1-2018

Three essays on corporate finance

Yuan ZHUANG

Singapore Management University, yuan.zhuang.2013@pbs.smu.edu.sg

THREE ESSAYS ON CORPORATE FINANCE

YUAN ZHUANG

SINGAPORE MANAGEMENT UNIVERSITY
2018

Three Essays on Corporate Finance

by
Yuan Zhuang

Submitted to Lee Kong Chian School of Business in partial fulfillment of the requirements for the Degree of Philosophy in Business (Finance)

<u>Dissertation Committee:</u>

Rong Wang (Supervisor/Chair)
Associate Professor of Finance
Singapore Management University

Roger K. Loh
Associate Professor of Finance
Singapore Management University

Fangjian Fu
Associate Professor of Finance
Singapore Management University

Xia Chen
Professor of Accounting
Singapore Management University

Singapore Management University
2018

ABSTRACT

Three Essays on Corporate Finance

Yuan Zhuang

My dissertation aims to better understand managers and financial analysts' behavior, incentives, and constraints, as well as their impacts on firm decisions and financial markets. In Chapter 1, I show that peer firms play an important role in determining U.S. corporate cash saving decisions. Using an instrument variable identification strategy, I find that one standard deviation change in peer firms average cash savings leads to a 2.63% same-direction change in firm's own cash savings, which exceeds the marginal effects of many previously identified determinants. The economic implications of such peer effects are large, which can significantly alter cash savings in a representative industry by 7.2%. In cross-sectional tests, I find that peer effects are stronger when the product market is highly competitive and when the economy is in recession. In addition, less powerful, smaller, and financially constrained firms respond more actively to their peers' cash saving decisions. Finally, I provide evidence that such peer effects are asymmetric — cash-rich firms, who already hold enough cash, are less likely to mimic peers' cash policies compared to cash-starved firms.

A recent strand of literature on stock market feedback examines how agents extract information from stock prices when making decisions. In Chapter 2, we investigate how analysts learn about the quality of their research from the stock-price reaction to

their reports. I find evidence of analyst learning from the stock market when there is a strong price reaction to their recommendation changes. Recently impactful analysts are more likely to issue recommendation changes and increase their total recommendation activity in the next period. These feedback effects are short-lived and also exist at the broker level, in which brokers with more influential recommendation changes in a month become more active in revising recommendations next month. Our results imply that short-term information in recently successful analyst reports gets incorporated with a lag to the rest of the coverage universe. A calendar-time strategy that seeks to benefit from such predictable spillover can earn abnormal returns of up to 0.6% per month.

Companies are run by a team of top managers. However, the literature normally focuses on CEO when studying managerial influence on firm decision-makings. In Chapter 3, I aim to examine the role of other senior managers. Specifically, the effect of non-CEO managers' over-optimism is studied, and it is found that other top managers are at least as important as CEOs in corporate decisions. The study shows that only the firms with both overoptimistic CEOs and overoptimistic non-CEO manager teams would make more investment, use more debt financing, and are less likely to pay dividends. Furthermore, overoptimistic CEOs need help of other overoptimistic senior managers in translating the growth opportunities into firm value, only overoptimistic CEOs alone cannot achieve such success. This result is consistent with the recent literature which documents the bright side of managerial over-optimism.

TABLE OF CONTENTS

Table of Contents

Acknowledgements v

1. Do Peer Firms Affect Corporate Cash Saving Decisions? 1

1.1 Introduction 1

1.2 Data and descriptive statistics 7

1.3 Identification of causal peer effect 9

1.3.1 Linear-in-means model 9

1.3.2 Identification strategy: IV-Peer firm relative idiosyncratic volatility... 11

1.3.3 Instrumental variable validity 14

1.4 Empirical results: IV estimation of peer effects in cash saving decisions ... 15

1.4.1 Main results.. 15

1.4.2 Robustness tests 18

1.5 Economic mechanisms of peer effects in cash saving decisions 21

1.5.1 Economic mechanisms.. 21

1.5.2 Evidence on the economic mechanisms 23

1.5.3 Is peer effect on cash saving decisions symmetric?.............................. 26

1.6 Economic implications of peer effects in cash savings............................... 27

1.6.1 Excess-variance identification strategy... 28

1.6.2 Results of excess-variance tests 31

1.7 Conclusion 32

2. Getting Feedback on Your Research: Evidence from Analaysts 34

2.1 Introduction 34

2.2 Data and sample 40

2.2.1 Analyst data 40

2.2.2 Descriptive statistics 42

2.3 Feedback effect of past success 44

2.3.1 Future recommendation change probability 44

2.3.2 Future total recommendation activity 49

2.3.3 Robustness tests 50

2.4 Is the feedback effect driven by learning about information or skill? 51

2.4.1 Feedback effect: Different horizons... 51

2.4.2 Feedback effect at the broker level 53

2.5 Additional tests: Evidence on capacity constraints 55

2.5.1 Are future recommendation changes still influential? 56

2.5.2 Feedback effect pre- and post-Reg FD 58

2.6 Additional tests and alternative hypotheses 59

2.6.1 Feedback effects for “failures” 59

2.6.2 Alternative hypothesis: Investor overreaction to past success.............. 60

2.6.3 A trading strategy that benefits from the feedback effect..................... 61

2.7 Conclusion 65

3. Is the CEO Solely Responsible for Corporate Decisions? 68

3.1 Introduction 68

3.2 Data and descriptive statistics 75

3.2.1 Measure of manager over-optimism 75

3.2.2 Classification of firms based on the manager over-optimism 78

3.2.3 Descriptive statistics 79

3.3 Econometric specifications 80

3.4 Main results 82

3.4.1 Investment and manager over-optimism 82

3.4.2 Financing choice and manager over-optimism: Debt vs. equity 87

3.4.3 Dividend payout and manager over-optimism 88

3.5 Robustness tests 90

3.5.1 Corporate governance effect 90

3.5.2 The effect of CEO power 91

3.5.3 Matching between manager over-optimism and firm decisions 92

3.5.4 Private information 93

3.5.5 Risk tolerance 94

3.6 Overoptimistic managers and firm value 94

3.7 Conclusion 96

References 98

Appendices 112

Appendix A Tables for Chapter 1 112

Appendix B Tables for Chapter 2 122

Appendix C Tables for Chapter 3 136

ACKNOWLEDGEMENTS

Writing this thesis was a challenging and fascinating process during which I had the chance to work and most importantly learn from many people. I would like to take this opportunity to express my gratitude.

First and foremost, my sincerest gratitude goes to my advisor Professor Rong Wang for her continuous encouragement, support, and valuable guidance. In the past few years, she gave me countless great suggestions on my research, our joint work, and my development as a scholar. Professor Wang led me into the door of academic research and provided me with numerous opportunities.

I would also like to thank my dissertation committee members, Professor Roger Loh, Fangjian Fu and Xia Chen. I deeply appreciate Professor Loh, who has been an outstanding mentor, example, and co-author to me. I am also very thankful to Professor Fu, whose comments and suggestions have significantly sharpened my thoughts and enriched my research. Further, comments from faculty in SMU, seminar speakers, and discussants were important in improving my work. I especially thank Professor Gennaro Bernile, Xiaodan Gao, Jarrad Harford, Sheng Huang and Hao Liang for their helpful suggestions.

Finally, my most heartfelt appreciation goes to my beloved family for their tremendous support, endless care, and unconditional love. To my parents, who have given me both roots and wings and have supported me in all my pursuits. To my husband Yueqi who has been my source of happiness and inspiration, and whose faithful support has been so appreciated. Also to my friends, thank you for being there whenever in need.

CHAPTER 1

DO PEER FIRMS AFFECT CORPORATE CASH SAVING DECISIONS?

1.1 Introduction

Firms do not operate in isolation, studies have uncovered many roles for peer groups in affecting various corporate policies (i.e., Shue (2013), Leary and Roberts (2014), Popadak (2017)). A recent strand of literature emphasizing the "strategic" role of cash implies that peer effects may matter for corporate cash policies.[1] Cash can help firms to finance competitive strategies, signal the possibility of aggressive behaviors, and protect firms from predation risk induced by the rivals. Therefore, paying attention to peers' cash saving decisions would enable firms to better understand the potential opportunities and risks, and then adjust their own cash accordingly. In this paper, I examine whether firm's cash changing behavior is influenced by peer effects. I also study the economic forces that might explain the existence of such peer effects.

Fresard (2010) shows that large cash reserves will lead to future market share gains at the expense of industry rivals. Therefore, a firm will face greater predation risk in the product market when its peer firms increase their cash holdings. Such threat can also spur the firm to hold more cash, since Haushalter, Klasa, and Maxwell (2007) find evidence that the extent to which a firm is exposed to product market risk is positively

[1] See Haushalter, Klasa, and Maxwell (2007), Fresard (2010), Lyandres and Palazzo (2012), Hoberg, Phillips, and Prabhala (2014), and Lyandres and Palazzo (2015).

associated with the amount of its cash holdings. On the other hand, when peer firms decrease their cash holdings, the firm may also find it optimal to hold less cash, because high level of extra cash is always related to the high opportunity cost and potential agency problems, it is inefficient for the firm to hold much more cash than that of peers.

The identification of peer effect is empirically challenging (see the "reflection problem" in Manski (1993)). Contextual and correlated effects are two economic forces that also induce firms to behave like their peers. *Contextual effects* are the propensity of an individual firm to change cash holdings in some way that varies with the exogenous characteristics of the industry peer group. For example, cash saving tends to vary with the average investment expenditures or growth opportunities of other firms in the same peer groups. *Correlated effects* wherein individual firms in the same reference group tend to behave similarly when they have similar/correlated firm-specific characteristics or face common institutional environments. For example, correlated effect occurs when firms change their cash ratio together because of financial crisis. These alternative industry effects, endogenous selection, or spurious correlation cannot be interpreted as causal interactions.

To address identification problem, I use the lagged relative idiosyncratic stock volatility (firm's own idiosyncratic stock volatility minus industry median idiosyncratic stock volatility) of peer firms as an instrument for peer firms' average cash savings. A valid instrument should be associated with the cash savings of peer firms, and it should not be driven by common factors. Existing studies document the relevance of lagged idiosyncratic stock volatility and firm cash savings (e.g., Riddick and Whited (2009), and Panousi and Papanikolaou (2012)). These studies find that an increase in uncertainty leads to an increase in corporate cash savings, which is consistent with precautionary motivation of holding cash. Similarly, when the average of the peer firms' idiosyncratic stock volatility increases, the average cash savings across peer firms should also increase. On the other hand, each firm's relative idiosyncratic stock volatility is unpredictable, distinct from industry stock volatility, and only captures firm-specific shocks. Consequently, other firms' relative idiosyncratic stock volatility cannot be directly linked to a firm's own cash saving decisions. This indirect relationship makes peers' lagged relative idiosyncratic stock volatility an ideal candidate for an instrumental variable because it likely satisfies the exclusion restriction. Taken together, my primary identification assumption is that, one-period-lagged relative idiosyncratic stock volatility across peer firms is correlated with their average cash savings, but it is orthogonal to common industry-wide and market-wide shocks, which cannot directly influence the firm's own cash savings.

Two-stage least square estimation (2SLS) shows that peer effects are statistically significant and economically meaningful in influencing corporate cash savings. The estimated marginal effect of peer influence is larger than many previously identified determinants, such as real size, market-to-book ratio, net equity issuance, net debt issuance, and the last period idiosyncratic stock volatility. Specifically, one standard deviation increase in the average cash savings of peer firms would lead to the 2.63% increase in a firm's own cash savings. The reverse is also true, that one standard deviation decrease in the peers' average cash savings would lead to the 2.63% decrease in a firm's own cash saving. In addition, the results continue to hold when I further control for cash mean-reverting dynamics, when I use an alternative definition of peer groups, and when I restrict the sample to the US domestic firms or the periods where cash trend disappears.

Having documented the existence, magnitude, and direction of the peer effect on cash savings decisions, I investigate the underlying mechanisms to better understand why peer effect matters for cash saving decisions. There are two theories related to the peer effects: rivalry-based theory and information-based theory. The rivalry-based theory regards imitation as a response designed to mitigate competitive rivalry or risk (see Lieberman and Asaba (2006)). A firm that imitates peers' cash policies could alleviate competitive risk from the aggressive actions of rivals, and hence maintain its relative position in the product market. On the other hand, imitating peers cash policies can not only make firms keeping their competitiveness, but at the same time make them avoid holding so much cash that is always related with high opportunity cost and potential agency problem. Therefore, if rivalry-based theory works for cash-saving peer effects, the learning behavior would be more pronounced in the competitive industries. The information-based theory explains peer effects from the aspects of social learning and reputation concern, where mimicking the cash policies of peer firms is an efficient approach when managers are unsure of the optimal amount of cash maintained within firms, or if direct analysis is difficult, costly, and time-consuming, or if a manager wants to avoid his/her bad reputation. Therefore, some less powerful firms might be more likely to imitate peers' cash policies, or it is more likely to observe the peer effect in bad time, say, financial crisis periods.

I extend the instrumental variable analyses to test both theories by interacting the peer firms' average cash savings with dummy variables indicating economic status, product market competitiveness, and some firm-specific characteristics, such as firm market power and financial conditions. The interaction term is also endogenous and instrumented for the peer firms' lagged average relative idiosyncratic stock volatility interacted with the indicators. The cross-sectional tests suggest that rivalry-based and information-based mechanisms are both economically important. Firms facing a more competitive environment, with less market power, as well as smaller and financially constrained firms are more sensitive to the cash policies of peer firms. I also find that the peer effect is more pronounced during economic recessions, which further supports the information-based channel. As the increased uncertainty in bad times make it harder for managers to determine firms' cash policies, learning from peers might be an efficient way for them to do so. Furthermore, I find that peer effects in cash savings are not symmetric where cash-rich firms, who had already held enough cash, are less likely to mimic peers' cash policies compared to cash-insufficient firms.

Finally, I examine the economic implications of peer effects in cash savings. Peer effect is the economic externality whereby changes to one firm affect the outcomes of other firms. If only one manager in an industry mimic its competitors' cash saving decisions, then it is very likely that other forces will pull it back and force a correction. However, if peer learning is common in an industry, this may lead to significant changes in the industry overall cash savings. By using an excess-variance test pioneered by Graham (2008), I find that peer effects can explain some of the variations in cash savings observed across industries.[2] To understand the economic magnitude, consider an industry with an expected cash change by 2% under the assumption of no peer

[2] I thank Professor Bryan S. Graham for making his sample code with regard to identifying social interactions through excess variance contrasts available online.

influence, the observed cash changes in that industry will be between 1.74% and 2.26% when peer effect exists.

The primary contribution of this paper is to provide new insights on corporate cash saving decisions. A large volume of the current literature is dedicated to understanding a firm's cash savings from growth and precautionary aspects. Prominent examples of those types of studies include Almeida, Campello, and Weisbach (2004), Acharya, Almeida, and Campello (2007), Dasgupta, Noe, and Wang (2011), Riddick and Whited (2009), Palazzo (2012), and Fresard (2012). These studies support the evidence that a firm's saving decisions are driven by the managers' expectations of future investment opportunities and future cash flow risk. In this paper, I argue that a firm's cash saving decisions are not independently determined; rather, the cash policies of peer firms also play an important role.

My study also highlights the strategic role of corporate cash holdings by demonstrating that firms facing greater product market competition pressures respond more actively to the cash policies of peer firms. Keeping close look at the peers' cash holding decisions could neutralize the aggressive actions of its competitors and maintain its relative position. Fresard (2010) shows that cash reserves could lead to systematic future market share gains and affect industry rivals' entry or expansion. Haushalter, Klasa, and Maxwell (2007) and Hoberg, Phillips, and Prabhala (2014) both indicate that a firm's cash holdings are significantly affected by predatory threats from rivals. Lyandres and Palazzo (2016) further stresse the importance of strategic considerations in shaping cash policies in innovative firms. Although this study provides some evidence regarding how two closest innovation firms' cash holding choices are interacted with each other, peer effect was not the purpose of their study. Considering the manifold uses of cash, I provide empirical evidence of general peer effect and find different results.

Last but not the least, this paper complements a growing body of literature that examines the peer effects in a number of corporate policies, such as capital structure decisions (Leary and Roberts (2014)), executive compensation and managerial decisions (Shue (2013) and Bizjak, Lemmon, and Naveen (2008)), dividends and share repurchases (Popadak (2017) and Massa, Rehman, and Vermaelen (2007)), firm investment decisions (Fracassi (2016) and Bustamante and Frésard (2017)), stock split decisions (Kaustia and Rantala (2015)), corporate disclosure (Seo (2016)), corporate governance (John and Kadyrzhanova (2008)), risk aversion and trust (Ahern, Duchin, and Shumway (2014)), the adoption of corporate social responsibility (Cao, Liang, and Zhan (2015)), and changes in tax paying and reporting behaviors (Bird, Edwards, and Ruchti (2016)). I contribute to this line of studies by providing empirical evidence of peer effects in corporate cash savings.

The paper proceeds as follows. Section 2 discusses the sample and descriptive statistics; Section 3 details the instrumental variable identification strategy and shows the main results as well as robustness checks. Section 4 explores the underlying mechanisms of peer effects; Section 5 examines the economic implication of cash-saving peer effect by studying the total incidence of peer effects at the industry level, and Section 6 concludes.

1.2 Data and descriptive statistics

This paper analyzes the cash saving decisions of U.S. firms publicly traded on the

New York Stock Exchange (NYSE), the American Stock Exchange (AMEX), and the NASDAQ. Firms' accounting data come from the Compustat database from the year 1980 through 2014. Stock return data for our sample of firms are obtained from the Center for Research in Security Prices (CRSP) daily stock price database. The data on lines of credit are from Capital IQ. Text-based network industry classification (TNIC), product market fluidity, and TNIC HHI (Herfindahl-Hirschman Index) are provided by Hoberg and Phillips in their website.[3] I exclude firms in financial industries (SIC code 6000-6999), utilities industries (SIC code 4900-4999), and government entities (SIC code greater than or equal to 9000). To ensure consistency throughout primary analysis, I require each firm-year observation to have non-missing data for the explanatory variables in each empirical model. To reduce the effect of outliers, all ratios are winsorized at the first and ninety-ninth percentile.

Table 1.1 presents descriptive statistics for the main variables in the final sample of 94,085 firm-year observations (9419 distinct firms) for the empirical analyses. The number of observations varies in different tests depending on the availability of data. I define peer groups for the primary analyses based on three-digit SIC industry groups.[4] There are 202 industry groups in our sample. I also require each firm has at least five other peer firms in each year.[5] Below, I also employ, for robustness, text-based network industry classification (TNIC) peer group definition that relying on the similarity of product characteristics (Hoberg and Phillips (2010), and obtain qualitatively similar

[3] I thank Professor Gordon Phillips and Professor Gerard Hoberg for making their text-based network industry classification (TNIC) data, and product market data based on their industry classification available online.

[4] The choice of three-digit SIC industry group is a balance between minimizing the possibility of grouping firms in unrelated business, and ensuring a meaningful number of peers.

[5] The results are qualitatively and quantitatively similar if the number of peer firms are not restricted.

results. In Table 1.1, I report the summary statistics for both firm-specific and peer firms' average characteristics. The peer firms' average characteristics are constructed as the equally weighted average of characteristics across all peer firms in the three-digit SIC group excluding i^{th} observation. Comparison between summary statistics for firm-specific and peer firms' average characteristics indicates that the two groups have similar mean values for most variables. At the bottom of the table, I report the number of industries and the distribution of the number of peer firms per industry-year combination. Over the entire sample, the average and median number of firms in each industry-year (peer group) are approximately 23 and 14, respectively.

1.3 Identification of causal peer effect

To test whether peer effects exist in cash saving decisions, I analyze the response of executives to peer influence based on the linear-in-means model (Manski (1993)) and use the instrumental variable strategy to estimate the causal peer effect.

1.3.1 Linear-in-means model

In this section, I first describe how linear-in-means model is applicable to test cash saving peer effects and proceed to discuss the identification strategy. Manski (1993) provides an empirical framework in estimating marginal peer effects based on a "linear-in-means" model. The model specification is as follows,

$$Y = \alpha + \beta E(Y|Z) + \gamma' E(X|Z) + \eta' X + \delta' Z + \varepsilon \tag{1}$$

where Y is an outcome variable of interest, Z are attributes characterizing a reference group, X and ε are observed and unobserved firm-specific characteristics that directly affect y. Both β and γ represent social interactions: β represents the (endogenous) peer effects wherein the propensity of a firm to behave in some way varies with the behavior of the peer group, and γ represents contextual (exogenous) effects wherein the propensity of a firm to behave in some way varies with the exogenous characteristics of the peer group, respectively (Manski (1993)). The reason why it is called the linear-in-means model is that the mean regression of y on X and Z has the linear form:

$$E(Y|X,Z) = \alpha + \beta E(Y|Z) + \gamma' E(X|Z) + \eta' X + \delta' Z \,. \tag{2}$$

I rewrite the equation (4) to apply it to peer effects (β) in corporate cash saving decisions, that is,

$$\Delta Cash_{ijt} = \alpha + \beta \overline{\Delta Cash}_{-ijt} + \eta \bar{X}_{-ijt} + \gamma X_{ijt} + \mu_i + \theta_t + \varepsilon_{ijt} \tag{3}$$

where $\Delta Cash_{ijt}$ represents cash changings for firm *i* in industry *j* in year *t*. The (endogenous) peer effect is captured by the effect of $\overline{\Delta Cash}_{-ijt}$, which is defined as the peer firms' average cash savings excluding firm *i* in industry *j* in year *t*. X_{ijt} and $\bar{X}_{-ijt}$ are vectors of firm-specific and peer firms' average characteristics (i.e., common and contextual effects) that influence the changes in cash holdings. ε_{ijt} is the firm-year specific error component. Firm fixed effects μ_i is included to control for omitted firm-specific factors that potentially influence cash saving decisions, which also allows me to identify within-firm variation in cash saving decisions and mitigates the concern on the "sticky" cash. I also include year fixed effect, θ_t, to control for unmeasured macro shocks.[6] In the model, the peer firms average cash saving variable is measured contemporaneously, which makes the identification of causal peer effect more difficult because it limits the amount of time for firms to respond to one another. Also, the

[6] If I include industry fixed effects and year fixed effects, the results are quantitatively and qualitatively similar.

measurement mitigates the scope for possible confounding effects resulting from other changes related to the firm's cash saving decisions (Leary and Roberts (2014)).

1.3.2 Identification strategy: IV-Peer firm relative idiosyncratic volatility

As mentioned in the introduction part, the main identification problem arises when I try to infer whether the average behavior in reference group influences the behavior of individual members that comprise the group. It is called the "reflection problem" in Manski (1993), as he explains that "the reflection problem is similar to that of interpreting the almost simultaneous movements of a person and his reflection in a mirror". Thus, an OLS regression could not provide the evidence of (endogenous) peer effects (Manski (1993), Angrist (2014)).

To address the identification problem, I use the lagged relative idiosyncratic stock volatility across peer firms as a source of exogenous variation in peer firms average cash savings. According to the cash saving literature, idiosyncratic stock volatility is a determinant of changes in cash holdings. For example, Riddick and Whited (2009) show that firms facing more uncertainty have a higher marginal propensity to save from their operating income. In addition, by regressing changes in cash on the last period idiosyncratic stock volatility, Panousi and Papanikolaou (2012) also find that an increase in uncertainty leads to an increase in corporate cash savings, which is consistent with precautionary motivation of holding cash. Similarly, when the average of the peer firms' idiosyncratic stock volatility increases, the average cash savings across peer firms should also increase.

Although the average value of idiosyncratic stock volatility across peer firms satisfies the correlation condition, i.e., correlates with the peer firms' average cash savings, a firm's own idiosyncratic stock volatility and peers average idiosyncratic stock volatility is likely to move together and contain some common industry information, this would go against the exclusion restriction. For example, the competition within an industry would lead to the increasing idiosyncratic stock volatility for the competitors in this industry (see Irvine and Pontiff (2009) and Philippon (2003)). Irvine and Pontiff (2009) envision a type of competition in which consumers shift their demand from firm A to firm B within an industry and induce more idiosyncratic stock volatility for these two firms. Therefore, the firm A's idiosyncratic stock volatility and firm A's peers (including firm B) average idiosyncratic stock volatility contain common factors—demand variation, which will drive the firm A's cash saving and the average cash savings across firm A's peer firms varying simultaneously, thus the identification of causal peer effects by using peers average idiosyncratic stock volatility as instrument would fail. To mitigate such concern, I construct a measure of relative idiosyncratic stock volatility based on the innovation in stock specific volatility. I follow a simple two-step procedure. First, for each firm *i* in industry *j*, I construct its relative idiosyncratic volatility, $RELIdioVol_{ijt}$ in year *t* as its actual idiosyncratic stock volatility $IdioVol_{ijt}$ minus the industry median idiosyncratic stock volatility $Median\_IdioVol_{jt}$. That is, $RELIdioVol_{ijt}$ measures for each firm in innovation in its own idiosyncratic stock volatility conditional on the industry and year. Next, I construct peer firms' average relative idiosyncratic volatility, denoted as $P\_RELIdioVol_{-ijt}$, as the equally weighted average of $RELIdioVol_{ijt}$ across all peers in the three-digit SIC group that the firm belongs. In other words, it measures for each firm the average innovation in idiosyncratic stock volatility among its peer firms. I lag this shock innovation one year $P\_RELIdioVol_{-ijt-1}$ and use it as the source of exogenous variation (instrument) for peer firms average cash savings $P\_\Delta Cash_{ijt}$.

To measure idiosyncratic stock volatility of an individual stock $IdioVol_{ijt}$, I firstly estimate equation (6) for each firm on a rolling month basis using daily returns in the past 12 months,

$$r_{ij\tau} = R_{ij\tau} - rf_\tau = \alpha_{ijt} + \beta_{ijt}^{M}(R_{m\tau} - rf_\tau) + \beta_{ijt}^{IND}(\bar{R}_{-ij\tau} - rf_\tau) + \varepsilon_{ij\tau} \qquad (4)$$

where τ is the subscript for the day and *t* is the subscript for the month, $R_{ij\tau}$ is the total return for firm *i* in industry *j* for the day τ, $\tau \in t$. $(R_{m\tau} - rf_\tau)$ is the daily excess return of market portfolio, and $(\bar{R}_{-ij\tau} - rf_\tau)$ is the daily excess return of equal-weighted industry portfolio excluding firm *i*'s return.[7,8] Then, the idiosyncratic return for each individual stock is computed as follows: [9]

$$\widehat{\varepsilon_{ij\tau}} = r_{ij\tau} - \widehat{r_{ij\tau}} = r_{ij\tau} - (\widehat{\alpha_{ijt}} + \hat{\beta}_{ijt}^{M}(R_{m\tau} - rf_\tau) + \hat{\beta}_{ijt}^{IND}(\bar{R}_{-ij\tau} - rf_\tau)). \qquad (5)$$

Next, the monthly idiosyncratic stock volatility is calculated as the standard deviation of the daily idiosyncratic stock return in that month and multiply the square root of the number of trading days in the month.[10] Moreover, to maintain consistency with the periodicity of the accounting data, I average the monthly idiosyncratic stock volatility

[7] As explained in Leary and Roberts (2014), "the last industry factor is to remove any variation in returns that is common across firms in the industry peer group, but not a priced risk factor".

[8] Consistent with the definition of peer groups in this paper, industries are defined by three-digit SIC code.

[9] For example, to construct daily idiosyncratic returns in February 1985, I estimate the equation (6) using daily returns from February 1984 to January 1985. Then using the estimated coefficients and the daily factor returns in February 1985 to compute the daily estimated residual (idiosyncratic stock return) in February 1985. To obtain daily idiosyncratic returns in March 1985, I repeat the process by updating the estimation sample from March 1984 to February 1985 and using daily factor returns during March 1985. I require at least 150 trading days in each regression. The trading days per year in my sample ranges from 150 to 255 days.

[10] I require a minimum of 15 trading days in a month. A similar procedure is used by French, Schwert, and Stambaugh (1987) and Fu (2009).

in each fiscal year to get the annualized idiosyncratic stock volatility $IdioVol_{ijt}$.

1.3.3 Instrumental variable validity

Although the exclusion restriction of instrument variable cannot be verifiable from the data, several arguments support the plausibility of satisfying the restriction. First, the instrument's construction ensures it to be orthogonal to market risk and industry risk, and unique to the specific peer firms. To further bolster this argument, I control for the industry competition and industry cash flow volatility in the following estimations, as well as the firm's own idiosyncratic stock volatility that is suggested by Leary and Roberts (2014) to absorb the remaining correlation. Second, the inclusion of a firm's own and peers average characteristics, as well as firm fixed effects and year fixed effect in the empirical regression would further mitigate the concern that peers relative idiosyncratic stock return affects corporate cash savings through its correlation with some omitted yet common factors rather than through its relevance for peer firms cash saving decisions.

Table 1.2 examines the partial correlations between peer firms' average relative idiosyncratic stock volatility and firm characteristics, to determine whether instrument contains some information about firm fundamental characteristics. The reason why it is necessary because "economically large correlation between the instrument and observable firm characteristics would raise concerns about the extent to which instrument may be correlated with unobservable factors" (Leary and Roberts (2014)). The results in Table 1.2 indicate that the economic magnitudes of the estimated coefficients are all tiny. For the only statistically significant coefficient, cash flow, a one standard deviation increase in this factor will lead to 1.63 base point increase in lagged average of peer firms' relative idiosyncratic stock volatility. Such change in instrument is about 0.009 standard deviations. Thus, to some extent, the lagged peer firms average relative idiosyncratic stock volatility contains no economically significant information related to firm's next period cash saving determinants. In addition, the correlation between firm's relative idiosyncratic stock volatility and peers average relative idiosyncratic stock volatility is -0.03, while the correlation between firm's idiosyncratic stock volatility and peers average idiosyncratic stock volatility is 0.4. The decline suggests that the method purges most of the intra-industry correlation in idiosyncratic stock volatility.

1.4 Empirical results: IV estimation of peer effects in cash saving decisions

1.4.1 Main results

In this section, I document the estimation results from the two-stage least square (2SLS) regression where the endogenous variable is the peer firms' average cash savings, and the associated instrument variable is the equal-weighted average of relative idiosyncratic stock return across peer firms in the last year $P\_RELIdioVol_{-ijt-1}$. The 2SLS regression includes firm-specific, industry-specific, and peer firms' average covariates as well as firm fixed effects and year fixed effects. The firm-specific covariates include firm size, cash flow, market-to-book ratio, as well as the sources and usage of funds from financing and investing activities in year *t*, i.e. the net equity issuance, the net debt issuance, and the net investment (Almeida, Campello, and Weisbach (2004) and Palazzo (2012)). These help to control for other factors that drive changes in cash holdings. The industry-specific covariates that associate with firm cash savings include industry competitiveness and industry cash flow volatility, which help to control for other industry dynamics that may cause changes in cash holdings. The results are presented in the Table 1.3 and reveal that peer effects in cash saving decision exist.

From the coefficients of the first-stage instrumental variable regressions reported at the bottom of Table 1.3, we can see that the instrument is strongly and positively associated with the peers average cash savings, this is consistent with the theoretical arguments on the precautionary motivation for holding cash. Statistically speaking, *Kleibergen-Paap rk wald F statistics* from the first-stage regression exceed the requisite 10 to reject the weak instrument null hypothesis (Stock and Yogo (2002)).

In terms of the second-stage results, the significantly positive coefficient of the instrumented peer firms' average cash savings in each specification supports the existence of peer effects in corporate saving decisions. To ease interpretation of magnitudes, all the independent variables included in the 2SLS regressions are standardized. Thus, the coefficient of $P\_\Delta Cash_{ijt}$ in column (1) is interpreted as follows: one standard deviation increase (decrease) in instrumented peer firms' average cash savings leads to 2.63% increase (decrease) in firm's cash savings on average. Interestingly, the peer effect for cash savings is economically meaningful and larger than many previously identified cash saving determinants. For example, a standard deviation increase in firm size only leads to cash saving increasing by 0.66%, compared to the 2.63% induced by such an increase in peer influence. This indicates that peer influence is at least as important an economic determinant of cash savings as other standard firm-specific covariates.[11]

[11] The results are quite similar if I control for industry fixed effects instead of firm fixed effects.

Although the instrument variable—peers relative idiosyncratic stock volatility has already removed the common trend of idiosyncratic volatility, I further control for the industry-specific covariates that may still influence the instrument variable and the dependent variable simultaneously, such as industry competition and industry risk. Industry competition is proxied by Herfindahl-Hirschman Index (HHI) and industry risk is measured by the industry average cash flow volatility.[12] From the results in column (2) to column (4), I find that the results are quite robust, where the estimated coefficients of peer firms' average cash savings are little affected by the inclusion of HHI and industry risk.

Opler *et al.* (1999) show that firms have target cash levels and cash holdings revert to the mean. If a firm held less cash than its target cash levels in the last year, and meanwhile its peer firms increase cash savings on average this year, it is possible that the peer effect inducing the firm to save more cash would be confounded by the firm's mean-reverting adjustment of their cash holdings to its own target cash ratio. Therefore, I further control for the firm prior-year cash savings in column (5), as well as the peer firms prior-year cash savings in column (6). The significantly negative coefficients of lagged cash savings support the mean reverting dynamics of cash holdings, and interestingly, the effect from peer firms average cash savings is still robust and become even stronger.

In contrast to the peer influence, other peer firm characteristics are less important for firm cash saving decisions and are sometimes statistically indistinguishable from

[12] The industry average cash flow volatility is calculated by following Bates, Kahle, and Stulz (2009). It is defined as the average of the firm cash flow standard deviations in each year across each three-digit SIC code.

zero. This suggests that cash saving peer effects are not simply the repackaging of peer effects associated with some other corporate policies, such as, leverage, financing, and investment. I also control for the fraction of peer firms who pay dividends in the year *t*, to exclude the possibility that the result of cash saving peer effect is the consequence of learning peers in dividend policy (see Popadak (2017)). The unreported results show that peer effect of cash savings is quite robust and is not influenced by the dividend peer effects.

Overall, the estimation results in Table 1.3 reveal the importance of peer effects in corporate cash saving decisions, these effects are economically large, significantly larger than many other cash-saving determinants.

1.4.2 Robustness tests

In this section, I check the robustness of the main results to some changes under the instrument test specification, including an alternative construction of peer groups based on the product market, two subsample tests to exclude the confounding effects of foreign cash and the trend of cash ratios, as well as a placebo test involving randomly selected peers. The results of these tests are included in Table 1.4, and it reveals that cash saving peer effects remain economically meaningful except for the pseudo peers in placebo test.

1.4.2.1 Text-based network industry classification (TNIC) peer group definition

I consider an alternative definition of the peer group by using the Text-based Network Industry Classification (TNIC) developed by Hoberg and Phillips (2010), which is based on firms' products description (from 10K filings). Specifically, they calculate firm-by-firm similarity measures based on the number of words that two firms' product description have in common. Using this similarity measure, they define each firm *i*'s industry to include all firm *j*s with pairwise similarities relative to firm *i* above a pre-specified minimum similarity threshold. These firm *j*s are TNIC peers of firm *i* in year *t*. Such peer groups change over time and are firm-specific. The TNIC peers are available from 1996 through 2013 because TNIC industries are based on the availability of 10-K annual filings in electronically readable format.

To perform the sensitivity tests, the peer firms average cash savings, the average relative idiosyncratic stock volatility, as well as the peer firms' covariate averages and industry characteristics are all recalculated based on the TNIC peer groups. Then, I re-estimate the 2SLS estimation for the effect of TNIC peers, and find that the peer effects in cash saving decisions are not sensitive to the definition of peer group. From the estimation results reported in the column (1) of Table 1.4, we can see that TNIC peer influence is larger than the three-SIC peer influence—one standard deviation increase in TNIC peers average cash savings leads to the 3.15% increase in firm's cash savings. The results remain statistically significant and economically meaningful.

1.4.2.2 Domestic and multinational firms

Foley *et al.* (2007) document that US multinational firms hold vast volume of cash overseas to defer the taxation of foreign cash. To alleviate the concern that the mimicking behavior of cash savings might be due to the wave of multinationalism in an industry and stockpiling foreign cash overseas simultaneously, I re-estimate the linear-in-means model of cash savings only for U.S. domestic firms. As suggested by Foley *et al.* (2007), Pinkowitz, Stulz, and Williamson (2012), and Harford, Wang, and Zhang (2015), the identification of domestic or multinational firms is based on whether foreign tax income (TXFO) or foreign pretax income is zero or not. Dyreng and Lindsey (2009) claim that "Visual inspection of several 10-K filings reveals that many of the missing values for tax-related and pretax-related variables in Compustat should be coded as zero". Therefore, I firstly replace some missing values as suggested in Dyreng and Lindsey (2009), and then identify domestic firm-years as the periods before the existence of the first nonzero value of TXFO or PIFO, or the firms who never report TXFO or PIFO in the whole sample period. Imposing these requirements on the data translate into a sample of 47081 firm-year observations. The estimated coefficients are illustrated in the column (2) of Table 1.4, suggesting that peer effects still exist for domestic firms.

1.4.2.3 The trend in cash holdings

Considerable attention has been payed to the growing cash holdings in U.S. firms. Bates, Kahle, and Stulz (2009) shows that time *t* has a significantly positive coefficient on average cash-to-assets ratio from 1980 to 2006. The peer effects may be mixed with the cash holding trend since it is difficult to explicitly isolate the trend from peer effects tests. To address this problem, I firstly draw the line of average cash ratios for U.S. firms from 1980 through 2014 in Figure 1. I find that the trend of cash holdings in U.S. firms disappears since the year of 2004. Then, I re-estimate the peer effects in the period spanning from 2004 through 2014. The estimate coefficients in column (3) of Table 1.4 are similar with those in main results, which indicate that the existence of peer effect in cash saving decisions is not driven by the cash holding trend in U.S. firms.

1.4.2.4 Placebo test: Pseudo peers

If the peer effect really matters for corporate cash savings, I should expect that firm's cash saving decision is not sensitive to the cash policies of unrelated firms. To this end, I artificially generate the "pseudo" peers. Specifically, each year, for each firm in the sample, I randomly select firms from outside of the firm's industry and let the number of "pseudo" peers matches the number of the true peers.[13] I recalculate the peers cash savings, instrument variable and peers average covariates based on the pseudo peers. The estimation results are illustrated in the column (4) of Table 1.4. Given that a peer group composed of randomly selected firms has no economic links, the estimated coefficient of instrumented peer firms average cash savings cannot influence firm cash savings. In addition, pseudo peer firms' other characteristics have no impact on firm's cash saving decisions either.

1.5 Economic mechanisms of peer effects in cash saving decisions

Having established that peer effects in cash saving decisions exist, I next explore the economic reasons to understand the origins and dynamics of peer effects. There are two broad theories of business imitation: (1) rivalry-based theories, where firms imitate others to maintain competitive parity or to neutralize the aggressive actions of rivals, and (2) information-based theories, where firms follow others that are perceived as having superior information (see Lieberman and Asaba (2006)). These reasons represent the potential mechanisms underlying the peer effects in cash savings.

1.5.1 Economic mechanisms

Cash holding is regarded as a preemptive device to gain market share and affect

[13] I require that the pseudo peers industry should be different from the firm's industry at the one-digit SIC level.

industry rivals' entry (Fresard (2010) and Haushalter, Klasa, and Maxwell (2007)), managers not only independently determine their optimal level of cash holdings, it is important for them to pay attention to that of peers, since lower cash holdings compared to peers high cash levels may impair firms competitiveness in product market (Fresard (2010)), such as losing out the investment opportunities to competitors. This is especially so in a competitive industry, where firms are exposed to higher risks from rivals and prices and profits are easily eroded. Since pursuing a differentiation strategy is often costly, difficult and risky, firms cannot be certain whether the new position will be superior. Given this, firms therefore often choose to pursue homogeneous strategies, where they match the behavior of rivals to ease the intensity of competition. Although holding enough cash can protect firm from predation risk, it does not mean that holding much more cash than peers is an insurance and it is not an efficient way to do so, as high level of extra cash holdings is always related to the high opportunity cost and potential agency problems. Therefore, it would be beneficial for firms to learn from their peers' cash policies and avoid holding too little or too much. I predict that *peer effect in cash savings is more pronounced if firms face greater competition pressures.*

Information-based theories explain mimicking behavior from "social learning" and "reputation concerns" aspects. It occurs when a manager is unsure about the optimal amount of cash maintained within firms, or the direct analysis is difficult, costly and time-consuming. Then, imitating cash holding policies of the industry peers without regard to his own information would become optimal. Sometimes, managers want to avoid their negative reputations and signal their "qualities" through mimicking peers financial policies, because they are afraid of proving to be wrong and suffering a loss or reputation. These situations are more likely to happen in relatively weak firms. Therefore, I expect that *firms with less market power, smaller firms, growing firms, and financially constrained firms would be more sensitive to the peer firms' cash holding decisions*.

In addition, it is acknowledged that in recessions and crisis (periods I call "bad times", Loh and Stulz (2017)), firms will experience greater uncertainty and volatility, which leads to the larger pressures, difficulties and cost for managers to make plans. Therefore, based on the information-based theory, I predict that *the peer effects would be more pronounced in bad times than in other times*.

1.5.2 Evidence on the economic mechanisms

To examine the economic channels, I extend the instrumental variable identification strategy wherein the endogenous variables are the peer firm average cash savings interacted with indicator variables, and the instruments are the lagged peer firms average relative idiosyncratic stock volatility interacted with the same indicator variables.

1.5.2.1 Rivalry-based mechanism

Table 1.5 assesses rivalry-based mechanism for peer effects. To examine this channel, I begin with the Herfindahl-Hirschman Index (HHI) to measure industry competitiveness, which is constructed for each three digit-SIC industry classification and for each fiscal year using all available firms in the Compustat database. Then I turn to the text-based network industry classification (TNIC) HHI developed by Hoberg and Phillips (2010). Compared to the Compustat HHI, TNIC HHI might be more accurate to measure product market competition as it is based on firms' products description. In terms of HHI measures, the lower the value of HHI, the higher competition within the industry. The third proxy for product market competition is excess price-cost margin (EPCM). Following Gaspar and Massa (2006), I subtract the industry average price-cost margin to control for heterogeneities across industries unrelated to the degree of competition. A larger excess price-cost margin indicates weaker competition since the closer to perfect competition, the greater extent that price will approximate the marginal cost. I also use cash flow volatility to proxy for the competition intensity, as prior studies show that "intense product market interactions increase fundamental cash flow volatilities because of the increasing sensitivity of firm performance to rival's behaviors" (Seo (2016) and Irvine and Pontiff (2009)). Last but not the least, I use "product market fluidity" to proxy the product market threats. This measure is constructed by Hoberg, Phillips, and Prabhala (2014) capturing how rivals are changing the product words that overlap with individual firm's vocabulary. The larger of this measure, the greater product market threat that firm would face. If rivalry-based peer effects channel exists, firms who face higher product market threat will be more sensitive to the peers' cash holding decisions.

In the tests, firms are sorted into terciles based on the values of these competition proxies in each year, the indicator variable D_{low} is equal to one if firms are ranked into the bottom tercile and zero if the firms are at the top tercile. Just the reverse, D_{high} is equal to one if firms are ranked into the top tercile and zero for bottom tercile. The results in Table 1.5 are consistent with my prediction, where the coefficients of the interaction term with high competition indicators are larger than that of the interaction variable with low competition indicator. In column (1), column (2) and column (4), the peer effects are only significant for firms who face high competition environment, but insignificant for those firms facing relative low-level competition.

1.5.2.2 Information-based mechanism

Table 1.6 assesses the information-based mechanism for peer effects on cash savings. In Panel A, for each industry-year combination, I rank firms into terciles based on the firm-specific measures of market share, gross margin, market cap, book size, market-to-book ratio, and firm age. Similarly, the D_{low} equals to one for firms at the bottom tercile and zero for firms at the top tercile. To the contrary, D_{high} equals to one for top tercile firms and zero for bottom tercile firms. The results in the Panel A of Table 1.6 show that firms with lower product market power (market share), smaller firms (market cap and book size), growing and young firms (market-to-book ratio and firm age) are more sensitive to their peers' cash policies than their counterparts.

In Panel B, I identify financially constrained firms by firstly using indirect proxies, such as whether firms have bond rating, pay dividend, or have lines of credit. Sufi (2007) provides evidence that lack of access to lines of credit is a more statistically powerful measure of financial constraints than other traditional measures used in the literature. Secondly, I use direct proxies constructed as linear combinations of observable firm characteristics, such as Hadlock and Pierce (2010) and Whited and Wu (2006) indices. Following convention, firms are ranked into terciles based on their index values in the preceding year. Firms in the top tercile are regarded as constrained firms (D_1) and those in the bottom tercile are unconstrained firms (D_2). The results in Panel B of Table 1.6 exhibit that more financially constrained firms respond more to the peer effects than less financially constrained firms. It is well-known that financially constrained firms rely more on internal financial resources, such as cash holdings and cash flows. If they hold less cash than that of peers, it is more likely for them to lose when a new investment opportunity arrives. Therefore, imitating peer firms cash policies can help them keep a "safe" position in the competition. Overall, the results in Table 1.6 are consistent with my prediction, they suggest that mimicking behavior is more pronounced among those firms with the greater learning motivation and perhaps the greater need to build reputation.

Table 1.7 assesses whether peer effects is stronger in bad times. The first definition of "bad times" uses NBER-defined recessions, which are the periods January-July 1980, July 1981-November 1982, July1990-March 1991, March-November 2001, and December 2007-June 2009. Second, considering that the last period recession is especially sharp, I separate this period as "Subprime Mortgage Crisis". The third definition uses *Crisis* defined in Loh and Stulz (2017) which are the periods September-November 1987 (1987 crisis), August-December 1998 (LTCM crisis), and July 2007-March 2009 (Credit crisis). I identify a fiscal year as a "bad year", if at least a half period of bad times is included in one fiscal year, except for 1987 crisis and LTCM crisis as these two crisis periods are quite short. Thus, I require that these two crises should completely fall into one fiscal year, then that year could be identified as "bad year". The results in Table 1.7 are consistent with the prediction, that firms are more sensitive to peer firms' behavior during bad times, which provide another evidence on the information-based channel.

1.5.3 Is peer effect on cash saving decisions symmetric?

After showing the economic channels underlying the cash saving peer effects, I find that such peer effect is not symmetric. Table 1.8 shows the evidence that cash-rich firms respond less to peer firms' cash policies than other firms. At the beginning of the tests, it is necessary to clarify the definition of "cash-rich". (1) I sort firms based on their last period cash holding levels within each year, and identify the upper and lower third as "cash-rich" firms and "cash-insufficient" firms, respectively; (2) Referring to Harford (1999), "cash-rich firm-years are years in which a firm's cash holdings are more than 1.5 standard deviations above the predicted cash holdings, where the standard deviation used is the time series standard deviation of the firm's cash holdings." According to the definition, there are 10095 cash-rich firm years, compared them to the rest of 65616 firm-year observations.[14] (3) To make sure the results are robust, I put a more stringent constraint on cash-rich definition, that firms whose cash holdings are more than 2 standard deviations above the predicated cash holdings can be regarded as *cash-rich* firms. Column (1) of Table 1.8 presents the cross-sectional estimation results when using the first definition of "cash-rich". Although the results are not very significant, the smaller and insignificant coefficient of the peer firms average cash savings interacted with the cash-rich indicator variables (D_{rich}) informs that cash-rich firms are insensitive to the peer firms' cash saving behaviors. In column (2) and column (3), when using Harford (1999) "cash-rich" definition, it becomes clearer that cash-rich firms respond less to the peer firms cash saving behaviors than other firms.

1.6 Economic implications of peer effects in cash savings

An important implication of peer effects is the economic externality whereby

[14] In Harford (1999), he identifies 1821 cash-rich firm-year observations and compares it to the other 21675 firm-years in the periods spanning from 1972 to 1994.

changes to one firm affect the outcomes of other firms. If only one manager in an industry mimics its competitors' cash saving decisions, then it is very likely that other forces will pull it back and force a correction. However, if peer learning is common in an industry, this may lead to significant changes in the industry overall cash savings. In this section, I evaluate whether peer influence is important enough to impact aggregate cash savings at the industry level.

1.6.1 Excess-variance identification strategy

To identify the total economic impact stemming from peer-influenced cash saving decisions at the industry level, I use an excess variance identification strategy pioneered by Graham (2008), which proposes an approach for identifying the existence and magnitude of social interactions based on the conditional variance restrictions. If firms within the same industry learn from one another on cash saving decisions, then individual firm cash savings will covary positively within an industry and display excess variation across industries. Thus, the ratio of between-industry variance over within-industry should be larger than one when peer effect exists. However, there is another explanation for excess variance—industry-level heterogeneity (i.e., the distribution of observed and unobserved industry and firm characteristics might vary across industries). Therefore, the unconditional between-group variance of cash savings is the sum of three terms: (1) the variance of any industry-level heterogeneity, (2) the between-industry variance of any firm-level heterogeneity, and (3) the strength of any social interactions. When identifying the peer effect component of excess variance, Graham (2008) compares the within- and between-group variances across large and small groups. The distribution of group-level heterogeneity is the same across large groups or across small groups, while the distribution of peer effect differs. The key identifying assumption for the excess variance method is that after controlling for observables, being in a small or large industry only affects the between-group variance in outcome variable via peer effects. To apply it in cash saving peer influence, the identification logic is as follows.

In large industries, clusters of firms with high cash savings are typically offset by corresponding clusters of firms with low cash savings, resulting in little variation in average cash savings across large industries, that is, the mean levels of cash savings are similar across large industries. In small industries, however, through learning from each other, the composition of firms with mostly above or below average cash savings are more frequently observed than that in large industries, because there are not enough firms in small industry to derive offsetting effect. That is to say, the variance of cash savings is greater across small industries than that across large industries in the presence of peer effects. The strengths of peer effects are different across small and large industries, while the variance of industry heterogeneity across large industries and that across small industries should be similar.[15] Thus, a ratio of the difference in between-group variance across small and large industries to the difference in within-group variance across small and large industries provides a measure of the existence and strength of peer effects.[16] This is described as "ratio-in-differences" in Popadak (2017).

---

[15] "Even if there is some variable that is unaccounted for that is correlated with industry size and outcome variable as long as it does not systematically inflate the observed variance in small industries across all observations over sample period, then the identification holds." See Popadak (2017).

[16] Some industries may have no peer effects, so their cash savings would exhibit no clustering regardless of whether they are small or large industries. However, by evaluating all the three-digit SIC industries over more than 30 years, it is possible to statistically detect the difference in the excess variance when conditional on small and large industries, and that is the evidence of peer effects.

Following Graham (2008) and Popadak (2017), the econometric specification of excess-variance test of cash saving decisions is given by:

$$\Delta Cash_{ij} = \alpha_j + (\gamma - 1)\bar{\varepsilon_j} + \varepsilon_{ij} \tag{6}$$

where α_j represents industry-level heterogeneity, ε_{ij} represents firm-level heterogeneity, and $\bar{\varepsilon_j}$ is the industry mean of ε_{ij}. γ represents the peer influence parameter and is dependent on $\bar{\varepsilon_j}$. In the absence of peer effect, the γ will be one. If peer effect exists, γ is greater than one, then cash saving decisions are influenced by the $\bar{\varepsilon_j}$ which involves the decisions of peer firms and moreover the characteristics of the peers. The greater the strength of peer effect, the greater γ will be. However, the γ cannot be directly identified because the presence of $\bar{\varepsilon_j}$ leads to a matrix that is not of full rank. Graham (2008) provides a way of estimating the square of peer influence, γ^2, which results from a ratio of actual (observed) difference in between-group variances across small and large industries to the corresponding difference in within-group variance.[17]

$$\gamma^2 = \frac{E\left(c_j^b \middle| S_j = 1\right) - E\left(c_j^b \middle| S_j = 0\right)}{E\left(c_j^w \middle| S_j = 1\right) - E\left(c_j^w \middle| S_j = 0\right)} \tag{7}$$

where $c_j^b = (\overline{\Delta C}_{j.} - \overline{\overline{\Delta C}}_s)^2$ is between-industry sum of squares for the vector of cash savings ΔC, with $\overline{\Delta C}_{j.}$ the mean cash savings in industry *j* and $\overline{\overline{\Delta C}}_s$ the grand mean cash savings in small or large industries. $c_j^w = \frac{1}{N_j}\frac{1}{N_j - 1}\sum_{i=1}^{N_j}\left[\Delta C_{ij} - \overline{\Delta C}_{j.}\right]^2$ is within-industry sum of squares with ΔC_{ij} the cash savings for firm *i* in industry *j* and N_j is the number of firms in industry j. S_j is an indicator for industry type, which equals to one for small

[17] The mathematical derivations are detailed in Graham (2008) and Popadak (2017).

industry and zero for large industry.

In order to reduce the amount of firm level and industry level heterogeneity, I orthogonalize the cash savings with respect to many explanatory variables such as firm size, cash flow, Tobin's Q, net equity issuance, net debt issuance, net investment, as well as industry-specific factors including industry competitiveness and industry cash flow volatility, and use the residuals $\hat{u}_{ij}$ to compute $c_j^b = \bar{\hat{u}}_{j.}{}^2$, and $c_j^w = \frac{1}{N_j}\frac{1}{N_j - 1}\sum_{i=1}^{N_j}\left[\hat{u}_{ij} - \bar{\hat{u}}_{j.}\right]^2$.

1.6.2 Results of excess-variance tests

To determine whether excess variance is coming from peer effects, I compare the excess variance across different sizes of peer groups defined by the number of firms in the industry. In each year, I rank industry peer groups from the largest to the smallest number of firms in the industry, and then the lower third industry groups are defined as small industries, while the middle and top third industry groups are regarded as large industries.[18] Estimation results are illustrated in Table 1.9. Column (1) conditions on observable firm-level and industry-level heterogeneity including firm-specific and industry-specific characteristics. Column (2) further conditions on peer firm average characteristics. The estimates of the square of peer effect parameter γ^2 is 1.832 given firm- and industry-specific variables which suggesting a peer effect multiplier of 1.354, and the related Chi-squared statistics is 7.76 indicating a rejection of no peer effects hypothesis at the 99% significance level. When further controlling for peer firms'

[18] If I classify the industry group as small and large by cutting at the median number of firms across industry peer group, the results are qualitatively similar.

average variables, the estimate changes little.

To interpret the economic significance of the peer effect multiplier, I estimate the relative cash changes due to peer effects in small and large industries, respectively. It shows that peer effects lead managers to enlarge or shrink cash savings by 12.8% in small industries and 6.18% in large industries.[19] To put these results into perspective, consider a small industry with an expected cash changes by 2% under the assumption of no peer influence, the results suggest that observed cash changes will be between 1.74% and 2.26%. Since the average cash level of sample firms is 200 million, and the average total asset is 2026 million, the peer effect in cash savings (0.26%) could result in substantial changes. Overall, the results of the excess variance-based tests for peer effects strongly support the hypothesis that peer effects significantly alter cash savings in an industry.

1.7 Conclusion

This paper provides evidence that corporate cash saving decisions are influenced in a meaningful way by the peer firms cash policies. Using instrumental variable identification approach to estimate the causal peer effect, I show that one standard deviation increase (decrease) in instrumented peer firms' average cash savings leads to 2.63% increase (decrease) in firm's cash savings on average. Such peer effect is economically meaningful and larger than many previously identified cash saving determinants. In addition, I also find that cash saving peer effects are important enough to impact total cash savings at the industry level.

[19] Graham (2008) provides a rough sense of the magnitude of the implied social multiplier, see Page 656-657. Such relative change is given by $(\gamma - 1)/\sqrt{N_j}$.

After examining the existence of peer effects in cash saving decisions, I also perform several cross-sectional tests to examine whether rivalry-based mechanism and (or) information-based mechanism could explain the peer effect in cash saving decisions. The sets of tests suggest that cash saving peer effects originate from both channels: (1) firms are more sensitive to peers' cash holding decisions when they face greater competitive pressures; (2) less powerful firms (with lower market share), smaller firms, young firms, and financially constrained firms respond more actively to the peers' cash policies; (3) peer effects on cash savings is more pronounced in bad times. Furthermore, I find that peer effect is asymmetric where cash-rich firms are less sensitive to peer firms cash policies than other firms.

Overall, this paper provides a positive answer that firms' cash saving decisions are remarkably influenced by peer firms' cash policies, and the peer effect is more important than many other determinants of cash savings. There is another related question: whether mimicking behavior in cash saving decisions could increase firm values. I believe this could provide an interesting avenue for future research.

CHAPTER 2

GETTING FEEDBACK ON YOUR RESEARCH: EVIDENCE FROM ANALYSTS

2.1 Introduction

A recent strand of literature examines how agents extract information from stock prices when making decisions.[20] Chen, Goldstein, and Jiang (2007) show that firm managers incorporate the private information contained in stock prices when they make corporate investment decisions. Because stock prices aggregate diverse information, including outsider opinions that firm managers might not easily access, stock-price information might shift manager beliefs about their own firms' fundamentals. In particular, Luo (2005) shows that firm managers, after observing the market reaction to their acquisition decisions, are more likely to abandon deals that the stock market does not react favorably to. Managers hence are able to learn from stock-market reactions about the quality of their managerial decisions.

In this paper, we investigate whether another group of important agents—sell-side analysts—also learn about the quality of their decisions from the stock market. Sell-side analysts issue research reports on the firms that they cover and the market then reacts to these reports. If the report elicits a large market reaction, this means that the analyst changed the market's priors about the covered firm (Loh and Stulz (2011)). The

[20] See Bond, Edmans, and Goldstein (2012) for a review. The idea that market prices are a useful source of information goes back to Hayek (1945).

analyst might also update her beliefs about the quality of her own research, hence influencing her future research effort and impact.[21] There are good reasons to expect that research effort might depend on past success. First, if the report contains new information about the firm, such as private information collected by the analyst or a better method/framework of understanding existing public information, such information can also be applied to all firms in the analyst's coverage portfolio. Veldkamp (2006) shows that agents have incentives to produce information that is usable across a subset of assets rather than for only one asset (see also Kacperczyk, Van Nieuwerburgh, and Veldkamp (2016)). This mirrors what analysts do since they typically specialize in covering firms in the same industry (e.g., see Boni and Womack (2006)). Second, besides information, the analyst might update her beliefs about her own skill in doing research and hence cause her future research effort to be related to past success. In the setting of traders, Seru, Shumway, and Stoffman (2010) show evidence of learning from past trading experiences and that past success influences future activity.

We define whether a recommendation change is successful by using the influential definition from Loh and Stulz (2011)—in which a recommendation change issued on a non-firm news day produces a stock-price impact that is visible to investors in the stock. We examine how a recently influential analyst learns from past success by measuring two dimensions of future effort: the number of recommendation changes

[21] Footnote #1 in Bond, Edmans, and Goldstein (2012) has an appropriate analogy for our paper's focus. "As an analogy, assume that there exist stock prices on individual researchers, which reflect the views of the general profession. If a researcher's stock price fell upon starting a new project, many such researchers would choose to abandon the project." Our focus is on analysts who produce research on the firms that they cover and the stock-price reaction to their research informs analysts about the quality of their research and/or their skill in doing research.

and the total number of recommendations including initiations and reiterations.

We find consistent evidence of learning. We show that if an analyst issues at least one influential recommendation change in a quarter, she is more likely to issue recommendation changes next quarter. The unconditional probability of observing that an analyst issues a recommendation change in a quarter is 65.6%. This probability goes up by 6.12% if the analyst has influential recommendation changes in the most recent quarter. The finding that recent success (influential changes) leads to increased research effort is robust to controlling for analyst and firm characteristics, and the analyst's history (beyond the most recent quarter) of issuing influential recommendation changes. We also find evidence that recent success leads to more recommendation activity in general, where activity includes initiations and reiterations. We therefore conclude that recent success does indeed lead to greater future effort.

Why are recently influential analysts more active? The first plausible explanation is a short-term information hypothesis. If analysts find that it is their models/frameworks or the private information in the reports that caused the stock-market reactions, the analyst will quickly use this to generate related reports. As such, the feedback effect should dissipate after such a time when the information gets fully incorporated into subsequent reports. The second is a learning about skill hypothesis where the analyst uses the stock-market reaction to infer her research skill. Having received validation of her skill from the strong stock-price reaction, the analyst increases effort and confidence and might become better at the job. Here, the feedback effect should be more long-lasting and persist beyond the short term, i.e. recent success should lead to increased effort beyond the next immediate period. Third and finally, an overconfidence hypothesis could explain the findings. Analysts experiencing recent success increase their activity but there is no real quality to this new output.

In tests to disentangle these hypotheses, we find that the feedback effects of recent success on future effort indeed last only for the short term. After four quarters, the feedback effects are all significantly lower, which supports the information hypothesis.[22] To test the overconfidence hypothesis, we examine the future influential likelihood of recently influential analysts. We find that recently influential analysts are also more likely to be influential in the next period, inconsistent with the overconfidence hypothesis.[23]

In further tests, we examine other implications of the short-term information hypothesis. That research effort can be predicted by past successes most likely implies that analysts have capacity constraints. Otherwise an analyst who receives feedback from the market that the thesis in her report is correct can immediately write more reports on other firms. Such instantaneous report writing would limit our ability to find any predictability from the current to the next quarter. Other professional investors have been shown to have capacity constraints, e.g. institutional investors (Kempf, Manconi, and Spalt (2016)), fund managers (Lu, Ray, and Teo (2016)), and Federal Reserve supervisors (Eisenbach, Lucca, and Townsend (2016)). Driskill, Kirk, and Tucker (2016) also find that analysts take more time to issue forecast revisions when a lot of

[22] To pin down whether this information is private information or better processing of public information, we show that the feedback effects are strong in both the pre- and post-Reg FD periods. This reveals that the information channel does not merely stem from private access to management, but also from the analyst's better ability to interpret available public information.

[23] The fact that recommendation changes in the next period have larger stock price reactions could also be consistent with overreaction if investors overreact to recently influential analysts. But in recommendation drift tests, we find no evidence of reversals for previously influential analysts or uninfluential analysts and hence conclude that investor overreaction is not responsible for these results.

news hit their coverage portfolios (see, e.g., the limited attention story in Hirshleifer, Lim, and Teoh (2009)). In Hong, Stein, and Yu (2007), agents who have limited attention shift from one simple model to another whenever enough evidence accumulates against the incumbent model. We find evidence consistent with capacity constraints—that when an analyst covers more firms, the predictability of this quarter's influential recommendations for next quarter's recommendation activity increases.

We also conduct our tests at the broker level and find a strong short-term feedback effect which dissipates after a few months. This is consistent with the information story. Analysts who move the market share the insights with the rest of the brokerage team and colleagues learn from each another. Such learning across stocks implies that influential recommendation changes can predict the future occurrence of recommendation changes for other firms in the same industry. To examine whether such spillover effects can lead to profitable trading strategies, we form a portfolio of firms without recommendation changes but whose industries had a large number of influential upgrades. If these influential upgrades contain common industry information that will be incorporated into future analyst reports, buying such firms in advance might earn abnormal returns when investors do not fully anticipate such predictability. We show that a long-short portfolio formed in this manner earns abnormal returns of up 0.6% per month and that the ability of these portfolios to predict future upgrades and downgrades likely contributes to these profits. We also provide cross-sectional regressions to show our results here are robust to controlling for the usual lead-lag effect between large and small firm returns in the same industry.

Our study is related to the literature on the real effects of financial market prices (e.g., Bond, Edmans, and Goldstein (2012)). Most of such studies examine whether managers learn from stock prices when making decisions (e.g., Bakke and Whited (2010), Foucault and Frésard (2012), Edmans, Goldstein, and Jiang (2015), Edmans, Jayaraman, and Schneemeier (2016), and Zuo (2016)). Besides firm managers, other decision makers also learn from stock prices, e.g. customers (Sun (2016)) and suppliers (Williams and Xiao (2014)). These studies typically use exogenous changes in stock prices caused by extreme mutual fund flows to test whether decision makers respond to such non fundamentals-driven price movements. Sulaeman and Wei (2014) show that analysts also appear to make recommendations in response to such flow-driven mispricing. The focus of our study is on analysts. While most studies explore on how agents learn from exogenous changes in stock prices, we examine how analysts learn from endogenous stock-price movements caused by their reports. The analyst setting is also better than the manager setting for studying individual learning because observable analyst output occurs frequently and can be tied to an individual, while observable managerial actions are infrequent and usually team-based.

Our study also relates to the broad literature on how security analysts respond to recent news. As prominent intermediaries, analysts' reports should incorporate recent stock market information. Some papers look at specific firm events, such as earnings announcements (e.g., Altınkılıç and Hansen (2009)), earnings guidance (e.g., Frankel, Kothari, and Weber (2006)), and large stock price movements (e.g., Conrad *et al.* (2006)). Other papers look at how analysts respond to other analysts, i.e. herding behavior, examined for example in Welch (2000), Hong and Kubik (2003), and Jegadeesh and Kim (2010). In addition, because we study the likelihood of a recommendation change for a given unit of time, our paper is related to recent work on the recommendation change frequency (e.g., Hobbs, Kovacs, and Sharma (2012), Boulland, Ornthanalai, and Womack (2016), and Bernhardt, Wan, and Xiao (2016)). We bring a new dimension to this issuance frequency literature by showing that the likelihood of issuing a revision is closely linked to whether the analyst's most recent recommendation changes have been influential.

The rest of the paper proceeds as follows. Section 2 discusses the data sources, sample, and key variables. Section 3 reports the results of feedback effect on analyst research. Section 4 identifies the mechanisms driving the feedback effect of analyst recommendation activity, Section 5 reports the results of additional tests, and Section 6 concludes.

2.2 Data and sample

2.2.1 Analyst data

The analyst stock recommendations are from Thomson Financial's Institutional Brokers Estimate System (I/B/E/S) U.S. Detail File, which spans the years 1993 to 2014.[24] Our sample period starts from 1994 since the recommendation change observations in 1993 are sparse (1993 data is used for prior ratings when available). A recommendation change is defined as the current rating minus the prior rating by the

[24] Ljungqvist, Malloy, and Marston (2009) report that matched records in the I/B/E/S recommendations data were altered between downloads from 2000 to 2007. Thomson, in response to their paper, fixed the alterations in the recommendation history file as of February 12, 2007. The dataset we use is dated December 17, 2015 and hence reflects these corrections. However, there are still some large brokers missing from the current I/B/E/S forecasts and recommendations files. To reinstate the missing years from these brokers, we use Capital IQ estimates to extract recommendations issued by these missing brokers and splice the collected data into our sample. Spliced observations make up about 0.45% of the observations in the recommendations sample.

same analyst. According to the definition in Ljungqvist, Malloy, and Marston (2009), a prior rating is assumed to be outstanding if it has not been stopped by the broker (in the I/B/E/S Stopped File) or has been confirmed by the analyst in the last twelve months (checking the I/B/E/S review date).

An analyst's total recommendation activity is computed by aggregating all rating activity including rating changes, initiations, and reiterations. Reiterations are commonly not recorded by the I/B/E/S recommendation file. So we follow the literature (e.g. Loh and Stulz (2017)) to assume that an analyst reiterates an outstanding recommendation when she issues 1) a Q1 earnings forecast in the I/B/E/S Detail File, or 2) a price target forecast in the I/B/E/S Price Target File. We exclude observations from anonymous analysts, recommendation changes where the lagged stock price is less than one dollar, and observations with no outstanding prior rating from the same analyst. For our main analysis, we also exclude the analyst codes in I/B/E/S which mostly likely do not represent individuals. These are analyst codes associated with industry names (e.g., Healthcare), obvious team-sounding names (e.g. Research DEPT), multiple analyst names, and broker codes associated with only one single analyst.

Stock returns are from CRSP. To be sure that stock-price reactions associated with recommendation changes can be reasonably attributed to analysts, we remove recommendation changes that occur on firm-news days following Loh and Stulz (2011). Firm-news contaminated days are defined as the three trading days centered around a Compustat earnings announcement date or a company earnings guidance date,[25] and days with multiple analysts issuing recommendations for the firm. We calculate the

[25] Guidance dates are from First Call Guidelines until it was discontinued on September 29, 2011, and from I/B/E/S Guidance file thereafter.

cumulative abnormal return (CAR) of recommendation changes from the recommendation date to the following trading day, i.e., a day [0,1] event window,[26] and check whether the two-day CAR is in the same direction and is statistically significant at the firm level based on the firm's prior stock-price volatility.[27] Such recommendation changes are classified as influential.

2.2.2 Descriptive statistics

Our main sample is constructed at the analyst-quarter level (averaging across the multiple firms covered by an analyst) where we estimate whether analysts are more likely to issue recommendation changes and increase their total recommendation activity in the next quarter *t*+1 conditional on having influential revisions this quarter *t*. The analyst-quarter observations are included in our sample only if the analyst issued uncontaminated recommendation changes in quarter *t*. We then define *Rec-change dummy* which equals one when the analyst-quarter observation is associated with at least one recommendation change in quarter *t*+1, and zero otherwise. When *Rec-change dummy* equals zero, it means that the analyst who is present in quarter *t* only issues initiations, re-initiations, reiterations, or does not issue any recommendations at all in quarter *t*+1 even though she is still present in I/B/E/S. An analyst's total

[26] If the recommendation is issued on a non-trading day or after trading hours, day 0 is defined as the next trading day.

[27] Specifically, CAR is computed as the cumulative return of the common stock less the cumulative return on an equally weighted characteristic-matched size, book-to-market ratio (BM), and momentum portfolio (following Daniel *et al.* (1997)), and then compare its absolute value with $1.96 \times \sqrt{2} \times \sigma_{\varepsilon}$. We multiply by $\sqrt{2}$ since the CAR is a two-day CAR. σ_{ε}, the Idiosyncratic volatility, is the standard deviation of residuals from a daily time-series regression of past three-month (days −69 to −6) firm returns against the Fama and French (1993). This measure roughly captures recommendation changes that are associated with noticeable abnormal returns that can be attributed to the recommendation changes.

recommendation activity (*#Total activity*) is measured as the total number of reports written by the analyst in a quarter. *ΔTotal activity (from t-1 to t+1)* is the change in the total number of analyst recommendation reports from quarter *t*-1 to quarter *t*+1. *Influential dummy*, our key explanatory variable, equals one if the analyst issues at least one influential recommendation change in the quarter *t*, and zero otherwise. *Influential before* is an indicator variable which equals one if analyst issued at least one influential recommendation changes in her full history before quarter *t*, and zero otherwise. Panel A of Table 2.1 shows the distribution of recommendation activity at the analyst-quarter level, which consists of 79,192 observations where at least one uncontaminated recommendation change is issued by an analyst in the current quarter. We see on average that 22.3% of analyst-quarters are associated with at least one influential recommendation change, and the unconditional next-quarter recommendation change probability is 65.6%. This percentage includes all next quarter recommendation changes, including those that are issued with firm news. When we examine only next-quarter changes that are uncontaminated by firm news, the percentage is 53.3%. In the next quarter, analysts on average write 14.4 reports, and issue 2.53 recommendation changes (of which 1.95 of the changes are uncontaminated). In multivariate regressions, we control for analyst and firm average characteristics (averaged within the analyst-quarter), whose distributions are reported in Panel C of Table 2.1.

We also construct a sample at the broker-month level (averaging across the multiple firms covered by a broker) to estimate whether brokers issue more recommendation changes and increase their total recommendation activity in the month *m*+1, conditional on having influential revisions in month *m*. A more frequent measurement of activity (monthly instead of quarterly) can be used here because at the broker level we are aggregating all analyst activity within a broker. When doing this, we reinstate team analysts who were removed earlier in the analyst level tests. The broker-month observations are included in our sample only if the broker issued at least one uncontaminated recommendation change in month *m*. *Influential dummy* here equals one if the broker has at least one influential recommendation change in month *m*, and zero otherwise. Other variables are also measured in the same way as those defined in the analyst-quarter setting. Additionally, we also define the fractions of recommendation changes, influential recommendation changes, uncontaminated recommendation changes, and total recommendation activity, where the denominator is the number of firms covered by the broker. The summary statistics of these variables are reported in the Panel B of Table 2.1. The broker-level sample consists of 25,628 observations. We can see that 40.6% of broker-months are associated with at least one influential recommendation change, which is much larger than that at the analyst-quarter setting, and the next-month recommendation change probability is 87.3%. When we limit to next-month revisions that are not contaminated by firm news, the percentage is still high, at 81.7%. In the next month, brokers on average have 80.3 recommendation reports, 8.41 of which are associated with recommendation changes, of which 5.59 are uncontaminated recommendation changes.

2.3 Feedback effect of past success

2.3.1 Future recommendation change probability

In this section, we estimate whether analysts are more likely to issue recommendation changes in the upcoming quarter if they have influential recommendation changes this quarter, controlling for analyst and firms' average characteristics. We use recommendation changes as one of our measures of discretionary analyst effort because analysts are not required to change their ratings according to any regular interval, unlike earnings forecasts which need to be issued every quarter. Also, in comparison to reiterations, recommendation changes are infrequent reports issued by the analyst when the analyst has accumulated enough evidence to move her prior on the firm while reiterations might merely repeat the information in a prior report.

We first provide univariate evidence on the relation between influential likelihood and the upcoming recommendation change probability, by estimating the probit regression of next-quarter *Rec-change dummy* on a constant and *Influential dummy*. Then we turn to multivariate tests where a battery of analyst and firms' average characteristics are added in. Boulland, Ornthanalai, and Womack (2016) observe that experienced analysts become more deliberate and change their recommendations less frequently than before. Analysts' career tenure also relates to their research performance. Mikhail, Walther, and Willis (1997) and Loh and Stulz (2011) show that experienced analysts make more accurate and influential earnings forecast and recommendation revisions. Therefore, we control for analyst experience and reputation proxied by an indicator variable to identify whether the analyst is ranked as an All-American team (whether as first-, second-, third-team, or runner-up statuses) in the latest October *Institutional Investor* magazine's annual poll.[28] Analyst experience is the

[28] All-American analyst ranking is published in the October issue and an analyst maintains the Star status for 12 months beginning the November after the publication of the poll results.

number of quarters since the analyst issued the first Q1 earnings forecast or stock recommendation on I/B/E/S. We use the earlier of two dates if the analyst issues both forecasts and recommendations. Next, because forecast accuracy can be a proxy for skill in stock picking (Loh and Mian (2006)), we define *Accuracy quintile* as the average forecast accuracy quintile of the analyst based on the firms covered in the past year, where the quintile rank is increasing in forecast accuracy. We further control for the analyst's prior year *Leader-Follower Ratio* (*LFR*) constructed following Cooper, Day, and Lewis (2001) where they use this ratio to gauge the extent to which a forecast event leads other analysts to revise their estimate. A ratio larger than one denotes a leader analyst.[29] More importantly, the number of firms that an analyst covers (*#Firmsperana*) in a quarter is added to our multivariate regressions, since analysts are more likely to issue more recommendation changes if they cover a larger number of firms. We also control for the following firm characteristics: *Size* is last June's market capitalization, *BM* is the book-to-market ratio (computed and aligned following Fama and French (2006)), *Momentum* is the buy-and-hold return for the 11-month period ending one month before beginning of the recommendation month, and *Stock volatility* is the standard deviation of daily stock returns in the prior month. These firm characteristics are averaged over all firms that analysts cover in analyst-quarter setting.

We now report estimates of next-quarter recommendation change probability conditional on having influential recommendation changes from probit regressions in

[29] To compute this, the gaps between the current recommendation and the previous two recommendations from other brokers are computed and summed. The same is done for the next two recommendations. The leader-follower ratio is the gap sum of the prior two recommendations divided by the gap sum of the next two recommendations. A ratio larger than one shows that other brokers issue new ratings quickly in response to the analyst's current recommendation.

Table 2.2. We focus on the marginal effect of the indicator variable—*Influential dummy*. The marginal effects, which measure the change in probability when changing the variable by one standard deviation centered around its mean ($\pm\frac{1}{2}\sigma$), or a 0 to 1 change for a dummy variable, are reported with *z*-statistics in parentheses (based on standard errors clustered by analyst).

We first examine the impact of influential recommendation changes on analyst effort proxied by the likelihood of the analyst issuing *any* recommendation change next quarter, i.e. including changes issued both on firm news days and on non-firm news days. The marginal effect of *Influential dummy* reveals that the increase in the likelihood of issuing recommendation changes next quarter conditional on having influential changes this quarter is 6.12%. When we add analyst and firms' average characteristics, as well as calendar quarter fixed effects to control for market-wide shocks, the marginal effect remains sizable at 3.71%.[30] In column 3, we further include an indicator variable—*Influential before*, to capture the analyst's propensity to be influential in general. Loh and Stulz (2011) show that being influential in the past is positively associated with the current likelihood of being influential, and it is related to analyst skill that is persistent. Thus, this variable can help us to control for some unobserved and persistent analyst characteristics that impact both the influential likelihood and the recommendation change probability. From column 4 to column 6, we examine analyst effort using only recommendation changes issued on non-firm news days. We find that analysts are also more likely to issue uncontaminated

[30] Quarter fixed effects are useful because market wide shocks might be important in affecting analyst behavior in general. For example, Loh and Stulz (2017) show that analysts produce better output in bad times. When we examine our feedback effects in good and bad times, there is some evidence that bad times see slightly stronger feedback effects.

recommendation changes if they have influential changes in this quarter, regardless of whether control variables and calendar quarter fixed effects are added.[31]

Looking at the marginal effects of the controls, we see that inexperienced analysts, those with a smaller leader-follower ratio, and non-Star analysts are more likely to issue recommendation changes. These results are consistent with Boulland, Ornthanalai, and Womack (2016) who show that experienced analysts revise their decisions more slowly, but are more influential and more likely to lead other analysts. These results imply that "success" as measured by Star status and analyst experience leads to lower effort as proxied by the number of revisions. While this appears to be in contrast with our main results that success leads to greater effort, we believe this can be understood in a simple Bayesian framework. Reputable analysts have stronger priors and better information and are unlikely to change their priors frequently. Hence it is not surprising that they make less frequent rating revisions (Boulland, Ornthanalai, and Womack (2016)). However, when a reputable analyst makes a rating change that is influential, she learns from the market reaction that her new private signal has high precision. Hence she updates her priors on the other firms that she covers and is now more likely to revise her existing ratings in her coverage universe. The multivariate analysis shows that analysts are more likely to issue recommendation changes if they cover a large number of firms, which is an intuitive result. Lastly, the probability to revise recommendations is greater when the average firm associated with the analyst-quarter has higher prior stock volatility.

[31] Although we already have several analyst characteristics as controls, it is possible that some other unobserved analyst characteristic influences our results. When we include analyst fixed effects to account for these potential unobserved characteristics, we find that our results are both qualitatively and quantitatively similar.

2.3.2 Future total recommendation activity

We use an alternative variable to measure analyst effort—total recommendation activity, instead of the recommendation change likelihood. From column 1 to column 3 of Table 2.3, we use the log of one plus the number of total reports written by an analyst next quarter as the dependent variable and estimate pooled OLS regressions with standard errors clustered by analyst. We see that the past success-future effort feedback effect is still economically and statistically significant, where successful analysts issue more reports next quarter than those of unsuccessful analysts who do not make any influential recommendation changes this quarter. The inclusion of analyst and firms' average characteristics, as well as analyst past performance does not remove the effect of the *Influential dummy*. In column 4 to column 6, we investigate the time-series change in analyst effort before and after issuing influential recommendation changes, where the dependent variable is the change in the number of total recommendation reports written by the analyst from quarter t-1 to t+1.[32] We can see that analysts significantly write more reports after issuing influential recommendation changes.

Overall, both univariate and multivariate tests show that analysts who receive feedback from the stock market that the content of their research reports is correct are more likely to issue recommendation changes and write more recommendation reports in the next quarter. This can be interpreted as evidence of increased research effort due to recent success.

[32] The change in number of total activity from quarter t-1 to quarter t+1 is winsorized at 1% and 99% percentile, to reduce the impact of extreme values.

2.3.3 Robustness tests

In order to exclude some possible confounding effects, we expand the analyst-quarter sample to an analyst-quarter-firm sample. For each analyst-quarter combination, we include all firms covered by an analyst as long as the analyst has an outstanding rating on the firm even if she does not issue a new rating in the quarter.[33] This allows us to construct a large panel in which an analyst-quarter-firm observation represents an analyst who is actively covering the firm that quarter. This large panel allows us to control for the following additional effects.

1) An analyst may revise a rating back to its original level because the prior influential recommendation change leads the stock price of that firm to the analyst's target. To account for such revisions, we add a dummy variable that equals one if the firm receives an influential recommendation change from the same analyst this quarter and zero otherwise.
2) An analyst may respond to other analysts' influential recommendation changes but not their own. We add the percentage of analysts who issue influential recommendation changes on the firm this quarter as a control
3) Firm-specific characteristics—firm size, book-to-market ratio, firm's stock return and total volatility are included as firm-level controls instead of analyst-level averages across firms.[34] Stock volatility is important to control for salient events in the current quarter, which might lead to an overall increase in

[33] A rating is considered outstanding following the definition in Ljungqvist, Malloy, and Marston (2009).

[34] Firm size and book-to-market ratio are defined previously. Total volatility is the average of the monthly standard deviation of daily returns that quarter, and stock return is the average monthly return in a quarter.

recommendation change likelihood next quarter.[35]

In unreported results, we find that including above control variables does not change our conclusion that analysts are more likely to issue recommendation changes next quarter on average across all the firms that they cover conditional on having influential recommendation changes this quarter on any firm.

2.4 Is the feedback effect driven by learning about information or skill?

In this section, we investigate why analysts devote more effort next quarter on their future research when the market reacts strongly to at least one of their current reports. An information hypothesis suggests that analysts update their beliefs about the quality of their research in terms of the models they used in the reports or the private information that they obtained. Such endorsement by the market leads them to increase their future research effort and impact. This is supported by the theoretical work of Veldkamp (2006) on information markets, which predicts that agents have incentives to produce information with implications for a subset of assets. A skill hypothesis suggests that it is because analysts update their beliefs about their own skill when the market endorses their reports. This motivates them to devote more effort on future research.

2.4.1 Feedback effect: Different horizons

To investigate whether it is the information hypothesis or (and) the skill hypothesis that drives the feedback effect (past success-future effort relation), we test the

[35] We do not control for the number of analysts per firm because this variable is highly correlated with firm size. In this sample, the correlation between these two variables is more than 0.70. We obtain similar results if we replace firm size with the number of analysts per firm.

persistence of feedback effect. If the analyst finds out that it is the information/model in their most recent report that elicits the large market reaction, she will apply it quickly to other firms. Such information is likely to be short-lived as the market soon figures out the implications of the new information for other firms. Therefore, the feedback effect should dissipate after some time. However, if the skill hypothesis drives the feedback effect, that the analyst and the market updates on how good the analyst is, the feedback effect should not dissipate as much.

Table 2.4 reports the marginal effect of having influential recommendation changes in this quarter on the quarter *t*+1 and quarter *t*+4 recommendation activity, controlling for analyst and firms' average characteristics. The Chi-square statistics and p-values reveal whether there is any difference between the *Influential dummy* coefficients associated with these two samples. From column 1 to column 4, we find that the marginal effect of *Influential dummy* is larger for quarter *t*+1's recommendation change probability than it is for quarter *t*+4. Importantly, the difference is both economically and statistically significant, which indicates that the information contained in the current quarter's influential recommendations does indeed contribute to the increasing likelihood of recommendation changes next quarter but this information becomes less useful one year later. In column 5 to column 8, we test how the feedback effect varies in analyst next-quarter and next-year's total recommendation activity. Although the decrease in the effect of *Influential dummy* on the number of total recommendation activity from quarter *t*+1 to quarter *t*+4 is not statistically significant, the magnitude of the decline appears economically meaningful. The dependent variable in column 7 (column 8) is the number of total activity in quarter *t*+1 (*t*+4) minus that in quarter *t*-1.

We can see that the feedback effect on the change of total activity disappears four quarters later.

These results generally support the information hypothesis, that the market's validation of the analyst's information leads the analyst to successfully apply the same framework or information quickly next quarter to other related firms. The skill hypothesis appears still relevant, since the feedback effects on the recommendation change probability and the number of total recommendation activity are mostly still statistically significant after four quarters.

2.4.2 Feedback effect at the broker level

In this section, we test the feedback effect at the broker level. If analysts share their information and learn from each other within brokerage house, the information contained in influential reports can be further applied by the analyst or her colleagues. At the broker level, it is harder to make a case for the skill hypothesis since brokers, especially large ones, are unlikely to need the market's endorsement of their research to learn about how good they are.

As the unconditional probability of recommendation changes at the broker level is very high, our dependent variable here is the fraction of activity, i.e. recommendation changes, uncontaminated recommendation changes, and total recommendations divided by the number of firms covered by the broker. The explanatory variable is the number of influential recommendation changes over the number of firms covered by a broker (*#InfluRecchg/#Firms*). In the multivariate tests, we control for the first lag of the dependent variable to capture the persistence in a broker's recommendation activity. We also control for analyst and firm characteristics, which are averaged within each broker-month combination. To control for broker size, we include the number of analysts per broker (*Log #Anaperbroker*).

Table 2.5 reports the estimates of broker future recommendation activity conditional on the fraction of influential recommendation changes this month. In the regressions, broker fixed effects and calendar month fixed effects are also included. From the positive coefficients of *#InfluRecchg/#Firms* in column 1 to column 3, we see that the fraction of influential recommendation changes this month is indeed positively associated with the fraction of recommendation changes and activity next month. To interpret the economic significance of the coefficients of 0.321, 0.157, and 1.154 (models 1 to 3), a one standard deviation increase in the fraction of influential recommendation changes this month leads to a 10%, 7%, and 5% increase in next-month fractions of recommendation changes, uncontaminated recommendation changes and total recommendation activity, respectively.[36] These results indicate that brokers are indeed more active next month if they have more influential reports this month.

The results in column 4 to column 9 inform us whether broker-level feedback effect is persistent. We find that the feedback effect dissipates and becomes insignificant after three months, and disappears completely after twelve months. The feedback effect hence appears short-lived. This supports the information hypothesis and is consistent with the earlier presented evidence at the analyst level.

---

[36] The economic significance is measured by the coefficient of *#InfluRecchg/#Firms* multiplied by its standard deviation and then divided by the mean of dependent variable (see Table 2.1).

2.5 Additional tests: Evidence on capacity constraints

In this section, we examine the impact of capacity constraints. Our result that research effort is predicted by past successes most likely implies that analysts have capacity constraints and are unable to write and publish reports simultaneously on all the firms that they cover. Otherwise an analyst who receives feedback from the market that she wrote a great report can immediately turn around and write more reports on other firms instantaneously. This would prevent us from finding any predictability from the current quarter to the next quarter.

However, recent literature shows that agents, even if professional investors, face capacity constraints and limited attention, e.g., directors (Fich and Shivdasani (2006), and Falato, Kadyrzhanova, and Lel (2014)), fund managers (Lu, Ray, and Teo (2016)), Federal Reserve supervisors (Eisenbach, Lucca, and Townsend (2016)), and institutional investors (Kempf, Manconi, and Spalt (2016)). The issue of limited attention seems particularly pertinent to sell-side analysts, who play a crucial information intermediary role in both analyzing public information and generating private information (see Driskill, Kirk, and Tucker (2016) and Harford *et al.* (2016)). Busy agents endowed with a limited amount of process capacity might take shortcuts whenever they can. Hong, Stein, and Yu (2007) show that agents who have limited attention shift from one univariate model to another whenever enough evidence accumulates against the incumbent model. Agents who have limited attention are also more likely to stick to one model if they receive feedback that the model is effective.

We use the number of firms that an analyst covers as a proxy for the analyst's capacity constraints. In each quarter, we sort analysts based on how many firms that they cover (using the number of firms on which they have outstanding ratings on). The analysts who cover an above-median number of firms are classified as the capacity constrained analysts and others are capacity unconstrained. We then compare the marginal effects of *Influential dummy* on the next-quarter recommendation change probability and total recommendation activity across capacity constrained and capacity unconstrained analysts in Table 2.6, and find that constrained and less constrained analysts both learn about the quality of their decisions from the stock market. However, constrained analysts display more predictability.

2.5.1 Are future recommendation changes still influential?

After showing that analysts update their beliefs about the quality of their research and devote more effort in the upcoming quarter conditional on having influential recommendation changes, we now test whether their future reports are also more likely to be influential. Loh and Stulz (2011) show that being influential in the past is significantly related to the current likelihood of being influential. From the above results, analysts who find that the market endorses the information/model in their most recent report start to apply it quickly to other firms. If the analyst is able to apply the valuable information and framework to the next report and this cannot be easily replicated by investors reading the first influential report, we expect the future recommendation changes from that analyst to also be influential. As we argued, such information is likely to be short lived, and thus the effect of the useful information will dissipate some time later. However, if the skill hypothesis dominates, the increased likelihood of influential reports should not dissipate.

Table 2.7, Panel A reports the marginal effect of having influential recommendation changes in this quarter on the influential probability in quarter t+1 and quarter t+4, controlling for analyst and firm average characteristics. The Chi-square statistics and p-values reveal whether there is any difference between the coefficients associated with these two samples. We find that the marginal effect of *Influential dummy* is larger for quarter t+1's recommendation change influential probability than it is for quarter t+4. Importantly, the difference is both economically and statistically significant, which indicates that the information contained in the current quarter's influential recommendations does indeed contribute to more influential recommendations changes next quarter but this information becomes less useful one year later.

In addition, we estimate the impact of current-month influential recommendation changes on the future influential probability at the broker-month setting. Panel B of Table 2.7 reports the marginal effect of having influential recommendation changes this month on the broker's influential probability in month m+1, m+3, and m+12, controlling for analyst and firm characteristics averaged within the broker-month, as well as the number of analysts per broker. Broker fixed effects and calendar-month fixed effects are also included in these estimations. We can see that the marginal effect of *Influential dummy* is statistically and economically significant in predicting the next-month influential probability, while the predictability decreases after three months and disappears one year later. The decreasing pattern of broker future influential probability is consistent with our evidence at the analyst level.

Overall, the findings of increasing future influential probability at the analyst and broker level further support the information hypothesis—that the market's validation of the analyst's information leads the analyst or other analysts within the same broker to successfully apply the same framework or information to other related firms in the subsequent periods. In addition, the fact that the current influential probability is positively related to future influential probability is not consistent with an overconfidence hypothesis. An overconfidence explanation of the feedback effect would say that analysts revise more frequently after recent success because they become overconfident. Overconfidence-motivated recommendation changes should not be associated with a higher influential likelihood.

2.5.2 Feedback effect pre- and post-Reg FD

We now examine whether the feedback effect is stronger or weaker in the post Reg FD period. We define the post Reg FD period as the fourth quarter of 2000 and after. Some studies find that channels of analyst's private access to management dry up after Reg FD (e.g., Cohen, Frazzini, and Malloy (2010)). The source of information included in an analyst's influential report can come from either private access to management, or the analyst's better ability to interpret available information. One possible mechanism of our information channel is that after an analyst issues an influential rating change, she is able to get access to more private channels to management afterwards due to the attention that her successful report generated. If the source of the analyst's future influential recommendation changes is due to such private channels of information access, these sources should dry up after Reg FD. But if the source of the information is the analyst's better interpretation of already public information, then the feedback effect should not dissipate after Reg FD.

Table 2.8 reports that the feedback effect after Reg FD remains statistically and economically significant. The results indicate that the feedback effect is not solely driven by private sources of information between the analyst and firm management. Analysts' better interpretation of already public information is also an important ingredient of the information in the successful report that gets transmitted to future reports.

2.6 Additional tests and alternative hypotheses

2.6.1 Feedback effects for "failures"

Thus far our results are based on successful analysts and we proxy for success using the presence of an influential recommendation change in the current quarter. We find that such success indeed changes the next period behavior and effort of analysts. Instead of examining the impact of success, some studies examine the impact of failures on agents. Coval and Shumway (2005) find that futures traders who experienced losses in the morning are more inclined to take above-average afternoon risks. Howell (2017) find that entrepreneurs who receive negative feedback in a business venture competition are more likely to abandon their ventures in the future. To investigate the effects of feedback using failures instead of successes, we define an analyst-quarter as a failure if that analyst issues more wrong-direction recommendation changes than right-direction recommendation changes, and she does not have any influential recommendation changes in a quarter. We examine the effect of failure on the next quarter recommendation activity. As the flip side of success, failure is expected to lead to less research effort. But it could be that bad analysts now have to work harder to make more recommendation changes to increase their chances of turning things around. In unreported results, we find that on average bad analysts are less likely to revise their recommendations and decrease their total recommendation activity in the next quarter.

2.6.2 Alternative hypothesis: Investor overreaction to past success

An alternative explanation for the greater impact of next-quarter recommendation changes issued by analysts who make influential revisions is that investors simply overreact to these analysts. To investigate this, we compare the next-quarter recommendation change performance of analysts who have successful recommendations and those who do not. Each of these two samples is further subdivided into recommendation upgrades and downgrades. Then, for each subsample, we form a daily-rebalanced calendar-time portfolio that buys stocks from trading day 2 following the revisions to day 21, i.e. a one-month drift. Following the standard approach in Barber, Lehavy, and Trueman (2007), we compute average daily returns where one dollar is placed in each revision and the weight of the revised stock varies from day 2 to day 21 according to its cumulative return since entering the portfolio. The portfolio's daily returns are then compounded to monthly returns, and the returns in excess of the risk-free rate are regressed on the Carhart (1997; Fama and French (2015) five factors. Consequently, the intercept measures the revision drift of each recommendation change portfolio.

In Table 2.9, we find that the intercepts of the regressions are significantly positive for upgrades and significantly negative for downgrades, indicating that there is a stock-price drift to analyst revisions. Of interest is the difference of intercepts between the influential portfolio (i.e. buy upgrades and short downgrades of analysts who make at least one influential recommendation change in the current quarter) and the non-influential portfolio (i.e. buy upgrades and short downgrades of analysts who do not make any influential recommendation changes in the current quarter), but we find that the difference is statistically insignificant. Similar results are also found for the three-month and six-month drifts. This is evidence that the post-revision drift of successful analysts is both economically and statistically indistinguishable from that of other analysts who do not make influential rating changes. Overall, we find no evidence that investor overreaction is the cause of the larger stock-price impact of next-quarter recommendation changes of previously influential analysts.

This test also excludes another potential channel—the observed feedback effect may be driven by the investors who notice influential recommendation changes, and then analysts adjust their behavior to cater to this increased investor attention. Higher attention to the recently successful analyst should then lead to a smaller drift in the analyst's next-quarter recommendation change portfolio compared to that of unsuccessful analysts but we do not find such evidence.

2.6.3 A trading strategy that benefits from the feedback effect

Our main evidence is that influential recommendation changes can predict the future issuance of recommendation changes by the analyst *and* also by the broker. Such spillover effects can lead to a profitable trading strategy but only under certain conditions. First, the sign of the influential recommendation changes should be able to predict the direction of future recommendation changes. If the information in influential recommendation changes is applicable for a subset of stocks as our information hypothesis suggests, it seems reasonable that this information can be applied in a directionally similar manner to other covered firms. Second, for such a trading strategy to be work, investors have to respond insufficiently to such predictability. If investors can figure out the implications of the influential recommendation changes for the value of all other firms covered by the same analyst and broker, they would react in anticipation of the feedback effect predicting that more similar-content reports will be issued.

Each month, we long a portfolio of firms without recommendation changes but whose industries (Fama-French 30 groups) on average had influential upgrades. We hold the stocks for three months to mirror the quarterly horizon we use for most of our earlier tests. We also short a similar portfolio of firms without recommendation changes but whose industries on average had influential downgrades. To differentiate industries based on the strength of their influential upgrades or downgrades, we compute the difference between the number of influential upgrades and influential downgrades (*DiffUpDown_Influ)*. Because large industries are more likely to have extreme values of *DiffUpDown_Influ*, we control for industry size by dividing industries first into two groups based on the number of firms in the industry, and then sorting industries into five quintiles based on *DiffUpDown_Influ* within each group.[37]

Firms in the industries that are sorted into each quintile are weighted using one plus the firm's prior-month return. Compared to equal-weighting firms, these weights mitigate microstructure biases such as the bid-ask bounce (see Asparouhova, Bessembinder, and Kalcheva (2013)). Panel A of Table 2.10 reports the average monthly raw returns, alphas, and summary statistics for each portfolio. One can see immediately that quintile 5, which contains firms in industries that have the most

[37] An alternative way is to sort industries based on the number of influential upgrades divided by the total number of influential recommendation changes. While we do get similar results, this approach causes smaller industries to dominate the extreme portfolios.

influential upgrades has the highest abnormal returns. This means that in industries that have the most influential upgrades, firms without recommendation changes can earn positive abnormal returns in the subsequent three months. In contrast, quintile 1, which contains firms in industries with the most influential downgrades is associated with negative alphas, although they are not statistically significant.[38] Finally, we see that the hedged portfolio that longs quintile 5 and shorts quintile 1 earns a statistically significant Fama-French three-factor (five-factor) alpha of 0.59% (0.60%) per month.

The results reveal that the information in influential recommendation changes in an industry contains a common (rather than competitive) component that is predictably incorporated into other firms over the next few months. We have shown in our earlier tables that influential recommendation changes lead to a higher likelihood of recommendation changes in the future months. Column 8 of Table 2.10 reports the average number of upgrades and downgrades as well as the ratio of upgrades over downgrades for each quintile in the months where stocks are held. In column 8 of Panel A, we see that the quintile portfolio 1 (5) has the lowest (highest) ratio of average number of upgrades to the average number of downgrades, and the difference of this ratio between quintile portfolios 5 and 1 is statistically significant at the 1% level. Overall, the findings in Panel A indicate that the ability of these portfolios to predict future upgrades and downgrades likely contributes to the abnormal alphas that we observe. These future upgrades and downgrades are predicted by the feedback effect in which the successful analysts or analysts from successful brokers predictably increase

[38] The weaker results with downgrades is consistent with the information in influential downgrades being a mix of common and competitive information so that it does not lead unambiguously to other firms in the same industry being downgraded later. A downgrade with competitive information might mean that other firms will likely get upgraded instead.

their likelihood of issuing recommendation changes. This evidence of within-industry predictability might be related to the lead-lag effect within industries since firms without recommendations might be smaller than firms with influential recommendation changes. Hou (2007) shows that within an industry, the returns of large firms can predict the returns of small firms due to the slow diffusion of common industry information. In our portfolios, firms without recommendations are indeed smaller than firms that experience influential recommendation changes. To investigate the role of the lead-lag effect, we partition our sample into large firms (those with market capitalization above the 80^{th} NYSE size percentile (highest quintile), and reported in Panel B) and small firms (all other firms, reported in Panel C). We can see that abnormal returns are still positive and statistically significant even for large firms. We see also that the predictability of future upgrades and downgrades is actually stronger in large firms. These findings indicate that our results are not fully driven by the lead-lag effect.[39]

We also estimate Fama and MacBeth (1973) cross-sectional regressions each quarter with an explicit control for the lead-lag effect. The dependent variable is the quarterly return for firms without recommendation changes in the prior month whose industries had influential recommendation changes. The independent variables are observed in the month-end before the start of the quarter. We proxy for the lead-lag effect using the value-weighted prior-month return of firms in the same industry that

[39] We also check whether the predictability in returns in simply due to past recommendation changes or due to only influential recommendations as we argue. When we form portfolios according to the uncontaminated but non-influential recommendation changes rather than only influential recommendation changes, we find that there is no predictability in the returns of firms without recommendations. This implies that our abnormal returns are driven by the spillover effect from influential recommendation changes, and not simply a general spillover effect from all recommendation changes.

are in the largest size quintile based on the NYSE breakpoints determined using the CRSP sample. We also control for firm size, book-to-market, lagged return, short-term and long-term momentum, return volatility, turnover, institutional ownership, and industry size. Table 2.11 reports time-series averages of the coefficients and the associated time-series *t*-statistics (in parentheses). The quarterly returns used as the dependent variable are non-overlapping to ensure the *t*-statistics will not be overstated.[40] Our variable of interest is *UpInfluQuintile*, which is the quintile rank indicating the favorableness of influential recommendation changes (favorableness is determined by the number of influential upgrades minus the number of influential downgrades).[41] Across four specifications in Table 2.11, the coefficient of *UpInfluQuintile* is positive, showing that the spillover effects of influential recommendation changes in the industry onto other firms. These coefficient estimates are robust to the control for the lead-lag effect.

The evidence of predictability in returns is also consistent with investors underreacting to the implications of the feedback effect. They do not incorporate the predictability in analysts' influential recommendation changes on the likelihood of observing more same-signed recommendation changes in the same industry in subsequent periods.

2.7 Conclusion

Recent literature examines how decision makers extract information from stock

---

[40] We use quarterly regressions here to mirror the quarterly horizon we use in most of our tests. We obtain similar results if we use a monthly cross-sectional regression instead.

[41] The results are qualitatively similar if we use the difference between the number of influential upgrades and influential downgrades *(DiffUpDown_Influ)* instead.

prices in making decisions. As prominent information intermediaries, sell-side analysts issue research reports on the covered firms, and some of these reports are associated with extremely large abnormal returns. We show strong evidence that analysts learn from the significant market reactions elicited by their influential recommendation changes and update beliefs about the quality of their research. Specifically, conditional on having influential revisions in this quarter, analysts devote more efforts in the upcoming quarter, for example, they are more likely to revise recommendations and increase their total recommendation activities.

We find that these results are mostly consistent with an information hypothesis. In essence, analysts who move the market treat this as positive feedback about the content or approach in their influential report. They then quickly apply this to the reports that they write in the next quarter and this leads to more active and influential recommendation activities. One year later, however, such effects reduce since the information is more fully incorporated into the analyst's coverage universe after a series of reports. The information hypothesis is also supported by the existence of feedback effect at the broker level. The evidence in this paper is also consistent with analysts facing capacity constraints, just like other professional investors. Getting positive feedback leads them to immediately implement their ideas on other firms so that the predictability we find is indeed stronger for busier analysts.

We also examine whether a trading strategy that seeks to benefit from such feedback effects can earn abnormal returns. If a firm that did not experience any recommendation change is in an industry with many influential recommendation changes in one direction, it is likely that this firm will soon experience a same-direction recommendation change if analysts' past success leads them to revise their ratings subsequently on other similar firms. We find indeed that such a trading strategy can earn abnormal returns of up to 0.6% per month. This is evidence that investors do not fully incorporate such analyst feedback effects to other firms when they observe influential recommendation changes in an industry.

Overall, this study sheds new light on how an important group of decision makers, analysts, whose research influences stock price, learn from stock-price reactions when producing future research reports.

CHAPTER 3

IS THE CEO SOLELY RESPONSIBLE FOR CORPORATE DECISIONS?

3.1 Introduction

Most academic literature focuses on the role of chief executive officers (CEOs) in firm decision-makings, the strand of studies typically examines how the CEO compensation, experience, behaviors and personalities affect firm policies and performance. In reality, however, the publicly listed companies are run by a team of top managers, where individuals with different beliefs and opinions collectively decide what the corporation should do. Therefore, only focusing on the CEO cannot provide a whole picture of how managerial team influences corporate decisions. This paper aims to fill this void by examining how other senior managers cooperate with CEOs in determining firm investment, financing and payout policies, as well as their impacts on firm value.

Recent studies have begun to lay stress on the importance of other senior managers, especially for chief financial officers (CFOs). They find that CFO equity incentives play a stronger role than those of the CEOs in earnings management, debt maturity choices, and stock price crash risk (see Jiang, Petroni, and Yanyan Wang (2010), Chava and Purnanandam (2010), and Kim, Li, and Zhang (2011)). The findings in Ben-David, Graham, and Harvey (2013) also imply that CFO actively contribute to firm investment and financing decisions. Different from these papers, we want to go a step further by looking at the role of whole top manager team in firm decisions and performance, instead of focusing on only one person or just comparing the relative influence of CEOs and CFOs on different firm policies.

We test our story by focusing on the impact of one of managerial personalities—over-optimism on firm policies. Over-optimism is defined as an excessive belief that future events will be positive, and is figured out by the prior literature as a strong and robust psychological trait across many samples of subjects, especially among top executives. Therefore, drawing on the findings of prior research that overoptimistic CEOs are more likely to make aggressive firm decisions,[42] we can examine whether overoptimistic (non-optimistic) non-CEO managers would reinforce (mitigate) the influence of overoptimistic CEOs on firm policies.

Following the methodology of Campbell *et al.* (2011), we construct a modified version of Malmendier and Tate (2005) stock options-based over-optimism measure. The measure captures the propensity of a manager to voluntarily hold vested in-the-money stock options. Although it is optimal for risk-averse, undiversified, and utility maximized executives to exercise their granted options early if it is sufficiently in the money, the overoptimistic managers believe that the stock prices of their companies will increase and postpone option exercise to earn more capital gains.

To test whether and how non-CEO managers cooperate with CEOs in firm decision-makings, we estimate and compare the investment, financing and payout policies across four groups of firms, where the CEO and non-CEO manager team are both overoptimistic (*Group1_both*), or only one of them is overoptimistic (*Group2_CEO* and *Group3_NonCEO*), or neither of them is overoptimistic

[42] For example, see Malmendier and Tate (2005), Malmendier and Tate (2008), Malmendier, Tate, and Yan (2011), Hirshleifer, Low, and Teoh (2012), Banerjee, Humphery-Jenner, and Nanda (2015), etc.

(*Group4_Neither*). We define a non-CEO manager team as over-optimism if at least half of the non-CEO top managers are overoptimistic.[43] We find that, on average, firms whose CEOs and non-CEO manager team are both overoptimistic or neither of them is overoptimistic account for a substantial part of our sample, about 76%, which suggests that CEOs and other c-suite executives perform similarly in holding/exercising stock options. It might be because they receive same information, or they mimic each other in holding/exercising firm options, or CEOs prefer to hire non-CEO managers with similar personalities. Nevertheless, there are still 24% firm-years where CEOs and other c-suite executives perform differently in holding/exercising their own firms' stock options.

We have several important findings. Firstly, we examine the investment choices of four groups of firms. Our results indicate that, firms with both overoptimistic CEOs and overoptimistic non-CEO manager teams invest 14.9% more than Group 4 firms do, where none of senior managers are overoptimistic. However, the capital expenditure in the Group 2 firms with only overoptimistic CEOs is very close to that of Group 4 firms. The pattern is similar for investment-to-cash-flow sensitivity, asset growth as well as property plant and equipment (PP&E) growth, where other non-optimistic managers mitigate the effect of overoptimistic CEOs on increasing investment-to-cash-flow sensitivity and asset growth. These results indicate that prior findings of the positive relation between overoptimistic CEOs and firm investment are driven by the firms whose overall senior managers are overoptimistic, but cannot be attributed to

[43] We require that each firm-year should have available total compensation (item TDC1 in Execucomp) for top five managers. Except for the CEO, we call other four top managers as non-CEO managers.

overoptimistic CEOs alone.

Next, we examine the managerial over-optimism on financing decisions. Malmendier, Tate, and Yan (2011) and Ben-David, Graham, and Harvey (2013) find that overoptimistic managers issue more debt than their industry peers, because they overestimate firm's future and ability to meet its liabilities. However, we find that such financing decisions only exist in those firms whose CEOs and non-CEO manager team are both overoptimistic. Another issue is that of payout decisions. We find that although the overoptimistic CEOs are reluctant to pay dividends, there is a significant increase in payout (or equivalently, mitigate the decreasing payout) in firms where other top managers are not overoptimistic.

We conduct a number of robustness tests to increase the credibility of our results and interpretation. A possible alternative explanation for the managers' late option exercise behavior could be that managers in firms with strong past stock performance retain their option holdings and also engage in investment, issue more debt, and retain money for future projects to alleviate the underinvestment problem. Therefore, we control for firm buy-and-hold returns over the past fiscal years in our main regressions. In addition, we also control for manager tenure and compensation incentives which may impact manager options holding/exercise behaviors as well as corporate policies. We find that our results are robust to these factors.

Another confounding issue is that the effect of whole senior manager team may reflect corporate governance factors. Goel and Thakor (2008) emphasize the importance of the interaction between manager overconfidence and the corporate governance, including internal organizational governance and board governance. Both concepts of corporate governance not only influence top manager team's constitution, they also affect the firm investment, financing, and payout policies. In addition to internal and board governance, we also consider the influence of CEO power. Powerful CEOs not only influence firm decisions (e.g., see Malmendier and Tate (2005), Bebchuk, Cremers, and Peyer (2011)), they may also foist their beliefs on other top managers, which could drive our results looking like aggressive decisions are made by whole overoptimistic manager team. Interestingly, including corporate governance measures and proxies of CEO power as additional controls does not change our coefficients of interest in a meaningful way.

It is also possible that firms who have opportunities or plan to implement the aggressive policies and hence appoint overoptimistic managers. To gain insight about whether our findings are driven by a causal effect of overoptimistic manager team on firm policies or solely by matching, we restrict our sample to a subset of firm-years for which matching is less likely to happen. We re-estimate the foregoing regressions after eliminating the firm-years in which the CEOs stay in the firms less than 3 years, or the average tenure of non-CEO managers is less than 3 years. We find that our results on the association between overoptimistic manager team and firm policies do not come mainly from the endogenous selection of overoptimistic managers by those firms.

Finally, we examine whether overoptimistic non-CEO managers are helpful in increasing firm value. Malmendier and Tate (2008) show that overoptimistic CEOs overpay for target companies and undertake value-destroying mergers. However, several papers suggest the possibility of an overoptimistic manager increasing firm performance (e.g., Bénabou and Tirole (2002), Compte and Postlewaite (2004), Van den Steen (2004), Hackbarth (2008), and Gervais, Heaton, and Odean (2011)). In recent years, some empirical work exists to support the beneficial aspects of overoptimistic managers (e.g., Galasso and Simcoe (2011), Hirshleifer, Low, and Teoh (2012), Hilary *et al.* (2016), and Phua, Tham, and Wei (2017)). Following the estimation method in Hirshleifer, Low, and Teoh (2012), we find that only firms with both overoptimistic CEOs and non-CEO manager team are able to transform the growth opportunities into firm value, while overoptimistic CEO alone cannot achieve such success. This result is consistent with that in Hilary *et al.* (2016), they show that over-optimism is a related but different bias from overconfident. Over-optimism mainly refers to an excessive belief that future state will be positive, while overconfident individuals place too much weight on the accuracy of private information and an excessive belief in their own skills. Over-optimism generates higher managerial effort, and importantly, this additional effort improves firm profitability and market value.

Recent work shows that overconfident CEOs have a significant impact on various corporate decisions, including investment (e.g. Malmendier and Tate (2005)), mergers and acquisitions (e.g. Malmendier and Tate (2008), Ferris, Jayaraman, and Sabherwal (2013)), financing decisions (Malmendier, Tate, and Yan (2011), Huang, Tan, and Faff (2016)), innovations (e.g. Galasso and Simcoe (2011), Hirshleifer, Low, and Teoh (2012)), stakeholder commitments (Phua, Tham, and Wei (2017)), and accounting practices (Ahmed and Duellman (2013), Schrand and Zechman (2012), Hribar and Yang (2010)). Our paper differs from these studies in focusing on the roles of non-CEO top managers in corporate decision-makings, specifically, we aim to test whether overoptimistic (non-optimistic) non-CEO managers would reinforce (mitigate) the aggressive decisions of overoptimistic CEOs.

Several studies explore the roles of other managers as well as directors in firm outcomes. In addition to the papers that explore the impacts of CFOs on firm policies as we mentioned above, Banerjee, Humphery-Jenner, and Nanda (2015) show that board independence improves decision makings by overoptimistic CEOs by using the passage of Sarbanes-Oxley Act of 2002 (SOX). Our paper, however, does not focus on one manager or directors' monitoring role, we attach the importance to how the whole non-CEO senior manager team helps CEO in firm investment, financing and payout decisions.

Our study also relates to a broader literature that examines the impact of managerial styles on firm decisions. The pioneering paper by Bertrand and Schoar (2003) shows that individual manager matters for corporate decisions, in addition to firm-, industry-, and market-level characteristics. Many subsequent studies have looked at the corporate decisions made by the heterogeneous decision makers in terms of gender, age, education, and experience, etc. Our paper contributes to the literature by looking at the relationship between corporate decisions and the aggregation of top managers' over-optimism. There is very few paper talking about the aggregation effect of managers within the company, except for Garlappi, Giammarino, and Lazrak (2017), who model the dynamic corporate investment where decisions are made collectively by a group of agents holding heterogeneous beliefs.

The paper is organized as follows. Section 2 describes the data and variables of interest. In section 3, we talk about the econometric specifications of our tests. The main empirical findings are provided in section 4. We consider alternative explanations and robustness check for our findings in section 5. Section 6 explores whether non-CEO managers have abilities to transfer the growing opportunities into firm value. Section 7 concludes.

3.2 Data and descriptive statistics

We use Compustat's Execucomp data to construct the over-optimism measure from 1993 to 2015. To be included in the sample, a firm must report the non-missing and non-negative total compensation (as reported in Execucomp item TDC1) for CEO and other four highest paid managers (excluding CEO) during the year.[44] We also restrict our sample to firm-years in which the CEO was in office for the entire year. All accounting data are from Compustat and stock returns are from CRSP. The financial firms (Standard Industrial Classification (SIC) codes 6000 to 6999) and utility firms (SIC codes 4900 to 4999) are excluded from our sample.

3.2.1 Measure of manager over-optimism

Manager over-optimism cannot be observed directly. Malmendier and Tate (2005) and Malmendier and Tate (2008) develop several measures of CEO optimism based on the CEO net stock purchases, options holding and exercising decisions, and the media's descriptions. Unlike CEO, it is difficult to collect enough information on the media's perception of other four top managers' personal characteristics. Considering that our sample contains more than 28,000 top managers, it is infeasible to hand collect each manager's portrayal from the media reports. On the other hand, matching top five

[44] For some firm-years, more than five executives are listed in Execucomp. In such cases, we use only the five executives with the highest compensation.

managers in Execucomp with their stock transaction data in Thomson Reuters Insider Filing will lose many observations. Therefore, we base our over-optimism measure on a manager's stock options holding/exercise decisions in Execucomp.

Options-based over-optimism measure is built on the assumption that it is optimal for risk-averse, undiversified, and utility maximized executives to exercise their granted options early if it is sufficiently in the money (Hall and Murphy (2002)). Top managers are granted large quantities of stock and options, but the transactions of these grants are restricted, which prohibits top managers from perfectly hedging against the risk and leaves them highly exposed to the idiosyncratic risk of their companies. Over-optimism, however, may lead top managers to overestimate the future success of their companies. These overoptimistic managers believe that the stock prices of their companies will increase and postpone option exercise to earn more capital gains. Following Malmendier and Tate (2005), we define a top manager as overoptimistic if she holds her own company's stock options that are more than 67% in the money. The choice of 67% in Malmendier and Tate (2005) comes from calibrating Hall and Murphy (2002) model using a detailed dataset on executive stock option holding and exercise decisions. Since we do not have the detailed options grant data, we take 67% moneyness cutoff as a given for the full sample of executives.[45] We classify a top manager as overoptimistic from the first time she fails to exercise more than 67% in-the-money options and if she subsequently exhibits the same behavior at least one time during the remaining sample period. This classification is consistent with our target that we are interested in exploring top managers who "habitually" exercise options late,

[45] Our results are robust if we use 100% moneyness cutoff.

rather than "transitory" over-optimism effect.

As we do not have detailed data on options holdings and exercise for each option grant, we follow Campbell *et al.* (2011) in calculating the average moneyness of the manager's option portfolio for each year. Specifically, the average moneyness of the options is estimated as the per-option realizable value divided by the average exercise price. For each manager-year, we calculate the average realizable value per option as the total realizable value of the exercisable options divided by the number of exercisable options held by the manager. We then subtract the per-option realizable value from the stock price as the fiscal year end to obtain an estimate of the average exercise price of the options.[46] As we want to identify managers who chose to hold options that could have been exercised, we include only vested options held by the top managers. Using this measure with the Execucomp sample allows us to compute optimism for each top-five manager and enables us to include more firms in our sample. The optimism measure in Campbell *et al.* (2011) can achieve similar classification and empirical results shown in Malmendier and Tate (2005) in which the optimism measure is built on the proprietary stock options holding and exercising data. In addition, Malmendier, Tate, and Yan (2011) show that the optimism measure based on the year-by-year aggregate data on manager vested option holdings available in Execucomp works well after controlling for past stock return performance.[47]

[46] By using this algorithm, we cannot classify managers who have all of their options out of the money or have no options at all. In addition, we cannot classify managers who have no options for every year they are in the sample. In the analyses, we exclude the unclassified managers.

[47] We control for firm past stock return performance in all empirical regressions.

3.2.2 Classification of firms based on the manager over-optimism

According to the above approach in constructing the over-optimism measure, we can identify each top-five manager in our sample as overoptimistic or not. In order to explore whether and how other non-CEO managers as a team cooperate with CEO in deciding investment, financing and payout policies, we aggregate the extent of over-optimism among other four senior managers and define that a non-CEO manager team as a whole is overoptimistic if at least two of the four non-CEO top managers are classified as overoptimistic, and it is non-optimistic if only one or none of the them is overoptimistic. As thus, we have two indicators, one is used to measure whether CEO is overoptimistic or not, another one aims to identify whether a firm has an overoptimistic non-CEO manager team during the year.

Left panel of Table 3.1 describes the fraction of firms with overoptimistic CEOs and that of firms with overoptimistic non-CEO manager team. Since a manager who is identified as overoptimistic in any year remains so throughout the sample period, this may mechanically induce an increase in the fraction of overoptimistic managers as time goes on. However, after the year 1997, the increasing pattern is not very obvious (except for the period 2003-2005). The pattern in the first half of the sample period is similar to that in Hirshleifer, Low, and Teoh (2012). From the column (4) and column (6), we can see that the average frequencies of firms with overoptimistic CEOs (62.97%) is larger than the frequencies of firms with overoptimistic non-CEO manager team (50.39%).

To estimate how the firm policies vary across firms with different combinations of CEO and non-CEO manager team who have the same or opposite extent of optimism, we classify firms into four categories: *Group1_both* includes firms who have an overoptimistic CEO as well as an overoptimistic non-CEO executive team. *Group2_CEO* includes firms whose CEOs are overoptimistic, but whose non-CEO manager team are not overoptimistic. *Group3_NonCEO*, just the other way around, includes firms who have overoptimistic non-CEO manager team but their CEOs are not overoptimistic. In *Group4_neither*, neither CEOs nor non-CEO manager team is overoptimistic.

In the right panel of Table 3.1, we can see that, on average, Group 1 and Group 4 firms account for a substantial part, 75.84%, of our sample period. It shows that CEOs and other c-suite executives perform similarly in holding/exercising stock options, which might be because they have same information, or they mimic each other in holding/exercising firm options, or CEOs prefer to hire people with similar personalities. Nevertheless, there are still 24.16% firm-years in which CEOs and other c-suite executives perform differently in holding/exercising their own firms' stock options.

3.2.3 Descriptive statistics

We use the firm-year panel to estimate the roles of top managers in determining firm investment (i.e. capital expenditure, investment-to-cash flow sensitivity and asset growth), financing (i.e. internal or external financing, debt or equity financing), and payout policies (i.e. dividend and total payout activity), as well as their influences on firm value. In the empirical estimations for different firm policies, we include different controls that are examined to be effective for investment, financing, and payout policies by the extant literature, respectively.

Table 3.2 provides the averages of dependent and independent variables for four groups of firms, respectively. We find that the aggressive behaviors, like large investment, high speed of asset growth, more debt issuance, and less payout concentrate in the firms where both CEO and non-CEO manager team are both overoptimistic, while overoptimistic CEOs alone (*Group2_CEO*) are not able to make that aggressive decisions. Interestingly, the smallest difference of firm investment, financing and payout decisions is found between Group 2 and Group 4 firms, which means that other non-optimistic c-suite managers restrain the aggressive behaviors of overoptimistic CEOs. From the last column, we can see that the difference of these decisions between Group 1 and Group 2 firms are statistically significant at the 1% level.

With respect to the controls, the overoptimistic top five managers in Group 1 manage firms with larger size, higher Tobin's Q, lower leverage, greater performance as measured by profitability, ROA, and annual stock returns. In addition, managers in Group 1 firms tend to have longer tenure and higher delta values. The average percentage of the total compensation to the top five executives that goes to the CEO (CEO pay slice, CPS) is largest for Group 2 firms where CEO is relatively more important (see Bebchuk, Cremers, and Peyer (2011)).

3.3 Econometric specifications

To test the roles that non-CEO managers play in corporate investment, financing and payout decisions, we use two regression specifications. Firstly, we include three dummy variables—*Group1_both*, *Group2_CEO*, *and Group3_nonCEO* into the regressions of firm policies.

$$Y_{it+1} = \alpha + \beta_1 Group1_{it} + \beta_2 Group2_{it} + \beta_3 Group3_{it} + \gamma' X_{it} + \varepsilon_{it} \quad (8)$$

Where *Y* represents investment, financing or payout policies, *X* includes the relevant determinants of *Y*, as well as the measures of corporate governance, manager stock ownership and compensation incentives. The intercept α represents the average value of the dependent variables in the forth-group firms (*Group4_neither*).

Another more intuitive specification is to directly include two dummy variables, *I(Opt_CEO)* and *I(Opt_nonCEO)*, which indicate whether the CEO or non-CEO manager team is overoptimistic or not, respectively. However, as shown in Table 3.1, there are about 76% firm-year combinations in which CEO and other c-suite managers have similar behavior in holding/exercising their own firms' stock options, it means *I(Opt_CEO)* and *I(Opt_nonCEO)* are highly correlated. In our sample, the correlation of these two variables is about 0.54. Therefore, we firstly regress the *I(Opt_nonCEO)* on *I(Opt_CEO)* as illustrated in Equation (2), and keep the residual as the proxy of the "pure" optimism level of non-CEO manager team that cannot be explained by the CEO optimism nor by other common factors, i.e. same insider information, board characteristics, and corporate governance effect etc., which drive CEO and other managers perform similarly. Then, we include the residual from the above regression as well as the indicator variable *I(Opt_CEO)* in the regressions of firm policies in Equation (3). The specification is as follows,

$$I(\text{Opt\_nonCEO})_{it} = \alpha + \beta * I(\text{Opt\_CEO})_{it} + \varepsilon_{it} \tag{9}$$

$$Y_{it+1} = \alpha + \beta_1 \text{I}(\text{Opt\_CEO})_{it} + \beta_2 \hat{\varepsilon}_{it} + \gamma' X_{it} + \eta_{it} \tag{10}$$

When examining the firm-year panel of observations based on the above two specifications, we control for industry and year fixed effects, and standard errors that are heteroskedasticity-consistent and clustered by firm. Industries are defined based on

Fama-French 48-industry groupings. All outcome variables are forwarded by one period and continuous variables are winsorized at the 1% and 99% percentile. We do not include firm fixed effects here, since the variables of interests is sticky. For example, only firms that are classified in each group at least once during the sample period can be included in the firm-fixed-effect estimations in Equation (1), which will reduce large number of observations and induces sample selection problem.

3.4 Main results

3.4.1 Investment and manager over-optimism

We begin by examining whether and how non-CEO manager team impacts corporate investment decisions. In particular, we test whether large increase in firm's investment, sensitivity of investment to cash flows, and asset growth comes from the firms as long as their CEOs are overoptimistic (as previous literature shows), or only comes from those companies whose CEOs and non-CEO manager teams are both overoptimistic. If the latter is the case, it implies that firms other senior managers also play an important role in investment decisions.

3.4.1.1 Capital expenditure

The regression results are provided in Table 3.3. From the column (1) to column (3), we estimate the regressions of capital expenditure by including the indicator variables of different groups of firms. The regression results support our conjecture: the coefficient on indicator variable *Group1_both* is positive and statistically significant at the 1% level, while, the indicator *Group2_CEO* becomes insignificance, and the magnitude of its coefficient is much smaller than that of *Group1_both*.

Economically, 0.945%, the coefficient estimate of *Group1_both*, represents 14.9% increase in capital expenditures from its mean and an increase of 0.15 standard deviations. The 0.178%, the coefficient estimate of *Group2_CEO*, only represents 2.8% increase in capital expenditures from its mean and an increase of 0.03 standard deviations. The p-value at the bottom of the table shows that the difference between the coefficients of *Group1_both* and *Group2_CEO* are statistically significant. These results indicate that the aggressive investment decisions of overoptimistic CEOs can be moderated by other c-suite managers who are not overoptimistic, which leads Group 2 firms much closer to the benchmark firms (Group 4 firms with neither overoptimistic CEOs nor overoptimistic non-CEO manager team) in their industries. Previous studies just examine the relation between overoptimistic CEOs and firm investment (see Malmendier and Tate (2005), Hirshleifer, Low, and Teoh (2012), Banerjee, Humphery-Jenner, and Nanda (2015)), they ignore the effect of other c-suite managers. Virtually, their findings are mostly driven by the firms whose overall senior manager team is overoptimistic. Our findings remind people that do not always attribute the aggressive expansion to CEOs, other c-suite managers are also boosters.

In the column (2), we additionally control for manager tenure and their compensation incentives, including delta and vega. Since the correlation of these variables between CEO and other top managers are high, we only include those variables of CEOs. Delta is defined as the dollar change in a manager's stock and option portfolio for a 1% change in stock price, and measures the manager's incentives to increase stock price. Vega is the dollar change in a manager's option holdings for a 1% change in stock return volatility, and measures the risk-taking incentives generated by the manager's option holdings.[48] It appears that younger managers, the managers with higher delta and lower vega will invest more. But these control variables do not influence our conclusion that firms make more investment is due to their whole top manager team is overoptimistic.

In the column (3), we further control for firm past performance. As stock options are often granted at the money, the moneyness of options is influenced by firm stock returns subsequent to the grant date. Thus, the option-based measure of manager over-optimism may also proxy for the relation between past performance and investment rather than manager personal characteristics. Thus, the *Group1_both* may represent those firms who perform well and earn a lot in stock market, which lead them to invest more in capital expenditures. Malmendier, Tate, and Yan (2011) find that the optimism measure based on the aggregate vested option holdings in Execucomp performs well after controlling for past stock performance. Therefore, in column (3), we further control for firm buy-and-hold returns over the past fiscal years by following Hirshleifer, Low, and Teoh (2012). Firstly, to determine the number of years of stock returns we should control for, we run regressions of natural logarithm of one plus moneyness on several lags of annual stock returns, including the annual stock return leading up to the fiscal year-end for which moneyness is being measured. We also include the natural logarithm of market capitalization as an additional control variable. Similar to Hirshleifer, Low, and Teoh (2012), we find that moneyness is significantly associated with contemporaneous annualized stock returns and up to 6 years of lagged stock returns. Then, we compute the cumulative stock return over the lesser of the CEO's

[48] We use the approximation method detailed in Core and Guay (2002) to calculate delta and vega of the stock and option portfolios.

tenure or 7 years, the cumulative return stops just before the start of the fiscal year when the dependent variable is measured. We use this cumulative past stock return as a control variable, and find that the results continue to hold, where firms with both overoptimistic CEO and non-CEO manager team have the largest capital expenditures than other groups of firms.

In column (4) and column (5), we use the econometric specification in Equation (2) - (3) to test whether overoptimistic non-CEO manager team also plays an important role in corporate investment decisions, after controlling for CEO over-optimism. The results in column (4) show that we can replicate results in the prior studies on the relation of overoptimistic CEO and corporate investment decisions, in which investment is positively related to the indicator variable of overoptimistic CEO. Furthermore, we find that the overoptimistic non-CEO manager team also positively and significantly associate with the investment decisions.

Overall, the empirical evidence in Table 3.3 reveals that non-CEO senior managers also play a crucial role in firm investment decisions. Without the overoptimistic colleagues, overoptimistic CEO alone cannot significantly influence firm investment decisions.

3.4.1.2 Sensitivity of investment to cash flows

We next examine how overoptimistic non-CEO manager team influences a firm's investment sensitivity to cash flows. Malmendier and Tate (2005) find that overconfident CEOs spend more of their cash flows on capital expenditures. We want to test whether such effect is driven by overoptimistic CEO alone or it needs other c-suite managers also to be optimistic. We examine the investment-cash-flow sensitivity model that is widely studied in the literature (e.g., Almeida, Campello, and Weisbach (2004); Malmendier and Tate (2005); Foucault and Fresard (2014)). The capital expenditure and cash flow in year *t*+1 are all normalized by total assets at the beginning of the year. Other control variables are in year *t*.

The results are presented in Table 3.4. We find that only Group 1 firms with both overoptimistic CEO and overoptimistic non-CEO manager team spend more of their cash flows. The results are also economically significant. Compared to the benchmark, firms in Group 1 will spend more than 30% of their cash flows to investment. However, the firms in Group 2 do not increase their spending of cash flow to capital expenditures, their investment-cash-flow sensitivity is very close to that of firms in Group 4 with neither overoptimistic CEO nor overoptimistic non-CEO manager team. The difference of the first two interactions are statistically significant at the 5% level from the p-value at the bottom of the table. In addition, the inclusion of manager incentive and past stock performance does not weaken our results. When we replace the group indicators with the manager over-optimism indicators in column (5), we find that overoptimistic non-CEO manager team plays an important role in the sensitivity of investment to cash flow.

3.4.1.3 Asset growth and manager over-optimism

Banerjee, Humphery-Jenner, and Nanda (2015) show that overconfident CEOs, with their overly positive views on firm prospects, seek greater asset growth, whether measured by total asset growth or property, plant, and equipment growth. In this paper, we test whether the effect of overoptimistic CEO on asset growth can be moderated by other non-overoptimistic senior managers. The PP&E growth represents the log increase in property, plant, and equipment from year *t* to year *t*+1, and similarly for total asset growth. The regression results are reported in Table 3.5. We find that all three groups, compared to the benchmark Group 4 firms who have neither overoptimistic CEO nor overoptimistic non-CEO manager team, are positively associated with the PP&E growth and total asset growth. For example, the managers in Group 1 firms tend to grow the PP&E more than 90% of its mean and 0.278 of its standard deviations. The estimated PP&E growth in Group 2 firms represents 18.3% of its mean and 0.06 of its standard deviations. From the p-value at the bottom of the table, we find that the coefficient of indicator variable *Group2_CEO* is significantly smaller than that of *Group1_both* at the 1% level, suggesting that non-CEO executive team also plays an important role in asset growth. Specifically, if the non-CEO senior managers are also optimistic, they will accelerate the aggressive decisions of overoptimistic CEOs. However, if the non-CEO colleagues are not overoptimistic, they would restrain the CEOs' aggressive actions.

When we further control for manager tenure and incentives, as well as the past stock performance in column (2) and column (3), the Group 2 firms do not associate with faster asset growth than benchmark group any more. Interestingly, the coefficient of *Group3_nonCEO* indicator is always significant, and its magnitude is larger than that of *Group2_CEO*. The empirical evidence of the managerial over-optimism on total asset growth illustrated in column (4) and column (8) are similar to that of PP&E growth. Overall, the results in Table 3.5 emphasize the important role of other c-suite managers in firm expansion decisions.

3.4.2 Financing choice and manager over-optimism: Debt vs. equity

Malmendier, Tate, and Yan (2011) show that, conditional on having to access public securities markets, overconfident managers choose debt over equity, since equity prices are more sensitive to differences in opinions about future cash flows. In addition, debt can allow current shareholders to remain the residual claimant on the firm's future cash flows. From the overall financing aspect, Hackbarth (2009) and Ben-David, Graham, and Harvey (2013) also find that overoptimistic manager chooses a higher level of debt because she is confident on firm's future and ability to meet its liabilities. In this section, we test whether overoptimistic CEO alone can significantly affect the corporate financing decisions.

The regression results are presented in Table 3.6. We find that Group 1 firms (*Group1_both*) prefer debt to equity across three specifications from column (1) to column (3). The coefficient estimates are significant at the 1% and 5% level and range from 0.00912 to 0.0133, representing a 33.3% to 48.5% increase in net debt issuance from its mean and an increase of 0.08 to 0.12 standard deviations. However, the Group 2 firms with only overoptimistic CEO display no preference on debt issuance, when controlling for manager characteristics and firm past performance. In addition, the results in column (5) indicate that non-CEO executive team is more important than CEOs in determining external financing choices. This result is not surprising as literature documents that CFOs play an equally or even more important role than that of CEOs in financing decision-makings.

3.4.3 Dividend payout and manager over-optimism

Deshmukh, Goel, and Howe (2013) show that an overconfident CEO views external financing as costly and hence builds financial slack for future investment needs by lowering the current dividend payout. We examine whether this reduction in dividends associated with overoptimistic CEO alone or it needs non-CEO manager team also to be overoptimistic. We use the same econometric specifications as those in the above sections, in which we estimate the effect of non-CEO manager team on firm payout decisions. The results are illustrated in Table 3.7. We find that the magnitude of the coefficient estimate on *Group2_CEO* is the half of that on *Group1_both*, the difference of these two coefficients are statistically significant at the 1% level. In terms of economically significance, the coefficient estimates of *Group1_both* range from -0.529 to -0.664 across three specifications in column (1) to column (3), representing a 50.7% to 63.6% decrease in dividend payment from its mean and a decrease of 0.359 to 0.45 standard deviations.

The results here are consistent with the findings documented in Deshmukh, Goel, and Howe (2013) that overoptimistic CEOs prefer to reduce dividend payment. Furthermore, we also find that if an overoptimistic CEO is accompanied by an overoptimistic non-CEO manager team, she is more likely to retain the money for the future investment and reduce the dividend to shareholders. However, if other c-suite managers are not overoptimistic, they would moderate the effect of overoptimistic CEO on the dividend reductions. The results are quite robust to the additional controls for manager characteristics and firm past performance. The column (4) further verifies that non-CEO managers also play an important role in determining the dividend payment. From column (5) to column (8), we can see that the results are similar when we replace the dividend payment with total payout—the sum of dividend and repurchase, then scaled by the market capitalization.

3.5 Robustness tests

Two main concerns about the interpretation of our results are omitted variables that simultaneously inspiring whole manager team to hold deeply in-the-money options and inducing firm to implement aggressive decisions, and the accuracy of option-based over-optimism measure. In this section, we conduct a number of robustness check to increase the credibility of our results and interpretation.

3.5.1 Corporate governance effect

We classify firms into four categories according to the CEO and other non-CEO managers' over-optimism, the category indicators may represent other effects rather than the manager over-optimism effect. For example, it may merely reflect corporate governance factors. Goel and Thakor (2008) emphasize the importance of the interaction between manager overconfidence and the corporate governance. There are two concepts of corporate governance, one is the "internal organizational governance" that refers to the internal promotion process by which managers move up through the corporate hierarchy, and the other is the "board governance" representing the board's decision to promote or fire a manager. Both concepts of corporate governance not only influence the optimism level of manager team, they also affect the firm investment, financing, and payout policies. To tease out these possible confounding effects, we further control for governance characteristics in our main regressions. Our measures of corporate governance are the board size and board independence.[49] Following Harford, Mansi, and Maxwell (2008), board size is generally measured as the number of

[49] Using E-index as corporate governance measure gives similar results.

directors on the board divided by the log of total assets, and board independence is computed as the ratio of independent directors to total directors.

The results are illustrated in Table 3.8, where corporate governance measures are included in the various regressions of firm policies. We can see that our results are still robust that firm policies are decided by a whole top manager team, overoptimistic CEO alone cannot make such aggressive decisions. Although the difference of capital expenditure is not statistically significant between Group 1 firms and Group 2 firms, it is still economically significant. In the regressions, we also control for the other determinants of outcome variables, including firm and manager characteristics, compensation incentives, as well as the past firm performance.

3.5.2 The effect of CEO power

Prior studies show that powerful CEOs can influence the firm policies. For example, Malmendier and Tate (2005) show that CEOs who have accumulated additional titles (e.g. the chairman of the board) display higher sensitivity of investment to cash flow. Bebchuk, Cremers, and Peyer (2011) suggest that dominant CEOs, as proxied by a high CEO pay slice (CPS) can provide a useful tool for studying the performance and behavior of firms. On the other hand, powerful CEOs might foist their beliefs on other top managers. If powerful CEOs postpone exercising their stock options, they may force other senior managers to do so, or other senior managers learn from the late exercising behavior of powerful CEOs. Thus, it is possible that our results—only firms with optimistic whole manager team make aggressive decisions are driven by the effect of powerful CEOs.

In order to mitigate such concern, we further control for the CEO power, as proxied by the CEO pay slice (CPS) defined in Bebchuk, Cremers, and Peyer (2011)—the fraction of the aggregate compensation of the top-five executive team captured by the CEO, and an indicator variable which equals one if CEO is the chairman of the board, and zero otherwise.[50] We re-estimate the foregoing regressions by including these two variables, and present the results in Table 3.9. We can see that the additional controls of CEO power do not influence our results that non-overoptimistic managers can attenuate the aggressive effect of overoptimistic CEOs on firm policies. In the regressions, we also control for firm and manager characteristics, as well as the past firm performance.

3.5.3 Matching between manager over-optimism and firm decisions

Graham, Harvey, and Puri (2013) find that growing firms tend to have more optimistic CEOs. It is possible that firms who have opportunities or plan to implement the aggressive policies and hence appoint overoptimistic managers. To gain insight about whether our findings are driven by a causal effect of overoptimistic manager team on firm policies or solely by matching, we restrict our sample to a subset of firm-years for which matching is likely to be less important. We measure manager over-optimism as a persistent trait. However, firm growth opportunities vary over time as its strategic resources and competitive environment shift (Hirshleifer, Low, and Teoh (2012)), which suggesting that matching effects between manager over-optimism and time-varying firm decisions are likely to be strongest when the managers are first appointed. Therefore, we re-estimate the foregoing regression after eliminating the

[50] We use data in ISS (Institutional Shareholder Services, formally RiskMetrics) database to construct the indicator variable that whether CEO is the chairman of the board.

firm-years in which the CEOs stay in the firms less than 3 years, and where the average tenure of non-CEO top managers is less than 3 years.

Table 3.10 summarizes the coefficients of the variables of our interest. The related control variables are included in but not reported in the table, as well as industry fixed effects and year fixed effects. In column (2), we report the coefficients of group indicators that interact with the cash flow, although we do not differentiate it with pure group indicators reported in other columns. Except that the investment to cash flow sensitivity is not significantly larger or lower in any group of firms, the *Group1_both* continues to be statistically and economically significant in other tests. These findings suggest that the relations between overoptimistic manager team and firm policies do not come mainly from the endogenous selection of overoptimistic managers by those firms.

3.5.4 Private information

Managers who fail to exercise their own firms' stock option may have positive private information about future stock prices that make holding options attractive. Such favorable information may also explain firms' subsequent behaviors on active financing, retaining money, and doing investment. Since Group 1 firms have both overoptimistic CEO and non-CEO manager team, it displays a strongest signal of private information, as whole managers refrain from exercising deeply in-the-money options. It seems that the previous results are driven by private information. In fact, private information should be short-lived and it is unlikely that the same manager repeatedly receives positive information. However, our over-optimism measure is persistent which targets manager habitual tendency to postpone exercising options. In addition, Carpenter and Remmers (2001) document that there is no evidence that managers exercise options based on their private inside information. Furthermore, Malmendier and Tate (2005) find that the manager failed to exercise in-the-money options in the past can predict the similar behavior in the future, but is not associated with the current or future stock price performance. They also find that the CEOs cannot beat the market by holding options beyond the threshold. Therefore, it is unlikely that the inside information drives our results.

3.5.5 Risk tolerance

Another possible alternative explanation for our findings is that managers who fail to exercise deeply in-the-money option are due to their high risk tolerant rather than over-optimism. Such high-risk tolerance can lead to more aggressive behaviors that we find from our above results. Even if this is true, it would not overturn our key insight, that non-CEO manager team who has opposite managerial traits from CEO can mitigate the impact of CEO traits on firm policies. It still suggests the important role of non-CEO managers and the cooperation among senior managers.

3.6 Overoptimistic managers and firm value

The evidence provided so far is all neutral, where we can see overoptimistic non-CEO managers reinforce the effect coming from the overoptimistic CEOs, while if other non-CEO managers are not overoptimistic, they can mitigate the effect produced by overoptimistic CEOs in the firms. In this section, we examine whether overoptimistic non-CEO managers are helpful in firm performance. Malmendier and Tate (2008) show that overoptimistic CEOs overpay for target companies and undertake value-destroying mergers. However, some studies support the beneficial aspects of overoptimistic managers, because overoptimistic managers are more creative and make more efforts to achieve their goals. For example, Hirshleifer, Low, and Teoh (2012) document that CEO over-optimism allows firm to translate growth opportunities into realized firm value, by using industry price to earnings (PE) ratio as an exogenous proxy for firm growth opportunities to address the endogenous issue under the interpretation of the regression of firm value on manager over-optimism. We follow their method and examine the variations in firm value across different groups of firms, and see whether the increasing firm value is due to overoptimistic CEOs alone or it needs that other c-suite managers are also optimistic. Firstly, we calculate the monthly industry PE ratio as the logarithmic transformation of the ratio of the industry's total market capitalization to the industry's total earnings. Then, we subtract the 60-month moving average of the PE ratio. Finally, we average the difference over the fiscal year to form the exogenous proxy of firm growth opportunities. The firm value is proxied by Tobin's Q, and the independent variables are the same as those in Hirshleifer, Low, and Teoh (2012). Using our sample, we can replicate their results by looking at the regression results in column (1) and column (3) in Table 3.11, where the industry PE ratio positively and significantly associates with Tobin's Q, and the coefficient of the interaction between industry PE ratio and the CEO over-optimistic measure in column (3) is also positive and statistically significant. In column (2), we interact the measure of growth opportunities-industry PE ratio with group indicators. Interestingly, we find that only group 1 firms are able to transform the growth opportunities into firm value, where both CEOs and non-CEO manager team are overoptimistic. The results in column (4) also suggest that non-CEO managers play a crucial in transforming growth opportunities into firm value. Therefore, the increasing firm value cannot only attribute to the optimistic CEOs, it is the result of cooperation of whole manager team. This result is consistent with the Hilary *et al.* (2016), they show that over-optimism is a related but different bias from overconfident, it generates higher managerial effort, and importantly, this additional effort improves firm profitability and market value.

3.7 Conclusion

This paper aims to explore the role played by the non-CEO top managers in firm investment, financing and payout policies, as well as their impacts on firm value. Drawing on the findings of prior research that overoptimistic CEOs are more likely to make aggressive firm decisions, we examine whether overoptimistic (non-optimistic) non-CEO managers would reinforce (mitigate) the effect of overoptimistic CEOs on firm policies. Over-optimism is defined as individual holding an excessive belief that future events will be positive, and is figured out by the prior literature as a strong and robust psychological trait across many samples of subjects, especially among top executives.

Using a large sample of top five managers from Execucomp database, we find that only the firms with both overoptimistic CEOs and overoptimistic non-CEO manager teams would make more investment, use more debt financing when accessing to the public security market, and are less likely to pay dividends. However, the investment and financing decisions in firms with only overoptimistic CEOs are close to those of firms with neither overoptimistic CEOs nor overoptimistic non-CEO manager teams. These results indicate that prior findings of the aggressive decisions made by overoptimistic CEOs are virtually driven by the firms whose overall senior managers are overoptimistic, but cannot be attributed to overoptimistic CEOs alone. We also find that only the firms with both overoptimistic CEOs and overoptimistic non-CEO manager teams are able to transform the growth opportunities into firm value, overoptimistic CEOs alone cannot achieve such success. This result supports the bright side of over-optimism, because it can boost manager creativity and generate higher managerial effort, which consequently improve firm value.

Our research is the first to explicitly show that non-CEO managers also play a significant role in firm investment, financing, and payout decisions. But the implications of this study are limited by the validity of our measures of group over-optimism. In addition, although we try to mitigate the concerns of endogeneity, this issue remains exist in our study. These limitations can be avenues for future research.

References

Acharya, V.V., Almeida, H., and Campello, M., 2007. Is Cash Negative Debt? A Hedging Perspective on Corporate Financial Policies. *Journal of Financial Intermediation* 16: 515-554.

Ahern, K.R., Duchin, R., and Shumway, T., 2014. Peer Effects in Risk Aversion and Trust. *Review of Financial Studies* 27: 3213-3240.

Ahmed, A.S. and Duellman, S., 2013. Managerial Overconfidence and Accounting Conservatism. *Journal of Accounting Research* 51: 1-30.

Almeida, H., Campello, M., and Weisbach, M.S., 2004. The Cash Flow Sensitity of Cash. *Journal of Finance* 59: 1777-1804.

Altınkılıç, O. and Hansen, R.S., 2009. On the Information Role of Stock Recommendation Revisions. *Journal of Accounting and Economics* 48: 17-36.

Angrist, J.D., 2014. The Perils of Peer Effects. *Labour Economics* 30: 98-108.

Asparouhova, E., Bessembinder, H., and Kalcheva, I., 2013. Noisy Prices and Inference Regarding Returns. *Journal of Finance* 68: 665-714.

Bakke, T.E. and Whited, T.M., 2010. Which Firms Follow the Market? An Analysis of Corporate Investment Decisions. *Review of Financial Studies* 23: 1941-1980.

Banerjee, S., Humphery-Jenner, M., and Nanda, V., 2015. Restraining Overconfident Ceos through Improved Governance: Evidence from the Sarbanes-Oxley Act. *Review of Financial Studies* 28: 2812-2858.

Barber, B.M., Lehavy, R., and Trueman, B., 2007. Comparing the Stock Recommendation Performance of Investment Banks and Independent Research Firms. *Journal of Financial Economics* 85: 490-517.

Bates, T.W., Kahle, K.M., and Stulz, R.M., 2009. Why Do Us Firms Hold So Much More Cash Than They Used To? *Journal of Finance* 64: 1985-2021.

Bebchuk, L.A., Cremers, K.J.M., and Peyer, U.C., 2011. The CEO Pay Slice. *Journal of Financial Economics* 102: 199-221.

Ben-David, I., Graham, J.R., and Harvey, C.R., 2013. Managerial Miscalibration. *Quarterly Journal of Economics* 128: 1547-1584.

Bénabou, R. and Tirole, J., 2002. Self-Confidence and Personal Motivation. *Quarterly Journal of Economics* 117: 871-915.

Bernhardt, D., Wan, C., and Xiao, Z., 2016. The Reluctant Analyst. *Journal of Accounting Research* 54: 987-1040.

Bertrand, M. and Schoar, A., 2003. Managing with Style: The Effect of Managers on Firm Policies. *Quarterly Journal of Economics* 118: 1169-1208.

Bird, A., Edwards, A., and Ruchti, T.G., 2016. Taxes and Peer Effects. Working paper, Carnegie Mellon University.

Bizjak, J., Lemmon, M., and Naveen, L., 2008. Does the Use of Peer Groups Contribute to Higher Pay and Less Efficient Compensation? *Journal of Financial Economics* 90: 152-168.

Bond, P., Edmans, A., and Goldstein, I., 2012. The Real Effects of Financial Markets. *Annual Review of Financial Economics* 4: 339-360.

Boni, L. and Womack, K.L., 2006. Analysts, Industries, and Price Momentum. *Journal of Financial and Quantitative analysis* 41: 85-109.

Boulland, R., Ornthanalai, C., and Womack, K.L., 2016. Speed and Expertise in Stock Picking: Older, Slower, and Wiser? Working paper, University of Toronto.

Bustamante, M.C. and Frésard, L., 2017. Does Firm Investment Respond to Peers' Investment? Woking paper, University of Maryland.

Campbell, T.C., Gallmeyer, M., Johnson, S.A., Rutherford, J., and Stanley, B.W., 2011. CEO Optimism and Forced Turnover. *Journal of Financial Economics* 101: 695-712.

Cao, J., Liang, H., and Zhan, X., 2015. Peer Effects of Corporate Social Responsibility. Working paper, Chinese University of Hong Kong.

Carhart, M.M., 1997. On Persistence in Mutual Fund Performance. *Journal of Finance* 52: 57-82.

Carpenter, J.N. and Remmers, B., 2001. Executive Stock Option Exercises and inside Information. *Journal of Business* 74: 513-534.

Chava, S. and Purnanandam, A., 2010. Ceos Versus Cfos: Incentives and Corporate Policies. *Journal of Financial Economics* 97: 263-278.

Chen, Q., Goldstein, I., and Jiang, W., 2007. Price Informativeness and Investment Sensitivity to Stock Price. *Review of Financial Studies* 20: 619-650.

Cohen, L., Frazzini, A., and Malloy, C., 2010. Sell-Side School Ties. *Journal of Finance* 65: 1409-1437.

Compte, O. and Postlewaite, A., 2004. Confidence-Enhanced Performance. *American Economic Review* 94: 1536-1557.

Conrad, J., Cornell, B., Landsman, W.R., and Rountree, B.R., 2006. How Do Analyst Recommendations Respond to Major News? *Journal of Financial and Quantitative Analysis* 41: 25-49.

Cooper, R.A., Day, T.E., and Lewis, C.M., 2001. Following the Leader: A Study of Individual Analysts' Earnings Forecasts. *Journal of Financial Economics* 61: 383-416.

Core, J. and Guay, W., 2002. Estimating the Value of Employee Stock Option Portfolios and Their Sensitivities to Price and Volatility. *Journal of Accounting Research* 40: 613-630.

Coval, J.D. and Shumway, T., 2005. Do Behavioral Biases Affect Prices? *Journal of Finance* 60: 1-34.

Daniel, K., Grinblatt, M., Titman, S., and Wermers, R., 1997. Measuring Mutual Fund Performance with Characteristic-Based Benchmarks. *Journal of Finance* 52: 1035-1058.

Dasgupta, S., Noe, T.H., and Wang, Z., 2011. Where Did All the Dollars Go? The Effect of Cash Flows on Capital and Asset Structure. *Journal of Financial and Quantitative Analysis* 46: 1259-1294.

Deshmukh, S., Goel, A.M., and Howe, K.M., 2013. CEO Overconfidence and Dividend Policy. *Journal of Financial Intermediation* 22: 440-463.

Driskill, M., Kirk, M., and Tucker, J.W., 2016. Earnings Announcement Clustering and Analyst Forecast Behavior. Working paper, University of Florida.

Dyreng, S.D. and Lindsey, B.P., 2009. Using Financial Accounting Data to Examine the Effect of Foreign Operations Located in Tax Havens and Other Countries on Us Multinational Firms' Tax Rates. *Journal of Accounting Research* 47: 1283-1316.

Edmans, A., Goldstein, I., and Jiang, W., 2015. Feedback Effects, Asymmetric Trading, and the Limits to Arbitrage. *American Economic Review* 105: 3766-3797.

Edmans, A., Jayaraman, S., and Schneemeier, J., 2016. The Source of Information in Prices and Investment-Price Sensitivity. Working paper, London Business School.

Eisenbach, T.M., Lucca, D.O., and Townsend, R.M., 2016. The Economics of Bank Supervision. Working paper, Federal Reserve Bank of New York.

Falato, A., Kadyrzhanova, D., and Lel, U., 2014. Distracted Directors: Does Board Busyness Hurt Shareholder Value? *Journal of Financial Economics* 113: 404-426.

Fama, E.F. and French, K.R., 1993. Common Risk Factors in the Returns on Stocks and Bonds. *Journal of Financial Economics* 33: 3-56.

Fama, E.F. and French, K.R., 2006. The Value Premium and the CAPM. *Journal of Finance* 61: 2163-2185.

Fama, E.F. and French, K.R., 2015. A Five-Factor Asset Pricing Model. *Journal of Financial Economics* 116: 1-22.

Fama, E.F. and MacBeth, J.D., 1973. Risk, Return, and Equilibrium: Empirical Tests. *Journal of Political Economy* 81: 607-636.

Ferris, S.P., Jayaraman, N., and Sabherwal, S., 2013. CEO Overconfidence and International Merger and Acquisition Activity. *Journal of Financial and Quantitative Analysis* 48: 137-164.

Fich, E.M. and Shivdasani, A., 2006. Are Busy Boards Effective Monitors? *Journal of Finance* 61: 689-724.

Foley, C.F., Hartzell, J.C., Titman, S., and Twite, G., 2007. Why Do Firms Hold So Much Cash? A Tax-Based Explanation. *Journal of Financial Economics* 86: 579-607.

Foucault, T. and Fresard, L., 2014. Learning from Peers' Stock Prices and Corporate Investment. *Journal of Financial Economics* 111: 554-577.

Foucault, T. and Frésard, L., 2012. Cross-Listing, Investment Sensitivity to Stock Price, and the Learning Hypothesis. *Review of Financial Studies* 25: 3305-3350.

Fracassi, C., 2016. Corporate Finance Policies and Social Networks. *Management Science* 63: 2420-2438.

Frankel, R., Kothari, S., and Weber, J., 2006. Determinants of the Informativeness of Analyst Research. *Journal of Accounting and Economics* 41: 29-54.

French, K.R., Schwert, G.W., and Stambaugh, R.F., 1987. Expected Stock Returns and Volatility. *Journal of Financial Economics* 19: 3-29.

Fresard, L., 2010. Financial Strength and Product Market Behavior: The Real Effects of Corporate Cash Holdings. *Journal of Finance* 65: 1097-1122.

Fresard, L., 2012. Cash Savings and Stock Price Informativeness. *Review of Finance* 16: 985-1012.

Fu, F., 2009. Idiosyncratic Risk and the Cross-Section of Expected Stock Returns. *Journal of Financial Economics* 91: 24-37.

Galasso, A. and Simcoe, T.S., 2011. CEO Overconfidence and Innovation. *Management Science* 57: 1469-1484.

Garlappi, L., Giammarino, R., and Lazrak, A., 2017. Ambiguity and the Corporation: Group Disagreement and Underinvestment. *Journal of Financial Economics* 125: 417-433.

Gaspar, J.M. and Massa, M., 2006. Idiosyncratic Volatility and Product Market Competition. *Journal of Business* 79: 3125-3152.

Gervais, S., Heaton, J.B., and Odean, T., 2011. Overconfidence, Compensation Contracts, and Capital Budgeting. *Journal of Finance* 66: 1735-1777.

Goel, A.M. and Thakor, A.V., 2008. Overconfidence, CEO Selection, and Corporate Governance. *Journal of Finance* 63: 2737-2784.

Graham, B.S., 2008. Identifying Social Interactions through Conditional Variance Restrictions. *Econometrica* 76: 643-660.

Graham, J.R., Harvey, C.R., and Puri, M., 2013. Managerial Attitudes and Corporate Actions. *Journal of Financial Economics* 109: 103-121.

Hackbarth, D., 2008. Managerial Traits and Capital Structure Decisions. *Journal of Financial and Quantitative Analysis* 43: 843-881.

Hackbarth, D., 2009. Managerial Traits and Capital Structure Decisions. *Journal of Financial and Quantitative Analysis* 43: 843.

Hadlock, C.J. and Pierce, J.R., 2010. New Evidence on Measuring Financial Constraints: Moving Beyond the KZ Index. *Review of Financial Studies* 23: 1909-1940.

Hall, B.J. and Murphy, K.J., 2002. Stock Options for Undiversified Executives. *Journal of Accounting and Economics* 33: 3-42.

Harford, J., 1999. Corporate Cash Reserves and Acquisitions. *Journal of Finance* 54: 1969-1997.

Harford, J., Jiang, F., Wang, R., and Xie, F., 2016. Career Concerns and Strategic Effort Allocation by Analysts. Working paper, University of Washington.

Harford, J., Mansi, S.A., and Maxwell, W.F., 2008. Corporate Governance and Firm Cash Holdings in the Us. *Journal of Financial Economics* 87: 535-555.

Harford, J., Wang, C., and Zhang, K., 2015. Foreign Cash: Taxes, Internal Capital Markets and Agency Problems. Working paper, University of Washington.

Haushalter, D., Klasa, S., and Maxwell, W., 2007. The Influence of Product Market Dynamics on a Firm's Cash Holdings and Hedging Behavior. *Journal of Financial Economics* 84: 797-825.

Hayek, F.A., 1945. The Use of Knowledge in Society. *American Economic Review* 35: 519-530.

Hilary, G., Hsu, C., Segal, B., and Wang, R., 2016. The Bright Side of Managerial Over-optimism. *Journal of Accounting and Economics* 62: 46-64.

Hirshleifer, D., Lim, S.S., and Teoh, S.H., 2009. Driven to Distraction: Extraneous Events and Underreaction to Earnings News. *Journal of Finance* 64: 2289-2325.

Hirshleifer, D., Low, A., and Teoh, S.H., 2012. Are Overconfident CEOs Better Innovators? *Journal of Finance* 67: 1457-1498.

Hobbs, J., Kovacs, T., and Sharma, V., 2012. The Investment Value of the Frequency of Analyst Recommendation Changes for the Ordinary Investor. *Journal of Empirical Finance* 19: 94-108.

Hoberg, G. and Phillips, G., 2010. Product Market Synergies and Competition in Mergers and Acquisitions: A Text-Based Analysis. *Review of Financial Studies* 23: 3773-3811.

Hoberg, G., Phillips, G., and Prabhala, N., 2014. Product Market Threats, Payouts, and Financial Flexibility. *Journal of Finance* 69: 293-324.

Hong, H. and Kubik, J.D., 2003. Analyzing the Analysts: Career Concerns and Biased Earnings Forecasts. *Journal of Finance* 58: 313-351.

Hong, H., Stein, J.C., and Yu, J., 2007. Simple Forecasts and Paradigm Shifts. *Journal of Finance* 62: 1207-1242.

Hou, K., 2007. Industry Information Diffusion and the Lead-lag Effect in Stock Returns. *Review of Financial Studies* 20: 1113-1138.

Howell, S., 2017. Learning from Feedback: Evidence from New Ventures. Working paper, New York University.

Hribar, P. and Yang, H., 2010. Does CEO Overconfidence Affect Management Forecasting and Subsequent Earnings Management. Working paper, University of Iowa.

Huang, R., Tan, K.J.K., and Faff, R.W., 2016. CEO Overconfidence and Corporate Debt Maturity. *Journal of Corporate Finance* 36: 93-110.

Irvine, P.J. and Pontiff, J., 2009. Idiosyncratic Return Volatility, Cash Flows, and Product Market Competition. *Review of Financial Studies* 22: 1149-1177.

Jegadeesh, N. and Kim, W., 2010. Do Analysts Herd? An Analysis of Recommendations and Market Reactions. *Review of Financial Studies* 23: 901-937.

Jiang, J., Petroni, K.R., and Yanyan Wang, I., 2010. CFOs and CEOs: Who Have the Most Influence on Earnings Management? *Journal of Financial Economics* 96: 513-526.

John, K. and Kadyrzhanova, D., 2008. Peer Effects in Corporate Governance. Working paper, New York University.

Kacperczyk, M., Van Nieuwerburgh, S., and Veldkamp, L., 2016. A Rational Theory of Mutual Funds' Attention Allocation. *Econometrica* 84: 571-626.

Kaustia, M. and Rantala, V., 2015. Social Learning and Corporate Peer Effects. *Journal of Financial Economics* 117: 653-669.

Kempf, E., Manconi, A., and Spalt, O., 2016. Distracted Shareholders and Corporate Actions. *Review of Financial Studies,* forthcoming.

Kim, J.-B., Li, Y., and Zhang, L., 2011. CFOs Versus CEOs: Equity Incentives and Crashes. *Journal of Financial Economics* 101: 713-730.

Leary, M.T. and Roberts, M.R., 2014. Do Peer Firms Affect Corporate Financial Policy? *Journal of Finance* 69: 139-178.

Lieberman, M.B. and Asaba, S., 2006. Why Do Firms Imitate Each Other. *Academy of Managment Review* 31: 366-385.

Ljungqvist, A., Malloy, C., and Marston, F., 2009. Rewriting History. *Journal of Finance* 64: 1935-1960.

Loh, R.K. and Mian, G.M., 2006. Do Accurate Earnings Forecasts Facilitate Superior Investment Recommendations? *Journal of Financial Economics* 80: 455-483.

Loh, R.K. and Stulz, R.M., 2011. When Are Analyst Recommendation Changes Influential? *Review of Financial Studies* 24: 593-627.

Loh, R.K. and Stulz, R.M., 2017. Is Sell-Side Research More Valuable in Bad Times? *Journal of Finance,* forthcoming.

Lu, Y., Ray, S., and Teo, M., 2016. Limited Attention, Marital Events and Hedge Funds. *Journal of Financial Economics* 122: 607-624.

Luo, Y., 2005. Do Insiders Learn from Outsiders? Evidence from Mergers and Acquisitions. *Journal of Finance* 60: 1951-1982.

Lyandres, E. and Palazzo, B., 2016. Cash Holdings, Competition, and Innovation. *Journal of Financial and Quantitative Analysis* 51: 1823-1861.

Malmendier, U. and Tate, G., 2005. CEO Overconfidence and Corporate Investment. *Journal of Finance* 60: 2661-2700.

Malmendier, U. and Tate, G., 2008. Who Makes Acquisitions? CEO Overconfidence and the Market's Reaction. *Journal of Financial Economics* 89: 20-43.

Malmendier, U., Tate, G., and Yan, J., 2011. Overconfidence and Early-life Experiences: The Effect of Managerial Traits on Corporate Financial Policies. *Journal of Finance* 66: 1687-1733.

Manski, C.F., 1993. Identification of Endogenous Social Effects: The Reflection Problem. *Review of Economics Studies* 60: 531-542.

Massa, M., Rehman, Z., and Vermaelen, T., 2007. Mimicking Repurchases. *Journal of Financial Economics* 84: 624-666.

Mikhail, M.B., Walther, B.R., and Willis, R.H., 1997. Do Security Analysts Improve Their Performance with Experience? *Journal of Accounting Research* 35: 131-157.

Opler, T., Pinkowitz, L., Stulz, R., and Williamson, R., 1999. The Determinants and Implications of Corporate Cash Holdings. *Journal of Financial Economics* 52: 3-46.

Palazzo, B., 2012. Cash Holdings, Risk, and Expected Returns. *Journal of Financial Economics* 104: 162-185.

Panousi, V. and Papanikolaou, D., 2012. Investment, Idiosyncratic Risk, and Ownership. *Journal of Finance* 67.

Philippon, T., 2003. An Explanation for the Joint Evolution of Firm and Aggregate Volatility. Working paper, New York University.

Phua, K., Tham, T.M., and Wei, C., 2017. Are Overconfident CEOs Better Leaders? Evidence from Stakeholder Commitments. *Journal of Financial Economics*.

Pinkowitz, L., Stulz, R.M., and Williamson, R., 2012. Multinationals and the High Cash Holdings Puzzle. Working paper, National Bureau of Economic Research.

Popadak, J., 2017. Dividend Payments as a Response to Peer Influence. *Journal of Financial Economics*, forthcoming.

Riddick, L.A. and Whited, T.M., 2009. The Corporate Propensity to Save. *Journal of Finance* 64: 1729-1766.

Schrand, C.M. and Zechman, S.L., 2012. Executive Overconfidence and the Slippery Slope to Financial Misreporting. *Journal of Accounting and Economics* 53: 311-329.

Seo, H., 2016. Peer Effects in Corporate Disclosure Decisions. Working paper, National Univeristy of Singapore.

Seru, A., Shumway, T., and Stoffman, N., 2010. Learning by Trading. *Review of Financial Studies* 23: 705-739.

Shue, K., 2013. Executive Networks and Firm Policies: Evidence from the Random Assignment of MBA Peers. *Review of Financial Studies* 26: 1401-1442.

Stock, J.H. and Yogo, M., 2002. Testing for Weak Instruments in Linear IV Regression. Working papar, National Bureau of Economic Research.

Sufi, A., 2007. Bank Lines of Credit in Corporate Finance: An Empirical Analysis. *Review of Financial Studies* 22: 1057-1088.

Sulaeman, J. and Wei, K.D., 2014. Sell-Side Analysts' Responses to Mutual Fund Trading Pressure. Working paper, National University of Singapore.

Sun, S.Y., 2016. Do Customers Learn from Stock Prices? Working paper, University of Texas at Austin.

Van den Steen, E., 2004. Rational Overoptimism (and Other Biases). *American Economic Review* 94: 1141-1151.

Veldkamp, L.L., 2006. Information Markets and the Comovement of Asset Prices. *Review of Economic Studies* 73: 823-845.

Welch, I., 2000. Herding among Security Analysts. *Journal of Financial Economics* 58: 369-396.

Whited, T.M. and Wu, G., 2006. Financial Constraints Risk. *Review of Financial Studies* 19: 531-559.

Williams, R. and Xiao, S.C., 2014. The Effect of Stock Prices on Real Investment in the Vertical Supply Chain. Working paper, University of Texas at Dallas.

Zuo, L., 2016. The Informational Feedback Effect of Stock Prices on Management Forecasts. *Journal of Accounting and Economics* 61: 391-413.

APPENDICES

Appendix A Tables for Chapter 1

Figure 1.1: Trend of cash ratio from 1980 to 2014

The sample includes all Compustat firm-year observations from 1980 to 2014 with positive total assets and sales for firms incorporated in the United States and publicly traded on the NYSE, AMEX and NASDAQ. Financial firms (SIC code 6000-6999), utilities (SIC code 4900-4999) and government entities (SIC code greater than or equal to 9000) are excluded from the sample. Cash ratio is cash and short-term investment scaled by total assets. The aggregate cash ratio is the sum of cash divided by the sum of assets for all sample firms.

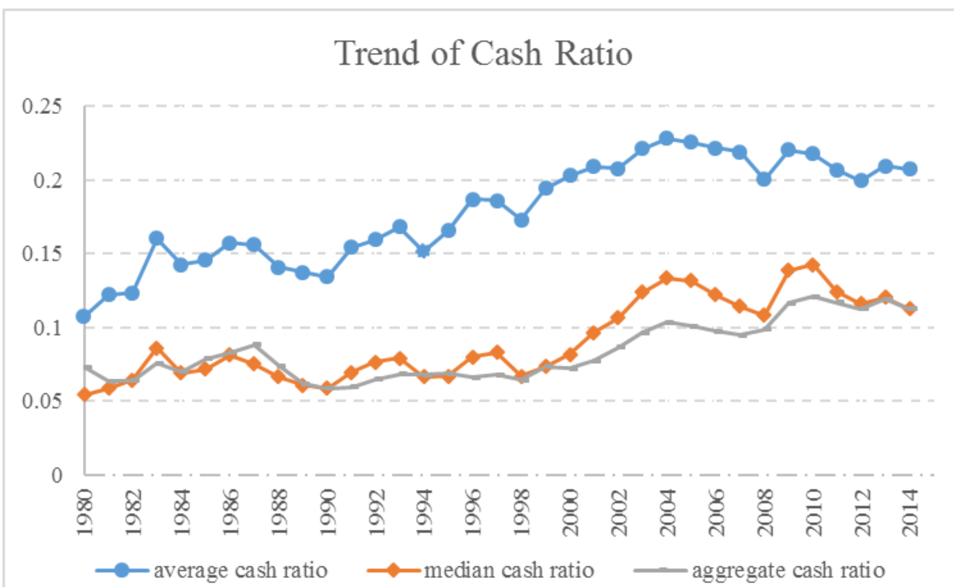

Table 1.1: Descriptive statistics

The sample includes all Compustat firm-year observations from 1980 to 2014 with positive total assets and sales for firms incorporated in the United States and publicly traded on the NYSE, AMEX and NASDAQ. $\Delta Cash_t$ is the change in cash ratio. $Real\ Size_t$ is the natural logarithm of total assets and adjusted by 2014 CPI. $Cash\ Flow_t$ is the ratio of income before extraordinary over total assets. MB_t market value divided by the book value of assets. Market value of assets is book value of asset mines book value of equity and plus market value of equity. Book value of equity is equal to stockholder equity plus balance sheet deferred taxes and investment tax credit, minus the book value of preferred stock. Market equity is the fiscal year-end equity price multiplied by the number of common shares outstanding. $Net\ Equity_t$ is the ratio of net equity issuance over total assets. Net equity issuance is defined as the sale of common and preferred stocks net of cash dividend and purchase of common and preferred stocks. $Net\ Debt_t$ is the ratio of net debt issuance over total assets. Net debt issuance is defined as long-term debt issuance net of long-term debt reduction. $Net\ Invest_t$ is the ratio of net investment over total assets. Net investment is the sum of capital expenditures plus acquisitions net of sales of property. Peer firms' average characteristics denote variables constructed as the average of all firms within an industry-year combination, excluding the i^{th} observation. Financial firms (SIC code 6000-6999), utilities (SIC code 4900-4999) and government entities (SIC code greater than or equal to 9000) are excluded from the sample. Industries are defined by three-digit SIC code.

	Mean	Median	SD	P1	P99
Firm-specific characteristics					
$\Delta Cash_t$	-0.004	-0.001	0.093	-0.379	0.316
$Real\ Size_t$	5.508	5.398	2.077	1.170	10.611
$Cash\ Flow_t$	-0.034	0.0320	0.237	-1.426	0.244
MB_t	1.889	1.393	1.513	0.578	10.358
$Net\ Equity_t$	-0.023	-0.003	0.046	-0.279	0
$Net\ Debt_t$	0.009	0	0.091	-0.289	0.403
$Net\ Invest_t$	0.076	0.050	0.084	-0.059	0.445
$IdioVol_{t-1}$	0.152	0.129	0.090	0.037	0.492
Peer firms' average characteristics					
$P\_\Delta Cash_t$	-0.008	-0.007	0.024	-0.08	0.054
$P\_Real\ Size_t$	5.420	5.292	1.0675	3.343	8.370
$P\_Cash\ Flow_t$	-0.059	-0.027	0.124	-0.649	0.112
P_MB_t	2	1.801	0.786	0.886	5.047
$P\_Net\ Equity_t$	-0.024	-0.019	0.017	-0.111	0
$P\_Net\ Debt_t$	0.010	0.009	0.029	-0.082	0.127
$P\_Net\ Invest_t$	0.077	0.068	0.038	0.007	0.244
$P\_RELIdioVol_{t-1}$ (IV)	0.020	0.017	0.016	-0.013	0.078
Industry Characteristics					
#Firms per industry-year	23.89	14	36.202		
#Industries	202				
#Obs.	94085				
#Firms	9419				

Table 1.2: Instrument variable validity

The table reports partial correlations between the instrument and firm-specific fundamentals. The sample includes all Compustat firm-year observations from 1980 to 2014 with positive total assets and sales for firms incorporated in the United States and publicly traded on the NYSE, AMEX and NASDAQ. Financial firms (SIC code 6000-6999), utilities (SIC code 4900-4999) and government entities (SIC code greater than or equal to 9000) are excluded from the sample. The dependent variable is the average of peer firm relative idiosyncratic stock volatility in the last year. Peer firm average factors are peer firm averages of the same variables listed under firm-specific factors in the table: firm size, cash flow, market-to-book ratio, net equity issuance, net debt issuance, and net investment. Peer firm averages are constructed as the average of all firms within an industry-year combination, excluding the i^{th} observation. Industries are defined by three-digit SIC code. All the variables are winsorized at 1% and 99% level. Column (1) includes firm-specific and peer firms' average characteristics, and column (2) further controls for industry characteristics: industry concentration and industry cash flow volatility. All test statistics are computed using standard errors that are robust to within-firm correlation and heteroscedasticity. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

	$P\_RELIdioVol_{t-1}$	
	(1)	(2)
Firm-specific characteristics		
$Real\ Size_t$	-0.0001	-0.0001
	(-0.82)	(-0.86)
MB_t	-0.000035	-0.0000327
	(-0.72)	(-0.67)
$Cash\ Flow_t$	0.000637**	0.000633**
	(2.23)	(2.22)
$Net\ Equity_t$	-0.00159	-0.00151
	(-1.26)	(-1.20)
$Net\ Debt_t$	0.00000835	0.0000186
	(0.01)	(0.03)
$Net\ Invest_t$	0.000328	0.000321
	(0.44)	(0.43)
Peer firms' average characteristics	Yes	Yes
Firm *i*'s *IdioVol*	Yes	Yes
Industry characteristics	No	Yes
Year F.E.	Yes	Yes
Firm F.E.	Yes	Yes
Adj. R^2	0.229	0.230
#Obs.	94085	94085

Table 1.3: 2SLS estimation of linear-in-means model

This table presents 2SLS estimated coefficients scaled by the corresponding variable's standard deviation, where the instrument is the lagged average of peer firms relative idiosyncratic stock volatility, relative idiosyncratic stock volatility is the difference between firm's idiosyncratic stock volatility and industry median idiosyncratic stock volatility. The endogenous variable is the peer firms average cash savings. The sample includes all Compustat firm-year observations from 1980 to 2014 with positive total assets and sales for firms incorporated in the United States and publicly traded on the NYSE, AMEX and NASDAQ. Financial firms (SIC code 6000-6999), utilities (SIC code 4900-4999) and government entities (SIC code greater than or equal to 9000) are excluded from the sample. Peer firms' average characteristics denote variables constructed as the average of all firms within an industry-year combination, excluding the i^{th} observation. Industries are defined by three-digit SIC code. All the variables are winsorized at 1% and 99% level. All test statistics are computed using standard errors that are robust to within-firm correlation and heteroscedasticity. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively. *K-P rk Wald F statistics* significance implying less than 15% or 10% size distortion is denoted by ** and ***, respectively.

Dependent variable: $\Delta Cash_t$	(1)	(2)	(3)	(4)	(5)	(6)
$P\_\Delta Cash_t$	0.026**	0.0264*	0.0264*	0.027**	0.032***	0.027***
	(2.23)	(2.24)	(2.23)	(2.24)	(2.66)	(2.61)
$\Delta Cash_{t-1}$					-0.024***	-0.024***
					(-50.76)	(-50.90)
$Real\ Size_t$	0.007***	0.007***	0.007***	0.007***	0.006***	0.006***
	(4.86)	(4.86)	(4.90)	(4.90)	(4.37)	(4.36)
$Cash\ Flow_t$	0.034***	0.034***	0.034***	0.034***	0.039***	0.039***
	(12.91)	(12.91)	(12.91)	(12.91)	(14.50)	(14.54)
MB_t	0.012***	0.012***	0.012***	0.012***	0.012***	0.012***
	(11.01)	(11.00)	(10.99)	(10.99)	(10.98)	(11.10)
$Net\ Equity_t$	0.022***	0.022***	0.022***	0.022***	0.019***	0.019***
	(23.61)	(23.60)	(23.60)	(23.59)	(20.93)	(21.06)
$Net\ Debt_t$	0.017***	0.017***	0.017***	0.017***	0.015***	0.015***
	(21.14)	(21.14)	(21.13)	(21.13)	(18.94)	(19.00)
$Net\ Invest_t$	-0.053***	-0.053***	-0.053***	-0.053***	-0.050***	-0.050***
	(-56.31)	(-56.31)	(-56.31)	(-56.31)	(-55.28)	(-55.38)
$IdioVol_{t-1}$	0.006***	0.006***	0.006***	0.006***	0.008***	0.008***
	(8.57)	(8.56)	(8.61)	(8.60)	(11.32)	(11.11)
$P\_\Delta Cash_{t-1}$						0.005***
						(3.07)
$P\_Real\ Size_t$	-0.003	-0.003	-0.003*	-0.003*	-0.004*	-0.003*
	(-1.47)	(-1.48)	(-1.70)	(-1.70)	(-1.84)	(-1.72)
$P\_Cash\ Flow_t$	-0.013*	-0.013*	-0.013*	-0.014*	-0.016**	-0.015**
	(-1.75)	(-1.77)	(-1.84)	(-1.86)	(-2.09)	(-2.08)
P_MB_t	-0.001	-0.001	-0.001	-0.001	-0.002	-0.001
	(-0.64)	(-0.65)	(-0.54)	(-0.55)	(-1.12)	(-0.87)
$P\_Net\ Equity_t$	-0.002	-0.002	-0.002	-0.002	-0.003*	-0.002
	(-1.35)	(-1.36)	(-1.34)	(-1.36)	(-1.94)	(-1.60)
$P\_Net\ Debt_t$	-0.001**	-0.001**	-0.001**	-0.001**	-0.002***	-0.002**
	(-2.02)	(-2.03)	(-1.98)	(-1.99)	(-2.64)	(-2.10)
$P\_Net\ Invest_t$	0.013***	0.013***	0.013***	0.013***	0.015***	0.013***
	(2.82)	(2.83)	(2.81)	(2.82)	(3.22)	(3.27)
HHI_{t-1}		0.000		0.000		
		(0.86)		(0.79)		
$Ind\ cash\ flow\ Vol_{t-1}$			-0.002*	-0.002*		
			(-1.86)	(-1.83)		
1st-stage Instrument	0.039***	0.039***	0.039***	0.039***	0.039***	0.045***
$P\_RELIdioVol_{t-1}$	(9.36)	(9.36)	(9.36)	(9.36)	(9.36)	(10.64)
K-P rk Wald F statistics	87.659***	87.651***	87.58***	87.566***	87.619***	113.302***
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
#Obs.	94085	94085	94085	94085	94085	94085

Table 1.4: Robustness tests

This table presents 2SLS estimated coefficients scaled by the corresponding variable's standard deviation, where the instrument is the lagged average of peer firm relative idiosyncratic risk, and the endogenous variable is the peer firm average cash savings. Column (1) employ TNIC peer groups, column (2) restricts the sample into US domestic firms, column (3) focuses on the period from 2004 to 2014 where no cash trend exists, and column (4) uses pseudo peers to implement placebo tests. Financial firms (SIC code 6000-6999), utilities (SIC code 4900-4999) and government entities (SIC code greater than or equal to 9000) are excluded from the sample. Peer firms' average characteristics denote variables constructed as the average of all firms within an industry-year combination, excluding the i^{th} observations. All the variables are winsorised at 1% and 99% level. All test statistics are computed using standard errors that are robust to within-firm correlation and heteroscedasticity. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively. *K-P rk Wald F statistics* significance implying less than 15% or 10% size distortion is denoted by ** and ***, respectively.

	TNIC peers (1)	Domestic firms (2)	2004 - 2014 (3)	Pseudo peers (4)
$P\_\Delta Cash_t$	0.032**	0.022*	0.021*	0.008
	(2.07)	(1.93)	(1.80)	(0.30)
$Real\ Size_t$	0.008**	0.0098***	0.012***	0.007***
	(2.38)	(4.45)	(2.88)	(5.14)
$Cash\ Flow_t$	0.022***	0.030***	0.021***	0.035***
	(5.46)	(12.30)	(3.70)	(12.72)
MB_t	0.011***	0.013***	0.013***	0.013***
	(5.47)	(7.60)	(6.62)	(12.19)
$Net\ Equity_t$	0.020***	0.020***	0.037***	0.022***
	(16.83)	(13.26)	(20.10)	(23.88)
$Net\ Debt_t$	0.024***	0.013***	0.029***	0.017***
	(18.14)	(11.07)	(18.05)	(20.94)
$Net\ Invest_t$	-0.062***	-0.052***	-0.062***	-0.053***
	(-43.47)	(-37.77)	(-43.51)	(-56.16)
$IdioVol_{t-1}$	0.006***	0.007***	0.004***	0.006***
	(3.76)	(7.37)	(2.76)	(9.41)
$P\_Real\ Size_t$	-0.002	-0.003	-0.003	-0.001
	(-0.87)	(-1.17)	(-1.39)	(-0.51)
$P\_Cash\ Flow_t$	-0.003	-0.003	-0.003	-0.002
	(-1.42)	(-1.10)	(-1.04)	(-0.37)
P_MB_t	-0.006	0.001	-0.004	-0.000
	(-1.52)	(0.30)	(-1.43)	(-0.37)
$P\_Net\ Equity_t$	-0.006*	-0.001	-0.005	-0.002
	(-1.94)	(-0.76)	(-1.63)	(-0.74)
$P\_Net\ Debt_t$	-0.005***	-0.001	-0.004**	-0.000
	(-2.58)	(-1.44)	(-2.12)	(-0.12)
$P\_Net\ Invest_t$	0.024***	0.012**	0.016**	0.002
	(3.05)	(2.43)	(2.37)	(0.28)
HHI_{t-1}	0.001	0.001	-0.000	0.001
	(1.35)	(0.65)	(-0.19)	(1.05)
$Ind\ Cash\ Flow\ Vol_{t-1}$	-0.001	-0.000	-0.006	-0.003**
	(-0.15)	(-0.16)	(-1.45)	(-2.25)
1st-stage Instrument	0.049***	0.055***	0.080***	0.013***
$P\_RELIdioVol_{t-1}$	(6.48)	(9.23)	(8.81)	(3.45)
K-P rk Wald F statistics	43.337***	85.235***	81.669***	11.913**
Year F.E.	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes
#Obs.	44878	47081	24613	94058

Table 1.5: Rivalry-based mechanism

This table reports 2SLS estimated coefficients for the peer firm average cash savings interacted with the indicator variables identifying industry concentration, the intensity of cash flow volatility, and the extent of product market threats. The dependent variable is the change in cash ratio. The coefficient estimates are scaled by the corresponding variable standard deviation. The endogenous variables are the peer firms average cash savings interacted with indicator variables, and the instrument variables are the one-period-lagged peer frim average relative idiosyncratic risk interacted with the same indicator variables. The indicator variable D_{low} is equal to one if firms are ranked into the bottom tercile and zero if the firms are at the top tercile based on the competition proxies listed in the top row. Just the reverse, D_{high} is equal to one if firms are ranked into the top tercile and zero for bottom tercile. The *K-P rk Wald F statistics* are reported at the bottom of the table. Industries are defined by 3-digit SIC code. All test statistics are computed using standard errors that are robust to within-firm correlation and heteroscedasticity. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively. *K-P rk Wald F statistics* significance implying less than 15% or 10% size distortion is denoted by ** and ***, respectively.

	Compustat HHI	TNIC HHI	EPCM	Cash flow volatility	Product market fluidity
	(1)	(2)	(3)	(4)	(5)
$P\_\Delta Cash_t * D_{low}$	0.019**	0.029***	0.033**	0.010	0.021**
	(2.51)	(2.63)	(2.29)	(1.37)	(1.97)
$P\_\Delta Cash_t * D_{high}$	0.016	0.012	0.029***	0.017*	0.030***
	(1.39)	(0.97)	(2.79)	(1.83)	(2.94)
D_{high}	0.001	-0.007	0.001	0.003	0.002
	(0.11)	(-1.37)	(0.11)	(0.82)	(0.48)
K-P rk Wald F statistics	11.165***	19.191***	20.414***	20.724***	16.979***
Firm-specific characteristics	Yes	Yes	Yes	Yes	Yes
Peers average characteristics	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
#Obs.	72394	31815	61785	56495	29423

Table 1.6: Information-based mechanism

This table reports 2SLS estimated coefficients for the peer firm average cash savings interacted with indicator variables identifying the lower and upper third of the within-industry-year distribution of market share, gross margin, market cap, book size, market-to-book ratio, and firm age in Panel A, as well as whether the firm has a bond rating, whether the firm paid a dividend, whether the firm has lines of credit, the Whited-Wu (2006) Index and HP Index (Hadlock and Pierce, 2010) in Panel B. The dependent variable is the change in cash ratio. The coefficient estimates are scaled by the corresponding variable standard deviation. The endogenous variables are the peer firms average cash savings interacted with indicator variables, and the instrument variables are the one-period-lagged peer frim average relative idiosyncratic risk interacted with the same indicator variables. The *K-P rk Wald F statistics* are reported at the bottom of the table. Industries are defined by 3-digit SIC code. All test statistics are computed using standard errors that are robust to within-firm correlation and heteroscedasticity. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively. *K-P rk Wald F statistics* significance implying less than 15% or 10% size distortion is denoted by ** and ***, respectively.

Panel A	Market share	Gross margin	Market Cap	Book size	Market-to-book	Firm age
	(1)	(2)	(3)	(4)	(5)	(6)
$P\_\Delta Cash_t * D_{low}$	0.029**	0.028**	0.198*	0.026**	-0.016	0.029**
	(2.11)	(1.99)	(1.66)	(2.07)	(-1.39)	(2.40)
$P\_\Delta Cash_t * D_{high}$	0.015**	0.028***	0.011	0.013*	0.034**	0.013
	(2.09)	(2.84)	(1.56)	(1.69)	(1.98)	(1.32)
D_{high}	0.002	0.002	-0.021***	-0.022***	0.022**	-0.005
	(0.24)	(0.28)	(-3.19)	(-3.19)	(2.53)	(-1.18)
K-P rk Wald F statistics	25.672***	25.316***	25.854***	36.915***	19.026***	23.573***
Firm-specific characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Peers average characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
#Obs.	61986	60762	61990	62018	61941	62378

Panel B	Bond rating (G2 = Y)	Dividend payment (G2 = Y)	Lines of credit (G2 = Y)	HP Index (G2 = Low)	WW Index (G2 = Low)
	(1)	(2)	(3)	(4)	(5)
$P\_\Delta Cash_t * D_1$	0.029***	0.018**	0.022**	0.032***	0.020**
	(2.70)	(2.17)	(2.49)	(2.76)	(2.44)
$P\_\Delta Cash_t * D_2$	0.005	0.014	0.011	0.016*	0.016*
	(0.95)	(1.44)	(1.43)	(1.91)	(1.74)
D_2	0.026***	-0.005	0.012***	0.023***	0.010*
	(5.84)	(-1.03)	(7.01)	(3.72)	(1.77)
K-P rk Wald F statistics	31.947***	11.045***	28.386***	24.657**	17.11***
Firm-specific characteristics	Yes	Yes	Yes	Yes	Yes
Peers average characteristics	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
#Obs.	94085	94085	61990	62209	60713

Table 1.7: Information-based mechanism – Bad times vs. Normal times

This table reports 2SLS estimated coefficients for the peer firm average cash savings interacted with indicator variables identifying the "bad times" in economics. Column (1) is based on the NBER-defined recessions; column (2) consider separately the Subprime mortgage crisis from December 2007 to June 2009; The column (3) set the indicator variable *Crisis* following the Loh and Sultz (2016) definition: September-November 1987 (1987 crisis), August-December 1998 (LTCM crisis), and July 2007-March 2009 (Credit crisis). The dependent variable is the change in cash ratio. The coefficient estimates are scaled by the corresponding variable standard deviation. The endogenous variables are the peer firm average cash savings interacted with indicator variables, and the instrument variables are the one-period-lagged peer frim average relative idiosyncratic risk interacted with the same indicator variables. The *K-P rk Wald F statistics* are reported at the bottom of the table. Industries are defined by 3-digit SIC code. All test statistics are computed using standard errors that are robust to within-firm correlation and heteroscedasticity. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively. *K-P rk Wald F statistics* significance implying less than 15% or 10% size distortion is denoted by ** and ***, respectively.

	NBER Recess	Dec 2007 – Jun 2009	Crisis
	(1)	(2)	(3)
$P\_\Delta Cash_{-ijt}$ * *Bad time dummy*	0.038**	0.023*	0.017***
	(2.06)	(1.71)	(3.52)
$P\_\Delta Cash_{-ijt}$ * *Other period dummy*	0.016**	0.019**	0.016*
	(2.18)	(2.27)	(1.78)
Bad time dummy	0.002	-0.029	0.018*
	(0.71)	(-1.00)	(1.67)
K-P rk Wald F statistics	7.247***	51.605***	46.095***
Firm-specific characteristics	Yes	Yes	Yes
Peers average characteristics	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes
#Obs.	94085	94085	94085

Table 1.8: Whether cash-rich firms are less sensitive to peer effect?

This table reports 2SLS estimated coefficients for the peer firms average cash savings interacted with indicator variables identifying cash-rich firms. The dependent variable is the change in cash ratio. The coefficient estimates are scaled by the corresponding variable standard deviation. All models are estimated by 2SLS method where the endogenous variables are the peer firm average cash savings interacted with indicator variables, and the instrument variables are the one-period-lagged peer frim average relative idiosyncratic risk interacted with the same indicator variables. The *K-P rk Wald F statistics* are reported at the bottom of the table. Industries are defined by 3-digit SIC code. The indicator variable in Column (1) identifying the lower and upper third of the within-industry-year distribution of last period cash holding levels. The indicator variables in Column (2) and Column (3) follows the Harford (1999), where cash-rich firm-years are years in which a firm's cash holdings are more than 1.5 standard deviations and 2 standard deviations above the predicted cash holdings, respectively. All test statistics are computed using standard errors that are robust to within-firm correlation and heteroscedasticity. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively. *K-P rk Wald F statistics* significance implying less than 15% or 10% size distortion is denoted by ** and ***, respectively.

	Lagged cash (1)	Cash rich 1.5X (2)	Cash rich 2X (3)
$P\_\Delta Cash_t * D_{low}$	0.016^{*}	0.026^{**}	0.024^{**}
	(1.70)	(2.10)	(2.44)
$P\_\Delta Cash_t * D_{rich}$	0.015	0.019^{**}	0.016^{**}
	(1.05)	(2.18)	(2.29)
D_{rich}	-0.099^{***}	-0.071^{***}	-0.062^{***}
	(-16.71)	(-10.66)	(-8.33)
K-P rk Wald F statistics	22.133^{***}	32.895^{***}	40.250^{***}
Firm-specific characteristics	Yes	Yes	Yes
Peers average characteristics	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes
#Obs.	61406	75272	75272

Table 1.9: Total economic impact of peer effect on industry cash savings

This table displays estimates from the excess variance-based tests pioneered by Graham (2008). The sample includes all Compustat firm-year observations from 1980 to 2014 with positive total assets and sales for firms incorporated in the United States and publicly traded on the NYSE, AMEX and NASDAQ. Financial firms (SIC code 6000-6999), utilities (SIC code 4900-4999) and government entities (SIC code greater than or equal to 9000) are excluded from the sample. When the estimate of the peer effect multiplier γ^2, is significantly different from 1, then peer effects of corporate cash saving decisions exist. Column (1) presents results for the changes of cash holdings, which conditions for firm-level characteristics such as cash flow to assets ratio, market-to-book ratio, firm real size, net equity issue, net debt issue, and net investment (Almeida, Campbell and Weisbach (2004), and Palazzo (2012)). Column (2) conditions for all firm-specific and peer firm average characteristics. The industry-specific factors are controlled in both models. Statistical significance at the 10%, 5% and 1% levels is denoted by *, **, and ***, respectively.

	(1)	(2)
Estimate of γ^2	1.832	1.809
Implied Peer Effect Multiplier	1.354	1.345
Chi-Squared Test (H0: There's no peer influence)	(7.76)***	(7.70)***
Implied effect of Multiplier (Small industry)	12.8%	12.5%
Implied effect of Multiplier (Large industry)	6.2%	6.0%
Firm-specific characteristics	Yes	Yes
Industry-specific characteristics	Yes	Yes
Peer firms' average characteristics	No	Yes
# Industry-year combinations	4445	4445

Appendix B Tables for Chapter 2

Table 2.1: Descriptive statistics

This table reports descriptive statistics of recommendation activity, analyst, and firm average characteristics. The recommendation sample is from I/B/E/S Detail U.S. File 1994-2014. Observations from anonymous analysts, recommendation changes where the lagged stock price is less than one dollar, observations with no outstanding prior rating from the same analyst, and team analysts are excluded. A recommendation change is defined as an analyst's current rating minus her prior outstanding rating (initiations, re-initiations, and reiterations are excluded). An uncontaminated recommendation change is one that does not occur on firm-news days following Loh and Stulz (2011). Firm-news contaminated days are defined as the three trading days centered around a Compustat earnings announcement date or a company earnings guidance date, and days with multiple analysts issuing recommendations for the firm. An analyst's total recommendation activity is computed by aggregating all rating activity including changes, initiations, and reiterations. We add to explicit reiterations in I/B/E/S by assuming that an analyst reiterates an outstanding rating when she issues a Q1 earnings forecast or a price target forecast. The rec-change probability and the number of total recommendation activity are based on the analyst-quarter sample which consists of only observations where the analyst makes at least one uncontaminated rec-change in the quarter. *Influential dummy* equals one if the analyst issued at least one influential recommendation change in the quarter *t*, and zero otherwise. Influential changes are those whose two-day CARs are in the same direction as the recommendation changes and is 1.96 times larger than expected based on the prior three-month idiosyncratic volatility of the stock, where CAR is the average day [0,1] cumulative abnormal return, whose benchmark return is the return from a characteristic-matched DGTW portfolio (Loh and Stulz (2011)). *Rec-change dummy* equals one if an analyst in quarter *t* with uncontaminated recommendation changes issues at least one recommendation change in quarter *t+1*, and zero otherwise. *ΔTotal activity (from t-1 to t+1)* is the difference of the number of total recommendation activity between quarter *t*+1 and quarter *t*-1. *Influential before* is a dummy variable which equals one if the analyst has been influential at least once before quarter *t*, and zero otherwise. For Panel B, to aggregate recommendation activity to the broker level, we reinstate team analysts. At the broker-month setting, *#InfluRecchg/#Firms* is the fraction of influential recommendation changes over the number of firms covered by a broker in month *m*+1. Similarly, %*TotalRec*, *%Recchg*, and *%CleanRecchg* are the fractions of total recommendation activity, recommendation changes, and uncontaminated recommendation changes, where the denominator is the number of firms covered by a broker in month *m*+1. Other variables at the broker-month level are defined in the same way as those at the analyst-quarter level. *Influential dummy* equals one if the broker has at least one influential recommendation change in month *m*, and zero otherwise. *Rec-change dummy* equals one if a broker in month *m* with uncontaminated recommendation changes issues at least one recommendation change in month *m*+1. *Experience* measures the number of quarters since the analyst issued the first earnings forecast or stock recommendation on I/B/E/S. *Accuracy quintile* is the average forecast accuracy quintile of the analyst based on the firms covered in the past year (5=most accurate). *LFR* is the analyst's prior-year leader-follower ratio (computed from recommendations). *Star analyst* equals one if the analysts are ranked as an All-American team in the latest October *Institutional Investor* magazine's annual poll. *#Firmsperana* is the number of firms that an analyst covers in a quarter. Firm characteristics are averaged across the firms that analysts cover in a quarter. *Size* is last June's market cap, *BM* is the book-to-market ratio, *Momentum* is the buy-and-hold return for the 11-month period ending one month before beginning of the recommendation month, and *Stock volatility* is the standard deviation of daily stock returns in the prior month (one month prior to the recommendation month).

Table 2.1: Descriptive statistics (Cont'd)

	Mean	Stdev	P25	Median	P75	#Obs.
Panel A: Analyst recommendation activity (analyst-quarter setting)						
Influential dummy	0.223	0.416	0	0	0	79192
Rec-change dummy (*t*+1)	0.656	0.475	0	1	1	79192
Clean rec-change dummy (*t*+1)	0.533	0.499	0	1	1	79192
#Total activity (*t*+1)	14.4	11.6	6	12	20	79192
#Rec-change (*t*+1)	2.53	2.16	1	2	3	79192
#Clean rec-change (*t*+1)	1.95	1.71	1	1	2	79192
ΔTotal activity (from *t*-1 to *t*+1)	0.090	8.04	-4	0	4	78371
Influential before	0.608	0.488	0	1	1	79192
Panel B: Broker recommendation activity (broker-month setting)						
Influential dummy	0.406	0.491	0	0	1	25628
#InfluRecchg / #Firms	0.005	0.015	0	0	0.005	25628
Rec-change dummy (*m*+1)	0.873	0.333	1	1	1	25628
Clean rec-change dummy (*m*+1)	0.817	0.387	1	1	1	25628
#Total activity (*m*+1)	80.3	109	15	40	98	25628
#Rec-change (*m*+1)	8.41	11.5	2	5	11	25628
#Clean rec-change (*m*+1)	5.59	8.5	1	3	7	25628
%TotalRec (*m*+1)	0.338	0.224	0.194	0.303	0.448	25628
%Recchg (*m*+1)	0.046	0.093	0.017	0.033	0.056	25628
%CleanRecchg (*m*+1)	0.031	0.079	0.009	0.020	0.036	25628
Panel C: Analyst and firm average characteristics (analyst-quarter setting)						
Experience (#qtrs)	26.9	20.9	10.3	22.2	38.8	79192
Accuracy quintile	2.99	0.436	2.77	3	3.22	71904
LFR	2.43	3.1	1.08	1.66	2.67	73213
Star analyst	0.126	0.331	0	0	0	79192
#Firmsperana	12.7	7.37	8	12	16	79192
Size ($m)_avg	8,573	22,173	713	2,201	7,168	79192
BM_avg	0.546	1.18	0.268	0.436	0.677	79192
Momentum_avg	0.166	0.61	-0.132	0.0952	0.339	79192
Total volatility_avg	0.029	0.018	0.017	0.024	0.035	79192

Table 2.2: Future rec-change probability conditional on having influential rec-changes

The probits estimate the marginal effect of having influential recommendation changes this quarter on the probability of issuing recommendation changes next quarter, controlling for analyst and firms' average characteristics. The sample here is based on analyst-quarter observations which have uncontaminated recommendation changes in a quarter *t*. The sample of recommendation changes are from I/B/E/S Detail U.S. File 1994-2014. In parentheses are *z*-statistics based on standard errors clustered by analysts, where *, **, *** denote statistical significance at 10%, 5%, and 1% respectively. Calendar quarter fixed effects are included when indicated. See Table 2.1 for definitions of variables.

	Rec-change dummy (t+1)			Uncontaminated Rec-change dummy (t+1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Influential dummy	0.061***	0.037***	0.035***	0.050***	0.030***	0.028***
	(14.40)	(8.76)	(8.36)	(10.74)	(6.58)	(6.20)
Influential before			0.053***			0.050***
			(9.95)			(8.54)
Log experience		-0.010***	-0.021***		-0.020***	-0.030***
		(-3.29)	(-6.15)		(-5.46)	(-7.59)
Accuracy quintile		0.011**	0.010**		0.002	0.001
		(2.31)	(2.06)		(0.40)	(0.17)
LFR		-0.001	-0.001		-0.002**	-0.002**
		(-0.85)	(-0.98)		(-2.42)	(-2.54)
Star analyst		-0.039***	-0.041***		-0.029***	-0.031***
		(-5.15)	(-5.52)		(-3.53)	(-3.82)
Log BM_avg		0.005*	0.005*		0.025***	0.025***
		(1.86)	(1.85)		(7.98)	(8.01)
Log size_avg		-0.000	-0.000		0.001	0.002
		(-0.17)	(-0.02)		(0.88)	(1.02)
Stock volatility_avg		0.590***	0.600***		0.215	0.226
		(4.38)	(4.48)		(1.48)	(1.56)
Momentum_avg		0.011***	0.011***		0.012***	0.012***
		(3.47)	(3.53)		(3.39)	(3.45)
Log #Firmsperana		0.196***	0.186***		0.209***	0.200***
		(40.04)	(37.79)		(36.32)	(34.56)
Predicted Prob.	0.656	0.695	0.696	0.533	0.557	0.557
Quarter F.E.	No	Yes	Yes	No	Yes	Yes
Pseudo R^2	0.0023	0.0553	0.0569	0.0012	0.0498	0.0510
#Obs.	79192	66393	66393	79192	66393	66393

Table 2.3: Future total recommendation activity conditional on having influential rec-changes

The pooled OLS regressions estimate the effect of having influential recommendation changes this quarter on the number of total recommendation activity from the same analyst next quarter, and the change in the number of total activity from quarter *t*-1 to *t*+1, controlling for analyst and firms' average characteristics. The sample here is based on analyst-quarter observations which have uncontaminated recommendation changes in a quarter *t*, using Loh and Stulz (2011)'s definition of firm news contamination. The sample of recommendation changes are from I/B/E/S Detail U.S. File 1994-2014. In parentheses are *t*-statistics based on standard errors clustered by analysts, where *, **, *** denote statistical significance at 10%, 5%, and 1% respectively. Calendar quarter fixed effects are included when indicated. See Table 2.1 for definitions of variables.

	Log #Total activity (*t*+1)			ΔTotal activity (from *t*-1 to *t*+1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Influential dummy	0.258***	0.062***	0.060***	0.195***	0.243***	0.259***
	(28.54)	(9.56)	(9.30)	(2.88)	(3.27)	(3.48)
Influential before			0.062***			-0.450***
			(6.25)			(-6.47)
Log experience		-0.047***	-0.059***		-0.537***	-0.449***
		(-7.55)	(-8.95)		(-14.97)	(-11.84)
Accuracy quintile		0.109***	0.107***		-0.094	-0.083
		(11.25)	(11.15)		(-1.51)	(-1.33)
LFR		-0.001	-0.001		-0.003	-0.003
		(-1.14)	(-1.24)		(-0.41)	(-0.33)
Star analyst		0.110***	0.107***		0.104	0.123
		(7.61)	(7.44)		(1.18)	(1.40)
Log BM_avg		0.071***	0.071***		-0.044	-0.043
		(9.54)	(9.56)		(-0.96)	(-0.92)
Log size_avg		0.053***	0.053***		0.008	0.004
		(15.08)	(15.27)		(0.40)	(0.20)
Stock volatility_avg		2.074***	2.084***		-18.240***	-18.310***
		(5.57)	(5.60)		(-6.65)	(-6.67)
Momentum_avg		0.031***	0.031***		0.453***	0.452***
		(4.45)	(4.47)		(7.07)	(7.06)
Log #Firmsperana		0.761***	0.749***		-0.630***	-0.545***
		(80.51)	(77.54)		(-11.52)	(-9.56)
Intercept	2.351***	-0.323***	-0.300***	0.046*	3.755***	3.585***
	(257.00)	(-5.18)	(-4.84)	(1.75)	(9.78)	(9.32)
Quarter F.E.	No	Yes	Yes	No	Yes	Yes
Adj. R^2	0.014	0.331	0.332	0.000	0.074	0.074
#Obs.	79192	67525	67525	78371	67253	67253

Table 2.4: Future recommendation activity conditional on having influential rec-changes: Different horizons

This table illustrates the effect of having influential rec-changes this quarter on the quarter *t*+1 and the quarter *t*+4 rec-change probability, uncontaminated rec-change probability, the level and the change of total recommendation activity, controlling for analyst and firms' average characteristics. The dependent variable in column 7 (column 8) is the difference in the number of total recommendation activity between quarter *t*+1 (*t*+4) and quarter *t*-1. The sample of recommendations are from I/B/E/S Detail U.S. File 1994-2014. The estimations here are based on analyst-quarter observations which have uncontaminated recommendation changes in a quarter *t*, using Loh and Stulz (2011)'s definition of firm news contamination. The Chi-square statistics and p-value are used to examine whether the effects of the current-quarter influential recommendation changes are different in quarter *t*+1 and quarter *t*+4 samples. In parentheses are *z*-statistics (column 1-column 4 and *t*-statistics (column 5-column 8) based on standard errors clustered by analysts, where *, **, *** denote statistical significance at 10%, 5%, and 1% respectively. Calendar quarter fixed effects are included. The R-sq. in column 1 to column 4 are pseudo R-squares based on the probit estimations, and the R-sq. in column 5 to column 8 are adjusted R-squares based on the pooled OLS regressions. See Table 2.1 for definitions of variables.

	Rec-change dummy		Uncontaminated Rec-change dummy		Log #Total activity		ΔTotal activity	
	t+1	*t*+4	*t*+1	*t*+4	*t*+1	*t*+4	*t*+1	*t*+4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Influential dummy	0.035***	0.020***	0.028***	0.015***	0.060***	0.048***	0.259***	0.151
	(8.36)	(4.298)	(6.20)	(3.062)	(9.30)	(5.05)	(3.48)	(1.54)
Influential before	0.053***	0.052***	0.050***	0.051***	0.062***	0.082*****	-0.450***	-0.451***
	(9.95)	(7.890)	(8.54)	(7.684)	(6.25)	(5.43)	(-6.47)	(-3.83)
Log experience	-0.021***	-0.024***	-0.030***	-0.032***	-0.059***	-0.089***	-0.449***	-0.789***
	(-6.15)	(-5.782)	(-7.59)	(-7.297)	(-8.95)	(-9.07)	(-11.84)	(-11.14)
Accuracy quintile	0.010**	0.019***	0.001	0.012**	0.107***	0.122***	-0.083	-0.133
	(2.06)	(3.343)	(0.17)	(1.969)	(11.15)	(9.00)	(-1.33)	(-1.26)
LFR	-0.001	0.000	-0.002**	-0.001	-0.001	-0.000	-0.003	0.004
	(-0.98)	(0.397)	(-2.54)	(-1.270)	(-1.24)	(-0.27)	(-0.33)	(0.30)
Star analyst	-0.041***	-0.014	-0.031***	-0.007	0.107***	0.172***	0.123	0.621***
	(-5.52)	(-1.574)	(-3.82)	(-0.767)	(7.44)	(8.36)	(1.40)	(3.90)
Log BM_avg	0.005*	0.004	0.025***	0.022***	0.071***	0.068***	-0.043	-0.151**
	(1.85)	(1.159)	(8.01)	(6.308)	(9.56)	(6.49)	(-0.92)	(-1.97)
Log size_avg	-0.000	-0.001	0.002	0.003	0.053***	0.043***	0.004	-0.143***
	(-0.02)	(-0.764)	(1.02)	(1.591)	(15.27)	(8.47)	(0.20)	(-4.07)
Stock volatility_avg	0.600***	-0.090	0.226	-0.136	2.084***	-1.178**	-18.31***	-51.60***
	(4.48)	(-0.587)	(1.56)	(-0.854)	(5.60)	(-2.11)	(-6.67)	(-11.50)
Momentum_avg	0.011***	0.017***	0.012***	0.014***	0.031***	0.075***	0.452***	0.778***
	(3.53)	(4.264)	(3.45)	(3.657)	(4.47)	(6.49)	(7.06)	(7.39)
Log #Firmsperana	0.186***	0.153***	0.200***	0.162***	0.749***	0.630***	-0.545***	-2.419***
	(37.79)	(25.96)	(34.56)	(24.78)	(77.54)	(43.90)	(-9.56)	(-22.95)
Intercept					-0.300***	-0.262***	3.585***	4.621***
					(-4.84)	(-2.61)	(9.32)	(5.77)
Comparing the marginal effects of *Influential dummy* in (*t*+1) and (*t*+4) samples								
Chi-square statistics	9.40***		5.70**		1.72		2.44	
[p-value]	[0.0022]		[0.0170]		[0.1893]		[0.1183]	
Predicted Prob.	0.696	0.622	0.557	0.497	-	-	-	-
Quarter F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0569	0.0334	0.0510	0.0346	0.332	0.164	0.074	0.085
#Obs.	66393	62065	66393	62065	67525	64878	67253	64625

Table 2.5: Future recommendation activity conditional on having influential rec-changes: Broker level analyses

This table illustrates the effect of having influential recommendation changes this month on the fractions of recommendation changes (%Recchg), uncontaminated recommendation changes (%Clean Recchg), and total recommendation activity (%TotalRec) over the number of firms covered in the month *m*+1, *m*+3, and *m*+12 at the broker level, controlling for analyst and firm average characteristics of the broker, as well as the number of analysts per broker in that month. The sample here is based on broker-month observations which have uncontaminated recommendation changes in a month *m*. The sample of recommendation changes is from I/B/E/S Detail U.S. File 1994-2014 and analyst codes associated with teams are included. In parentheses are *t*-statistics based on standard errors clustered by broker, where *, **, *** denote statistical significance at 10%, 5%, and 1% respectively. Calendar month fixed effects and broker fixed effects are included. See Table 2.1 for definitions of variables.

	Month *m*+1			Month *m*+3			Month *m*+12		
	%Recchg	%Clean Recchg	%Total Rec	%Recchg	%Clean Recchg	%Total Rec	%Recchg	%Clean Recchg	%Total Rec
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
#InfluRecchg/#Firms	0.321***	0.157**	1.154**	0.234	0.173	0.433	0.0063	0.076	-0.115
	(3.14)	(2.00)	(2.29)	(1.16)	(0.91)	(0.95)	(0.14)	(0.94)	(-0.72)
Dependent var (*m*)	0.454***	0.468***	0.156***	0.393***	0.365***	0.325***	0.125***	0.114***	0.204***
	(3.41)	(3.42)	(3.59)	(4.09)	(3.99)	(12.16)	(6.48)	(6.89)	(13.10)
Log Experience_avg	0.001	0.001	-0.003	0.000	0.000	-0.004	-0.001	-0.001	-0.006
	(0.65)	(0.67)	(-0.66)	(0.36)	(0.26)	(-1.10)	(-0.68)	(-0.63)	(-1.62)
Accuracy Quintile_avg	-0.001	-0.001	0.004	-0.001	-0.002	0.002	-0.001	-0.001	0.000
	(-0.54)	(-0.77)	(0.90)	(-0.82)	(-1.04)	(0.61)	(-0.58)	(-1.04)	(0.04)
LFR_avg	-0.000	-0.000	-0.001	-0.000	-0.000	-0.001	-0.000	-0.000	-0.000
	(-1.17)	(-1.54)	(-0.72)	(-0.47)	(-0.64)	(-1.58)	(-0.45)	(-0.72)	(-0.46)
Star Analyst_avg	0.006	0.006	0.016	0.005	0.005	0.020	0.011*	0.009*	0.016
	(1.25)	(1.35)	(0.44)	(1.04)	(1.25)	(0.66)	(1.80)	(1.67)	(0.44)
Log BM_avg	-0.003***	-0.002***	-0.0001	-0.001	-0.000	-0.0005	-0.0003	-0.0001	-0.003
	(-3.02)	(-2.95)	(-0.02)	(-1.10)	(-0.14)	(-0.20)	(-0.18)	(-0.04)	(-1.20)
Log Size_avg	-0.002*	-0.001*	0.002	-0.001*	-0.001*	0.001	-0.001	-0.001*	-0.001
	(-1.73)	(-1.66)	(1.54)	(-1.91)	(-1.82)	(0.59)	(-1.42)	(-1.71)	(-0.72)
Stock Volatility_avg	0.035	0.025	0.107	0.024	0.037	0.041	0.040	0.047	0.029
	(0.84)	(0.62)	(0.76)	(0.39)	(0.62)	(0.32)	(1.13)	(1.52)	(0.23)
Momentum_avg	0.001	0.001	0.005*	0.001	0.001	0.001	0.001	0.001	0.006*
	(1.04)	(1.21)	(1.76)	(0.71)	(1.05)	(0.53)	(0.87)	(0.85)	(1.87)
Log #Anaperbroker	0.005*	0.005*	0.023**	0.005*	0.003	0.029***	0.000	-0.001	0.017*
	(1.73)	(1.80)	(2.40)	(1.83)	(1.40)	(3.75)	(0.11)	(-0.54)	(1.71)
Intercept	0.041***	0.029**	0.168***	0.036***	0.031***	0.080**	0.056***	0.051***	0.185***
	(3.36)	(2.23)	(3.90)	(3.20)	(2.72)	(2.16)	(4.51)	(4.90)	(4.61)
Month F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Broker F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.525	0.505	0.479	0.478	0.442	0.518	0.366	0.341	0.494
#Obs.	25628	25628	25628	25467	25467	25467	24475	24475	24475

Table 2.6: Future recommendation activity conditional on having influential rec-changes: Evidence from capacity constrained vs. unconstrained analysts

This table illustrates the effect of having influential recommendation changes this quarter on the probability of issuing recommendation changes and the number of total recommendation activity next quarter for capacity constrained (C) and unconstrained (U) analysts, respectively, controlling for analyst and firms' average characteristics. The estimations here are based on analyst-quarter observations which have uncontaminated recommendation changes in a quarter *t*, using Loh and Stulz (2011)'s definition of firm news contamination. The capacity constrained (unconstrained) analysts are those whose number of covered firms is above (below) the median. The sample of recommendation changes are from I/B/E/S Detail U.S. File 1994-2014. The Chi-square statistics and p-value are used to examine whether the effects of the current-quarter influential recommendation changes are different for the capacity constrained and unconstrained subsamples. In parentheses are z-statistics (column 1-column 4) and t-statistics (column 5 and column 6) based on standard errors clustered by analysts, where *, **, *** denote statistical significance at 10%, 5%, and 1% respectively. Calendar quarter fixed effects are included. The R-sq. in column 1 to column 4 are pseudo R-squares based on the probit estimations, and the R-sq. in column 5 and column 6 are adjusted R-squares based on the pooled OLS regressions. See Table 2.1 for definitions of variables.

	Rec-change dummy (*t*+1)		Uncontaminated Rec-change dummy (*t*+1)		Log #Total activity (*t*+1)	
	C	U	C	U	C	U
	(1)	(2)	(3)	(4)	(5)	(6)
Influential dummy	0.040***	0.027***	0.034***	0.020***	0.061***	0.054***
	(7.660)	(4.081)	(5.641)	(2.926)	(7.132)	(5.922)
Influential before	0.065***	0.044***	0.063***	0.042***	0.066***	0.055***
	(8.086)	(6.249)	(6.904)	(5.584)	(4.036)	(4.929)
Log experience	-0.017***	-0.022***	-0.031***	-0.027***	-0.023**	-0.082***
	(-3.335)	(-5.226)	(-5.114)	(-5.704)	(-2.098)	(-11.71)
Accuracy quintile	0.009	0.010*	-0.001	0.002	0.195***	0.061***
	(1.193)	(1.679)	(-0.098)	(0.391)	(9.920)	(6.225)
LFR	-0.000	-0.001	-0.001	-0.002**	-0.003***	0.001
	(-0.297)	(-0.992)	(-1.444)	(-2.069)	(-2.634)	(0.718)
Star analyst	-0.036***	-0.053***	-0.028***	-0.040***	0.105***	0.087***
	(-4.175)	(-4.477)	(-2.869)	(-3.304)	(5.937)	(4.402)
Log BM_avg	0.004	0.007*	0.025***	0.025***	0.102***	0.056***
	(0.908)	(1.904)	(5.617)	(6.053)	(8.132)	(6.704)
Log size_avg	0.003	-0.002	0.004*	-0.000	0.060***	0.049***
	(1.269)	(-1.059)	(1.705)	(-0.168)	(10.45)	(13.08)
Stock volatility_avg	0.636***	0.556***	0.227	0.212	3.314***	1.044**
	(3.210)	(3.048)	(1.038)	(1.123)	(5.361)	(2.402)
Momentum_avg	0.006	0.015***	0.007	0.015***	0.026**	0.033***
	(1.117)	(3.585)	(1.203)	(3.391)	(2.025)	(3.985)
Log #Firmsperana	0.146***	0.205***	0.205***	0.186***	0.668***	0.753***
	(11.41)	(28.02)	(13.21)	(23.27)	(22.93)	(68.85)
Intercept					-0.252*	0.186**
					(-1.832)	(2.383)
Comparing the marginal effects of *Influential dummy* for constrained vs. unconstrained analysts						
Chi-square statistics	5.88**		2.71*		0.38	
[p-value]	[0.0153]		[0.0998]		[0.5391]	
Predicted Prob.	0.769	0.612	0.642	0.470	-	-
Quarter F.E.	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0266	0.0452	0.0274	0.0374	0.167	0.266
#Obs.	33418	32975	33418	32975	34012	33513

Table 2.7: Future influential probability conditional on having influential rec-changes

The probits in Panel A estimate the marginal effect of having influential recommendation changes this quarter on the quarter *t*+1 and the quarter *t*+4 recommendation change influential probability, controlling for analyst and firm average characteristics. The sample of recommendation changes is from I/B/E/S Detail U.S. File 1994-2014. The estimation in Panel A is based on analyst-quarter observations which have uncontaminated recommendation changes in a quarter *t*, using Loh and Stulz (2011)'s definition of firm news contamination. The Chi-square statistics and p-value are used to examine whether the effects of the current-quarter influential recommendation changes are different for quarter *t*+1 and quarter *t*+4 recommendation change influential probability. The probits in Panel B estimate the marginal effect of having influential recommendation changes this month on the month *m*+1, *m*+3, and *m*+12 recommendation change influential probability, controlling for analysts and firm average characteristics of the broker in that month. *Influential dummy* in the Panel B regressions equals one if the broker has at least one influential recommendation change in a month, and zero otherwise. The estimation in Panel B is based on broker-month observations which have uncontaminated recommendation changes in a month *m*, using Loh and Stulz (2011)'s definition of firm news contamination. Calendar quarter fixed effects are included in the regressions in Panel A, and broker fixed effects and calendar month fixed effects are included in the regressions in Panel B. In parentheses are *z*-statistics based on standard errors clustered by analysts, where *, **, *** denote statistical significance at 10%, 5%, and 1% respectively. See Table 2.1 for definitions of variables.

Panel A: Analyst-quarter setting

	Influential dummy (*t*+1) (1)	Influential dummy (*t*+4) (2)	Influential dummy (*t*+1) (3)	Influential dummy (*t*+4) (4)
Influential dummy	0.057***	0.039***	0.032***	0.018***
	(15.03)	(11.25)	(8.934)	(5.197)
Influential before			0.041***	0.038***
			(11.60)	(10.22)
Log experience			-0.012***	-0.015***
			(-4.791)	(-5.894)
Accuracy quintile			0.015***	0.011***
			(4.387)	(3.066)
LFR			-0.000	-0.000
			(-0.642)	(-0.837)
Star analyst			0.008*	0.016***
			(1.687)	(3.007)
Log BM_avg			0.005**	0.006***
			(2.316)	(2.632)
Log size_avg			-0.007***	-0.004***
			(-6.341)	(-3.402)
Stock volatility_avg			-0.404***	-0.128
			(-3.956)	(-1.221)
Momentum_avg			0.005**	0.001
			(2.039)	(0.405)
Log #Firmsperana			0.088***	0.075***
			(23.00)	(18.68)
Comparing the marginal effects of *Influential dummy* in (*t*+1) and (*t*+4) samples				
Chi-square statistics	12.07***		8.37***	
[p-value]	[0.0005]		[0.0038]	
Predicted Prob.	0.128	0.116	0.127	0.119
Quarter F.E.	Yes	Yes	Yes	Yes
Pseudo R^2	0.0060	0.0034	0.0484	0.0407
#Obs.	79192	75600	66393	62065

Table 2.7: Future influential probability conditional on having influential rec-changes (Cont'd)

Panel B: Broker-month setting

	Influential dummy (m+1) (1)	Influential dummy (m+3) (2)	Influential dummy (m+12) (3)
Influential dummy	0.031***	0.017**	0.001
	(4.63)	(2.54)	(0.18)
Log Experience_avg	0.017**	0.015**	0.011
	(2.37)	(1.99)	(1.39)
Accuracy Quintile_avg	0.000	0.001	0.015
	(0.00)	(0.09)	(1.46)
LFR_avg	0.000	-0.001	0.001
	(0.10)	(-0.48)	(0.59)
Star Analyst_avg	0.092*	0.123***	0.145***
	(1.95)	(2.84)	(2.89)
Log BM_avg	0.000	0.004	0.002
	(0.01)	(0.71)	(0.27)
Log Size_avg	0.004	0.000	0.001
	(1.25)	(0.14)	(0.38)
Stock Volatility_avg	-0.449	-0.262	-0.263
	(-1.55)	(-0.86)	(-0.85)
Momentum_avg	0.009	0.011	-0.008
	(1.23)	(1.64)	(-1.06)
Log #Anaperbroker	0.186***	0.188***	0.175***
	(13.71)	(14.41)	(11.76)
Predicted Prob.	0.361	0.366	0.373
Month F.E.	Yes	Yes	Yes
Broker F.E.	Yes	Yes	Yes
Pseudo. R^2	0.282	0.277	0.277
#Obs.	24283	23814	22131

Table 2.8: Feedback effect pre- and post-Reg FD

This table illustrates the effects of having influential rec-changes this quarter on the (uncontaminated) rec-change probability and the number of total activity in the pre- and post-Reg FD periods. The analyst and firms' average characteristics are controlled for in each estimation. The sample of recommendations are from I/B/E/S Detail U.S. File 1994-2014. Post-Reg FD period starts from 2000q4 to 2014q4, and pre-Reg FD period starts from 1994q1 to 2000q3. The estimations here are based on analyst-quarter observations which have uncontaminated recommendation changes in a quarter t, using Loh and Stulz (2011)'s definition of firm news contamination. The Chi-square statistics and p-value are used to examine whether the effects of the current-quarter influential recommendation changes are different in the post and pre-Reg FD samples. In parentheses are z-statistics (column 1-column 4) and t-statistics (column 5 and 6) based on standard errors clustered by analysts, where *, **, *** denote statistical significance at 10%, 5%, and 1% respectively. Calendar quarter fixed effects are included. The R-sq. in column 1 to column 4 are pseudo R-squares based on the probit estimations, and the R-sq. in column 5 and column 6 are adjusted R-squares based on the pooled OLS regressions. See Table 2.1 for definitions of variables.

	Rec-change dummy (t+1)		Uncontaminated Rec-change dummy (t+1)		Log #Total activity (t+1)	
	Pre Reg FD (1)	Post Reg FD (2)	Pre Reg FD (3)	Post Reg FD (4)	Pre Reg FD (5)	Post Reg FD (6)
Influential dummy	0.041***	0.033***	0.025***	0.029***	0.080***	0.051***
	(5.097)	(6.677)	(2.898)	(5.501)	(6.015)	(7.083)
Influential before	0.065***	0.045***	0.052***	0.051***	0.070***	0.056***
	(8.217)	(6.361)	(5.936)	(6.600)	(4.142)	(4.850)
Log experience	-0.012**	-0.024***	-0.013**	-0.038***	0.004	-0.086***
	(-2.404)	(-5.546)	(-2.204)	(-7.639)	(0.316)	(-11.04)
Accuracy quintile	0.016**	0.004	0.001	0.000	0.145***	0.082***
	(2.276)	(0.689)	(0.184)	(0.0417)	(8.786)	(7.322)
LFR	-0.001	-0.000	-0.004***	-0.001	-0.001	-0.001
	(-1.089)	(-0.572)	(-2.938)	(-1.338)	(-0.413)	(-1.189)
Star analyst	-0.017	-0.054***	-0.042***	-0.025**	0.068***	0.129***
	(-1.527)	(-5.598)	(-3.616)	(-2.444)	(2.679)	(8.219)
Log BM_avg	0.163***	0.197***	0.184***	0.208***	0.628***	0.808***
	(22.22)	(34.47)	(22.03)	(30.71)	(33.82)	(75.49)
Log size_avg	-0.008	0.011***	0.010*	0.031***	0.090***	0.071***
	(-1.539)	(3.282)	(1.812)	(8.483)	(6.679)	(8.290)
Stock volatility_avg	0.004	-0.002	0.001	0.002	0.062***	0.052***
	(1.471)	(-1.118)	(0.268)	(0.945)	(10.17)	(12.83)
Momentum_avg	0.670***	0.556***	0.212	0.171	4.391***	0.972**
	(2.828)	(3.395)	(0.841)	(0.988)	(6.336)	(2.240)
Log #Firmsperana	0.007*	0.013***	0.009*	0.013***	0.006	0.044***
	(1.649)	(2.762)	(1.897)	(2.585)	(0.675)	(3.944)
Intercept					-0.673***	-0.139*
					(-6.404)	(-1.895)
Comparing the marginal effects of *Influential dummy* in the post and pre-Reg FD samples						
Chi-square statistics	0.98		0.06		3.84*	
[p-value]	[0.3217]		[0.8031]		[0.0501]	
Predicted Prob.	0.717	0.686	0.605	0.536	-	-
Quarter F.E.	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0509	0.0595	0.0533	0.0470	0.261	0.338
#Obs.	20559	45834	20559	45834	20632	46893

Table 2.9: Stock recommendation drift for previously influential vs. uninfluential analysts

All recommendation changes in the next quarter (t+1) are placed into four portfolios based on whether the analyst issues at least one influential recommendation change in the current quarter (*Influential dummy*) and the direction of each revision. Each portfolio is a daily-rebalanced calendar-time portfolio that buys stocks from trading day 2 following the revision to day 21, i.e. a one-month drift. The daily average returns of each portfolio are computed following the standard approach in Barber, Lehavy, and Trueman (2007), in which one dollar is placed in each revision and the weight of the revised stock varies from day 2 to day 21 according to its cumulative return since entering the portfolio. The portfolio's daily returns are then compounded to monthly returns. The portfolio return in excess of the risk-free rate is then regressed against the Fama and French (2015) five factors and the coefficients reported. Sample data are from I/B/E/S Detail U.S. File and CRSP 1994-2014. In parentheses are t-statistics, where *, **, *** denote statistical significance at 10%, 5%, and 1% respectively based on a null hypothesis of zero for the coefficient (null hypothesis of one for the MKTRF coefficient of upgrade and downgrade portfolios).

		Intercept (%)	MKTRF	SMB	HML	RMW	CMA	Adj. R^2	Avg #Firms perday
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(9)
Influential dummy=1	Upgrades (t+1)	0.467***	1.198***	0.430***	0.210***	0.124	0.126	0.811	57.8
		(2.67)	(4.24)	(7.18)	(2.68)	(1.52)	(1.17)		
	Downgrades (t+1)	-0.686***	1.100**	0.396***	0.311***	0.038	-0.253**	0.802	58.4
		(-3.89)	(2.12)	(6.58)	(3.95)	(0.46)	(-2.33)		
	Difference (1)	1.154***	0.098	0.034	-0.101	0.087	0.379***	0.017	104.5
		(5.01)	(1.61)	(0.43)	(-0.98)	(0.81)	(2.68)		
Influential dummy=0	Upgrades (t+1)	0.565***	1.111***	0.442***	0.216***	0.094*	-0.127*	0.895	217.4
		(4.67)	(3.45)	(10.71)	(4.00)	(1.67)	(-1.71)		
	Downgrades (t+1)	-0.651***	1.106***	0.463***	0.137**	-0.020	-0.143*	0.902	233.0
		(-5.37)	(3.28)	(11.18)	(2.53)	(-0.35)	(-1.91)		
	Difference (0)	1.217***	0.005	-0.021	0.079	0.114*	0.015	0.057	406.9
		(9.45)	(0.15)	(-0.48)	(1.38)	(1.89)	(0.19)		
	[Difference (1) – Difference (0)]	-0.063	0.093	0.055	-0.180*	-0.027	0.364**	0.020	473.8
		(-0.26)	(1.47)	(0.67)	(-1.69)	(-0.24)	(2.49)		

Table 2.10: Alphas of firms without rec-changes sorted on the favorableness of recent influential rec-changes in their industry

In each month, we divide industries into two groups based on the number of firms in the industry (this controls for industry size), and then sort industries into five quintiles based on the favorableness of the influential recommendation changes in their industry. Favorableness is proxied by the difference between the number of influential upgrades and influential downgrades in the industry. We then hold stocks of firms without recommendation changes in these quintiles for three months. The average calendar-time monthly return of firms in each quintile is computed using one plus the firm's prior-month return as the weight, following Asparouhova, Bessembinder, and Kalcheva (2013). The average portfolio monthly returns in excess of the risk-free rate (*Rprf*) is reported in the column 1. *Rprf* is regressed against the Fama and French (1993) three factors or the Fama and French (2015) five factors and the respective alphas are reported in column 2 and column 3. The average number of firms (industries) per month of each portfolio is reported in column 4 (column 5). Column 6 to column 8 reports the average number of upgrades and downgrades and the ratio of upgrades over downgrades for the period where stocks are held (average here uses the same weights as the weights used in the returns computations). Recommendation data is from I/B/E/S Detail U.S. File 1994-2014. In parentheses are *t*-statistics, where *, **, *** denote statistical significance at 10%, 5%, and 1% respectively based on a null hypothesis of zero for the coefficient being tested.

Panel A: Full sample of firms without recommendation changes in sorting month

Portfolio	Avg Rprf (1)	FF-3 Alpha (2)	FF-5 Alpha (3)	Avg #Firms permth (4)	Avg #Ind permth (5)	Avg #Up permth (6)	Avg #Down permth (7)	Avg #Up/#Down permth (8)
1	0.874**	-0.182	-0.121	958	7.465	0.099	0.125	0.864
	(2.34)	(-1.20)	(-0.79)					
2	1.045***	-0.035	0.001	1165	12.343	0.105	0.127	0.884
	(2.77)	(-0.27)	(0.000)					
3	0.883**	-0.202*	-0.130	1111	11.354	0.107	0.131	0.880
	(2.35)	(-0.10)	(-1.03)					
4	0.981***	-0.081	0.006	1029	11.413	0.108	0.130	0.892
	(2.63)	(-0.77)	(0.006)					
5	1.452***	0.412***	0.481***	1027	7.937	0.106	0.124	0.921
	(3.95)	(3.47)	(4.13)					
5-1	0.578***	0.594**	0.602***	-	-	0.007***	-0.002	0.058***
	(2.87)	(3.06)	(3.11)			(5.34)	(-0.99)	(3.27)

Panel B: Subsample of large firms without recommendation changes in sorting month

Portfolio	Avg Rprf (1)	FF-3 Alpha (2)	FF-5 Alpha (3)	Avg #Firms permth (4)	Avg #Ind permth (5)	Avg #Up permth (6)	Avg #Down permth (7)	Avg #Up/#Down permth (8)
1	0.974***	-0.056	-0.028	522	7.465	0.147	0.180	0.903
	(2.68)	(-0.38)	(-0.18)					
2	0.974***	-0.070	-0.069	656	12.343	0.152	0.178	0.924
	(2.72)	(-0.55)	(-0.52)					
3	0.860**	-0.199*	-0.169	635	11.331	0.155	0.183	0.920
	(2.41)	(-1.73)	(-1.42)					
4	0.947***	-0.071	-0.067	583	11.402	0.156	0.182	0.931
	(2.74)	(-0.67)	(-0.63)					
5	1.368***	0.372***	0.342***	563	7.937	0.155	0.176	0.963
	(4.01)	(3.27)	(3.01)					
5-1	0.394**	0.428**	0.370**	-	-	0.008***	-0.004	0.06***
	(2.07)	(2.32)	(2.02)			(3.87)	(-1.49)	(3.27)

Panel C: Subsample of small firms without recommendation changes in sorting month

Portfolio	Avg Rprf	FF-3 Alpha	FF-5 Alpha	Avg #Firms permth	Avg #Ind permth	Avg #Up permth	Avg #Down permth	Avg #Up/#Down permth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1	0.786*	-0.297	-0.200	438	7.429	0.043	0.061	0.866
	(1.94)	(-1.52)	(-0.99)					
2	1.172***	0.052	0.142	510	12.114	0.045	0.062	0.804
	(2.77)	(0.30)	(0.79)					
3	0.958**	-0.156	-0.029	478	11.236	0.044	0.062	0.784
	(2.27)	(-0.87)	(-0.15)					
4	1.032**	-0.086	0.095	447	11.276	0.047	0.064	0.824
	(2.41)	(-0.54)	(0.59)					
5	1.582***	0.475***	0.659***	467	7.890	0.048	0.061	0.894
	(3.72)	(2.73)	(3.77)					
5-1	0.796***	0.772***	0.860***	-	-	0.005***	0.000	0.027
	(3.47)	(3.46)	(3.80)			(4.34)	(0.22)	(0.37)

Table 2.11: Quarterly Fama-MacBeth regressions of firms without rec-changes on the favorableness of recent influential rec-changes in their industry

This table reports the time-series average of coefficients from quarterly Fama-MacBeth regressions of returns of firms without recommendation changes on the favorableness of recent influential rec-changes in their industry from 1994:Q2 to 2015:Q1. Firms are defined as having no recommendation changes using the most recent prior month of each calendar quarter in industries (Fama-French 30 industry groups) which had influential recommendation changes. Firms with stock prices less than $1 at the end of the prior month are excluded. *UpInfluQuintile* is the difference between the number of influential upgrades and influential downgrades in the industry, measured in the month prior to the calendar quarter. *LagLargeFirmRet* is the value-weighted prior-month return of same-industry firms that are in the largest size quintile (based on the NYSE breakpoints in the CRSP sample). *Size* is the log of prior-month market capitalization. *Log(BM)* is the log of book-to-market ratio (computed and aligned following Fama and French (2006)). *LagRet* is the prior-month return. *Ret_lag3mths* is the buy-and-hold return for the 3-month period ending one month before the beginning of the calendar quarter. *Ret_lag4to12mths* is the buy-and-hold return for the 9-month period ending four months prior to the calendar quarter. *Volatility* is standard deviation of monthly returns over the 12 months ending in the prior month. *Turnover* is average monthly turnover over the 12 months ending in the prior month. *IOownership* is the institutional ownership as a fraction of shares outstanding in the prior quarter. *IndustrySize* is the log of the number of firms in Fama-French 30 industry groups. *, **, *** denote statistical significance at 10%, 5%, and 1%, respectively, with associated time-series *t*-statistics in parentheses.

	Quarterly Return			
	(1)	(2)	(3)	(4)
UpInfluQuintile	0.004**	0.004*	0.004**	0.003**
	(1.99)	(1.87)	(2.32)	(2.20)
LagLargeFirmRet		0.282***		0.230***
		(3.17)		(3.50)
Size			-0.003**	-0.003**
			(-2.36)	(-2.23)
Log(BM)			0.003	0.003
			(1.27)	(1.49)
LagRet			-0.042**	-0.047**
			(-2.17)	(-2.56)
Ret_lag3mths			0.027***	0.026***
			(3.02)	(2.94)
Ret_lag4to12mths			0.005	0.004
			(0.65)	(0.60)
Volatility			-0.052	-0.055
			(-1.19)	(-1.25)
Turnover			-0.025	-0.025
			(-1.41)	(-1.49)
IOownership			0.010	0.009
			(1.40)	(1.15)
IndustrySize			0.004*	0.004*
			(1.70)	(1.78)
Intercept	0.024	0.021	0.043	0.039
	(1.57)	(1.50)	(1.64)	(1.51)
Avg. #Firms/Mth	2185.822	2181.25	2132.25	2127.83
Avg. R^2	0.008	0.016	0.075	0.079
Startdate	1994Q2	1994Q2	1994Q2	1994Q2
Enddate	2015Q1	2015Q1	2015Q1	2015Q1

Appendix C Tables for Chapter 3

Table 3.1: Frequency of overoptimistic executives

This table gives the yearly breakdown of the number of total firms, the number of firms with overoptimistic CEOs, and the number of firms with overoptimistic non-CEO manager team in our sample. The table also gives the distribution of firms with different combinations of CEO and non-CEO manager team who have the same or opposite characteristics of optimism. The sample of firms is from Execucomp for the 1993-2015 period, with available compensation data (item TDC1) of CEOs and other four highest ranked executives. Financial firms (SIC 6000-6999) and utility firms (SIC 4900-4999) are deleted. An executive is measured as overoptimistic for all years from the first time when CEO holds options that are more than 67% in the money, and at least two times during their sample tenure. A firm who is regarded as the one with overoptimistic non-CEO manager team (*Opt_NonCEO*) should have at least two of other four highest ranked executives (half) measured as over-optimism. *Group1_both* includes firms who have both overoptimistic CEOs and overoptimistic non-CEO executives, *Group2_CEO* includes firms with only overoptimistic CEOs, *Group3_NonCEO* includes firms with only overoptimistic non-CEO executive teams, and *Group4_Neither* includes firms with neither overoptimistic CEOs nor overoptimistic non-CEO manager team. For brevity, overoptimistic executives are labeled as "Opt" in the table.

Year	#Firms	Group1 Both	(%)	Group2 CEO	(%)	Group3 NonCEO	(%)	Group4 Neither	(%)
1993	464	131	28.23	44	9.48	30	6.47	259	55.82
1994	612	200	32.68	61	9.97	34	5.56	317	51.80
1995	642	249	38.79	72	11.21	43	6.70	278	43.30
1996	662	300	45.32	71	10.73	42	6.34	249	37.61
1997	719	366	50.90	74	10.29	39	5.42	240	33.38
1998	747	398	53.28	88	11.78	46	6.16	215	28.78
1999	760	384	50.53	111	14.61	46	6.05	219	28.82
2000	765	377	49.28	128	16.73	46	6.01	214	27.97
2001	762	363	47.64	150	19.69	32	4.20	217	28.48
2002	841	343	40.78	192	22.83	41	4.88	265	31.51
2003	865	385	44.51	189	21.85	45	5.20	246	28.44
2004	861	419	48.66	197	22.88	50	5.81	195	22.65
2005	793	388	48.93	173	21.82	51	6.43	181	22.82
2006	767	360	46.94	176	22.95	39	5.08	192	25.03
2007	825	357	43.27	192	23.27	42	5.09	234	28.36
2008	813	339	41.70	202	24.85	36	4.43	236	29.03
2009	810	305	37.65	200	24.69	48	5.93	257	31.73
2010	770	319	41.43	175	22.73	53	6.88	223	28.96
2011	731	313	42.82	160	21.89	55	7.52	203	27.77
2012	661	294	44.48	146	22.09	40	6.05	181	27.38
2013	595	313	52.61	99	16.64	38	6.39	145	24.37
2014	543	274	50.46	103	18.97	27	4.97	139	25.60
2015	503	226	44.93	104	20.68	28	5.57	145	28.83
Avg.			44.60		18.38		5.79		31.24

Table 3.2: Average characteristics across different groups

This table gives the summary statistics of the variables used in this study. The sample consists of all nonfinancial and nonutility firms in Execucomp from 1993 to 2015, and firms are required to have available compensation data for one CEO and other four highest ranked executives. The table illustrates the summary statistics for Group 1, Group 2, Group 3, and Group 4 firms, respectively. The p-value in the last column indicates whether the difference between Group 1 and Group 2 is significant or not. The meaning of each group is illustrated in Table 3.1. *Opt_CEO* equals one if an executive is measured as overoptimistic for all years from the first time when CEO holds options that are more than 67% in the money with at least two times during their sample tenure, and zero otherwise. *Opt_NonCEO* equals one if two of other four highest ranked executives are measured as overoptimism. *Invest* is the firm capital expenditures normalized by total asset at the beginning of the year. *PP&E growth* is the natural logarithm of the PP&E divided by the PP&E in the prior year. *Asset growth* is the natural logarithm of the total asset divided by the total asset at the beginning of the year. *Net debt issuance* is the ratio of net debt issuance over total assets. Net debt issuance is defined as long-term debt issuance net of long-term debt reduction. *Dividend ratio* is the firm's dividend payment over market capitalization. *Total payout* is the ratio firm's dividend payment plus share repurchases over market capitalization. *Tobin's Q* is constructed as the market value divided by the book value of assets. Market value of assets is book value of asset mines book value of equity and plus market value of equity. Book value of equity is equal to stockholder equity plus balance sheet deferred taxes and investment tax credit, minus the book value of preferred stock. Market equity is the fiscal year-end equity price multiplied by the number of common shares outstanding. *Cash flow* is earnings before extraordinary items plus depreciation and is normalized by total assets at the beginning of the year. *Firm size* is natural logarithm of total asset. Leverage is the debt in current liabilities plus long-term debt divided by the total asset. Tangibility is defined as net property, plant, and equipment divided by total assets. *Profitability* is the operating income before depreciation normalized by the total assets at the beginning of year. *IntanAssets* is the firm's intangible assets scaled by its total assets. *Log (PPE/Emp)* is the natural logarithm of the net property, plant, and equipment per employee. *Annual ret* is cumulative stock return over year *t*. *Industry PE* is average monthly industry PE over the fiscal year. The monthly industry PE is calculated as the natural logarithm of the industry's total market capitalization to total earnings less a 60-month moving average. *Stock vol* is the annualized standard deviation of stock returns estimated over the 60 months prior to the beginning of the fiscal period. *CEO Pay Slice* (CPS) is the percentage of the total compensation to the top five executives that goes to the CEO. *CEO stkown* is the fraction of company stock owned by the CEO. *CEO tenure* is the number of years the CEO has held that position. *CEO delta* is the dollar change in CEO stock and option portfolio for 1% change in stock price. *CEO vega* is the dollar change in CEO option holdings for a 1% change in stock return volatility. *Chair/President CEO* equals to one for all CEO-years if the CEO is also president and chairman of the board.

Table 3.2: Average characteristics across different groups (Cont'd)

	Group1 Both	Group2 CEO	Group3 NonCEO	Group4 Neither	P-value (G1-G2≠0)
Dependent variables (*t*+1)					
Invest	0.073	0.050	0.065	0.057	0.000
PP&E growth	0.12	0.032	0.078	0.016	0.000
Asset growth	0.131	0.053	0.087	0.0356	0.000
NetDebt	0.036	0.020	0.030	0.018	0.000
DivPayout (%)	0.71	1.09	1.04	1.52	0.000
Total payout (%)	2.92	3.60	3.45	3.78	0.000
Control variables (*t*)					
Cash flow	0.122	0.081	0.103	0.076	0.000
Log (TA)	7.34	7.62	7.19	7.47	0.000
Log (Sale)	7.27	7.48	7.14	7.40	0.004
ΔLog (Sale)	0.135	0.057	0.1	0.041	0.000
Tobin's Q	2.47	1.84	2.08	1.60	0.000
Leverage	0.208	0.223	0.221	0.252	0.042
Tangibility	0.281	0.259	0.287	0.311	0.016
ΔTangibility	-0.003	-0.003	-0.004	-0.004	0.708
Profitability	0.160	0.124	0.150	0.121	0.000
ΔProfitability	-0.001	-0.003	0.002	-0.001	0.008
SGA/Sale	0.254	0.273	0.250	0.236	0.013
IntanAssets	0.178	0.194	0.176	0.171	0.059
#Business segments	2.33	2.74	2.46	2.71	0.000
Log (PPE/Emp)	3.93	3.94	3.92	4.07	0.930
Annual ret	0.277	0.116	0.218	0.099	0.000
Stock vol	0.443	0.451	0.441	0.406	0.317
Manager characteristics (*t*)					
CEO Stkown	0.021	0.015	0.024	0.012	0.000
Non-CEO avgStkown	0.011	0.006	0.013	0.007	0.000
CEO Tenure	9.6	8.91	7.67	6.17	0.039
Non-CEO avgTenure	4.01	3.37	3.51	2.86	0.000
CEO Delta	921	678	439	314	0.000
Non-CEO avgDelta	175	99.7	122	74.6	0.000
CEO Vega	153	207	125	128	0.000
Non-CEO avgVega	42.9	48.1	35.6	33.2	0.075
CPS	0.391	0.400	0.380	0.384	0.025

Table 3.3: Executive over-optimism and capital expenditure

The table presents the results from regressions of firm capital expenditure on CEO and non-CEO manager team over-optimism. The sample consists of all nonfinancial and nonutility firms from Execucomp for the 1993-2015 period, with available compensation data (item TDC1) of CEOs and other four highest ranked executives. The number of observations varies with the data availability for each variable. An executive is measured as overoptimistic for all years from the first time when CEO holds options that are more than 67% in the money, and at least two times during their sample tenure. A firm who is regarded as the one with overoptimistic non-CEO manager team should have at least two of other four highest ranked executives (half) are measured as over-optimism. Variable definitions are provided in Tables 3.1 and 3.2. All regressions include year and industry fixed effects, defined based on Fama-French 48-industry groupings. In parentheses are *t*-statistics based on standard errors clustered by firms, where *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent variable: CAPX/Total assets at the year (*t*+1)				
	(1)	(2)	(3)	(4)	(5)
Group1_both	0.010***	0.007***	0.006***		
	(4.87)	(3.33)	(2.74)		
Group2_CEO	0.002	0.001	0.001		
	(1.06)	(0.34)	(0.36)		
Group3_NonCEO	0.0045*	0.0032	0.0029		
	(1.71)	(1.20)	(1.09)		
I (Opt_CEO)				0.003*	0.004**
				(1.87)	(2.17)
Opt_NonCEO (residual)					0.004***
					(2.91)
Cash flow	0.126***	0.128***	0.127***	0.128***	0.127***
	(14.32)	(13.57)	(13.37)	(13.49)	(13.37)
Log (TA)	-0.004***	-0.006***	-0.005***	-0.005***	-0.005***
	(-6.68)	(-6.23)	(-5.93)	(-6.03)	(-5.93)
MB	0.004***	0.002**	0.001	0.001	0.001
	(4.41)	(2.24)	(1.22)	(1.33)	(1.23)
CEO stkown	0.035*	-0.049*	-0.032	-0.037	-0.033
	(1.68)	(-1.77)	(-1.15)	(-1.29)	(-1.17)
CEO tenure		-0.002*	-0.003**	-0.003**	-0.003**
		(-1.79)	(-2.44)	(-2.48)	(-2.44)
CEO delta		0.005***	0.004***	0.005***	0.004***
		(4.49)	(3.55)	(3.83)	(3.55)
CEO vega		-0.003***	-0.003***	-0.003***	-0.003***
		(-4.05)	(-3.39)	(-3.72)	(-3.40)
Cumulated return			0.002***	0.002***	0.002***
			(4.42)	(4.75)	(4.47)
Intercept	0.087***	0.089***	0.092***	0.092***	0.092***
	(17.22)	(16.16)	(16.60)	(16.67)	(16.76)
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effect	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.404	0.414	0.417	0.416	0.417
#Obs.	15001	14167	14167	14167	14167
P-value (G1-G2≠0)	0.000	0.000	0.004	-	-

Table 3.4: Executive over-optimism and sensitivity of investment to cash flows

The table presents the estimation results of how the CEO and non-CEO manager team over-optimism impacts the firm investment to cash flow sensitivity. The sample consists of all nonfinancial and nonutility firms from Execucomp for the 1993-2015 period, with available compensation data (item TDC1) of CEOs and other four highest ranked executives. The number of observations varies with the data availability of each variable. Variable definitions are provided in Tables 3.1 and 3.2. All regressions include year and industry fixed effects, defined based on Fama and French 48-industry groupings. In parentheses are *t*-statistics based on standard errors clustered by firms, where *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent variable: CAPX/Total assets at the year (*t*+1)				
	(1)	(2)	(3)	(4)	(5)
Group1_both * Cash flow	0.030*	0.033*	0.031*		
	(1.69)	(1.77)	(1.65)		
Group2_CEO * Cash flow	-0.007	-0.004	-0.003		
	(-0.45)	(-0.23)	(-0.21)		
Group3_NonCEO * Cash flow	0.016	0.020	0.019		
	(0.58)	(0.70)	(0.69)		
Group1_both	0.007***	0.004	0.003		
	(3.04)	(1.55)	(1.12)		
Group2_CEO	0.002	0.001	0.001		
	(1.41)	(0.55)	(0.53)		
Group3_NonCEO	0.003	0.002	0.001		
	(1.10)	(0.59)	(0.47)		
I (Opt_CEO) * Cash flow				0.018	0.019
				(1.21)	(1.25)
I (Opt_CEO)				0.002	0.002
				(0.85)	(1.02)
Opt_NonCEO (residual) * Cash flow					0.030**
					(1.97)
Opt_NonCEO (residual)					0.002
					(0.86)
Cash flow	0.114***	0.113***	0.113***	0.117***	0.115***
	(8.86)	(8.20)	(8.16)	(9.14)	(8.99)
Log (TA)	-0.004***	-0.006***	-0.005***	-0.005***	-0.005***
	(-6.58)	(-6.26)	(-5.96)	(-6.03)	(-5.96)
MB	0.0037***	0.002**	0.0011	0.0014	0.001
	(4.09)	(2.00)	(1.04)	(1.28)	(1.03)
CEO stkown	0.0351*	-0.051*	-0.034	-0.0365	-0.035
	(1.67)	(-1.83)	(-1.21)	(-1.29)	(-1.24)
CEO tenure		-0.002*	-0.003**	-0.003**	-0.003**
		(-1.78)	(-2.42)	(-2.45)	(-2.43)
CEO delta		0.006***	0.004***	0.005***	0.004***
		(4.55)	(3.62)	(3.83)	(3.63)
CEO vega		-0.003***	-0.003***	-0.003***	-0.003***
		(-3.97)	(-3.32)	(-3.68)	(-3.34)
Cumulated return			0.002***	0.002***	0.002***
			(4.35)	(4.71)	(4.41)
Intercept	0.088***	0.090***	0.093***	0.093***	0.093***
	(17.69)	(16.57)	(16.97)	(16.88)	(16.98)
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effect	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.404	0.414	0.417	0.416	0.417
#Obs.	15001	14167	14167	14167	14167
P-value (G1-G2≠0)	0.027	0.032	0.047	-	-

Table 3.5: Executive over-optimism and asset growth

The table presents the results from regressions of firm tangible asset and total asset growth on the CEO and non-CEO manager team over-optimism. The sample consists of all nonfinancial and nonutility firms from Execucomp for the 1993-2015 period, with available compensation data of CEOs and other four highest ranked executives. The number of observations varies with the data availability. Variable definitions are provided in Tables 3.1 and 3.2. All regressions include year and industry fixed effects, defined based on Fama and French 48-industry groupings. In parentheses are *t*-statistics based on standard errors clustered by firms, where *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	PP&E growth: Log(PP&E(t+1)/PP&E(t))				Total asset growth: Log(AT(t+1)/AT(t))			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Group1_both	0.062***	0.036***	0.029***		0.057***	0.030***	0.027***	
	(12.10)	(6.40)	(5.25)		(12.51)	(6.03)	(5.30)	
Group2_CEO	0.013**	-0.000	0.001		0.015***	0.002	0.003	
	(2.25)	(-0.05)	(0.12)		(3.09)	(0.43)	(0.55)	
Group3	0.032***	0.023***	0.020**		0.028***	0.020***	0.019**	
NonCEO	(3.68)	(2.63)	(2.34)		(3.68)	(2.71)	(2.53)	
I (Opt_CEO)				0.018***				0.017***
				(3.68)				(3.93)
Opt_NonCEO				0.026***				0.022***
(residual)				(5.49)				(5.42)
Log (Sale)	-0.010***	-0.028***	-0.025***	-0.025***	-0.011***	-0.033***	-0.031***	-0.031***
	(-6.03)	(-11.62)	(-10.40)	(-10.40)	(-7.48)	(-14.54)	(-13.78)	(-13.78)
SGA/SALE	-0.019	-0.047**	-0.042*	-0.042*	-0.011	-0.038*	-0.036*	-0.036*
	(-0.81)	(-1.99)	(-1.78)	(-1.79)	(-0.54)	(-1.85)	(-1.73)	(-1.73)
Leverage	-0.090***	-0.059***	-0.054***	-0.054***	-0.083***	-0.051***	-0.048***	-0.048***
	(-6.51)	(-4.00)	(-3.71)	(-3.71)	(-6.73)	(-3.90)	(-3.73)	(-3.73)
RD/Sale	0.081*	-0.002	-0.010	-0.011	0.058	-0.024	-0.029	-0.029
	(1.69)	(-0.04)	(-0.22)	(-0.23)	(1.31)	(-0.56)	(-0.65)	(-0.66)
EBIT/AT	0.505***	0.372***	0.338***	0.337***	0.478***	0.329***	0.310***	0.309***
	(15.86)	(10.60)	(9.63)	(9.62)	(16.36)	(10.40)	(9.75)	(9.75)
Intangibility	0.053***	0.036**	0.034**	0.034**	0.009	-0.010	-0.011	-0.011
	(3.56)	(2.32)	(2.20)	(2.20)	(0.67)	(-0.71)	(-0.81)	(-0.81)
Invest	0.534***	0.445***	0.398***	0.398***	0.386***	0.296***	0.270***	0.270***
	(11.29)	(9.93)	(9.07)	(9.08)	(8.74)	(7.19)	(6.66)	(6.67)
CEO stkown		-0.636***	-0.483***	-0.485***		-0.669***	-0.584***	-0.586***
		(-7.49)	(-5.88)	(-5.90)		(-8.56)	(-7.59)	(-7.60)
CEO tenure		-0.013***	-0.017***	-0.017***		-0.016***	-0.018***	-0.018***
		(-4.61)	(-5.83)	(-5.83)		(-6.32)	(-7.11)	(-7.12)
CEO delta		0.044***	0.034***	0.034***		0.048***	0.043***	0.043***
		(12.31)	(9.78)	(9.81)		(14.98)	(13.21)	(13.24)
CEO vega		-0.012***	-0.008***	-0.008***		-0.011***	-0.008***	-0.008***
		(-4.82)	(-3.23)	(-3.25)		(-4.99)	(-3.96)	(-3.98)
CumulatedRet			0.011***	0.011***			0.006***	0.006***
			(9.53)	(9.58)			(6.21)	(6.24)
Intercept	0.083***	0.104***	0.107***	0.110***	0.110***	0.136***	0.138***	0.141***
	(4.56)	(5.20)	(5.42)	(5.60)	(6.83)	(7.78)	(7.95)	(8.14)
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.150	0.165	0.175	0.175	0.152	0.179	0.182	0.182
#Obs.	13144	12150	12150	12150	13156	12162	12162	12162
P-value (G1-G2≠0)	0.000	0.000	0.000	-	0.000	0.000	0.000	-

Table 3.6: Executive over-optimism and net debt issuance

The table presents the results from regressions of firm net debt issuance on the CEO and non-CEO manager team over-optimism. The sample consists of all nonfinancial and nonutility firms from Execucomp for the 1993-2015 period, with available compensation data (item TDC1) of CEOs and other four highest ranked executives. The number of observations varies with the data availability in Compustat. Variable definitions are provided in Tables 3.1 and 3.2. All regressions include year and industry fixed effects, defined based on Fama and French 48-industry groupings. In parentheses are *t*-statistics based on standard errors clustered by firms, where *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent variable: Net Debt Issuance (*t*+1)				
	(1)	(2)	(3)	(4)	(5)
Group1_both	0.013***	0.010***	0.009***		
	(6.21)	(4.03)	(3.57)		
Group2_CEO	0.004*	0.002	0.002		
	(1.76)	(0.61)	(0.62)		
Group3_NonCEO	0.009**	0.006	0.006		
	(2.02)	(1.50)	(1.42)		
I (Opt_CEO)				0.005**	0.006***
				(2.38)	(2.78)
Opt_NonCEO (residual)					0.007***
					(3.23)
ΔTangibility	0.143***	0.140***	0.140***	0.140***	0.140***
	(5.30)	(5.04)	(5.03)	(5.06)	(5.03)
ΔProfitability	-0.035	-0.022	-0.018	-0.020	-0.018
	(-1.34)	(-0.84)	(-0.70)	(-0.79)	(-0.70)
MB	0.006***	0.004**	0.004**	0.004**	0.004**
	(3.23)	(2.45)	(2.21)	(2.21)	(2.21)
ΔLog (Sale)	0.039***	0.036***	0.033***	0.034***	0.033***
	(6.08)	(5.16)	(4.68)	(4.90)	(4.68)
Leverage	-0.026***	-0.031***	-0.030***	-0.030***	-0.030***
	(-3.42)	(-4.19)	(-3.97)	(-4.01)	(-3.97)
CEO stkown		-0.084***	-0.072**	-0.076**	-0.072**
		(-2.62)	(-2.25)	(-2.41)	(-2.26)
CEO tenure		-0.002*	-0.003**	-0.003**	-0.003**
		(-1.81)	(-2.16)	(-2.28)	(-2.16)
CEO delta		0.004***	0.004***	0.004***	0.004***
		(3.88)	(3.08)	(3.63)	(3.10)
CEO vega		-0.002*	-0.001	-0.002*	-0.001
		(-1.81)	(-1.32)	(-1.89)	(-1.33)
Cumulated return			0.001**	0.001***	0.001**
			(2.18)	(2.59)	(2.20)
Intercept	0.019***	0.012**	0.013**	0.013**	0.014**
	(4.21)	(2.02)	(2.28)	(2.31)	(2.44)
Year F.E.	Yes	Yes	Yes	Yes	Yes
Industry F.E.	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.037	0.038	0.038	0.038	0.038
#Obs.	14601	13579	13579	13579	13579
P-value (G1-G2≠0)	0.000	0.001	0.002	-	-

Table 3.7: Executives over-optimism and dividend payout

The table presents the results from regressions of firm payout policies on the CEO and non-CEO manager team over-optimism. The sample consists of all nonfinancial and nonutility firms from Execucomp for the 1993-2015 period, with available compensation data of CEOs and other four highest ranked executives. The dependent variable is the firm's total dividend payment (or total payout amount) scaled by its market capitalization in year *t*+1. The number of observations varies with the data availability in Compustat. Variable definitions are provided in Tables 3.1 and 3.2. All regressions include year and industry fixed effects, defined based on Fama and French 48-industry groupings. In parentheses are *t*-statistics based on standard errors clustered by firms, where *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent variable: Dividend/MarketCap (*t*+1)				Dependent variable: Total payout/MarketCap (*t*+1)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Group1_both	-0.664***	-0.547***	-0.529***		-1.097***	-0.974***	-0.897***	
	(-12.45)	(-10.05)	(-9.76)		(-8.97)	(-7.39)	(-6.82)	
Group2_CEO	-0.346***	-0.284***	-0.285***		-0.448***	-0.501***	-0.504***	
	(-5.65)	(-4.52)	(-4.54)		(-3.33)	(-3.69)	(-3.73)	
Group3 NonCEO	-0.373***	-0.307***	-0.302***		-0.425**	-0.355*	-0.334*	
	(-4.93)	(-4.00)	(-3.95)		(-2.26)	(-1.87)	(-1.77)	
I (Opt_CEO)				-0.413***				-0.733***
				(-8.67)				(-6.61)
Opt_NonCEO (residual)				-0.260***				-0.377***
				(-6.06)				(-3.43)
MB	-0.034**	0.015	0.030*	0.030*	-0.176***	-0.205***	-0.141***	-0.142***
	(-2.45)	(0.88)	(1.74)	(1.76)	(-4.58)	(-4.73)	(-3.18)	(-3.18)
Cash flow	1.336***	1.498***	1.532***	1.530***	6.382***	6.342***	6.485***	6.487***
	(7.05)	(7.83)	(8.02)	(8.02)	(12.92)	(12.74)	(13.17)	(13.17)
Log (Sale)	0.245***	0.298***	0.294***	0.294***	0.514***	0.350***	0.333***	0.332***
	(15.57)	(14.33)	(14.10)	(14.11)	(15.61)	(7.62)	(7.23)	(7.21)
Tangibility	0.201	0.162	0.153	0.153	-1.564***	-1.485***	-1.522***	-1.522***
	(1.40)	(1.10)	(1.04)	(1.04)	(-4.82)	(-4.54)	(-4.67)	(-4.67)
Leverage	-0.297**	-0.298**	-0.309**	-0.309**	-1.392***	-1.404***	-1.449***	-1.449***
	(-2.20)	(-2.13)	(-2.21)	(-2.21)	(-4.56)	(-4.53)	(-4.71)	(-4.71)
CEO stkown	-0.427	2.347***	2.071***	2.058***	-1.533	2.379	1.221	1.234
	(-0.85)	(3.22)	(2.81)	(2.80)	(-1.09)	(1.30)	(0.67)	(0.67)
CEO tenure	0.063***	0.105***	0.116***	0.116***	0.057	0.037	0.084	0.084
	(2.82)	(4.42)	(4.82)	(4.82)	(0.93)	(0.58)	(1.33)	(1.33)
CEO delta		-0.193***	-0.175***	-0.175***		-0.157**	-0.083	-0.083
		(-5.34)	(-4.73)	(-4.73)		(-2.09)	(-1.07)	(-1.08)
CEO vega		0.086***	0.077***	0.077***		0.443***	0.405***	0.406***
		(3.27)	(2.89)	(2.87)		(8.54)	(7.77)	(7.78)
CumulativeRet			-0.028***	-0.028***			-0.117***	-0.118***
			(-4.20)	(-4.15)			(-6.21)	(-6.22)
Intercept	-0.130	-0.109	-0.143	-0.188	-0.015	0.428	0.287	0.240
	(-0.94)	(-0.79)	(-1.03)	(-1.38)	(-0.04)	(1.27)	(0.84)	(0.71)
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.258	0.271	0.272	0.272	0.160	0.167	0.170	0.170
#Obs.	14952	14124	14124	14124	14976	14147	14147	14147
P-value (G1-G2≠0)	0.000	0.000	0.000	-	0.000	0.000	0.003	-

Table 3.8: Robustness test: Further controlling for corporate governance effect

The table presents the results from regressions of firm investment, financing and payout policy on the CEO and non-CEO manager team over-optimism, when additionally controlling for the proxies of corporate governance: board size and independent board ratio (*Board Indpt*). Other control variables, including determinants of outcome variables, manager tenure, compensation incentives, and firm past performance, are also included but not reported in the table. The sample consists of all nonfinancial and nonutility firms from Execucomp for the 1993-2015 period, with available compensation data (item TDC1) of CEOs and other four highest ranked executives. In column (2), we report the coefficients of *Group1_both*, *Group2_CEO*, and *Group3_NonCEO* interacted with cash flows. The number of observations varies with the data availability. Variable definitions are provided in Table 3.1 and 3.2. All regressions include year and industry fixed effects, defined based on Fama-French 48-industry groupings. In parentheses are *t*-statistics based on standard errors clustered by firms, where *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	CAPX (1)	CAPX-CF (2)	PP&E growth (3)	Total asset growth (4)	Net debt issuance (5)	Total dividend (6)	Total payout (7)
Group1_both	0.004	0.049**	0.030***	0.029***	0.010***	-0.548***	-0.896***
	(1.43)	(1.97)	(4.74)	(5.12)	(3.50)	(-8.39)	(-5.78)
Group2_CEO	0.001	0.004	0.007	0.009	0.005	-0.323***	-0.574***
	(0.32)	(0.15)	(1.06)	(1.55)	(1.60)	(-4.29)	(-3.59)
Group3_NonCEO	0.001	0.057	0.008	0.009	0.005	-0.414***	-0.304
	(0.29)	(1.52)	(0.89)	(1.15)	(1.06)	(-4.56)	(-1.31)
Board size	-0.002	-0.003	-0.016*	-0.006	0.004	0.577***	0.451**
	(-0.68)	(-0.70)	(-1.73)	(-0.68)	(1.02)	(5.57)	(2.04)
Board Indpt	-0.000	-0.000	-0.016	0.009	0.018**	0.446***	1.001***
	(-0.05)	(-0.03)	(-0.97)	(0.62)	(2.29)	(2.73)	(2.58)
Intercept	0.094***	0.096***	0.087***	0.080***	0.011	-1.647***	-0.464
	(10.35)	(10.40)	(3.08)	(3.43)	(1.04)	(-6.36)	(-0.76)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.432	0.433	0.142	0.149	0.040	0.283	0.173
#Obs.	9601	9601	8498	8504	9205	9584	9600
P-value (G1-G2≠0)	0.140	0.030	0.001	0.000	0.051	0.000	0.029

Table 3.9: Robustness test: Further controlling for the CEO power

The table presents the results from regressions of firm investment, financing and payout policy on the CEO and non-CEO manager team over-optimism, when additionally controlling for CEO power: CEO pay slice (*CPS*) and an indicator variable which equals one if CEO is the chairman of the board, and zero otherwise. Other control variables, including determinants of outcome variables, manager tenure, compensation incentives, and firm past performance, are also included but not reported in the table. The sample consists of all nonfinancial and nonutility firms from Execucomp for the 1993-2015 period, with available compensation data (item TDC1) of CEOs and other four highest ranked executives. In column (2), we report the coefficients of *Group1_both*, *Group2_CEO*, and *Group3_NonCEO* interacted with cash flows. The number of observations varies with the data availability. Variable definitions are provided in Table 3.1 and 3.2. All regressions include year and industry fixed effects, defined based on Fama-French 48-industry groupings. In parentheses are *t*-statistics based on standard errors clustered by firms, where *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	CAPX (1)	CAPX-CF (2)	PP&E growth (3)	Total asset growth (4)	Net debt issuance (5)	Total dividend (6)	Total payout (7)
Group1_both	0.005**	0.042*	0.029***	0.027***	0.009***	-0.550***	-0.887***
	(1.99)	(1.79)	(4.86)	(4.96)	(3.30)	(-8.33)	(-5.65)
Group2_CEO	0.001	0.014	0.006	0.005	0.003	-0.292***	-0.543***
	(0.68)	(0.57)	(0.85)	(0.83)	(1.16)	(-3.89)	(-3.38)
Group3_NonCEO	0.001	0.035	0.015	0.011	0.002	-0.361***	-0.360
	(0.40)	(0.99)	(1.59)	(1.29)	(0.42)	(-4.10)	(-1.57)
CPS	-0.010	-0.009	0.028	0.028	0.026**	0.419**	0.243
	(-1.56)	(-1.47)	(1.24)	(1.42)	(2.42)	(2.26)	(0.47)
I (CEO_chairman)	-0.002	-0.002	-0.002	0.001	0.000	0.086*	0.150
	(-1.23)	(-1.24)	(-0.45)	(0.26)	(0.17)	(1.79)	(1.18)
Intercept	0.093***	0.095***	0.040*	0.071***	0.025***	-0.655***	0.419
	(13.38)	(13.77)	(1.82)	(3.51)	(2.96)	(-3.57)	(0.95)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.440	0.441	0.150	0.153	0.040	0.267	0.174
#Obs.	10104	10104	8899	8911	9696	10079	10097
P-value (G1-G2≠0)	0.071	0.161	0.000	0.000	0.028	0.000	0.024

Table 3.10: Robustness test: Restricting to a subsample where managers stay in firms at least 3 years

The table presents the results from subsample regressions of firm investment, financing and payout policy on the CEO and non-CEO manager team over-optimism, where CEOs should stay at firm at least 3 years, and the average tenure of other non-CEO managers should be at least three years. Other control variables, including determinants of outcome variables, manager tenure, compensation incentives, and firm past performance, are also included but not reported in the table. The sample consists of nonfinancial and nonutility firms from Execucomp for the 1993-2015 period, with available compensation data (item TDC1) of CEOs and other four highest ranked executives. The number of observations varies with the data availability. In column (2), we report the coefficients of *Group1_Both*, *Group2_CEO*, and *Group3_NonCEO* interacted with cash flows. Variable definitions are provided in Table 3.1 and 3.2. All regressions include year and industry fixed effects, defined based on Fama-French 48-industry groupings. In parentheses are *t*-statistics based on standard errors clustered by firms, where *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	CAPX (1)	CAPX-CF (2)	PP&E growth (3)	Total asset growth (4)	Net debt issuance (5)	Dividend (6)	Total payout (7)
Group1_both	0.006**	0.038	0.018***	0.021***	0.007**	-0.655***	-1.145***
	(2.13)	(1.30)	(2.70)	(3.27)	(2.19)	(-8.68)	(-6.42)
Group2_CEO	0.002	0.003	-0.008	0.003	0.001	-0.371***	-0.631***
	(0.66)	(0.12)	(-1.10)	(0.45)	(0.36)	(-4.33)	(-3.33)
Group3_NonCEO	0.005	0.032	0.015	0.009	0.009	-0.441***	-0.416
	(1.21)	(0.74)	(1.24)	(0.87)	(1.36)	(-3.93)	(-1.49)
Intercept	0.111***	0.113***	0.128***	0.150***	0.040***	- 0.254	1.242**
	(11.99)	(12.30)	(4.46)	(5.54)	(3.32)	(-1.28)	(2.21)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.438	0.439	0.155	0.149	0.034	0.259	0.164
#Obs.	8101	8101	7102	7108	7758	8059	8072
P-value (G1-G2≠0)	0.039	0.118	0.000	0.002	0.044	0.000	0.002

Table 3.11: Executive over-optimism and firm value

This table presents the results from regressions of Tobin's Q on an instrument for firm growth opportunities and on CEO and non-CEO manager team over-optimism. Industry PE is the proxy for growth opportunities is calculated as the average monthly industry PE over the fiscal year. The monthly industry PE is calculated as the log transformation of the industry's total market capitalization to total earnings less a 60-month moving average. The sample consists of all nonfinancial and nonutility firms from Execucomp for the 1993-2015 period, with available compensation data (item TDC1) of CEOs and other four highest ranked executives. Variable definitions are provided in Tables 3.1 and 3.2. All regressions include year and industry fixed effects. In parentheses are *t*-statistics based on standard errors clustered by firms, where *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Dependent variable: Tobin's Q (*t*+1)			
	(1)	(2)	(3)	(4)
Industry PE	0.143***	0.0308	0.0267	0.0188
	(5.99)	(1.10)	(0.99)	(0.70)
Group1_both * Industry PE		0.214***		
		(4.39)		
Group2_CEO * Industry PE		0.0462		
		(1.11)		
Group3_NonCEO * Industry PE		-0.0270		
		(-0.34)		
Group1_both		0.448***		
		(11.53)		
Group2_CEO		0.086**		
		(2.11)		
Group3_NonCEO		0.203***		
		(3.67)		
I (Opt_CEO) * Industry PE			0.178***	0.175***
			(4.46)	(4.41)
Opt_NonCEO (residual)* Industry PE				0.105**
				(2.37)
I (Opt_CEO)			0.299***	0.317***
			(8.92)	(9.45)
Opt_NonCEO (residual)				0.314***
				(9.22)
Log (Sale)	-0.089***	-0.083***	-0.089***	-0.082***
	(-6.35)	(-6.02)	(-6.44)	(-6.01)
Log (PPE/Emp)	-0.021	-0.025	-0.024	-0.025
	(-0.99)	(-1.19)	(-1.15)	(-1.17)
Stock return	0.041***	0.033***	0.037***	0.033***
	(5.41)	(5.15)	(5.30)	(5.14)
Stock return * Industry PE	-0.021**	-0.020**	-0.021**	-0.019**
	(-2.13)	(-2.14)	(-2.18)	(-2.09)
ROA	4.155***	3.740***	3.962***	3.740***
	(11.51)	(10.49)	(11.03)	(10.47)
# Business segments	-0.058***	-0.052***	-0.055***	-0.052***
	(-5.18)	(-4.66)	(-4.89)	(-4.66)
Intercept	1.977***	1.870***	1.916***	1.899***
	(14.57)	(13.88)	(14.29)	(14.14)
Year F.E.	Yes	Yes	Yes	Yes
Industry F.E.	Yes	Yes	Yes	Yes
Adj. R^2	0.317	0.341	0.330	0.341
#Obs.	12451	12451	12451	12451