisis. The change in firms' default risk during the 2008 financial crisis is measured by the change in the firm's Z-score from fiscal year 2007 to 2008. We calculate each firm's change of the inside debt measures over the crisis period (the change from fiscal year 2007 to 2008). We then report each group's mean change during the crisis period with standard deviations in parentheses. The difference-differences are calculated by subtracting the mean change of the control group from that of the treatment group. P-values associated with t-tests for different means between groups are also reported. Significance at the 1, 5, and 10% levels are signified with ***, **, and *, respectively.

Raw change (2008 – 2007)	Control group	Treatment group	Difference-in-differences	p-value
CEO inside debt holdings	-0.05	-0.69	-0.64	0.07*
	(4.14)	(6.15)		
CEO inside debt\cash compensation	-0.25	-0.46	-0.21	0.50
	(4.23)	(3.62)		
CEO to firm leverage ratio	2.67	0.07	-2.60	0.03**
	(16.26)	(9.93)		
CEO relative incentive ratio	1.93	0.00	-1.93	0.02**
	(12.01)	(7.22)		
No. Obs.	217	816		

Table 2.8: Difference-in-differences mean tests of inside debt across the control and treatment groups.
Difference-in-differences mean tests of inside debt across the control and treatment groups. This table reports the results from the difference-in-differences regressions. The sample includes 217 firms in the control group and 816 firms in the treatment group, a total of 2,066 firm-year observations in fiscal years 2007 and 2008. Crisis is a dummy variable that takes the value of one for fiscal year 2008 and zero for fiscal year 2007. Treatment is a dummy variable that equals one if the firm belongs to the treatment group and zero otherwise. Firm leverage is excluded from regression (3) and (4) as it's the denominator of CEO to firm leverage ratio and highly correlated with the denominator of CEO relative incentive ratio. The control group is composed of those firms with increased default risk during the 2008 financial crisis. The treatment group is composed of those firms with decreased default risk during the 2008 financial crisis. The change in firms' default risk during the 2008 financial crisis is measured by the change in the firm's Z-score from fiscal year 2007 to 2008. The table reports t-statistics based on standard errors clustered at the firm level in parentheses. Significance at the 1, 5, and 10% levels are signified with ***, **, and *, respectively.

	(1) CEO inside debt holdings	(2) CEO inside debt/ cash compensation	(3) CEO to firm leverage ratio	(4) CEO relative incentive ratio
Crisis	0.05	-0.18	2.62**	1.90**
	(0.18)	(-0.65)	(2.40)	(2.35)
Treatment	0.83	0.15	0.21	0.16
	(1.20)	(0.30)	(0.28)	(0.29)
$Crisis * treatment$	-0.75**	-0.27	-2.63**	-1.95**
	(-2.15)	(-0.88)	(-2.28)	(-2.29)
Firm size	3.34***	1.83***	-0.19	-0.29
	(9.94)	(10.79)	(-0.73)	(-1.60)
Firm leverage	-0.06***	-0.04***		
	(-3.53)	(-3.54)		
R&D	6.63*	0.24	12.74	7.61
	(1.89)	(0.12)	(1.53)	(1.26)
CEO age	0.19***	0.13***	0.12**	0.10***
	(5.44)	(5.17)	(2.45)	(2.70)
Constant	-31.51***	-16.94***	-3.29	-1.92
	(-9.22)	(-8.79)	(-1.13)	(-0.93)
No. Obs.	2066	2066	2066	2066
adjusted R^2	0.23	0.17	0.01	0.01
p-value	0.00	0.00	0.07	0.06

Table 2.9: Difference-in-differences components of inside debt.
Difference-in-differences mean tests of inside debt across the control and treatment groups. This table reports the results from the difference-in-differences regressions. The sample includes 217 firms in the control group and 816 firms in the treatment group, a total of 2,066 firm-year observations in fiscal years 2007 and 2008. Crisis is a dummy variable that takes the value of one for fiscal year 2008 and zero for fiscal year 2007. Treatment is a dummy variable that equals one if the firm belongs to the treatment group and zero otherwise. Firm leverage is excluded from regression (3) and (4) as it's the denominator of CEO to firm leverage ratio and highly correlated with the denominator of CEO relative incentive ratio. The control group is composed of those firms with increased default risk during the 2008 financial crisis. The treatment group is composed of those firms with decreased default risk during the 2008 financial crisis. The change in firms' default risk during the 2008 financial crisis is measured by the change in the firm's Z-score from fiscal year 2007 to 2008. The table reports t-statistics based on standard errors clustered at the firm level in parentheses. Significance at the 1, 5, and 10% levels are signified with ***, **, and *, respectively.

Equity (2008 – 2007)	Control group	Treatment group	Difference-in-differences	p-value
CEO equity	-0.33	-0.28	0.05	0.83
performance sensitivity ∗100	(3.64)	(2.19)		
CEO equity	-17.6	-62.7	-45.1	0.00***
	(136.4)	(368.6)		
Firm equity	-1720	-3242	-1522	0.03**
	(9532)	(8431)		
Stock option	-5.98	-10.4	-4.42	0.01***
	(20.82)	(23.55)		
Stock holding	-11.6	-52.3	-40.7	0.01***
	(131.8)	(364.7)		
Debt (2008 – 2007)				
CEO debt pay	1.42	-0.43	-1.85	0.01**
performance sensitivity ∗100	(10.41)	(7.63)		
CEO inside debt holdings	-0.05	-0.69	-0.64	0.07*
	(4.14)	(6.15)		
Firm debt	-59.3	219.1	278.4	0.00***
	(590.9)	(1146)		
Pension	0.20	0.08	-0.12	0.54
	(2.83)	(2.51)		
Defered compensation	-0.25	-0.76	-0.51	0.04**
	(2.56)	(5.37)		
Deferred Compensation 2008				
CEO contributions	255.2	243.1	-12.1	0.84
	(757.0)	(877.3)		
Firm contributions	67.37	109.6	42.23	0.20
	(200.4)	(859.2)		
Earnings	-350	-427	-77.0	0.63
	(2189)	(1648)		
Withdrawal	26.53	210.7	184.2	0.01***
	(188.5)	(1966)		
No. Obs.	217	816		

CHAPTER 3

THE ROLE OF INSIDE DEBT IN THE OPTIMAL STRUCTURE OF CEO PAY

Merton (1974) models the equity of the firm as a call option on the underlying asset of the firm with the strike price equal to the face value of the firm's debt. In this chapter, we adopt the Merton framework to analyze the firm's rationale for inside debt. Given the observed magnitudes of equity incentives and debt incentives in chief executive officer (CEO) compensation, we first characterize the set of positive net present value (NPV) projects that CEO prefer to forego (underinvestment) and the set of negative NPV projects that CEO want to accept (overinvestment). The magnitudes of underinvestment and overinvestment problems appear to be small on average in the sample. The observed magnitudes of inside debt are hard to reconcile with the hypothesis that providing debt incentives is the main reason for inside debt. Finally, the savings from deferral of personal tax appear to moderate in magnitude.

3.1 Introduction

Investment distortions arising from the conflict between debt holders and equity holders have been one major argument explaining the role of inside debt in firms' optimal contracting with managers. This chapter uses numerical methods to quantify investment distortions in a setup that takes managerial incentives into consideration. We analyze the project selection problem in which the manager makes investment decisions conditional on both equity and debt incentives in his contract. Quantifying the magnitudes of investment distortions is important to understand their relative importance in optimal contracting with managers.

"Inside debt" refers to the part of executive pay with payoffs similar to the debt security held by external creditors. It usually includes both pension and deferred compensation. Similarly, "inside equity" refers to the part of executive pay with payoffs similar to the equity security held by external investors. It usually includes both stock options and stock holdings. Theoretically, Jensen and Meckling (1976) show that inside debt exposes chief executive officers (CEOs) to default risk as inside debt represents unsecured and unfunded liabilities of the firm. Hence, inside debt helps align managerial incentives with those of debt holders. Consistent with this view, there has been empirical evidence showing that firms with more debt incentives provided through inside debt adopt less risky firm investment and financing policies and exhibit higher debt ratings (Cassell et al. (2012) among others).

To the extent that inside debt helps align managerial incentives with those of debt holders through the channel of affecting project selection decisions, it is important to understand the magnitudes of investment distortions associated with project selection problems. In this chapter, we research how the combination of debt and equity incentives affects investment decisions and quantify the magnitudes of investment distortions. One related work is Parrino and Weisbach (1999). They use discounted cash flow techniques to research the magnitudes of investment distortions when investment decisions are made to maximize equity value. They show that the magnitudes of the distortions cannot explain the cross section variation of firm's capital structure.

When investment decisions are made to maximize CEO wealth, it is straightforward that more debt incentives than equity incentives (a debt bias) induce more conservative investment decisions. And more equity incentives than debt incentives (an equity bias) induce more risky investment decisions. To measure debt and equity incentives, we follow the literature and use debt and equity pay-performance sensitivity. Debt or equity pay-performance sensitivity is defined as the change in the CEO's wealth when the firm's debt or equity value increases by one dollar. When debt and equity pay-performance sensitivity are not equal, there will be investment distortions as investment decisions are no longer made to maximize firm value. Empirically, debt pay-performance sensitivity is calculated as the ratio of inside debt value to face value of debt. Equity pay-performance sensitivity is calculated as the ratio of inside equity value to market value of equity. To evaluate the effect of investment opportunities on equity and debt holders, we need a framework to value market value of equity and debt. Current market value of equity is available from the Compustat database and we adopt the Merton (1974) framework to value the current value of risky debt. The model assumes that the firm has one zero-coupon bond. Firm equity can be viewed as a call option on the firm's asset and the risky debt can be viewed as a risk-free bond coupled with selling a put option on the firms asset. The strike price for both options is the face value of the firm's debt, and the expiration date is the same as the term of debt. Merton (1974) provides formulas to derive current firm value (equity plus risky debt value) and firm volatility from current equity value and equity volatility. Starting from the current value, when we consider investment opportunities that will affect firm value and firm volatility, we can value new equity and debt value by black-scholes option formula.

First, we construct a hypothetical firm with mean values in our sample for firm specific variables, such as face value of debt, market value of equity and equity volatility. We evaluate the magnitudes of underinvestment and overinvestment problems when the ratio of debt to equity pay-performance sensitivity varies in the range of zero to ten. This exercise helps provide a benchmark for the magnitudes of project selection problems that firms face when the CEO compensation exhibits either a debt or equity bias. Assuming that the project can increase or decrease the firm volatility by 10 or 20% from its current level, we show that the negative (positive) NPV projects that CEO will take (forego) vary from 0.01 to 1% of the underlying asset value on average. Despite the variation, it appears that the project selection problem is reasonably small in magnitude for a typical large United States firm. This is not surprising as the sample firm equity is deep in the money for the level of debt outstanding.

Next, we apply this methodology to each individual firm and evaluate the magnitudes of underinvestment and overinvestment problems with observed debt and equity pay-performance sensitivity. Specifically, we take a specific firm's face value of debt, market value of equity and equity volatility into consideration and assume that the project can change the asset volatility by 10 to 90% from its current level. The magnitudes of underinvestment and overinvestment problems vary up to 4% of the underlying asset value.

Also, we consider a potential friction that might underestimate investment distortions: if the renegotiation costs prevent the firm from changing the contract constantly, the incentive contracts may be set optimally to firm parameters later. Further, the level of investment distortions are calculated when the manager makes a single investment decision, based on current parameters. The current results might be less relevant to the results to real world firms which make a series of investment decisions over time, based on parameters (e.g., firm value) that will only become known later. We evaluate investment distortions for possible firm value in one year and calculate the mean investment distortions according to the distribution of firm value in one year, keeping other parameters unchanged. For the firms in our sample, we provide summary statistics for the mean investment distortions in one year. Overall the mean magnitudes of investment distortions are up to 0.5% of asset value on average in one year.

This chapter is related to the literature examining the relation between CEO compensation (managerial compensation) and firm investment policies. Coles, Daniel, and Naveen (2006) among others show that risk taking incentives in CEO compensation are positively correlated with the riskiness of firm policies. Most work in this literature has focused on only equity incentives until recently. Cassell et al. (2012) among others show that debt incentives are positively correlated with less risky investment and financial policies. We quantify the magnitudes of investment distortions that debt incentives are used to alleviate. Overall, the magnitudes of underinvestment and overinvestment problems are small under the assumptions of Merton (1974) for the set of firms in S&P 1500.

The contribution is threefold. First, we take managerial incentives into consideration when evaluating investment distortions arising from the conflict between debt holders and equity holders. Second, this simulation exercises contribute to the debate regarding firms' rationale for using inside debt. Inside debt has been documented to help alleviate agency costs of debt. This exercise directly quantifies the agency costs that are supposed to be alleviated by inside debt and indicates that investment distortions are minimal. This result challenges the conventional wisdom that the literature of debt incentives is built on. Though admittedly, the small magnitudes of investment distortions may result from the assumptions in Merton (1974). Finally, we extend the implications of this experiment to the under-leverage puzzle. Agency costs of debt have been referred as one possible explanation for the under-leverage puzzle. Consistent with Parrino and Weisbach (1999), this simulation exercise shows that this cost appears to be small compared to tax benefits. It is also puzzling that firms do not use more debt if project selection related costs can be eliminated by providing debt incentives. This at least indicates that other frictions than investment distortions are responsible for the under leverage puzzle.

3.2 Setup for the simulation

In general researchers separately studied the agency problem between managers and equity holders (effort choice problem), and the conflict between debt holders and equity holders (project selection problem). For the agency problem between managers and equity holders, researchers usually consider a setup with a risk-averse manager, risk-neutral equity holders and an all equity firm where equity holders are maximizing firm value (i.e., equity value in this setup). Two types of incentives are researched. First, in order for the manager to act in interests of the firm (equity holders for an all equity firm), part of the manager's compensation is linked to the payoff of equity. For example, restricted stocks and stock options are common in CEO compensation. Second, with risky payoff in compensation, risk-averse CEO may pass up positive net-present-value (i.e., firm value increasing) but risk increasing projects. Hence, certain convex payoff compensation is granted to promote risk-seeking behavior. For example, researchers have argued that stock options are part of the optimal compensation package because they help to induce risk-taking behavior.

On the other hand, the conflict between debt holders and equity holders is usually researched in a setup with both risk-neutral equity holders and debt holders. Optimal compensation is derived when firms face two projects, one safe and one risky. John and John (1993) derive the relation between debt incentives and equity incentives when the project has discrete payoffs. The underlying intuition for optimal project choice is to ultimately let the compensation include a proportion of firm value, i.e., the manager is maximizing firm value as opposed to equity value to alleviate the conflict between debt holders and equity holders. This statement holds as long as the manager only cares about his wealth.

Edmans and Liu (2011) show that granting the manager equal proportions of debt and equity incentives may not always be optimal. An equity bias is desired to induce effort for most situations. However, when bankruptcy is likely or the manager is efficient in increasing bankruptcy value, a debt bias is preferred. Edmans and Liu (2011) help lay out the intuition of the interplay between effort choice problem and project selection problem. The intuition is that the relative efficiency for the manager to increase solvency and bankruptcy value and the project selection problem are simultaneously evaluated in equilibrium.

To research whether the observed incentive contracts can be justified by a principal agent model, the technical challenge is to model both the effort choice problem and the project selection problem in a tractable form with parameters that can be estimated from data. Further, specific firm level assumptions need to be made about the possible project choices. In this chapter, we take a different approach and ask the research question of how large the distortion is when the debt and equity incentives in CEO compensation are not equal.

We use the framework of Merton (1974) to quantify the distortion when the debt and equity incentives in CEO compensation are not equal. In this framework, the equity of the firm is a call option on the underlying value of the firm and the strike price is the face value of the firm's debt. There are two main assumptions. The model assumes that the underlying value of each firm follows geometric Brownian motion and each firm issued only one zero-coupon bond. Further, neither the underlying value of the firm nor its volatility is directly observable. Under the model's assumptions both can be inferred from the value of equity, the volatility of equity, the face value of debt, and the expiration date of debt.

After getting the value and the volatility of the firm, we start the simulation process. The firm's equity is a call option on the underlying value of the firm, and the firm's risky debt is a risk-free bond together with selling a put option on the underlying value of the firm. The call and put option have the same maturity of the risk-free bond. Consider a CEO's compensation exhibiting a debt bias, i.e., the debt pay-performance sensitivity is larger than equity pay-performance sensitivity. With a decrease in firm volatility all else equal, the CEO's compensation is higher. This indicates that the CEO with a debt bias in his compensation will accept zero NPV but risk decreasing projects. With similar reasoning, the CEO will accept negative NPV but risk decreasing projects. The cutoff value for the type of negative NPV but risk decreasing projects is calculated when the firm's asset decreases to the point where the CEO's compensation is the same as that under the original firm volatility. With an increase in firm volatility all else equal, the CEO's compensation is higher. This indicates that the CEO with a debt bias in his compensation will reject zero NPV but risk increasing projects. With similar reasoning, the CEO will reject positive NPV but risk increasing projects. The cutoff value for the type of positive NPV but risk increasing projects is calculated when the firm's asset increases to the point where the CEO's compensation is the same as that under the original firm volatility. Similar reasoning can be applied to a CEO's compensation exhibiting an equity bias. The cutoff value for the type of negative NPV but risk increasing projects is calculated when the firm's asset decreases to the point where the CEO's compensation is the same as that under the original firm volatility. The cutoff value for the type of positive NPV but risk increasing projects is calculated when the firm's asset increases to the point where the CEO's compensation is the same as that under the original firm volatility.

3.3 The sample for simulation

As described in Section 3.2, we need various firm and CEO level variables to evaluate the CEO's project selection problem under the setup of Merton (1974) model. We start by collecting the debt and equity incentive components in CEO compensation from the ExecuComp Database. Inside debt includes pension and deferred compensation and inside equity includes stock options and stock holdings. Because ExecuComp starts to report complete information on inside debt holdings from 2006 onward, we restrict my sample period to fiscal years 2006 through 2011. In addition, we remove firms in the financial (firms with SIC code between 6000 and 6999) and utility (firms with SIC code between 4900 and 4999) industries. We require all firm-year observations have non-missing data for the main inputs to the Merton model, including firm debt and equity, inside debt and equity, and firm equity volatility. Finally, we exclude firm-years with zero equity incentive compensation as we use debt to equity pay-performance sensitivity to categorize firms into subcategories with either a debt or an equity bias. This results in a final sample of 5,447 firm-year observations. All ratio variables are winsorized at the upper 1 percentile to avoid data errors and outliers. We do not winsorize the ratio variables at the lower 1 percentile as they are truncated at zero.

In Table 3.1 Panel A, we present summary statistics for the full sample including 5,447 firm-year observations. We start by presenting firm level variables that serve as inputs into the Merton model. The average firm debt is \$1772 million and the average firm equity is \$7632 million. The median firm debt is \$436 million and the median firm equity is \$1753 million. The firm equity's annual volatility is 42% on average, and the median is 37%. The firm's market leverage is 0.52 on average, and the median is 0.224. It is worth noting that these firm level variables are highly skewed. We continue to provide summary statistics for CEO variables. The average CEO's inside debt holdings are \$5 million, and the average CEO's inside equity holdings are \$57 million. These two variables are highly skewed also. The median inside debt is \$0.7 million and the median inside equity is \$16 million. Further, debt pay-performance sensitivity is 2.67% on average, while the median is 0.12%. Equity pay-performance sensitivity is 5.14%, while the median is 0.83%. The average debt to equity pay-performance sensitivity is 5.14. However, the median is only 0.19. Out of the 5,447 firm-year observations, 28% exhibits a debt bias.

In Table 3.1 Panel B and C, we present summary statistics for subsamples with an equity bias and a debt bias. Firms with a debt bias have an average debt of \$2 billion, while firms with an equity bias have an average debt of \$1.6 billion. Firms with a debt bias have an average debt of \$13 billion, while firms with an equity bias have an average debt of \$5 billion. Firms with a debt bias have an average market leverage of 0.246, while firms with an equity bias have an average market leverage of 0.634. Firms with a debt bias have an average equity volatility of 35%, while firms with an equity bias have an average equity volatility of 45%. In summary, firms with a debt bias appear to have larger size and smaller market leverage, and exhibit smaller equity volatility and smaller equity pay-performance sensitivity. These statistics are consistent with those documented in this literature.

3.4 Simulation results

This section starts by presenting the set of project selection problems that a hypothetical firm faces when the CEO has some hypothetical debt to equity pay-performance sensitivity, ranging from 0 to 10. The hypothetical firm has a mean or median level of firm debt, equity and equity volatility. Then the summary statistics for firms' project selection problems are presented using observed CEO debt and equity incentive compensation for each individual firm. Finally, we present summary statistics for firms' project selection problems for both the observed CEO debt and equity incentive compensation, and only the observed CEO equity incentive compensation.

3.4.1 A hypothetical firm with hypothetical contracts

In this section, we describe the project selection problem for a hypothetical firm with hypothetical incentive contracts. This will help quantify the project selection problems and provide a benchmark for individual firm analysis.

In Table 3.2, we evaluate the project selection problem for a sample firm with mean value of firm debt, firm equity and equity volatility. First as the procedure outlined in Section 3.2, the firm value and firm volatility are calculated by assuming the sample firm has a zero coupon debt equal to the mean value of firm debt in the whole sample, and the zero coupon debt matures in 5 years. For each hypothetical debt to equity pay-performance sensitivity which varies from 0 to 1 and a 10% increase (decrease) in firm volatility (not equity volatility), the cutoff value for the negative (positive) NPV projects that the CEO will take (forgo) is calculated for a CEO with equity bias compensation. Similarly, for each hypothetical debt to equity pay-performance sensitivity which varies from 1 to 10 and a 10% decrease (increase) in firm volatility (not equity volatility), the cutoff value for the negative (positive) NPV projects that the CEO will take (forgo) is calculated for a CEO with debt bias compensation. Both the cutoff value and the cutoff value as a proportion of firm market value are reported. The cutoff value for negative NPV projects is the smallest negative NPV project that the CEO will take. And the cutoff value for positive NPV projects is the largest positive NPV project that the CEO will forgo. For example, for a CEO with zero debt to equity pay-performance sensitivity, with 10% decrease in firm volatility, the CEO will not take any positive NPV projects with NPV smaller than $6.11 million. For the same CEO with zero debt to equity pay-performance sensitivity, with 10% increase in firm volatility, the CEO will take any negative NPV projects with NPV bigger than negative $10.16 million. These cutoff values represent 0.065% and 0.108% of firm market value, respectively. As the distortion in debt to equity pay-performance sensitivity gets smaller, i.e., closer to 1, the cutoff value and the cutoff value as a proportion of firm market value decrease. For example, with a 10% decrease in firm volatility, a CEO with debt to equity pay-performance sensitivity equal to 0.1 will not take any positive NPV projects unless its NPV exceeds 0.058% of firm value. On the other hand, with a 10% decrease in firm volatility, a CEO with debt to equity pay-performance sensitivity equal to 0.9 will not take any positive NPV projects unless its NPV exceeds 0.006% of firm value. When the debt pay-performance sensitivity equals equity pay-performance sensitivity, there is no project selection problem and correspondingly the cutoff value is zero.

A similar pattern exists for firms with a debt bias in their CEO compensation. As the distortion in debt to equity pay-performance sensitivity gets larger, i.e., further from the value of 1, the cutoff value and the cutoff value as a proportion of firm market value increase. For example, with a 10% decrease in firm volatility, a CEO with debt to equity pay-performance sensitivity equal to 2 will take any negative NPV project as long as its negative NPV is smaller than 0.065% of firm value. On the other hand, with a 10% decrease in firm volatility, a CEO with debt to equity pay-performance sensitivity equal to 9 will take any negative NPV project as long as its negative NPV is smaller than 0.511% of firm value. Note that debt to equity pay-performance sensitivity equal to 9 represents a bigger distortion than debt to equity pay-performance sensitivity equal to 2.

It is also noteworthy that the distortion arisen when debt and equity pay-performance sensitivity are not equal is not symmetric. For example, debt to equity pay-performance sensitivity equal to 0.5 represents that the debt pay-performance sensitivity is half of equity pay-performance sensitivity. Debt to equity pay-performance sensitivity equal to 2 represents that the equity pay-performance sensitivity is half of debt pay-performance sensitivity. With a 10% decrease in firm volatility, a CEO with debt to equity pay-performance sensitivity equal to 2 will take any negative NPV projects as long as its negative NPV is smaller than 0.065% of firm value. On the other hand, with a 10% increase in firm volatility, a CEO with debt to equity pay-performance sensitivity equal to 0.5 will take any negative NPV project as long as its negative NPV is smaller than 0.054% of firm value. Even more drastically, with a 10% increase in firm volatility, a CEO with debt to equity pay-performance sensitivity equal to 2 will not take any positive NPV project unless the positive NPV is larger than 0.107% of firm value. On the other hand, with a 10% decrease in firm volatility, a CEO with debt to equity pay-performance sensitivity equal to 0.5 will not take any positive NPV project unless the positive NPV is larger than 0.032% of firm value.

In untabulated tables, we study project selection problems for the same sample firm except that the debt maturity is set to be 7 years. We also study project selection problems for the same sample firm with debt maturity equal to 5 years but facing a 20% increase (decrease) in firm volatility changes when calculating cutoff values. There is a pattern that the project selection problems get more severe when the debt maturity is longer and when the CEO has a greater influence on firm volatility.

As described in Section 3.3, firm debt and equity value are highly skewed. Hence in untabulated tables, we study the project selection problems for a sample firm with median firm debt and equity value, median equity volatility. The results are similar to those in previous panels in Table 3.2.

3.4.2 Firms with existing contracts

In this section, the project selection problems are calculated using each individual firm's face value of debt, market value of equity, equity volatility and each individual CEO's observed debt and equity incentive compensation. As the project selection problem takes completely opposite sign for firms with a debt bias and an equity bias in CEO compensation, the results for firms with a debt bias and an equity bias in CEO compensation are presented separately in Table 3.3 Panel A and Panel B. Similar to Table 3.2, the cutoff values for project selection problems are calculated using debt maturity of 5 years and 7 years, and a 10% and 20% changes in firm volatility.

It is worth noting that under the assumptions in Section 3.2, the distortions introduced by either an equity bias, or a debt bias in CEO compensation is trivial. This is not surprising in the sense that the firms in the sample are those in the S&P 1500 index and are in good financial health. To see how sensitive the results are to the assumption of changes in firm volatility, the project selection problems are calculated when assuming the CEO can change the firm volatility by 90%. The magnitudes of the cutoff value as a proportion of firm value are still not very large, with a mean of 4% at most.

3.5 A potential friction

It is worth noting that under the assumptions in Section 3.2, the distortions introduced by either an equity bias, or a debt bias in CEO compensation are trivial. This is calculated assuming that the firm manager makes a single investment decision, based on current firm parameters. We further research how relevant are the results to real world firms which make a series of investment decisions over time, based on parameters (like firm value) that will only become known later. One potential friction omitted from current quantification is renegotiation cost of managerial contracts. The rationale is that the current contract is set to help alleviate investment distortions in the future with revealed new asset price.

In this section, we explore the possibility that there are significant renegotiation costs for managerial contracts. The rationale is that the current agency costs of debt are minimal; however there are significant renegotiation costs. Hence, the observed managerial contracts are set in a level that helps alleviate the agency costs of debt when firms experience negative shocks and hence significant decline in market value. In Table 3.4, we present summary statistics of investment distortions for firms one year from now. The results are similar to those in Table 3.3. We decrease the equity value to half of the current value and recalculate the agency costs of debt. For firms with an equity bias, the related costs are still minimal. The type of positive (negative) projects that CEO will forgo (take) are on average 0.5% of asset value. However, for firms with a debt bias, the type of positive (negative) projects that CEO will forgo (take) are on average 17% of asset value. This manifests a fact that arises from the simulation exercise. The costs related to project choice are not symmetrical. For a certain amount of distress risk, the costs related to a debt bias are much more prominent than the costs related to an equity bias. This is not surprising in the sense that debt holders prefer to invest in t-bills if things are risky enough to leave debt risky. The 17% of asset value potential loss indicates that for firms with a debt bias, if the firm's equity decreases by half, the related agency costs of debt can be significant. This raises the question of why firms give CEO more inside debt in the first place. If project choice costs are the main concern to use inside debt, it is puzzling why firms do not stop giving their chief executive officers inside debt when debt to equity pay-performance sensitivity equals one.

3.6 The role of inside debt in optimal contracting

In particular, we double the existing debt face value and recalculate the potential investment distortions in Table 3.5. The magnitudes are around 2% of the underlying asset value when the projects are associated with a 10% change in firm volatility. This indicates that firms in the sample can double their debt outstanding and the agency costs of debt are still minimal in this numerical setup. This result indicates that agency costs of debt do not appear to explain the under-leverage puzzle. We consider a potential friction that may prevent the firm from using more debt, i.e., a cost of renegotiating managerial contracts. We test whether the existing magnitudes of debt incentives are crucial if the firm continues to receive negative shocks to their asset value. We decrease the firm's equity value by half and recalculate the potential investment distortions in Table 3.6. For firms with an equity bias, the magnitudes run around 2% of the asset value when the projects are associated with 10% change in firm volatility. However, for firms with a debt bias, the magnitudes run around 20% of the asset value when the projects are associated with 10% change in firm volatility. These distortions are significant and it is hard to justify the rationale that inside debt is used to alleviate investment related costs. One possible explanation is that the firms with a debt bias are financially healthy and it is not quite possible that the firms' equity will decrease significantly.

Inside debt helps align managerial incentives with those of external debt holders, while inside equity helps align managerial incentives with those of external equity holders. Intuitively, inside debt (inside equity) represents debt (equity) incentives in CEO compensation as it links CEO compensation directly to the payoffs of debt (equity) holders. The agency problem between the manager and equity holders and the conflict between equity holders and debt holders has been separately and well researched. Further, this exercise helps quantify the investment distortion associated with the agency costs of debt.

This intuition has been explored in the empirical inside debt literature. There is mixed empirical evidence about whether inside debt is used to alleviate the agency costs of debt. On one hand, inside debt helps align CEO incentives with external debt holders and induce less risky firm policies. In 2007, U.S. Securities and Exchange Commission disclosure reform required firms to disclose CEOs' inside debt positions. In response to this disclosure, Wei and Yermack (2012) show that for firms with sizeable inside debt, bond prices rise and equity prices fall. Cassell et al. (2012) further show that inside debt is correlated with less risky investment and financial policies. Specifically, they show that CEO inside debt holdings are positively correlated with asset liquidity and negatively correlated with research and development expenditures and financial leverage. On the other hand, in Chapter 2 we show that less distressed firms provide more debt incentives, contrary to the prediction of an equilibrium principal agent model. In this chapter, we attempt to directly quantify the magnitudes of project choice problems introduced by the relative magnitudes of debt and equity incentives.

Edmans and Liu (2011) model both the effort choice and project selection problem in an equilibrium model, where the firm is maximizing firm value by choosing the incentive contracts. They show that granting the manager equal proportions of debt and equity may not always be optimal when project choice is not the only concern. An equity bias is desired to induce effort for most situations. However, when bankruptcy is likely or the manager is efficient in increasing bankruptcy value, a debt bias is preferred. Edmans and

Liu (2011) help lay out the interplay between effort choice problem and project selection problem. The intuition is that the relative efficiency for the manager to increase solvency and bankruptcy value and the project selection problem are simultaneously evaluated in equilibrium. However, they also note that the two problems can get very complex even under specific assumptions that mechanically separated the two problems. The results in Section 3.4.2 indicate that the project selection problem is trivial for observed debt and equity incentive compensation. As outlined in Section 3.2, the equity incentive compensation is used to help align managerial incentives with those of equity holders, while the debt incentive compensation is used to help align managerial incentives with those of debt holders. At the same time, the relative weight between debt and equity incentive compensation will introduce related project selection problems. There has been substantial evidence indicating that equity incentives play an important role in optimal contracting. Hence, we research the related project selection problems introduced by the observed magnitudes of equity incentives. Similar to former results, the cutoff value of underinvestment and overinvestment problem as a proportion of firm market value is small even without debt incentives to mitigate the project selection problem. For firms with an equity bias in CEO compensation, the usage of inside debt only helps improve the project selection problem marginally. For firms with a debt bias in CEO compensation, the usage of inside debt introduces bigger project selection problem.

Overall, the simulation results indicate that the project selection problem arisen from the debt or equity bias in CEO compensation does not appear to be large in magnitude. Further, we assume that the equity incentives are used to induce effort and debt incentives are used to mitigate project selection problems. We show that the magnitudes of related project selection problems are small even without related debt incentives. For firms with an equity bias in CEO compensation, the usage of inside debt only helps improve the project selection problem marginally. For firms with a debt bias in CEO compensation, the usage of inside debt introduces bigger project selection problem, although marginally.

The low agency costs of debt presented in Section 3.4 further contribute to the under-leverage puzzle. We increase the firm's outstanding debt to twice the level of existing face value of debt. The related agency costs of debt are similar to those presented in Section 3.4.

3.7 Potential explanation

As a supplemental analysis, we calculate the savings from deferral of personal taxes since CEOs can defer taxes on inside debt until withdrawal. The average age for the CEO in the sample is 55 years old. We assume that all CEOs retire at age of 65 and face a marginal tax rate of 40%. We assume all CEOs get a lump sum of their pension and deferred compensation at the age of 65 and the risk free rate is 5%. Under these assumptions, we calculate the difference between the taxes that they have to pay today and those at age of 65. The difference is $0.58 million dollars on average, and the median is only $0.07 million dollars. This difference appears to be small in magnitude to explain that personal tax concern is a main driver for inside debt usage.

3.8 Conclusion

The investment distortions arising from the conflict between debt holders and equity holders have been one major argument supporting firms' optimal contracting, and one of the key frictions behind firms' financing choices. This chapter uses numerical methods to quantify investment distortions in a setup that takes managerial incentives into consideration. We analyze the project selection problem in which the manager makes investment decisions conditional on both equity and debt incentives in his contract. Quantifying the magnitudes of investment distortions is important to understand their relative importance in optimal contracting with managers and firms' financing decisions.

The intuition of inside debt to mitigate agency costs of debt is straightforward. However, the empirical evidence has been mixed. Furthermore, there is no formal analysis by linking the theoretical modeling to observed optimal contracts. In this chapter, we adopt the Merton (1974) framework and quantify the distortion in project selection problem.

Merton (1974) models the equity of the firm as a call option on the underlying asset of the firm with the strike price equal to the face value of the firm's debt. We attempt to shed light on the firm's rationale for inside debt usage by quantifying the project selection problem. Given the observed magnitudes of equity incentives and debt incentives in chief executive officer compensation, we first characterize the set of NPV projects that CEOs prefer to forego and the set of negative NPV projects that CEOs want to accept. The magnitudes of underinvestment and overinvestment problems appear to be small on average in my sample. The observed magnitudes of inside debt are hard to reconcile with the hypothesis that providing debt incentives is the main reason for inside debt. Finally, the savings from deferral of personal tax appear to moderate in magnitude.

Table 3.1: Summary statistics of the sample for simulation.
This table contains summary statistics for the variables used in the analysis over the full sample period. Our sample consists of 5,447 firm-year observations from fiscal years 2006 to 2011.

	N	Standard Deviation	Mean	25 Percentile	50 Percentile	75 Percentile
Panel A: all sample firm years						
Firm level variables						
Outstanding debt (millions of dollars)	5447	1772	3933	106	436	1497
Outstanding equity (millions of dollars)	5447	7632	19131	632	1753	5219
Risk free rate 5 year (%)	5447	2.75	1.08	1.93	2.34	3.49
Risk free rate 7 year (%)	5447	3.18	0.89	2.66	3.07	3.74
Firm equity volatility	5447	0.42	0.21	0.28	0.37	0.51
Firm market leverage	5447	0.523	1.041	0.083	0.224	0.497
CEO variables						
Inside debt (thousands of dollars)	5447	5003	10248	0	722	5179
Inside equity (thousands of dollars)	5447	57461	153103	6283	15708	41460
Debt pay-performance sensitivity (%)	5447	2.67	12.84	0.00	0.12	0.76
Equity pay-performance sensitivity (%)	5447	2.41	4.91	0.37	0.83	1.92
Debt to equity pay-performance sensitivity	5447	5.14	94.24	0.00	0.19	1.23
Panel B: firm years with an equity bias						
Firm level variables						
Outstanding debt (millions of dollars)	3895	1627	3666	109	416	1394
Outstanding equity (millions of dollars)	3895	5336	14268	530	1379	3898
Risk free rate 5 year (%)	3895	2.74	1.08	1.93	2.34	3.49
Risk free rate 7 year (%)	3895	3.17	0.89	2.66	3.07	3.74
Firm equity volatility	3895	0.45	0.22	0.30	0.40	0.53
Firm market leverage	3895	0.634	1.168	0.104	0.272	0.611
CEO variables						
Inside debt (thousands of dollars)	3895	2151	5478	0	89	1670
Inside equity (thousands of dollars)	3895	66566	175478	5918	15504	43448

Table 3.1 – continued

	N	Standard Deviation	Mean	25 Percentile	50 Percentile	75 Percentile
Debt pay-performance sensitivity (%)	3895	0.23	0.62	0.00	0.01	0.20
Equity pay-performance sensitivity (%)	3895	2.97	5.49	0.47	1.06	2.47
Debt to equity pay-performance sensitivity	3895	0.17	0.26	0.00	0.01	0.27
Panel C: firm years with a debt bias						
Firm level variables						
Outstanding debt (millions of dollars)	1552	2139	4514	100	500	1844
Outstanding equity (millions of dollars)	1552	13394	26972	1173	3211	10875
Risk free rate 5 year (%)	1552	2.77	1.08	1.93	2.34	3.49
Risk free rate 7 year (%)	1552	3.20	0.88	2.66	3.07	3.74
Firm equity volatility	1552	0.35	0.18	0.23	0.31	0.43
Firm market leverage	1552	0.246	0.519	0.053	0.150	0.282
CEO variables						
Inside debt (thousands of dollars)	1552	12161	14889	2460	6724	15087
Inside equity (thousands of dollars)	1552	34609	65319	7024	16167	38631
Debt pay-performance sensitivity (%)	1552	8.80	22.92	0.52	1.34	3.71
Equity pay-performance sensitivity (%)	1552	1.01	2.51	0.20	0.50	0.96
Debt to equity pay-performance sensitivity	1552	17.60	175.97	1.59	2.81	6.34

Table 3.2: Investment distortions for a sample firm with mean values of outstanding debt, outstanding equity, and firm equity volatility. This table contains the underinvestment and overinvestment cutoff values for a sample firm with mean values of outstanding debt, outstanding equity, and firm volatility. The debt to equity ratio is 23% for the firm that takes the mean values of outstanding debt and outstanding equity. The firm's mean equity volatility is 42%. We present the investment distortions for a firm with mean values of outstanding debt, outstanding equity, and firm equity volatility. We assume that the project results in a 10% change of volatility from the firm's original volatility and the firm's debt has a maturity of 5 years.

	90% of original volatility	90% of original volatility	110% of original volatility	110% of original volatility
Hypothetical debt to equity pay performance sensitivity	Underinvestment cutoff value (millions of dollars)	Underinvestment cutoff value / market value of the firm (%)	Overinvestment cutoff value (millions of dollars)	Overinvestment cutoff value/ market value of the firm (%)
0	6.11	0.065	-10.16	-0.108
0.1	5.50	0.058	-9.14	-0.097
0.2	4.89	0.052	-8.12	-0.086
0.3	4.27	0.045	-7.10	-0.075
0.4	3.66	0.039	-6.08	-0.065
0.5	3.05	0.032	-5.06	-0.054
0.6	2.44	0.026	-4.05	-0.043
0.7	1.83	0.019	-3.03	-0.032
0.8	1.22	0.013	-2.02	-0.021
0.9	0.61	0.006	-1.01	-0.011
1	0	0	0	0
Hypothetical debt to equity pay performance sensitivity	Overinvestment cutoff value (millions of dollars)	Overinvestment cutoff value/ market value of the firm (%)	Underinvestment cutoff value (millions of dollars)	Underinvestment cutoff value / market value of the firm (%)
2	-6.09	-0.065	10.04	0.107
3	-12.15	-0.129	19.95	0.212
4	-18.19	-0.193	29.75	0.316
5	-24.20	-0.257	39.44	0.419
6	-30.19	-0.321	49.01	0.521
7	-36.16	-0.385	58.48	0.622
8	-42.11	-0.448	67.84	0.721
9	-48.03	-0.511	77.09	0.820
10	-53.93	-0.574	86.25	0.917

Table 3.3: Summary statistics for investment distortions for firms with observed debt and equity incentives.
This table presents summary statistics of investment distortions for firms with observed debt and equity incentives. Panel A presents results for firm years with equity bias and Panel B presents results for firm years with debt bias. We present results when the project results in a 10% or a 20 % change of volatility from the firm's original volatility. We assume that the firm's debt has a maturity of 5 or 7 years.

	N	Standard Deviation	Mean	25 Percentile	50 Percentile	75 Percentile
Panel A: firm years with an equity bias						
Debt to equity pay-performance sensitivity	3895	0.170	0.261	0.000	0.012	0.265
0.9 times firm volatility / 5 years						
Underinvestment cutoff value (millions of dollars)	3895	12.434	62.876	0.004	0.410	4.892
Underinvestment cutoff value / firm value (%)	3895	0.343	0.688	0.000	0.023	0.304
1.1 times firm volatility / 5 years						
Overinvestment cutoff value (millions of dollars)	3895	-13.245	60.431	-6.197	-0.731	-0.014
Overinvestment cutoff value / firm value (%)	3895	-0.359	0.651	-0.386	-0.040	0.000
0.8 times firm volatility / 5 years						
Underinvestment cutoff value (millions of dollars)	3895	24.002	127.672	0.004	0.603	8.441
Underinvestment cutoff value / firm value (%)	3895	0.668	1.413	0.000	0.033	0.529
1.2 times firm volatility / 5 years						
Overinvestment cutoff value (millions of dollars)	3895	-27.240	118.202	-13.513	-1.865	-0.054
Overinvestment cutoff value / firm value (%)	3895	-0.732	1.268	-0.842	-0.101	-0.002
0.9 times firm volatility / 7 years						
Underinvestment cutoff value (millions of dollars)	3895	16.177	67.250	0.028	1.045	8.060
Underinvestment cutoff value / firm value (%)	3895	0.433	0.732	0.001	0.059	0.514
1.1 times firm volatility / 7 years						
Overinvestment cutoff value (millions of dollars)	3895	-16.958	63.518	-9.492	-1.658	-0.077
Overinvestment cutoff value / firm value (%)	3895	-0.444	0.689	-0.595	-0.092	-0.003

Table 3.3 – continued

	N	Standard Deviation	Mean	25 Percentile	50 Percentile	75 Percentile
0.8 times firm volatility / 7 years						
Underinvestment cutoff value (millions of dollars)	3895	31.476	138.220	0.035	1.689	14.653
Underinvestment cutoff value / firm value (%)	3895	0.850	1.511	0.001	0.092	0.932
1.2 times firm volatility / 7 years						
Overinvestment cutoff value (millions of dollars)	3895	-34.597	123.557	-20.112	-3.900	-0.235
Overinvestment cutoff value / firm value (%)	3895	-0.896	1.337	-1.231	-0.221	-0.010
Panel B: firm years with a debt bias						
Debt to equity pay-performance sensitivity	1552	17.598	175.973	1.594	2.810	6.343
1.1 times firm volatility / 5 years						
Underinvestment cutoff value (millions of dollars)	1552	5.698	27.769	0.000	0.040	1.761
Underinvestment cutoff value / firm value (%)	1552	0.200	0.834	0.000	0.001	0.056
0.9 times firm volatility / 5 years						
Overinvestment cutoff value (millions of dollars)	1552	-4.556	25.707	-0.934	-0.009	0.000
Overinvestment cutoff value / firm value (%)	1552	-0.170	0.778	-0.027	0.000	0.000
1.2 times firm volatility / 5 years						
Underinvestment cutoff value (millions of dollars)	1552	12.758	57.668	0.001	0.150	4.621
Underinvestment cutoff value / firm value (%)	1552	0.432	1.719	0.000	0.004	0.146
0.8 times firm volatility / 5 years						
Overinvestment cutoff value (millions of dollars)	1552	-8.171	49.295	-1.342	-0.011	0.000
Overinvestment cutoff value / firm value (%)	1552	-0.312	1.495	-0.038	0.000	0.000
1.1 times firm volatility / 7 years						
Underinvestment cutoff value (millions of dollars)	1552	9.253	35.521	0.002	0.203	4.235
Underinvestment cutoff value / firm value (%)	1552	0.287	0.989	0.000	0.006	0.139
0.9 times firm volatility / 7 years						
Overinvestment cutoff value (millions of dollars)	1552	-7.349	32.369	-2.664	-0.077	0.000

Table 3.3 – continued

	N	Standard Deviation	Mean	25 Percentile	50 Percentile	75 Percentile
Overinvestment cutoff value / firm value (%)	1552	-0.247	0.933	-0.085	-0.002	0.000
1.2 times firm volatility / 7 years						
Underinvestment cutoff value (millions of dollars)	1552	20.728	74.939	0.012	0.647	10.069
Underinvestment cutoff value / firm value (%)	1552	0.613	2.025	0.000	0.020	0.325
0.8 times firm volatility / 7 years						
Overinvestment cutoff value (millions of dollars)	1552	-13.119	61.912	-4.082	-0.102	0.000
Overinvestment cutoff value / firm value (%)	1552	-0.455	1.803	-0.128	-0.003	0.000

Table 3.4: Summary statistics for investment distortions for firms one year from now.
This table presents summary statistics of investment distortions (the underinvestment and overinvestment cutoff values) that firms face when we assume that the firm's asset one year from follow a normal distribution with mean and variance. We present results for firms with observed debt and equity incentives. Panel A presents results for firm years with equity bias and Panel B presents results for firm years with debt bias. We present results when the project results in a 10% change of volatility from the firm's original volatility. We assume that the firm's debt has a maturity of 7 years.

Variable	N	Mean	Standard deviation	25th percentile	50th percentile	75th percentile
Panel A: firm years with an equity bias						
0.9 times firm volatility / 7 years						
Mean underinvestment cutoff value	3895	15.271	62.832	0.037	1.110	7.595
Mean underinvestment cutoff value / firm value	3895	0.436	0.703	0.002	0.080	0.559
1.1 times firm volatility / 7 years						
Mean overinvestment cutoff value	3895	-16.029	59.680	-9.015	-1.608	-0.089
Mean overinvestment cutoff value / firm value	3895	-0.443	0.660	-0.618	-0.113	-0.004
Panel B: firm years with a debt bias						
1.1 times firm volatility / 7 years						
Mean underinvestment cutoff value	1552	8.880	33.319	0.003	0.239	4.189
Mean underinvestment cutoff value / firm value	1552	0.304	1.018	0.000	0.009	0.161
0.9 times firm volatility / 7 years						
Mean overinvestment cutoff value	1552	-7.042	29.635	-2.834	-0.102	-0.001
Mean overinvestment cutoff value / firm value	1552	-0.264	0.934	-0.108	-0.004	0.000

Table 3.5: Summary statistics for investment distortions for firms with double current debt outstanding.
This table presents summary statistics of investment distortions (the underinvestment and overinvestment cutoff values) that firms face when we assume that the firm's debt level is twice as much as current face value. We present results for firms with observed debt and equity incentives. Panel A presents results for firm years with equity bias and Panel B presents results for firm years with debt bias. We present results when the project results in a 10% change of volatility from the firm's original volatility. We assume that the firm's debt has a maturity of 7 years.

Variable	N	Mean	Standard deviation	25th percentile	50th percentile	75th percentile
Panel A: firm years with an equity bias						
0.9 times firm volatility / 7 years						
Uderinvestment cutoff value	3895	22.937	108.703	0.054	1.600	10.551
Uderinvestment cutoff value / firm value	3895	0.432	0.757	0.002	0.071	0.473
1.1 times firm volatility / 7 years						
Oerinvestment cutoff value	3895	-24.164	103.193	-12.747	-2.393	-0.135
Oerinvestment cutoff value / firm value	3895	-0.451	0.715	-0.554	-0.108	-0.005
Panel B: firm years with a debt bias						
1.1 times firm volatility / 7 years						
Uderinvestment cutoff value	1552	12.743	47.156	0.005	0.363	6.388
Uderinvestment cutoff value / firm value	1552	0.315	1.111	0.000	0.009	0.157
0.9 times firm volatility / 7 years						
Oerinvestment cutoff value	1552	-10.004	41.524	-4.014	-0.143	-0.001
Oerinvestment cutoff value / firm value	1552	-0.266	1.009	-0.102	-0.003	0.000

Table 3.6: Summary statistics for investment distortions for firms with half current equity market value.
This table presents summary statistics of investment distortions (the underinvestment and overinvestment cutoff values) that firms face when we assume that the firm's equity market value is half of current equity value. We present results for firms with observed debt and equity incentives. Panel A presents results for firm years with equity bias and Panel B presents results for firm years with debt bias. We present results when the project results in a 10% change of volatility from the firm's original volatility. We assume that the firm's debt has a maturity of 7 years.

Variable	N	Mean	Standard deviation	25th percentile	50th percentile	75th percentile
Panel A: firm years with an equity bias						
0.9 times firm volatility / 7 years						
Uderinvestment cutoff value	3895	11.469	54.351	0.027	0.800	5.275
Uderinvestment cutoff value / firm value	3895	0.432	0.757	0.002	0.071	0.473
1.1 times firm volatility / 7 years						
Oerinvestment cutoff value	3895	-12.082	51.597	-6.374	-1.197	-0.067
Oerinvestment cutoff value / firm value	3895	-0.451	0.715	-0.554	-0.108	-0.005
Panel B: firm years with a debt bias						
1.1 times firm volatility / 7 years						
Uderinvestment cutoff value	1552	6.372	23.578	0.002	0.182	3.194
Uderinvestment cutoff value / firm value	1552	0.315	1.111	0.000	0.009	0.157
0.9 times firm volatility / 7 years						
Oerinvestment cutoff value	1552	-5.002	20.762	-2.007	-0.072	0.000
Oerinvestment cutoff value / firm value	1552	-0.266	1.009	-0.102	-0.003	0.000

APPENDIX A

VARIABLE CONSTRUCTION AND DEFINITIONS

In this appendix, we describe how the variables used in this dissertation are constructed and details about the data used in the construction.

Table A.1: Variable definition.

Variable Name	Variable Definition
Four-factor-model abnormal return	Raw return minus four-factor-model expected return.
CAPM abnormal return	Raw return minus CAPM expected return.
Market capitalization	The market value of equity, price times shares outstanding. Absolute value of (PRC*SHROUT).
Research and development expense	Research and development expense scaled by total assets. XRD/AT
Capital expenditure	The firm's capital expenditure scaled by total assets. CAPX/AT
Tangibility	The firm's property plant and equipment scaled by total assets. PPENT/AT
Return on assets	The firm's income before extraordinary item scaled by total assets. IB/AT
Profitability	The firm's operating income before depreciation scaled by total assets. OIBDP/AT
Net income	The firm's net income scaled by total assets. NI/AT
Book-to-market	The firm's book equity scaled by market value of equity. The book equity is calculated as common equity plus deferred taxes and investment tax credit minus the first non-missing item from preferred stock liquidating value, preferred stock redemption value, and preferred stock. BE = CEQ + TXDITC PS PS is defined as the first non-missing item from 1) PSTKL, 2) PSTKRV, 3) PSTK. BE / (PRCC_F*CSHO)
Log(sales)	The natural log of the firm's sales. Log (SALE)
Log(assets)	The natural log of the firm's total assets. Log (AT)
Dividend payout ratio	The firm's common stock dividends, preferred stock dividends and the purchase of

Table A.1 – continued

Variable Name	Variable Definition
	common stock and preferred stock scaled by operating income before depreciation. (DVC + DVP + PRSTKC) / OIBDP
Last 12 months return	The excess holding return 12 months preceding the natural disaster event date. $(1+RET_{-1})*(1+RET_{-2})*(1+RET_{-3})*(1+RET_{-4})*(1+RET_{-5})$ $*(1+RET_{-6})*(1+RET_{-7})*(1+RET_{-8})*(1+RET_{-9})*(1+RET_{-10})$ $*(1+RET_{-11})*(1+RET_{-12})-1$, where RET_{-i} represent monthly return i months before the event date.

APPENDIX B

CALCULATE CUMULATIVE RETURN AND ASSOCIATED T-STATISTICS

Let Return stand for any return that we use in the paper, for example four-factor-model abnormal return, CAPM abnormal return or raw return. Cumulative return for an event window is defined as the sum of each daily return for the particular event window. For example, for event window from $t = l$ to $t = k$, $Cumulative\ return_{l\ to\ k} = Return_l + Return_{l+1} + \cdots + Return_{k-1} + Return_k$. The corresponding t-statistic that measures whether the cumulative return is significantly different from zero over the $t = l$ to $t = k$ event window is calculated using the dependence adjustment method as described by Brown and Warner (1985) with a holdout period $t = l - 30$ to $t = l - 1$ (we use 30 time periods in this paper). Specifically, we first calculate the variance of Return in the holdout period and let $\sigma^2_{holdout}$ stand for it. And the t-statistic is specified as: $Cumulative\ return_{l\ to\ k}$ / square root of $(\sigma^2_{holdout} * (k - l + 1))$.

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