THREE ESSAYS ON INFORMATION INTERMEDIARIES IN FINANCIAL MARKETS

HAMDI DRISS

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ABSTRACT

This dissertation examines novel issues related to information intermediaries in financial markets and consists of three essays on the following three distinct topics: (1) the monitoring role of credit rating agencies, (2) heterogeneity in the influence of sell-side analyst recommendation changes, and (3) the information flow from sell-side analysts to their affiliated asset managers.

In the first essay (Chapter Two), I focus on a sample of Moody's issuer-level credit watch actions on U.S. nonfinancial public borrowers between 1992 and 2011 and study the relations between credit watch resolution outcomes and the dynamics of corporate fundamentals. Using firms with watch-preceded rating downgrades (control firms) as a benchmark, I document that firms with watch-preceded rating confirmations (treatment firms) experience an increase in long-term financing, investment, and profitability immediately following the credit watch period. These patterns are stronger for treatment firms of lower credit quality, for which external monitoring is more valuable. I further show that financially constrained treatment firms substantially increase their long-term financing immediately following the watch period, indicating that agency's monitoring can help alleviate capital constraints resulting from information asymmetry. Additional tests show that treatment firms are more profitable regardless of corporate governance, implying that agency's monitoring is independent of corporate governance. These findings suggest that credit agencies have real effects on corporate behavior, and that a credit watch with direction downgrade acts as an effective monitoring mechanism.

The second essay (Chapter Three) presents empirical and theoretical evidence that heterogeneity in the influence of analysts is consistent with ambiguity aversion in the equity

market. Specifically, I show that investors are much more responsive to analyst recommendation changes when they perceive higher levels of "ambiguity in the firm environment", or difficulty in processing and acting on firm incomplete information. Ambiguity-averse investors act as if they underweight prior signals that imply fundamental uncertainty, underweight firm signals of uncertain quality, and overweight analyst signals. My findings are independent of other relevant factors, such as recommendation deviation from consensus or stock price momentum, and persistent irrespective of the direction of recommendation changes and the choice of empirical proxies for ambiguity. This essay contributes to the analyst literature by providing a behavioral explanation for puzzling heterogeneity in the influence of recommendation changes.

The third essay (Chapter Four) extends research on informed trading prior to recommendations from sell-side analysts. My analysis of a sample of downgrades indicates that some affiliated asset managers may receive tips from their brokerage analysts and engage in selling of the soon to be downgraded stocks. This evidence is limited to the period prior to the Global Research Analyst Settlement and stronger for those downgrades that are from the sanctioned brokerage firms or issued by affiliated analysts. Further analysis focuses on downgrades from the sanctioned firms and shows that informed selling is related to the downgrade characteristics that would imply early knowledge of forthcoming downgrades. Informed selling is relatively more prevalent prior to strong downgrades, downgrades of high magnitude, downgrades that are issued before Regulation Fair Disclosure came into effect, or downgrades that are issued on high institutional ownership stocks. Overall, my results support the tipping hypothesis.

DEDICATION

This dissertation is dedicated to my mother Baya Triki, father Tahar Driss, and wife Hana Ben Arab for their unconditional love and support.

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CHAPTER ONE: MOTIVATION AND OVERVIEW

1.1. Motivation

Information intermediaries, such as credit rating agencies and sell-side analysts, play the important role of collecting and analyzing information and conveying expert opinions to the market, helping alleviate the information asymmetry that typically exists between investors and firms. Acting as information equalizers, these intermediaries can help expand the supply of capital and lower the informational cost of capital that firms would otherwise have to pay. This dissertation goes beyond this traditional view on information intermediation and empirically investigates novel issues, which have important implications for our understanding of the economic functions of information intermediaries in financial markets. Motivated by anecdotal and theoretical evidence, this dissertation focuses on the following three distinct topics: (1) the monitoring role of credit rating agencies, (2) heterogeneity in the influence of sell-side analyst recommendation changes, and (3) the information flow from sell-side analysts to their affiliated asset managers.

The first topic is motivated by the credit rating model of Boot et al. (2006). In their model, a credit watch with direction downgrade has real effects on a firm's choice between a risky and a safe investment project. They demonstrate that if a sizeable proportion of investors condition their decisions on the ratings of a rating agency, then the agency can discipline the firm through the credit watch period, inducing first-best project choice. Based on the insights of this model, I empirically investigate whether a third-party rating agency can effectively monitor rated borrowers through the mechanism of a credit watch with direction downgrade.

As an effective third-party monitor, the agency can help mitigate moral hazard faced by investors and better alleviate information asymmetry in financial markets.

The second topic is motivated by anecdotal evidence suggesting that ambiguity in the firm environment may influence the way investors interpret and respond to information signaled by sell-side analysts. Empirical evidence in this respect contributes to our understanding of the phenomenon of heterogeneity in investors' responses to the announcements of recommendation changes (Loh and Stulz, 2011). By investigating the relation between ambiguity in the firm environment and the influence of recommendation changes, my objective is to provide a behavioral explanation for puzzling heterogeneity in the influence of recommendation changes.

The third topic is motivated by anecdotal evidence suggesting that sell-side analysts may tip off their affiliated asset managers regarding the content of forthcoming research reports. Empirical evidence in this direction highlights the ethical issues that may arise as a result of the interdependence among the divisions of a full-service brokerage firm. In this respect, I ask the following question: how did recent regulatory reforms (collectively known as the Global Research Analyst Settlement) affect the interdependence between brokerage analysts and their affiliated asset managers?

1.2. Overview

This dissertation is organized in five chapters including the introduction (Chapter One). Chapter Two investigates whether a third-party rating agency can effectively monitor rated borrowers through the mechanism of a credit watch with direction downgrade. Chapter Three employs ambiguity aversion as a behavioral explanation for puzzling heterogeneity in the influence of sell-side analyst recommendation changes. Chapter Four investigates whether sell-side analysts tip off their affiliated asset managers prior to their downgrade announcements. Chapter Five concludes this dissertation. The main chapters, Chapter Two, Three, and Four, are self-explanatory and independent from each other. Thus, there is no need to read Chapter Two first in order to understand Chapter Three and so on. In what follows, I provide a brief overview of the findings of each of these chapters.

In Chapter Two, I investigate whether credit rating agencies act as third-party monitors through the mechanism of a credit watch with direction downgrade. Two main channels are in play in this respect. First, by exerting informal influence the agencies can induce firms to undertake safe projects, helping improve their profitability. Second, when the agencies reveal the true credit quality of firms following the monitoring episode, they can help reduce information asymmetry and thus facilitate access to capital markets. Accordingly, firms increase their long-term financing and ramp up their investment activities. Based on a difference-in-differences panel regression framework, I find that, relative to firms with watch-preceded rating downgrades (control firms), firms with watch-preceded rating confirmations (treatment firms) tend to increase their long-term financing, employ more investment capital, and experience an improvement in profitability beginning immediately following the credit watch period. These patterns are stronger for treatment firms of lower credit quality, for which third-party monitoring is more valuable. I conduct two additional tests that support the information asymmetry and informal influence channels. First, I find that financially constrained treatment firms substantially increase their long-term financing at the expense of short-term financing immediately following the watch period, indicating that these firms attempt to benefit from reduction in information asymmetry and the resulting better access to capital markets. Second, I show that treatment firms are more profitable regardless of corporate governance, implying that agencies' informal influence is not an artifact of strong corporate governance.

Chapter Three presents and empirically evaluates a theory of ambiguity that predicts stronger influence of recommendation changes issued on firms with more ambiguous environments. I focus on two aspects of ambiguity in the firm environment: the ambiguity aspect that emerges from lack of knowledge of nonexistent relevant information and that manifests itself through the difficulty investors face in formulating prior beliefs about firm fundamentals (ambiguity in fundamentals or AIF); and the ambiguity aspect that pertains to the difficulty investors face in updating their prior beliefs in response to firm information of uncertain quality, (ambiguity in information or AII). I provide evidence that both aspects of ambiguity substantially increase the influence of recommendation changes (the "mean" effect). Moreover, I show that the influence of recommendation changes issued on higher AII firms substantially increases when their fundamentals are perceived to be more ambiguous (the "interaction" effect). One of the striking findings of this chapter is that recommendation changes issued on highest AIF, highest AII firms are about *four* times as influential as those issued on lowest AIF, lowest AII firms.

Chapter Four investigates whether affiliated asset managers engage in informed selling prior to downgrade releases from their brokerage analysts. I find that, in the period prior to the Global Settlement and related regulations, the selling activity of affiliated asset managers is associated with significantly negative pre-announcement abnormal returns and abnormally high share turnovers, implying pre-downgrade informed selling. Because these regulations include provisions that would limit the information flow from sell-side analysts to their affiliated asset managers, I can conclude that some of the selling activity is likely to be induced by tips received from those analysts. Focusing on downgrades issued by the sanctioned firms before the Global Settlement, I show that pre-downgrade informed selling of affiliated asset managers is related to the downgrade characteristics that would imply early knowledge of the soon to be released downgrades. Specifically, I find that informed selling is relatively more prevalent prior to strong downgrades, downgrades of high magnitude, downgrades that are issued before Regulation Fair Disclosure, or downgrades that are issued on high institutional ownership stocks. This provides further evidence that some affiliated asset managers may have received tips from their brokerage analysts before downgrade releases and engaged in selling of the soon to be downgraded stocks.

CHAPTER TWO: DO RATING AGENCIES ACT AS THIRD-PARTY MONITORS? EVIDENCE FROM MOODY'S CREDIT WATCHES

2.1. Introduction

In a well-publicized move, on July 13, 2011, Moody's Investors Service (Moody's) placed the Aaa bond rating of the U.S. government on watch for possible downgrade given the rising possibility that the statutory debt limit would not be raised on a timely basis, potentially leading to a default on US Treasury debt obligations. On the same day, Steven Hess, a senior credit officer at Moody's, said in a Bloomberg interview: "What we're looking for is a raising of the limit. It doesn't matter the process that they get there", a statement by which Moody's explicitly specifies the action politicians should take to avoid a rating downgrade on the U.S. debt, and attempts to influence the Congress to take that action. On August 02, 2011, Moody's confirmed the Aaa rating and said: "The initial increase of the debt limit by $900 billion and the commitment to raise it by a further $1.2-1.5 trillion by year end have virtually eliminated the risk of such a default, prompting the confirmation of the rating at Aaa."[1]

Evidence on the informal (behind-the-scenes) influence of a credit rating agency (CRA) extends to the universe of corporations, from both anecdotal and theoretical perspectives. In a survey paper, Graham and Harvey (2001) document that credit ratings are one of the most important factors in corporate financing decisions according to the surveyed CFOs, indicating that firms are concerned about their credit ratings.[2] In an influential

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[1] The above example is from the following article: "Moody's Places U.S. on Review for Downgrade As Debt Talks Stall", by John Detrixhe, Bloomberg News, July 13, 2011. See also www.moodys.com.

[2] The other most important factor affecting debt policy is financial flexibility.

theoretical paper, Boot et al. (2006) present a model in which a credit watch with direction downgrade has real effects on a firm's choice between a risky and a safe investment project. They demonstrate that if a sizeable proportion of investors condition their decisions on the ratings of a CRA, then the CRA can discipline the firm through the credit watch period, inducing first-best project choice.[3] Hence, according to Boot et al. the watch procedure with direction downgrade is much more than a simple auditing mechanism, it allows the agency to discipline (monitor) borrowers when their credit quality is at risk.

In light of the anecdotal and theoretical evidence outlined above, the main empirical question I am interested in is the following: can a third-party rating agency effectively monitor rated borrowers through the mechanism of a credit watch with direction downgrade? To answer this question I analyze the relations between watch resolution decisions and the dynamics of firms' profitability as well as their financing and investment policies. The premise of my empirical strategy is that effective third-party monitoring helps improve corporate profitability through an informal influence channel, and allows firms to have better access to capital markets via reduction in information asymmetry.

To establish the rationale for the relation between third-party monitoring and corporate profitability through an informal influence channel, consider the following. If a CRA initiates a credit watch with direction downgrade on a given firm, then the CRA could induce the firm to put in turnaround effort through the watch period, and gain information on whether the effort was successful in addressing the concerns that led to the watch action. The CRA can then condition its watch resolution decision on this information by either confirming or downgrading the rating under watch, and better anticipate future changes in the firm's profitability. Thus, effective third-party monitoring through a credit watch should result in

[3] The credit watch period is the period from the watch assignment to the watch resolution announcement.

predictable relations between watch resolution outcomes and corporate profitability. I can expect the profitability of firms with watch-preceded rating confirmations (treatment firms) to substantially improve following the watch period when benchmarked against firms with watch-preceded rating downgrades (control firms).

Effective third-party monitoring can also help reduce information asymmetry in capital markets. Treatment firms are set to benefit from a decrease in information asymmetry when the agency reveals their true credit quality following the monitoring episode. As a result, these firms would have better access to capital markets, in which case they would alter their financing mix to rely more on long-term financing and less on short-term financing. Provided that these firms have profitable investment opportunities, they would employ the additional long-term funds to ramp up their investment activities.

In sum, effective third-party monitoring should result in better corporate profitability through an informal influence channel, as well as higher long-term financing and investment through an information asymmetry channel. I can also expect these effects to be stronger among treatment firms of lower credit quality, for which third-party monitoring is more valuable.

To test these ideas, I obtain a sample of 509 rating confirmations and 1,381 rating downgrades, for a total of 1,890 issuer-level watch assignments with direction downgrade reported by Moody's for the universe of U.S. nonfinancial public firms between 1992 and 2011. Watch assignments can be issued with direction downgrade, uncertain, or upgrade. However, in this chapter I exclusively focus on watch assignments with direction downgrade because third-party monitoring is more likely to be effective when firms are being pressured to take specific actions to avoid being downgraded. For the rest of the chapter, I use the terminology "watch" to mean "credit watch with direction downgrade" for brevity of presentation.

My primary objective is to test two main hypotheses, which are as follows. (1) *The mean effects of treatment*: relative to control firms, treatment firms increase their long-term financing, invest more, and are more profitable beginning immediately after the watch period, and (2) *The interaction effects of treatment*: among treatment firms, the positive changes in long-term financing, investment, and profitability are more pronounced for firms of lower credit quality.

In the empirical tests, I first employ a difference-in-differences panel regression framework to analyze the dynamic pattern of corporate fundamentals from four quarters before to four quarters after the watch period. I provide evidence consistent with my first hypothesis and document a substantial increase in the long-term financing, investment, and profitability of treatment firms when benchmarked against control firms beginning immediately following the watch period. In the first quarter following the watch period, treatment firms increase their long-term financing by 3.14% as a percentage of average assets, and by four quarters post-watch they have raised their long-term financing by 3.54%, which translates to a nearly 100% increase above its pre-watch level of 3.3%. Treatment firms also experience a sizeable increase in capital expenditures as a percentage of average assets. In the fourth quarter post-watch, the increase amounts to 1.33%, which is equivalent to a roughly 22% increase above its pre-watch level of 6.1%. Meanwhile, profitability of treatment firms, as measured by operating income scaled by average assets, rises by about 1.2% immediately after the watch period, and continues on a positive trend until the fourth quarter post-watch, in which quarter the increase roughly amounts to 2%. Relative to its pre-watch level of 13.2%, the 2% increase in the operating income ratio translates into a nearly 15% improvement in profitability. Consistent with my second hypothesis, I further show that treatment firms of lower credit quality experience a more pronounced increase in long-term financing, investment, and profitability. Using a triple difference framework, I find that non-investment grade treatment firms increase their long-term financing by an incremental 6.4%, spend an additional capital equivalent to 1.35% of average assets, and are more profitable based on either return on assets or return on equity.

I conduct two additional tests that provide support for the information asymmetry and informal influence channels resulting from third-party monitoring. First, I condition upon corporate financial constraints, as proxied by the cash flow-investment gap as in Rajan and Zingales (1998), and study the dynamic pattern of corporate fundamentals around the watch period. I find that following the watch period financially constrained treatment firms behave differently from unconstrained firms. Constrained treatment firms display greater increases in their long-term financing at the expense of short-term financing, indicating that these firms have propensity to alter their financing mix to benefit from reduction in information asymmetry. Second, I condition upon corporate governance, as measured by the G Index of Gompers, Ishii, and Metrick (2003), to check whether the positive effect of treatment on corporate profitability is in fact an artifact of stronger corporate governance. I find that this effect holds regardless of corporate governance, implying that third-party monitoring could be a substitute for corporate governance. I also find weaker evidence that the positive effect of treatment on profitability is more pronounced among better governance firms.[4] This indicates that third-party monitoring is more effective among firms with stronger governance, perhaps because these firms are more inclined to embrace the implicit contract a la Boot et al. (2006).

[4] This evidence lacks statistical significance.

To check the robustness of the results, I conduct three robustness tests. First, I test the parallel trends assumption underlying the difference-in-differences framework, and provide evidence that the results are not driven by long-term trends in corporate fundamentals. The positive post-watch changes in long-term financing, investment, and profitability do not obtain if I falsely assume that treatment occurs two years before it actually does. Second, I demonstrate that the results are robust to different empirical methodologies. Using a propensity score matching algorithm, I match treatment firms to control firms based on relevant factors including the standard determinants of corporate policies, and show that the prior conclusions remain valid. Third, I address potential endogeneity in watch resolution decisions. In my sample, although treatment and control firms are comparable along a number of relevant dimensions prior to the watch period, treatment firms tend to be smaller, have better growth profiles, and are more profitable and less financially constrained, implying a non-random matching between watch resolution decisions and firms. To control for potential endogeneity in watch resolution decisions, I utilize a switching regression model with endogenous switching, an empirical design that makes it possible to hold the firm profile fixed and to infer the net effects due to watch resolution decisions. That is, I compare the actual changes in fundamentals measures for treatment firms with the predicted changes in those measures that would obtain had these firms been control firms instead. The findings point to significant increases in long-term financing, investment, and profitability for treatment firms when compared to their counterfactual peers, implying that the prior conclusions are robust to endogeneity concerns.

It is also important to discuss the incentives faced by CRAs to monitor firms. An intermediary must have an incentive to monitor a borrower. As noted by Holmstrom and

Tirole (1997), an intermediary must hold a financial stake in a firm's project to be a credible monitor. Of course, the agencies do not hold the debt of the firms they rate. So why are CRAs willing to monitor the firms they rate? My answer draws upon the literature on delegated monitoring. In particular, Strausz (1997) presents a principal-agent model in which the principal can monitor the agent's action or delegate monitoring to a third-party supervisor, and demonstrates that in equilibrium the principal gains by delegating monitoring to the supervisor. This is because under direct monitoring the principal has inadequate incentives to monitor on his own and faces a lack of commitment problem, which would lead to a lower payoff.[5] In their model, the principal offers a contract to the supervisor which induces him to monitor.

In the context of this chapter, the model implies that it is optimal for bondholders to delegate monitoring of borrowers to a third-party rating agency.[6] However, since the agency is usually not paid by the bondholders but by the borrowers soliciting rating services, how can bondholders induce the agency to enforce monitoring? The answer I propose relates to the general interest of bondholders in the agency's ratings, which is from the agency's perspective the reputational capital at stake. If more and more investors condition their decisions on the agency's ratings, then the agency will enjoy better reputation, and inevitably more and more borrowers will solicit the rating services of the agency, generating additional revenues. If the agency, however, turns out to be an ineffective monitor, then bondholders would show little interest in the agency's ratings, harming its reputation, future income, and

[5] In the literature review section, I provide a more detailed discussion on these issues.

[6] Other arguments in favor of delegation of monitoring are motivated by the efficiency gains resulting from delegation. For example, diversified lenders have limited ability to monitor the firms they lend to, and thus face incentives to use the services of a specialized monitor. Moreover, delegation of monitoring also allows lenders to avoid duplication of monitoring effort. These arguments are discussed in several papers, such as Ramakrishnan and Thakor (1984) and Millon and Thakor (1985).

potentially its survival. Thus, the need to protect reputational capital induces the agency to solve its incentive problems and fulfill its role as a third-party monitor.[7]

The main contribution of this chapter is to provide novel empirical evidence on third-party rating agency monitoring through the mechanism of credit watches. This chapter is related to an emerging strand of literature examining the implications of agencies' actions for corporate financing and investment behavior. Examples of papers in this literature are Sufi (2009) who argues that agencies' debt certification has real effects on firm financial and investment policies, and Tang (2009) who demonstrates that agencies can help alleviate information asymmetry in credit markets. Thus, this chapter builds on this literature and is the first to document empirical evidence on the monitoring function of the agencies.[8]

The remainder of this chapter is organized as follows. Section 2.2 reviews the related literature and develops two main hypotheses. Section 2.3 describes the sample formation process and presents descriptive statistics. Section 2.4 lays the foundations for the empirical tests. Section 2.5 presents the main results. Section 2.6 conducts robustness tests, and Section 2.7 concludes the chapter.

2.2. Related literature and hypotheses

2.2.1. Literature review

One important role of CRAs emerges from the information asymmetry that typically exists between lenders and borrowers. Acting as information equalizers, the agencies can help expand the supply of capital and lower the informational cost of capital that borrowers would

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[7] Since the issuer-pay model is standard, it seems unreasonable that CRAs receive fees from the issuers they are supposed to control. My view is that issuers seeking to access the public credit market have no choice but to solicit the rating services of CRAs and pay the associated fees. Faulkender and Petersen (2006) provide evidence in this respect.

[8] In the literature review section, I provide detailed discussion on how this chapter relates to prior literature.

otherwise have to pay. The empirical evidence on the effectiveness of CRAs in achieving this role is somewhat mixed. Early studies, such as Liu and Thakor (1984) and Ederington, Yawitz, and Roberts (1987), demonstrate that credit ratings have explanatory power in the cross-section of yield spreads. More recently, academic researchers have mostly been skeptical about the incremental information value of credit ratings. In general, the literature shows that credit rating upgrades do not have material announcement effects in both equity and bond markets. In contrast, the market reaction to rating downgrades is generally statistically significant, but also economically weak. Hand, Holthausen, and Leftwich (1992), Goh and Ederington (1993), and Dichev and Piotroski (2001) are examples of papers in this literature.[9] This chapter builds on this literature and investigates the idea that agencies can help mitigate information asymmetry in capital markets through the mechanism of a credit watch with direction downgrade.

This chapter also belongs to an emerging strand of literature, which advocates for feedback effects of CRAs. Credit actions are not just a response to changes in firm characteristics, but they could themselves have real effects on firms' behavior. Empirical studies within this literature primarily focus on corporate capital structure and investment decisions. For example, Sufi (2009) shows that the introduction of syndicated bank loan ratings induced firms to use more debt and increase their investment, indicating that third-party debt certification has real effects on firm financial and investment policies. Tang (2009) investigates the effects of the introduction of Moody's 1982 credit rating refinement on the rated firms. He demonstrates that firms with higher refined ratings (e.g., a rating refinement from A to A1 as opposed to A3 rating) enjoy better access to credit markets and have more capital investments, indicating that rating agencies can help alleviate firms' capital constraints

[9] See Cantor (2004) for a summary of this literature.

resulting from information asymmetry in credit markets. Faulkender and Petersen (2006) show that firms with access to the public bond markets, as measured by having a bond rating, choose higher levels of debt financing. Kisgen (2006) provides evidence that firms adjust their capital structure to maintain a particular bond rating, indicating that credit ratings can influence capital structure decisions.

Among theoretical papers within this literature, the most relevant paper to this chapter is Boot et al. (2006), who present a model in which a CRA can employ a credit watch procedure with direction downgrade to discipline a firm, helping to mitigate moral hazard faced by investors. In their model, the firm can choose between a risky and a safe project. After receiving a bad signal on the credit quality of the safe project, the CRA could initiate a credit watch procedure with direction downgrade, in which case a monitoring regime is put in place. In their language, the monitoring regime is equivalent to an implicit contract under which the firm is asked to undertake specific turnaround effort to avoid a rating downgrade. After observing the firm’s turnaround effort, the agency determines whether it was successful and then resolves the watch action accordingly. Boot et al. show that if a sufficient number of investors condition their decisions on the rating, then the agency can induce the firm to put in turnaround effort and choose the safe project.

Empirical research on the monitoring role of CRAs is scarce. One study that attempts to investigate the issue is Bannier and Hirsch (2010), who argue that the watch procedure has a dual role. For high credit quality borrowers, the agencies use the watch procedure mainly to deliver information to investors. In contrast, for low credit quality borrowers the agencies employ the watch procedure primarily for monitoring purposes. Bannier and Hirsch base their conclusions on the market response to the agencies’ announcements, implicitly assuming that the market response truly reflects agencies' monitoring effort. Unlike this paper, my main analysis does not rely on investors' perception of the agencies' announcements because this perception could be confounded by investors' anticipation or overreaction/underreaction effects, in which cases conclusions may be misleading. In this chapter, I adopt a more direct approach and employ an empirical framework that analyzes changes in corporate fundamentals, which likely better capture the effects of monitoring. Another related paper is Bannier et al. (2012), who attempt to investigate whether CRAs affect firm investment decisions. They find that firms reduce (increase) their investment around rating downgrades (upgrades), and interpret this result as being consistent with the monitoring role of CRAs. My chapter is different along several dimensions. First, my sample consists of watch actions with direction downgrade while in their main analysis they employ direct rating changes. Second, I study corporate profitability, financing, and investment. In contrast, they focus on corporate investment only. Third, I control for endogeneity in watch resolution decisions which they do not.

At a broader level, this chapter is also related to the literature on delegated monitoring. In particular, Strausz (1997) presents a principal-agent model in which the principal can monitor the agent's action or delegate monitoring to a third-party supervisor, and demonstrates that in equilibrium the principal gains by delegating monitoring to the supervisor. Delegation is profitable for the principal for two reasons: incentive and commitment effects. First, it is assumed that commitment to monitoring is not possible, implying that the principal has inadequate incentives to monitor on his own. With delegation of monitoring, the principal has to create two types of incentives: one for the agent and the other for the monitor. Because of this separation, the principal is able to regulate the two incentives more accurately (incentive-effect). Second, it is also assumed that information obtained from the monitoring process is private, which implies that if the principal acts as the monitor, he could not credibly commit to revealing positive information when the agent puts in high effort because to do so would mean making public profitable private information. As the agent can anticipate the principal's behavior, the principal's financial claim becomes weaker, and the resulting contract is unprofitable for the principal. Delegation of monitoring, however, solves the problem of lack of commitment. The supervisor has no financial interest in the agent's action, and thus has no incentive to withhold positive information should the agent choose a high effort level. Hence, delegation of monitoring allows the principal to make optimal use of a carrot and stick approach to discipline the agent (commitment-effect).[10]

2.2.2. Hypothesis development

The nature of the informal influence of CRAs over firms makes it difficult to document empirical evidence on the monitoring role of the agencies. Nonetheless, by focusing on credit watch actions with direction downgrade, I can obtain an ideal setup to test whether CRAs act as third-party monitors. I employ the occurrence of watch assignment and resolution announcements as points in time between which I know that interactions have taken place between the agencies and firms. Changes in corporate fundamentals that

[10] In another paper, Berlin and Loeys (1988) present a model in which a firm seeking to finance an investment project can choose between two contractual arrangements: loan contracts with covenants, which are written in terms of readily observable but noisy indicators of the firm's ability to repay, and loan contracts enforced by a monitoring specialist. Because the covenants are based on imperfect information, Berlin and Loeys demonstrate that default policies based on these covenants are inefficient, and it is optimal for the firm to hire the services of a delegated monitor, ensuring a more efficient liquidation policy. Taken to the context of CRAs, the model implies that an issuer is better off obtaining financing and engaging a rating agency to monitor its activities rather than obtaining financing with covenants. The agency has incentives to monitor because it collects fees from the issuer in exchange for its ongoing monitoring services. It is important to note, however, that in Berlin and Loeys's model monitoring is defined in the sense of gathering and analyzing detailed information rather than disciplining firms. For this particular reason, I favor the insights gained from the model of Strausz (1997) to motivate the monitoring role of CRAs.

immediately follow these interactions can provide evidence on the monitoring role of the agencies.

Consider the following theoretical framework, which is inspired by the credit rating model of Boot et al. (2006). Assume that firms can choose between a risky and a safe investment project. A safe project is defined as that with high expected investment returns (or high credit quality), and a risky project is defined as having low expected investment returns (or low credit quality). In the presence of moral hazard, investors cannot ex-ante determine whether these firms will opt for the safe or risky project so that they are reluctant to supply capital. A CRA can help mitigate moral hazard faced by capital suppliers by placing firms under watch with direction downgrade, in which case a monitoring regime begins. Firms threatened with a rating downgrade may have incentives to comply with the agency's directives and choose to undertake the safe project. The CRA can discover through the watch period the type of project chosen and accordingly reveals its true credit quality via watch resolution announcements. In this respect, firms choosing the safe project would receive rating confirmations (treatment firms), and firms opting for the risky project would be subsequently downgraded (control firms). It follows that watch resolution decisions and future patterns of corporate profitability should be related in a predictable manner. I can expect treatment firms to be more profitable than control firms following the watch period. In addition, by revealing the true credit quality of the selected project, the CRA can help mitigate capital market information asymmetry for treatment firms, which would have better access to capital markets. Accordingly, I can expect these firms to rely more on long-term financing and ramp up their investment activities.

In summary, third-party rating agency monitoring via a watch procedure with direction downgrade can help resolve project-choice moral hazard and alleviate the related capital market information asymmetry faced by capital suppliers, resulting in predictable patterns of corporate financing, investment, and profitability. My first main hypothesis illustrates these projections.

Hypothesis 1. The mean effects of treatment. Relative to control firms, treatment firms rely more on long-term financing and less on short-term financing, ramp up their investment activities, and are more profitable beginning immediately after the watch period.

I can expect third-party monitoring to add more value for firms of lower credit quality because these firms typically have no established record of addressing deterioration in their credit quality on their own. The CRAs allocate more resources and engage in more intense interactions with these firms, in which cases the CRAs would have stronger influence and better mitigate information asymmetry. In contrast, firms with high-quality credit standing typically have a long history of maintaining capability and promptness in meeting financial obligations. These firms need no external monitoring to address potential deterioration in their credit quality; they have the ability to exercise adequate turnaround effort on their own. In this respect, the credit watch procedure, as a monitoring mechanism, is likely redundant, and the CRAs would have less intense interactions and less influence on these firms.

If CRAs fail to exercise their monitoring function, I would expect to observe, ceteris paribus, comparable patterns of subsequent changes in fundamentals for firms with high credit quality (less valuable monitoring) and firms with low credit quality (more valuable monitoring). In contrast, if CRAs fulfill their monitoring role, I would expect to observe more pronounced subsequent changes in the fundamentals of firms of lower credit quality, for which monitoring is more valuable. My second main hypothesis summarizes these views.

Hypothesis 2. The interaction effects of treatment. Among treatment firms, the post-watch increase in long-term financing, investment, and profitability is more pronounced for firms of lower credit quality.

2.3. Sampling procedure and summary statistics

The sample of credit watch actions is from Moody's senior rating database, which contains *estimated senior unsecured ratings*. According to Moody's Investors Service (2009), "a company's estimated senior rating is set equal to its actual senior unsecured debt rating or, if there is none, by implying such a rating on the basis of rated subordinated or secured debt." I choose to work with issuer-level data to abstract from issue-level differences in seniority or security, so that I can better capture changes in issuers' fundamental credit quality. For each credit watch observation, Moody's data provide detailed information that includes the name of the issuer, credit rating placed under watch, watch initial direction, and watch announcement date. Once the credit watch is resolved, I observe the watch final direction, final rating, and watch resolution date.

I begin the sampling procedure by selecting credit watches with direction downgrade that are resolved with either a rating confirmation or downgrade at any date between 1992 and 2011, yielding an initial sample of 3,184 credit watch observations.[11] The set of Moody's

[11] Although Moody's has been publishing credit watches since 1985, watches gained the status of formal credit actions only beginning in 1991. Since there are few observations in 1991, I restrict my sample to start in 1992.

ratings consists of 21 letter ratings, which are formed based on generic rating categories (Aaa; Aa; A; Baa; Ba; B; Caa; Ca; and C) along with numerical modifiers (1; 2; and 3) appended to each generic classification from Aa through Caa. Consistent with prior papers, I translate these ratings into numerical scores on the following scale: Aaa = 1; Aa1 = 2; Aa2 = 3; Aa3 = 4; …; and C = 21.

Next, I merge credit watch observations with quarterly data from Compustat. I follow the literature on firm financing and investment decisions and exclude financial firms, and then build a watch-quarter level data set of firms for which accounting information is available from Compustat.[12] Based on this information, I construct several financing, investment, and profitability measures.[13] For financing, I consider five measures, which are: the change in long-term debt ratio, equity issuance ratio, change in long-term financing ratio, change in short-term debt ratio, and change in cash holdings ratio. For investment, I employ the following three measures: the capital expenditures ratio, PPE growth rate, and asset growth rate. For profitability, I focus on three measures, which are: the operating income ratio, return on assets, and return on equity. These variables and others are defined in Appendix A. To mitigate the effects of outliers, I follow the literature and winsorize Compustat variables each calendar quarter at the 1st and 99th percentile. The resulting baseline sample consists of 509 rating confirmations and 1,381 rating downgrades, for a total of 1,890 watch actions on 802 unique issuers and for a total of 17,010 watch-quarter observations.

Table 2.1 shows the distribution of watches by calendar year. As expected, the number of watches peaks in the years 2002 and 2009, which both coincide with times of slow

[12] I exclude firms with SIC codes 6000-6999.

[13] The ratios used in this chapter are based on either average assets or average equity as a scaling factor. If I scale by either lagged assets or lagged equity I obtain similar results (untabulated).

economic activity. Most watches are resolved with rating downgrades rather than confirmations. Indeed, out of 1,890 watches 1,381 (73.1%) watches are resolved with rating downgrades, and only 509 (26.9%) watches are resolved with rating confirmations. The incidence of rating downgrades appears to be influenced by concurrent economic conditions, peaking during the years 2002 and 2009.

Table 2.2 reports the transition probabilities of rating downgrades (Panel A) and their distribution by magnitude (Panel B). The last column in Panel A shows that most downgrades are issued on investment grade firms (Baa or higher). Downgrades issued on investment grade firms represent more than 70% of total downgrades. Meanwhile, the last row indicates a high incidence of migration from the investment to the non-investment grade category as a result of rating downgrades. Panel B shows that downgrades are typically of small magnitude. Together, one-notch and two-notch downgrades represent more than 90% of total downgrades, suggesting that Moody's favors gradual changes in ratings.

Table 2.3 reports the means of several watch characteristics (Panel A) and firm attributes (Panel B) as of the end of the quarter prior to the watch period, or quarter 0, for treatment and control firms along with their difference tests.[14] The results indicate that the two sets of firms are different along some dimensions and comparable along others. Moody's takes roughly 92 days to resolve a watch action with a rating downgrade compared with 143 days for a rating confirmation. A longer watch period for treatment firms is consistent with more interactions and longer discussions between Moody's and these firms. Among downgraded ratings, only about 28% are initially categorized as non-investment grade ratings. In contrast, roughly 33% of confirmed ratings belong to the non-investment grade category. The numerical scores associated with ratings before and after watch resolutions indicate that

[14] Appendix A provides variable definitions.

control firms have roughly comparable initial ratings but end up with lower ratings following watch resolutions. The average magnitude of a rating downgrade is equal to 1.5, and about 16% of downgraded ratings are fallen angels, which cross the investment grade boundary.

In Panel B of Table 2.3, the two sets of firms exhibit no significant differences in terms of their financing metrics. Relative to control firms, treatment firms grow their total assets faster, but employ similar investment capital as a percentage of average assets and have comparable PPE growth rates. Meanwhile, treatment firms are more profitable and smaller, have better growth profiles based on Tobin's Q, and are less financially constrained based on the cash flow investment gap ratio. However, treatment firms are comparable to control firms in terms of either asset tangibility or corporate governance. Collectively, the results indicate that the two sets of firms do not exhibit clear systematic differences, which is a desirable feature of the sample for my empirical analysis.

2.4. Empirical design

To investigate the effects of treatment on corporate fundamentals while controlling for the standard determinants of corporate policies, I analyze the dynamic pattern of corporate fundamentals from four quarters before to four quarters after the watch period. Specifically, I employ a panel regression model of the following specification

$$Y_{it} = \alpha + \sum_{q=1}^{4} \delta_q Before_{it}^{q} \times Treatment_i + \sum_{q=1}^{4} \mu_q After_{it}^{q} \times Treatment_i$$

$$+ \sum_{q=1}^{4} \varphi_q Before_{it}^{q} + \sum_{q=1}^{4} \omega_q After_{it}^{q} + \theta Treatment_i$$

$$+\gamma X_{it} + Industry_i + Quarter_t + FiscalQuarter_{it} + \varepsilon_{it}, \qquad (2.1)$$

where Y_{it} is a measure of corporate fundamentals (a financing, investment, or profitability measure); X_{it} is a set of control variables (measures of firm attributes and credit characteristics); $Before_{it}^{q}$ is a dummy variable equal to 1 if the observation is q quarters prior to the watch period, where $q =$ 1, 2, 3, or 4 quarters; $After_{it}^{q}$ is a dummy variable equal to 1 if the observation is q quarters after the watch period, where $q =$ 1, 2, 3, or 4 quarters; $Treatment_i$ is a dummy variable equal to 1 if the credit watch is resolved with a rating confirmation; i indexes firms; t indexes time measured in calendar quarters; $Industry_i$ are 38 SIC-based industry fixed effects; $Quarter_t$ are calendar quarter fixed effects; and $FiscalQuarter_{it}$ are fiscal quarter fixed effects. Comparable specifications to Equation (2.1) have been used in prior literature to study firm behavior around corporate events (see, e.g., Schoar, 2002; Bertrand and Mullianathan, 2003; and Chemmanur et al., 2009).

In the above specification, the group of interest consists of firms with confirmed ratings (treatment firms), and the control group consists of firms with downgraded ratings (control firms). For the latter group of firms, the interaction variables $Before_{it}^{q} \times Treatment_i$ and $After_{it}^{q} \times Treatment_i$ are always equal to 0. I also note that the reference quarter, quarter 0, is the quarter ending prior to the watch period. By construction, Equation (2.1) is a difference-in-differences specification, and the coefficients δ_q and μ_q identify the residual changes in measures of corporate fundamentals around the watch period for treatment firms relative to control firms. The industry fixed effects, $Industry_i$, control for differences across industries; the calendar quarter fixed effects, $Quarter_t$, are meant to account for changes in market conditions that may influence credit actions or affect firm fundamentals;

and the fiscal quarter fixed effects, $FiscalQuarter_{it}$, are intended to account for seasonal patterns of corporate fundamentals related to fiscal quarters. Following Petersen (2009) and Thompson (2011), in my empirical tests I estimate standard errors clustered at the firm and calendar quarter levels to control for potential biases related to heteroskedasticity and serial correlation of the residuals.

2.5. Treatment and the dynamic pattern of corporate fundamentals

I hypothesize that third-party monitoring facilitates access to capital markets by alleviating information asymmetry for treatment firms, in which case they would increase their long-term financing and ramp up their investment activities. I also hypothesize that third-party monitoring helps improve the profitability of treatment firms through an informal influence channel. Collectively, I refer to these predictions as the mean effects of treatment. I further hypothesize that these effects are stronger for treatment firms of lower credit quality. I label these additional predictions the interaction effects of treatment.

In this section, I present evidence on the mean and interaction effects of treatment, and conduct additional tests that provide support for the information asymmetry and informal influence channels. First, I examine the unconditional patterns of several measures of corporate financing, investment, and profitability in the quarters surrounding the watch period in a univariate setting. Second, I test the mean effects of treatment in a multivariate setting, allowing for the effects of other relevant factors. Third, I investigate the interaction effects of treatment by conditioning upon firm credit quality. Fourth, I condition upon firm financial constraints to test the information asymmetry channel. Fifth, I condition upon corporate governance to test the informal influence channel.

2.5.1. Univariate patterns of corporate financing, investment, and profitability

As a preliminary test for the mean effects of treatment, I examine the unconditional patterns of corporate fundamentals around the watch period in a univariate setting. Figure 2.1 depicts the means of corporate financing (Panel A), investment (Panel B), and profitability (Panel C) measures for treatment and control firms from four quarters before to four quarters after the watch period. I ensure that each firm is associated with non-missing values for the fundamentals measure of interest in every quarter within the event window. The figure provides evidence largely consistent with my first main hypothesis. Relative to control firms, treatment firms increase their long-term financing, investment, and profitability in the post-watch period.

Panel A of Figure 2.1 shows that the change in the long-term debt ratio exhibits similar patterns for both sets of firms in the pre-watch period. After watch, however, the long-term debt ratio substantially increases for treatment firms, while that of control firms remains little changed. The gap in the equity issuance ratio between treatment and control firms narrows in the few quarters leading up to quarter 0, at which quarter both sets of firms have similar equity issuance ratios. Interestingly, the biggest increase in the equity issuance ratio for treatment firms occurs between quarter 0 and quarter 1, while that of control firms remains unchanged. Focusing on the change in the long-term financing ratio (change in the long-term debt ratio plus the equity issuance ratio), I document similar pre-watch patterns for both sets of firms. As expected, I observe a substantial increase in the long-term financing ratio of treatment firms when benchmarked against control firms in the post-watch period. In contrast, treatment and control firms do not exhibit noticeable differences in the patterns of short-term financing. Both short-term debt and cash holdings, as a percentage of average assets, are little changed around quarter 0 for both groups of firms.

Panel B of Figure 2.1 shows that both sets of firms have comparable investment metrics prior to the watch period. After watch, however, treatment firms invest more capital than their peers. I observe that the difference in capital expenditures reverses sign from quarter 0 to 1, subsequent to which treatment firms employ substantially more investment capital. Consistent with this observation, I also document a sharp increase in the growth rates of PPE and total assets for treatment firms in the post-watch period.

Panel C of Figure 2.1 shows that treatment firms outperform their peers, most notably in the quarters following the watch period. Profitability of treatment firms remains positive and mostly stable throughout the event window. In contrast, profitability of control firms begins to decline slightly in the few quarters leading up to quarter 0, subsequent to which deterioration in profitability accelerates to the downside.

In sum, Figure 2.1 provides evidence that relative to control firms treatment firms increase their long-term financing, ramp up their investment activities, and are more profitable in the post-watch period. These patterns are not part of long-term trends in corporate fundamentals, but instead they are changes that occur only after the watch period. This observation supports the parallel trends assumption necessary for the validity of the difference-in-differences tests, which I present in the subsequent section. I provide a formal test of this assumption as a robustness check in Section 2.7.

2.5.2. The effects of treatment on corporate financing, investment, and profitability

To test the mean effects of treatment while controlling for other relevant factors, I run difference-in-differences panel regressions following the specification in Equation (2.1). Table 2.4 reports the estimation results, where the dependent variable is a measure of firm financing (Panel A), investment (Panel B), or profitability (Panel C), and the regressors of interest are the interaction variables $Before^q \times Treatment$ and $After^q \times Treatment$, where $q = 1$, 2, 3, or 4 quarters. As discussed in Section 2.4, the coefficients of these interaction variables identify the residual changes in the outcome variable of interest around the watch period for treatment firms relative to control firms. The control variables include the natural logarithm of a rating change and a dummy indicating a fallen angel. Since a multi-notch downgrade or a fallen angel signals potentially more severe deterioration in firm credit quality, I would expect to observe weaker fundamentals for firms with stronger downgrades or firms whose ratings have crossed the investment grade boundary. Following Rajan and Zingales (1995), I also include a set of control variables that may reflect either variations in firms' preferences for financing and investment, the supply of external financing, or future investment opportunities. These additional control variables are: firm size, Tobin's Q, and asset tangibility.[15] Size has been found in the literature to be positively correlated with corporate leverage because larger firms are expected to have lower costs of financial distress (Graham, Lemmon, and Schallheim, 1998; and Hovakimian, Opler, and Titman, 2001). Size could also be used as an inverse proxy for information asymmetry and thus is expected to be positively correlated with equity issuance (Rajan and Zingales, 1995). I measure firm size by the natural logarithm of firm total assets. I include Tobin's Q as an additional control because

[15] Since I use firm profitability as an outcome variable, I do not include it as a control in any of my regressions.

it is a standard proxy for growth opportunities, and is expected to be positively related to firm external financing and investment (Baker and Wurgler, 2002). Tobin's Q is measured as total assets minus book equity plus market equity all divided by total assets. Tangible assets could be used as collateral and thus is expected to be positively correlated with leverage (Baker and Wurgler, 2002). Asset tangibility could also be used as an inverse proxy for information asymmetry and thus is expected to be positively related to equity issuance.[16]

Panel A of Table 2.4 employs several proxies for corporate financing. These are: the change in the long-term debt ratio (Model [1]), equity issuance ratio (Model [2]), change in the long-term financing ratio (Model [3]), change in the short-term debt ratio (Model [4]), and change in the cash holdings ratio (Model [5]). Consistent with my first hypothesis, the results indicate that treatment firms rely more on long-term financing and less on short-term financing when benchmarked against control firms in the post-watch period. In Model [1], the positive coefficients of the interaction dummies $Before^q \times Treatment$ and $After^q \times Treatment$ substantially increase in magnitude and become statistically significant beginning in quarter 1, implying that treatment firms issue substantially higher levels of long-term debt following the watch period. In Model [2], the coefficients on the interaction dummies switch sign around quarter 0 and remain positive and significant over the subsequent three quarters, suggesting that treatment firms issue more equity in the post-watch period. Results from Model [3] confirm that treatment firms substantially increase their long-term financing in the post-watch period. The incremental long-term financing for treatment firms amounts to more than 3% of average assets in every quarter within the post-watch period. In particular, by four

[16] The estimated coefficients of the control variables are not always consistent with the associated expected effects. One potential explanation for this is that my sample is nonstandard because it consists of large, well-established firms for which issuer-level ratings are available. Therefore, the estimated effects of the control variables are actually conditional effects, which may diverge from the unconditional effects that would obtain in a standard sample.

quarters following the watch period treatment firms have raised their long-term financing by 3.54%, which represents a more than 100% increase above its pre-watch level of 3.3%.[17] In contrast, Models [4] and [5] indicate that treatment firms do not increase their short-term financing in the post-watch period. The change in short-term debt is mostly negative, but also insignificant. Cash holdings increase in the first quarter following the watch period, indicating that treatment firms tend to use less cash. However, this effect appears to be short-lived and disappears in the subsequent three quarters, indicating that the cash balance is mostly unchanged relative to its pre-watch level.

Panel B of Table 2.4 shows estimation results based on three measures of corporate investment, which are: the capital expenditures ratio (Model [6]), PPE growth rate (Model [7]), and asset growth rate (Model [8]). The general picture that emerges from this panel is that treatment firms ramp up their investment activities only after the watch period. In Model [1], the coefficients of the interaction dummies $Before^q \times Treatment$ and $After^q \times Treatment$ suggest that treatment firms employ comparable levels of investment capital before the watch period and significantly more capital afterwards when compared to control firms. Indeed, I observe that subsequent to quarter 0 capital expenditures, as a percentage of average assets, increase monotonically, and as of quarter 4 they amount to 1.33% of average assets, which represents a roughly 22% increase above its pre-watch level of 6.1%. Results from Models [7] and [8] provide evidence that treatment firms tend to grow their fixed and total assets in the post-watch period. Focusing on the fourth quarter subsequent to the watch period, treatment firms grow their fixed and total assets by an incremental 8.90% and an

[17] For all variables, pre-watch levels (levels as of quarter 0) are from the summary statistics of treatment firms in Table 2.3. The relative increase is computed as 3.54% divided by 3.3%.

additional 7.21%, respectively, which both represent a nearly 100% increase above their respective pre-watch levels of 6.70% and 8.10%.

Panel C of Table 2.4 employs three measures of corporate profitability: the operating income ratio (Model [9]), return on assets (Model [10]), and return on equity (Model [11]). The results indicate that treatment firms substantially outperform their peers, and most importantly the higher profitability attributed to treatment firms is not part of a long-term trend. Instead, treatment firms are significantly more profitable only in the post-watch period. In Model [9], I observe that treatment firms are about 2% more profitable by the end of quarter 4. Relative to its pre-watch level of 13.2%, the 2% increase in the operating income ratio represents a nearly 15% improvement in profitability. In Model [10], treatment firms outperform their peers by an incremental return on assets of 2.54% as of quarter 4. In the same quarter, Model [11] shows that the incremental return on equity for treatment firms is equal to a substantial 9.11%.

To summarize, the results reported in Table 2.4 strongly support the hypothesis on the mean effects of treatment. The occurrence of treatment represents a turning point for the pattern of corporate financing, investment, and profitability. Relative to control firms, treatment firms achieve higher levels of long-term financing, invest more capital, and experience a sharp increase in profitability immediately following the watch period.

2.5.3. The effects of treatment on corporate financing, investment, and profitability conditional upon firm credit quality

I hypothesize that treatment firms have better access to capital markets as a result of reduction in information asymmetry following the watch period, and accordingly they tend to increase their long-term financing and investment. Moreover, I hypothesize that treatment firms experience better profitability as a result of third-party monitoring. Among treatment firms, I know that those of lower credit quality are more sensitive to information asymmetry and are more susceptible to pressure from the agencies. If this is the case, then consistent with the hypothesis on the interaction effects of treatment I can expect the increase in long-term financing, investment, and profitability experienced by treatment firms to be stronger for those treatment firms of lower credit quality.

To test the interaction effects of treatment, I condition upon firm credit quality and study the dynamic pattern of firm fundamentals around the watch period. First, I divide the sample firms into two groups based on Moody's investment (Baa or higher)/non-investment (Ba or lower) grade status. Second, I repeat the analysis in Table 2.4 for each group of firms and report difference tests for the coefficients of the interaction variables $Before^q \times Treatment$ and $After^q \times Treatment$.

Table 2.5 reports the estimation results, where the coefficients of the control variables are omitted for brevity of presentation.[18] In support of my predictions, the results show that the increase in long-term debt, investment, and profitability of treatment firms is mainly driven by non-investment grade firms. Panels A-E indicate that non-investment grade firms issue substantially higher long-term debt. The fourth quarter post-watch increase in long-term debt is greater than 5% as a percentage of average assets for non-investment grade treatment firms when compared to their investment grade peers. Non-investment grade treatment firms also marginally reduce their short-term debt and do not alter their cash holdings on a relative basis. Panels F-H show that non-investment grade treatment firms increase their investment

[18] All specifications in Table 2.5 include the control variables reported in Table 2.4, with the exception of the non-investment grade dummy.

activities faster than their investment grade counterparts. For example, Panel G shows that as of quarter 4 post-watch non-investment grade treatment firms grow their fixed assets by a roughly 18% compared to only about 5% for investment grade firms. This amounts to a difference in PPE growth rate of 13%, which is statistically significant at the 1% level. Panels I-K show that non-investment grade treatment firms are more profitable, although results for some ratios lack statistical significance. Based on return on equity in Panel K, the outperformance of non-investment grade treatment firms ranges between roughly 10% and 16% in the post-watch period.

2.5.4. The effects of treatment on corporate financing conditional upon firm financial constraints

In this section, I investigate whether agencies can help alleviate firm financial constraints resulting from information asymmetry in credit markets. Among treatment firms, I can expect financially constrained firms to benefit the most from the reduction in information asymmetry and take advantage of the resulting better access to capital markets to alter their financing mix, using the sources of funds that are most sensitive to information asymmetry. Accordingly, I can expect financially constrained treatment firms to rely more on long-term financing and less on short-term financing when compared to their financially unconstrained counterparts in the post-watch period.

To implement this test, I condition upon firm financial constraints and study the dynamic pattern of firm financing around the watch period. Following Rajan and Zingales (1998), I employ the cash-flow investment gap to classify firms as financially constrained or unconstrained. I measure the cash-flow investment gap as cash flow minus capital expenditures all divided by average assets. I divide the sample firms into two groups based on the median value of the cash-flow investment gap as of quarter 0. Next, I repeat the analysis in Table 2.4 for each group of firms and report difference tests for the coefficients of the interaction variables $Before^q \times Treatment$ and $After^q \times Treatment$.

Table 2.6 reports the estimation results, where the coefficients of the control variables are omitted for brevity of presentation.[19] As expected, the results indicate that constrained treatment firms engage in financing substitution: they increase their long-term financing and decrease their short-term financing, a financing behavior that is consistent with a decrease in information asymmetry and a better access to capital markets. Panel C indicates that constrained treatment firms significantly increase their long-term financing in the quarters following the watch period. The incremental long-term financing ranges between roughly 3% and 6% of average assets. Panels A and B show that the incremental long-term financing is mainly driven by a sharp increase in long-term debt issuance. Turning to Panels D and E, I observe that constrained treatment firms reduce their short-term financing and marginally increase their cash holdings in the post-watch period, indicating that these firms rely less on short-term financing. Overall, the results indicate that constrained treatment firms substitute long-term financing for short-term financing to benefit from the post-watch reduction in information asymmetry and the resulting better access to capital markets.

2.5.5. The effects of treatment on corporate profitability conditional upon corporate governance

I am interested in how third-party monitoring interacts with corporate governance. In other words, is third-party monitoring a substitute for and/or a complement to corporate

[19] All specifications in Table 2.6 include the control variables reported in Table 2.4.

governance? Prior papers show that firms with stronger governance are more profitable (see, e.g., Gompers, Ishii, and Metrick, 2003). To test the substitution role, I ask the following: does third-party monitoring have a positive effect on corporate profitability beyond that attributed to better corporate governance? If corporate profitability improves as a result of treatment regardless of corporate governance, then I can conclude that third-party monitoring can be a substitute for corporate governance. To test the complement role, I ask whether the positive effect of third-party monitoring on corporate profitability increases with stronger corporate governance. If this is the case, then I can claim that third-party monitoring can be a complement to corporate governance.

To answer these questions, I condition upon corporate governance and study the effects of treatment on corporate profitability. I employ the G Index of Gompers, Ishii, and Metrick (2003) as a proxy for corporate governance. Firms with values for the Index below the median value of 10 are classified as firms with strong governance, and other firms are categorized as firms with weak governance. Next, among each corporate governance group, I identify treatment and control firms, yielding four subgroups: strong governance treatment firms, strong governance control firms, weak governance treatment firms, and weak governance control firms.

Table 2.7 reports conditional difference-in-differences estimates for the three measures of corporate profitability conditional upon corporate governance. The table also reports a triple difference test, where the last difference is computed as the difference-in-differences estimate for strong governance firms less that for weak governance firms. I document that among firms with strong corporate governance, treatment firms are more profitable than control firms. For example, Panel A shows that strong governance treatment firms outperform their control peers by about 2% to 3% when the operating income ratio is considered. The same effect holds among firms with weak corporate governance, although it is economically weaker. For example, Panel A shows that the operating income ratio is higher for weak governance treatment firms by about 1% when compared to their control counterparts. In sum, the effect of treatment on corporate profitability is not subsumed by that resulting from corporate governance. Because treatment has a positive effect on corporate profitability regardless of corporate governance, I can conclude that third-party monitoring can be a substitute for corporate governance. Interestingly, the results in Table 2.7 also indicate that the triple difference estimates are positive and significant albeit at lower confidence levels, consistent with an interaction effect between treatment and corporate governance. Agency's monitoring is more effective among firms with better corporate governance, perhaps because those firms are more likely to embrace the implicit contract a la Boot et al. (2006).

Table 2.8 reports regression results conditional upon corporate governance. I divide the sample firms into two groups: strong and weak governance groups. For each group, I run a difference-in-differences panel regression to estimate the coefficients of the interaction variables $Before^q \times Treatment$ and $After^q \times Treatment$. The regressions include all the control variables used in Table 2.4, but I do not report their coefficients for brevity of presentation. After controlling for other relevant factors and various fixed effects, I observe that the substitution role of third-party monitoring persists. The effect of treatment on corporate profitability is positive and mostly significant within each corporate governance group. The results also indicate that after considering other controls the interaction effect between third-party monitoring and corporate governance is weaker. The difference in the conditional effects of treatment on corporate profitability lacks statistical significance.

In sum, results from Tables 2.7 and 2.8 are consistent with the view that third-party monitoring can be a substitute for corporate governance: the agency can exert informal influence over firms to undertake profitable projects regardless of their corporate governance. The results also point to weaker evidence that third-party monitoring is more effective among firms with stronger governance.

2.6. Robustness tests

In this section, I conduct various robustness tests on my main results. First, I formally test the parallel trends assumption underlying the difference-in-differences framework. Second, I check whether the results are sensitive to empirical methodologies by considering propensity score matching as an alternative to the difference-in-differences panel regression framework. Third, I address potential endogeneity in agency's watch resolution decisions and provide an assessment of their net effects on measures of corporate fundamentals.

2.6.1. Testing the parallel trends assumption

A valid concern with the difference-in-differences results is that the changes in corporate financing, investment, and profitability measures that I observe in the post-watch period may be part of long-term trends. If this is the case, then I cannot attribute these changes to treatment, but rather to other unobservable factors that matter for the long-term trends in corporate fundamentals.

To address this concern, I test the parallel trends assumption underlying the difference-in-differences framework (see, e.g., Roberts and Whited, 2012). This assumption states that the pre-watch trends in corporate financing, investment, and profitability measures for treatment and control firms should be statistically indistinguishable. To implement this test, I falsely assume that the onset of treatment occurs two years before it actually does, and then I re-estimate the difference-in-differences model in Equation (2.1) based on this assumption.[20]

Table 2.9 reports estimation results of the difference-in-differences model based on the falsification test. Consistent with the parallel trends assumption, the results indicate that the evidence from my main analysis in Table 2.4 does not reproduce based on the false treatment. In all panels, the coefficients of the interaction variables $Before^q \times Treatment$ and $After^q \times Treatment$ are generally statistically insignificant. In those cases in which the coefficients are significant they always take the unexpected sign. Thus, I can be confident that the prior difference-in-differences results are valid, and the documented post-watch patterns in corporate financing, investment, and profitability are due to treatment rather than part of long-term trends.

2.6.2. Propensity score matching

So far, the difference-in-differences results are based on a panel regression model. To explore whether the results are sensitive to this specific empirical methodology, I employ propensity score matching, which allows for direct matching between treatment and control firms based on the characteristics that matter for corporate fundamentals. I run a probit regression to estimate propensity scores. The dependent variable is a dummy equal to one for treatment firms and zero for control firms. In the first set of matching variables, I consider

[20] I obtain comparable results when I assume that the onset of treatment occurs one year before it actually does.

proxies for the standard determinants of corporate policies, which are: firm size, Tobin's Q, and asset tangibility. The second set consists of the variables I employed in this chapter to investigate various conditional effects of treatment. These are dummy variables indicating non-investment grade status, financial constraints, and strong governance.[21] The third set consists of dummy variables that indicate calendar years and 38 SIC-based industries. In the matching, I determine candidate control firms by allowing for an absolute difference between propensity scores of 1%. Then, I match each treatment firm to a control firm chosen randomly with no replacement from the group of candidate control firms.[22]

Table 2.10 reports the results based on 250 matched treatment firms that could be matched to control firms based on non-missing values for the matching variables. Focusing on the difference-in-differences estimates, I observe that as expected treatment firms rely more on long-term financing, invest more, and are much more profitable in the four quarters following the watch period. Panel A shows that treatment firms issue more long-term debt and do not increase their short-term financing subsequent to the watch period. Panel B demonstrates that treatment firms spend more on capital expenditures and grow faster their fixed and total assets in the post-watch period. Panel C indicates that treatment firms are consistently more profitable in every quarter following the watch period. Collectively, these results are highly consistent with those based on the panel regression framework, indicating that my prior conclusions with respect to post-watch patterns in corporate fundamentals are robust to different empirical methodologies.

[21] The financial constraints dummy indicates the cash flow investment gap ratio is below its median. The corporate governance dummy indicates the G Index is below its median.

[22] I obtain similar results if I choose one-to-three matching with a propensity score distance of 3% or one-to-five matching with a propensity score distance of 5%. In both cases, the number of matched treatment firms is smaller.

2.6.3. Addressing endogeneity

The panel regression model may fail to account for potential endogeneity in agency's watch resolution decisions. As shown in Table 2.3, treatment firms tend to be smaller, more profitable, have better growth profiles, and are less financially constrained. If watch resolution decisions are influenced by firm characteristics, then the estimated effects from the panel regression are likely contaminated with the effects resulting from endogeneity in watch resolution decisions.

To address selectivity in watch resolution outcomes and establish their direct causal effects on firm fundamentals, I consider an empirical framework that makes it possible to hold the firm profile fixed and measure the effects due to watch resolution decisions only. The empirical model I propose is a switching regression model with endogenous switching, which is discussed in Maddala (1983). The model is used by Fang (2005) to control for endogeneity in the issuer-underwriter matching in her study of the effects of investment bank reputation on the price and quality of debt underwriting services.[23] I formally present the model in detail in Appendix B.

Table 2.11 shows the estimation results for Equation (B.10), which computes the difference between actual and hypothetical changes in a firm's fundamentals measure. As discussed in Appendix B, the hypothetical change in a fundamentals measure is obtained by holding the firm's profile fixed and computing the change in that measure as if the firm's rating were downgraded rather than confirmed. Each panel of the table reports for a given fundamentals measure the means of the actual and hypothetical values along with their difference tests. Consistent with the main results, the evidence I gather from the table suggests

[23] In general, the switching regression model is suitable to model endogenous dichotomous choices where the researcher observes data for the two regimes resulting from these choices.

that treatment firms obtain higher long-term financing, invest more, and are more profitable when benchmarked against their counterfactual counterparts. To illustrate, I focus on the difference-in-differences estimates for the period from quarter 0 to 4. In Panel A, results show that for treatment firms the mean change in long-term financing as a percentage of average assets is equal to 5.58%, 3.99% higher than the hypothetical 1.59% change that would obtain for counterfactual firms. Panel B shows that treatment firms ramp up their investment in PPE at an annual growth rate equal to 7.08%; if these firms had been downgraded instead, they would achieve a negative growth rate equal to -7.47%, resulting in a significant deterioration in fixed asset growth of 14.56%. In Panel C, results indicate that for treatment firms the mean actual change in return on equity is equal to 2.45%, which is 10.15% higher than the hypothetical -7.69%.

In sum, Table 2.11 provides evidence that after accounting for potential endogeneity in watch resolution decisions treatment firms obtain higher long-term financing, invest more, and are more profitable beginning immediately following the watch period, a set of results that is consistent with my prior conclusions.

2.7. Conclusion

In this chapter, I investigate whether credit rating agencies act as third-party monitors through the mechanism of a credit watch with direction downgrade. To this end, I examine the relations between credit watch resolution outcomes and the subsequent changes in corporate financing, investment, and profitability. Based on the insights provided in Boot et al. (2006), the watch procedure with direction downgrade allows for a monitoring regime, whereby the agencies exert behind-the-scenes influence over firms placed under watch to extend turnaround effort. I conjecture that if the agencies condition their watch resolution decisions on the effectiveness of firm turnaround effort, then the relations between these decisions and the patterns of subsequent changes in corporate fundamentals are predictable. Two main channels are in play in this respect. First, by exerting informal influence the agencies can induce firms to undertake safe projects, helping improve their profitability. Second, when the agencies reveal the true credit quality of firms following the monitoring episode, they can help reduce information asymmetry and thus facilitate access to capital markets. Accordingly, firms increase their long-term financing and ramp up their investment activities.

Based on a difference-in-differences panel regression framework, I find that, relative to firms with watch-preceded rating downgrades (control firms), firms with watch-preceded rating confirmations (treatment firms) tend to increase their long-term financing, employ more investment capital, and experience an improvement in profitability beginning immediately following the credit watch period. These patterns are stronger for treatment firms of lower credit quality, for which third-party monitoring is more valuable.

I conduct two additional tests that support the information asymmetry and informal influence channels. First, I find that financially constrained treatment firms substantially increase their long-term financing at the expense of short-term financing immediately following the watch period, indicating that these firms attempt to benefit from reduction in information asymmetry and the resulting better access to capital markets. Second, I show that treatment firms are more profitable regardless of corporate governance, implying that agencies' informal influence is not an artifact of strong corporate governance.

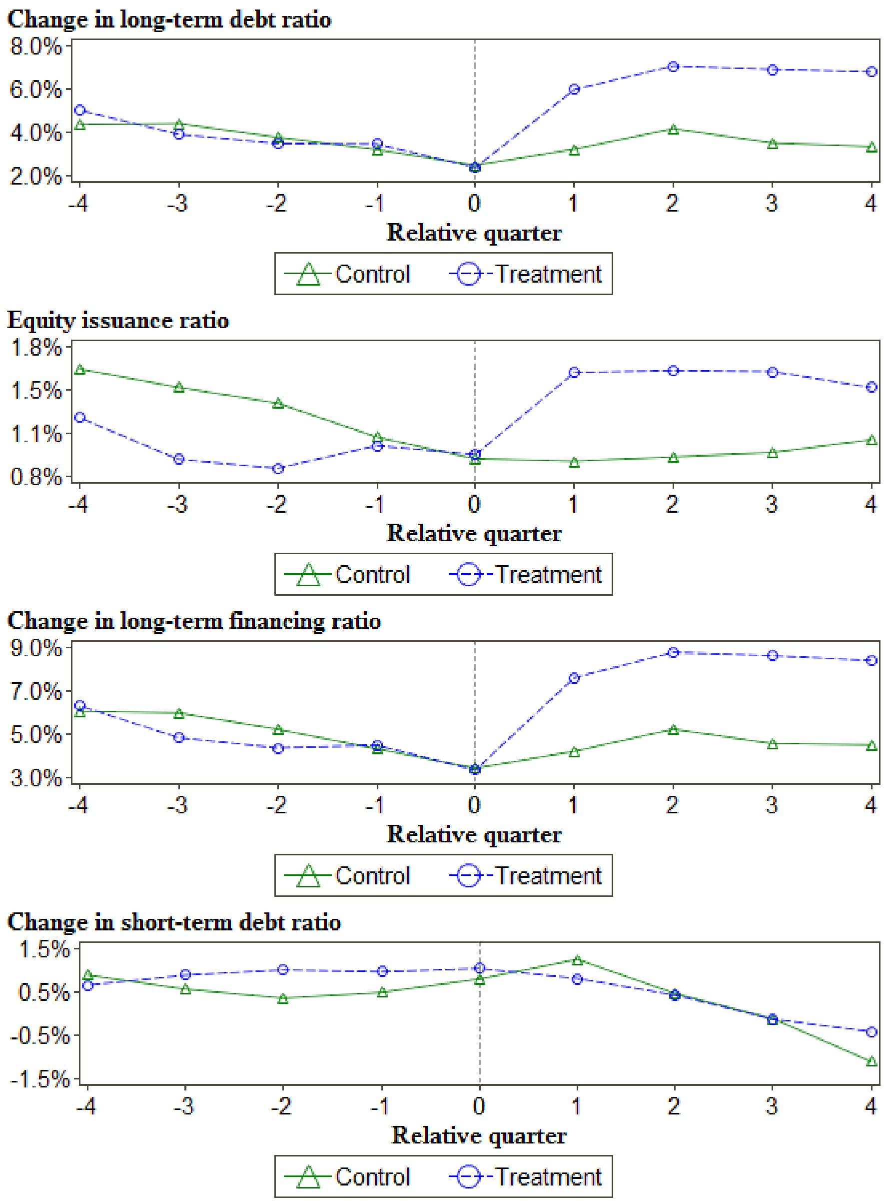
I present three robustness checks on the main results. First, I provide evidence in support of the parallel trends assumption underlying the difference-in-differences framework. Second, I demonstrate that my results are not sensitive to empirical methodologies. In this respect, I show that the documented post-watch patterns of corporate financing, investment, and profitability still hold under a propensity score matching framework. Third, I employ an endogenous switching regression model to address endogeneity concerns. I show that after controlling for endogeneity treatment firms still experience an increase in long-term financing, investment, and profitability following the watch period.

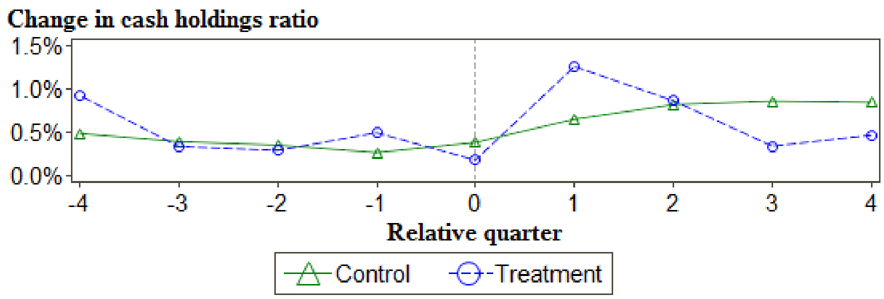
This chapter contributes to our understanding of the economic functions of rating agencies in financial markets. Based on the mechanism of a credit watch with direction downgrade, this chapter provides evidence that rating agencies act as third-party monitors, helping mitigate moral hazard faced by investors and alleviate information asymmetry in capital markets. In terms of future research, there are a number of interesting issues, which I do not address in this chapter. First, despite the evidence my results could be driven by a reverse causality problem, whereby watch resolution decisions are influenced by agencies' anticipation of future corporate dynamics. In this respect, further testing is needed. Second, an alternative explanation of my results could be motivated by agencies' debt certification rather than monitoring. Third, firms placed under watch with direction downgrade may have an incentive to manipulate earnings. It would be interesting to investigate whether firms report artificially high profitability, avoiding a rating downgrade.

Figure 2.1. Watch resolutions and corporate fundamentals.

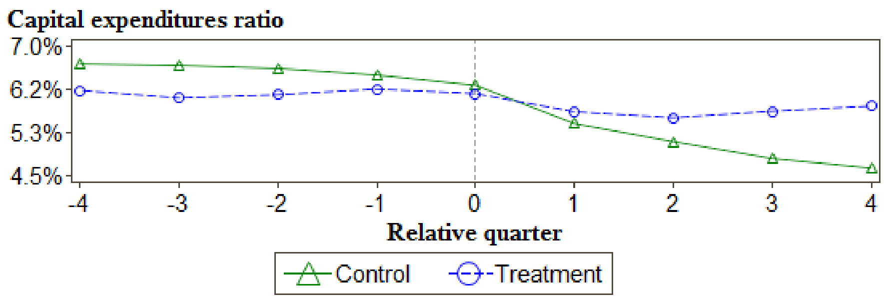
The figure displays the pattern of corporate financing (Panel A), investment (Panel B), and profitability (Panel C) for firms with confirmed ratings (treatment firms) and firms with downgraded ratings (control firms) from four quarters before to four quarters after the watch period. I ensure non-missing values for each corporate fundamentals measure in every quarter within the event window. Variables are defined in Appendix A. The baseline sample consists of 1,381 rating downgrades and 509 rating confirmations, for a total of 1,890 Moody's issuer-level watch actions between 1992 and 2011.

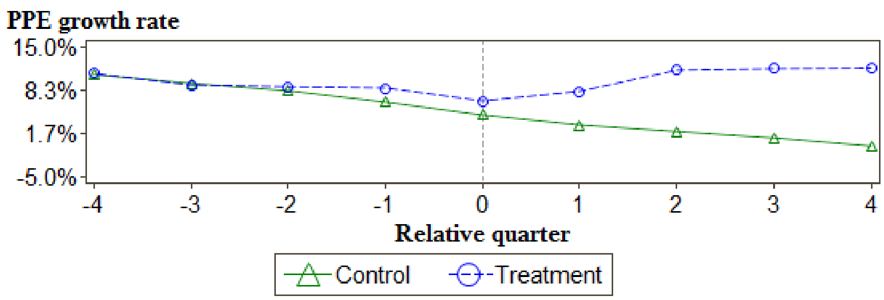
Panel A: Financing measures

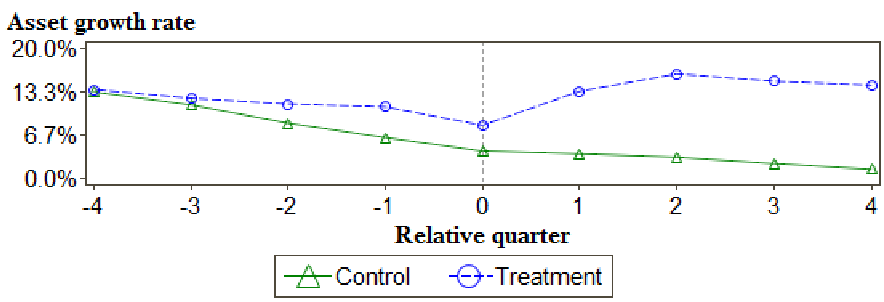




Panel B: Investment measures







Panel C: Profitability measures

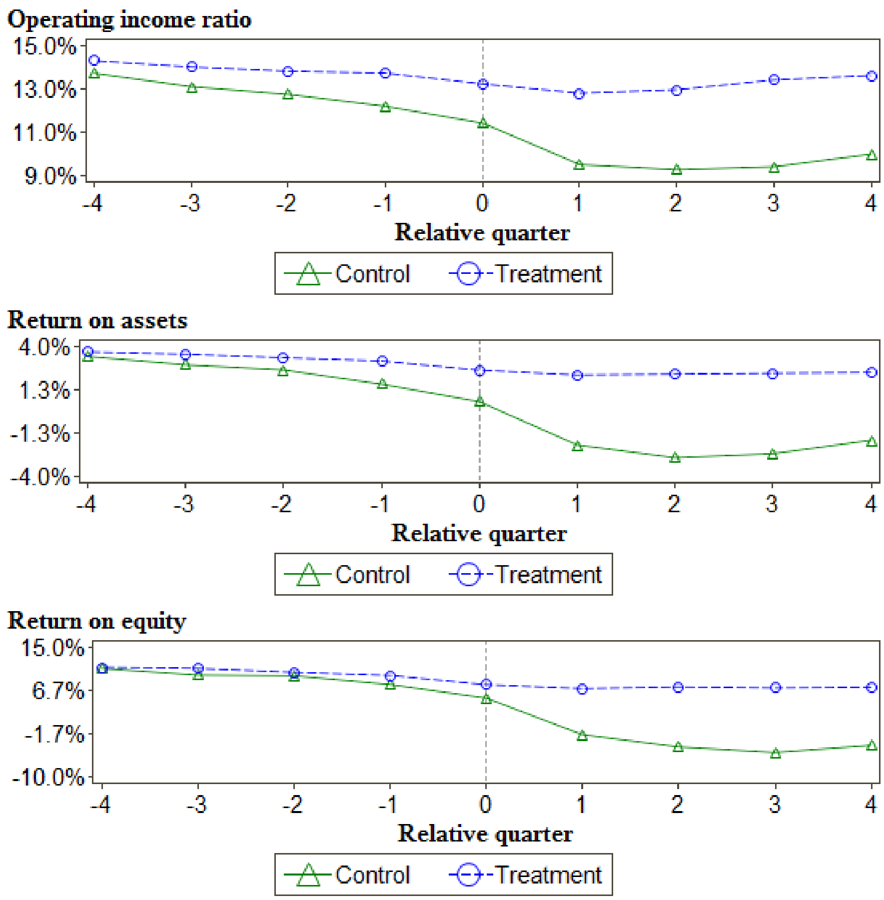

Operating income ratio
15.0%
13.0%
11.0%
9.0%
-4 -3 -2 -1 0 1 2 3 4
Relative quarter
Control
Treatment
Return on assets
4.0%
1.3%
-1.3%
-4.0%
-4 -3 -2 -1 0 1 2 3 4
Relative quarter
Control
Treatment
Return on equity
15.0%
6.7%
-1.7%
-10.0%
-4 -3 -2 -1 0 1 2 3 4
Relative quarter
Control
Treatment

Table 2.1. Sample distribution by calendar year.

The table shows the distribution of credit watches, rating confirmations, and rating downgrades by calendar year. The baseline sample consists of 509 rating confirmations and 1,381 rating downgrades, for a total of 1,890 Moody's issuer-level watch actions between 1992 and 2011.

	Watches		Rating confirmations		Rating downgrades	
Year	*N*	%	*N*	%	*N*	%
1992	17	0.9%	3	17.6%	14	82.4%
1993	64	3.4%	27	42.2%	37	57.8%
1994	56	3.0%	23	41.1%	33	58.9%
1995	52	2.8%	22	42.3%	30	57.7%
1996	66	3.5%	25	37.9%	41	62.1%
1997	68	3.6%	27	39.7%	41	60.3%
1998	82	4.3%	34	41.5%	48	58.5%
1999	107	5.7%	40	37.4%	67	62.6%
2000	135	7.1%	45	33.3%	90	66.7%
2001	180	9.5%	39	21.7%	141	78.3%
2002	252	13.3%	34	13.5%	218	86.5%
2003	158	8.4%	25	15.8%	133	84.2%
2004	93	4.9%	31	33.3%	62	66.7%
2005	97	5.1%	21	21.6%	76	78.4%
2006	93	4.9%	26	28.0%	67	72.0%
2007	95	5.0%	28	29.5%	67	70.5%
2008	98	5.2%	24	24.5%	74	75.5%
2009	110	5.8%	16	14.5%	94	85.5%
2010	34	1.8%	11	32.4%	23	67.6%
2011	33	1.7%	8	24.2%	25	75.8%
Overall	1,890	100.0%	509	26.9%	1,381	73.1%

Table 2.2. Rating downgrade statistics.
The table shows the transition probabilities of credit rating downgrades (Panel A) and their distribution by magnitude (Panel B). The baseline sample contains 509 rating confirmations and 1,381 rating downgrades, for a total of 1,890 Moody's issuer-level watch actions between 1992 and 2011.

Panel A: Transition probabilities of rating downgrades

	New rating category									
Prior rating category	Aaa	Aa	A	Baa	Ba	B	Caa	Ca	C	Overall
Aaa		14	1							15
		93.3%	6.7%							1.1%
Aa		32	63	2						97
		33.0%	64.9%	2.1%						7.0%
A			182	193	1					376
			48.4%	51.3%	0.3%					27.2%
Baa				297	207	4	3			511
				58.1%	40.5%	0.8%	0.6%			37.0%
Ba					85	111	3			199
					42.7%	55.8%	1.5%			14.4%
B						70	67	3		140
						50.0%	47.9%	2.1%		10.1%
Caa							30	11	1	42
							71.4%	26.2%	2.4%	3.0%
Ca									1	1
									100.0%	0.1%
Overall	0	46	246	492	293	185	103	14	2	1,381
	0.0%	3.3%	17.8%	35.6%	21.2%	13.4%	7.5%	1.0%	0.1%	100.0%

Panel B: Distribution of rating downgrades by magnitude

Magnitude	*N*	%
1	873	63.2%
2	379	27.4%
3	95	6.9%
4	24	1.7%
5	5	0.4%
6	2	0.1%
8	2	0.1%
9	1	0.1%
Overall	1,381	100.0%

Table 2.3. Summary statistics.

The table shows summary statistics of several credit watch characteristics (Panel A) and firm characteristics (Panel B) in the subsamples of credit rating confirmations (treatment firms) and downgrades (control firms). Variables are measured as of quarter 0, or the quarter ending prior to the watch period. The table also provides difference tests. All variables are defined in Appendix A. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The baseline sample consists of 509 rating confirmations and 1,381 rating downgrades, for a total of 1,890 Moody's issuer-level watch actions between 1992 and 2011.

	Treatment firms		Control firms		Treatment - Control	
	N	Mean	*N*	Mean	Mean	*t*-value
Panel A: Watch characteristics						
Watch days	509	143.3	1,381	92.5	50.8***	8.59
Non-investment grade	509	33.0%	1,381	27.7%	0.053**	2.22
Rating on watch	509	9.4	1,381	9.0	0.3	1.63
Rating off watch	509	9.4	1,381	10.6	-1.2***	-6.05
Rating change	509	0.0	1,381	1.5	-	-
Fallen angel	509	0.0%	1,381	15.6%	-	-
Panel B: Firm characteristics						
Financing						
Change in long-term debt ratio	455	2.4%	1,257	2.5%	-0.1%	-0.19
Equity issuance ratio	457	1.0%	1,271	0.9%	0.1%	0.23
Change in long-term financing ratio	455	3.3%	1,257	3.4%	-0.1%	-0.15
Change in short-term debt ratio	429	1.0%	1,170	0.8%	0.2%	0.68
Change in cash holdings ratio	457	0.2%	1,271	0.4%	-0.2%	-0.93
Investment						
Capital expenditures ratio	428	6.1%	1,213	6.2%	-0.1%	-0.51
PPE growth rate	453	6.7%	1,265	4.5%	2.2%	1.53
Asset growth rate	457	8.1%	1,271	4.2%	3.9%***	3.23
Profitability						
Operating income ratio	347	13.2%	993	11.4%	1.8%***	3.95
Return on assets	445	2.5%	1,245	0.6%	1.9%***	4.17
Return on equity	415	7.7%	1,123	5.1%	2.6%**	2.02
Others						
Assets (million $)	466	13,255.0	1,287	14,853.5	-1,598.5*	-1.73
Tobin's Q	426	1.10	1,177	0.92	0.18***	4.01
Tangibility	462	41.1%	1,282	39.2%	1.9%	1.56
Governance index	387	9.6	1,039	9.8	-0.2	-1.23
Cash flow investment gap ratio	420	1.7%	1,192	-0.5%	2.2%***	4.34

Table 2.4. Dynamic pattern of corporate fundamentals around the watch period.

This table presents the dynamic pattern of corporate financing (Panel A), investment (Panel B), and profitability (Panel C) from four quarters before to four quarters after the watch period. Results are for firms with confirmed ratings (treatment firms) benchmarked against firms with downgraded ratings (control firms). The dynamic pattern is estimated based on the following regression specification

$$Y_{it} = \alpha + \sum_{q=1}^{4} \delta_q Before_{it}^q \times Treatment_i + \sum_{q=1}^{4} \mu_q After_{it}^q \times Treatment_i + \sum_{q=1}^{4} \varphi_q Before_{it}^q + \sum_{q=1}^{4} \omega_q After_{it}^q + \theta Treatment_i$$

$$+\gamma X_{it} + Industry_i + Quarter_t + FiscalQuarter_{it} + \varepsilon_{it}, \qquad (1)$$

where Y_{it} is a financing, investment, or profitability measure; X_{it} is a set of control variables, which are: Ln(rating change + 1), Fallen angel, Ln(assets), Tobin's Q, and Tangibility; all variables are defined in Appendix A; $Before_{it}^q$ is a dummy variable equal to 1 if the observation is q quarters prior to the watch period, where $q = 1$, 2, 3, or 4 quarters; $After_{it}^q$ is a dummy variable equal to 1 if the observation is q quarters after the watch period, where $q = 1$, 2, 3, or 4 quarters; $Treatment$ is a dummy variable equal to 1 if the credit watch is resolved with a rating confirmation; i indexes firms; t indexes time measured in calendar quarters; $Industry_i$ are 38 SIC-based industry fixed effects; $Quarter_t$ are calendar quarter fixed effects; and $FiscalQuarter_{it}$ are fiscal quarter fixed effects. Standard errors are clustered at the firm and calendar quarter levels. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The baseline sample consists of 509 rating confirmations and 1,381 rating downgrades, for a total of 1,890 Moody's issuer-level watch actions between 1992 and 2011.

	Panel A: Financing									
Dependent variable	Change in long-term debt ratio		Equity issuance ratio		Change in long-term financing ratio		Change in short-term debt ratio		Change in cash holdings ratio	
	[1]		[2]		[3]		[4]		[5]	
	Coefficient	*t*-Stat	Coefficient	*t*-Stat	Coefficient	*t*-Stat	Coefficient	*t*-Stat	Coefficient	*t*-Stat
$Before^4$ × Treatment	0.0180	1.58	-0.0027	-0.95	0.0151	1.19	-0.0073	-1.05	0.0032	0.77
$Before^3$ × Treatment	0.0028	0.26	-0.0055**	-2.43	-0.0034	-0.29	-0.0004	-0.05	-0.0003	-0.13
$Before^2$ × Treatment	0.0014	0.15	-0.0050***	-3.30	-0.0040	-0.42	0.0036	0.55	-0.0012	-0.42
$Before^1$ × Treatment	0.0062	0.66	-0.0006	-0.39	0.0054	0.53	0.0018	0.27	0.0031	1.20
$After^1$ × Treatment	0.0272**	2.40	0.0047**	2.34	0.0314***	2.66	-0.0078	-0.93	0.0082***	3.02
$After^2$ × Treatment	0.0291**	2.35	0.0043**	2.00	0.0329**	2.50	-0.0065	-0.73	-0.0001	-0.04
$After^3$ × Treatment	0.0341**	2.21	0.0038*	1.74	0.0385**	2.40	-0.0066	-0.66	-0.0039	-1.11
$After^4$ × Treatment	0.0330***	2.65	0.0019	0.90	0.0354***	2.70	0.0007	0.09	-0.0034	-0.90
Treatment	-0.0056	-0.46	0.0042	1.56	-0.0004	-0.04	0.0076	0.95	0.0026	1.00

Before^4	0.0089	1.43	0.0043**	2.22	0.0139*	1.87	0.0001	0.03	0.0008	0.42
Before^3	0.0129**	2.23	0.0044***	2.80	0.0181***	2.62	-0.0033	-1.10	-0.0001	-0.10
Before^2	0.0097**	2.40	0.0036***	3.58	0.0141***	3.05	-0.0051	-1.52	-0.0009	-0.60
Before^1	0.0054	1.50	0.0013***	3.82	0.0070*	1.78	-0.0035	-1.24	-0.0019	-1.57
After^1	0.0133**	2.24	0.0005	0.60	0.0143**	2.18	0.0058*	1.66	0.0028*	1.72
After^2	0.0262***	3.31	0.0008	0.69	0.0281***	3.32	-0.0010	-0.24	0.0043**	2.37
After^3	0.0214**	2.46	0.0013	1.04	0.0232**	2.51	-0.0062	-1.30	0.0045**	2.47
After^4	0.0204***	2.72	0.0019*	1.67	0.0230***	2.84	-0.0162***	-3.34	0.0034	1.64
Ln(rating change + 1)	0.0098	1.03	0.0052	1.62	0.0162	1.56	0.0072	1.37	0.0009	0.55
Fallen angel	-0.0119**	-2.24	0.0005	0.35	-0.0118**	-1.99	-0.0062***	-2.62	0.0020	1.53
Ln(assets)	0.0016	0.81	-0.0016***	-2.72	-0.0003	-0.17	0.001	1.50	0.0007	1.27
Tobin's Q	0.0111***	3.55	0.0067***	5.05	0.0169***	4.40	0.0023*	1.89	0.0035***	3.25
Tangibility	-0.016	-0.83	-0.0010	-0.17	-0.0182	-0.78	0.0000	0.00	-0.0195***	-4.98
Intercept	0.0622	1.21	0.0172	1.34	0.0810	1.32	-0.0033	-0.39	-0.0173**	-2.05
N	14,139		14,175		14,139		14,076		14,175	
Adjusted R^2	0.074		0.072		0.074		0.053		0.041	
Difference tests										
After^1 × Treatment - Before^1 × Treatment	0.0209**	2.25	0.0054**	2.07	0.0260**	2.54	-0.0096*	-1.87	0.0051	1.53
After^2 × Treatment - Before^1 × Treatment	0.0228**	2.45	0.0049*	1.89	0.0274***	2.68	-0.0083	-1.63	-0.0032	-0.98
After^3 × Treatment - Before^1 × Treatment	0.0278***	2.99	0.0045*	1.72	0.0330***	3.22	-0.0084	-1.63	-0.0071**	-2.13
After^4 × Treatment - Before^1 × Treatment	0.0267***	2.87	0.0025	0.97	0.0300***	2.92	-0.0010	-0.21	-0.0065**	-1.96

Panel B: Investment

Dependent variable	Capital expenditures ratio		PPE growth rate		Asset growth rate	
	[6]		[7]		[8]	
	Coefficient	*t*-Stat	Coefficient	*t*-Stat	Coefficient	*t*-Stat
Before^4 × Treatment	-0.0017	-0.64	-0.0006	-0.02	-0.0157	-0.61
Before^3 × Treatment	-0.0032	-1.54	-0.0074	-0.34	-0.0124	-0.50
Before^2 × Treatment	-0.0021*	-1.85	-0.0037	-0.23	-0.0007	-0.04
Before^1 × Treatment	-0.0003	-0.38	0.0025	0.12	0.0130	0.71

$After^1 \times$ Treatment	0.0037	1.40	0.0224	1.15	0.0429*	1.83
$After^2 \times$ Treatment	0.0060**	2.26	0.0669***	3.02	0.0668***	2.68
$After^3 \times$ Treatment	0.0100***	3.21	0.0765***	3.11	0.0662**	2.52
$After^4 \times$ Treatment	0.0133***	3.98	0.0890***	3.59	0.0721**	2.59
Treatment	-0.0029	-0.79	-0.0210	-0.89	-0.0102	-0.43
$Before^4$	0.0009	0.48	0.0387**	2.47	0.0579***	3.42
$Before^3$	0.0016	1.05	0.0316**	2.54	0.0481***	3.30
$Before^2$	0.0017*	1.79	0.0258**	2.52	0.0279***	3.66
$Before^1$	0.0012**	2.09	0.0141**	2.26	0.0125**	2.40
$After^1$	-0.0057***	-5.99	-0.0022	-0.27	0.0090	0.99
$After^2$	-0.0089***	-6.72	-0.0068	-0.55	0.0112	0.83
$After^3$	-0.0118***	-7.11	-0.0121	-0.93	0.0052	0.36
$After^4$	-0.0137***	-6.62	-0.0260	-1.59	-0.0079	-0.47
Ln(rating change + 1)	0.0027	0.86	-0.0225	-1.12	-0.0254	-1.13
Fallen angel	-0.0010	-0.46	-0.0133	-1.14	-0.0214*	-1.68
Ln(assets)	-0.0006	-0.62	0.0131***	2.83	0.0180***	3.66
Tobin's Q	0.0141***	5.59	0.0268***	3.14	0.0300***	3.74
Tangibility	0.1036***	10.03	0.0600	1.16	-0.1596***	-3.25
Intercept	0.0513	1.34	-0.0370	-0.35	0.0799	0.77
N	13,527		14,166		14,175	
Adjusted R^2	0.417		0.087		0.129	
Difference tests						
$After^1 \times$ Treatment - $Before^1 \times$ Treatment	0.0041	1.28	0.0199	0.97	0.0298	1.39
$After^2 \times$ Treatment - $Before^1 \times$ Treatment	0.0064**	2.02	0.0644***	3.15	0.0537**	2.51
$After^3 \times$ Treatment - $Before^1 \times$ Treatment	0.0104***	3.25	0.0740***	3.62	0.0532**	2.48
$After^4 \times$ Treatment - $Before^1 \times$ Treatment	0.0137***	4.27	0.0865***	4.22	0.0590***	2.75

Panel C: Profitability

Dependent variable	Operating income ratio		Return on assets		Return on equity	
	[9]		[10]		[11]	
	Coefficient	*t*-Stat	Coefficient	*t*-Stat	Coefficient	*t*-Stat

$Before^4 \times$ Treatment	-0.0004	-0.08	-0.0018	-0.38	0.0098	0.53
$Before^3 \times$ Treatment	0.0021	0.48	0.0002	0.06	0.0168	1.14
$Before^2 \times$ Treatment	0.0015	0.29	-0.0020	-0.36	0.0045	0.28
$Before^1 \times$ Treatment	0.0011	0.21	-0.0012	-0.27	0.0002	0.02
$After^1 \times$ Treatment	0.0120***	2.93	0.0209***	3.31	0.0591***	3.62
$After^2 \times$ Treatment	0.0178***	4.58	0.0306***	4.20	0.0873***	4.81
$After^3 \times$ Treatment	0.0213***	3.88	0.0292***	3.60	0.0976***	4.09
$After^4 \times$ Treatment	0.0199***	4.17	0.0254***	3.48	0.0911***	4.16
Treatment	-0.0179**	-2.43	-0.0295****	-3.65	-0.1133***	-4.33
$Before^4$	0.0090***	4.26	0.0114***	3.65	0.0229**	2.57
$Before^3$	0.0067***	3.57	0.0096***	3.05	0.0168*	1.82
$Before^2$	0.0055***	2.78	0.0099***	2.84	0.0221**	2.15
$Before^1$	0.0042***	5.92	0.0058*	1.79	0.0159**	2.06
$After^1$	-0.0138***	-11.57	-0.0209***	-4.87	-0.0571***	-5.65
$After^2$	-0.0166***	-9.82	-0.0299***	-5.91	-0.0799***	-8.61
$After^3$	-0.0156***	-6.02	-0.0291***	-5.12	-0.0908***	-6.97
$After^4$	-0.0118***	-3.75	-0.0236***	-4.19	-0.0820***	-5.83
Ln(rating change + 1)	-0.0212***	-3.47	-0.0380***	-4.71	-0.1100***	-4.45
Fallen angel	-0.0048	-1.47	-0.0026	-0.64	-0.0046	-0.40
Ln(assets)	0.0033**	2.16	0.0069***	3.91	0.0246***	4.44
Tobin's Q	0.0530***	8.94	0.0416***	10.66	0.0945***	8.15
Tangibility	0.0419***	3.26	-0.0013	-0.10	0.0399	1.00
Intercept	0.0453*	1.72	-0.0079	-0.31	-0.0533	-0.85
N	11,178		13,851		12,879	
Adjusted R^2	0.470		0.297		0.238	
Difference tests						
$After^1 \times$ Treatment - $Before^1 \times$ Treatment	0.0108**	2.18	0.0221***	3.39	0.0588***	3.18
$After^2 \times$ Treatment - $Before^1 \times$ Treatment	0.0166***	3.35	0.0318***	4.88	0.0871***	4.71
$After^3 \times$ Treatment - $Before^1 \times$ Treatment	0.0201***	4.05	0.0304***	4.67	0.0973***	5.26
$After^4 \times$ Treatment - $Before^1 \times$ Treatment	0.0188***	3.77	0.0267***	4.09	0.0908***	4.91

Table 2.5. The effects of treatment on the dynamic pattern of corporate fundamentals conditional upon credit quality.

Firms are sorted into non-investment (Moody's Ba rating or lower) and investment (Moody's Baa rating or higher) grade groups. This table presents for each of those groups the dynamic pattern of corporate financing (Panel A), investment (Panel B), and profitability (Panel C) from four quarters before to four quarters after the watch period. Results are for firms with confirmed ratings (treatment firms) benchmarked against firms with downgraded ratings (control firms). The dynamic pattern is estimated for the non-investment and investment grade firm groups separately (along with difference tests) based on the following regression specification

$$Y_{it} = \alpha + \sum_{q=1}^{4} \delta_q Before_{it}^{q} \times Treatment_i + \sum_{q=1}^{4} \mu_q After_{it}^{q} \times Treatment_i + \sum_{q=1}^{4} \varphi_q Before_{it}^{q}$$

$$+ \textstyle\sum_{q=1}^{4} \omega_q After_{it}^{q} + \theta Treatment_i + \gamma X_{it} + Industry_i + Quarter_t + FiscalQuarter_{it} + \varepsilon_{it}, \quad (1)$$

where Y_{it} is a financing, investment, or profitability measure; X_{it} is a set of control variables, which are: Ln(rating change + 1), Fallen angel, Ln(assets), Tobin's Q, and Tangibility; for brevity of presentation, only the coefficients on the variables of interest are reported here; all variables are defined in Appendix A; $Before_{it}^{q}$ is a dummy variable equal to 1 if the observation is q quarters prior to the watch period, where q = 1, 2, 3, or 4 quarters; $After_{it}^{q}$ is a dummy variable equal to 1 if the observation is q quarters after the watch period, where q = 1, 2, 3, or 4 quarters; $Treatment_i$ is a dummy variable equal to 1 if the credit watch is resolved with a rating confirmation; i indexes firms; t indexes time measured in calendar quarters; $Industry_i$ are 38 SIC-based industry fixed effects; $Quarter_t$ are calendar quarter fixed effects; and $FiscalQuarter_{it}$ are fiscal quarter fixed effects. Standard errors are clustered at the firm and calendar quarter levels. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The baseline sample consists of 509 rating confirmations and 1,381 rating downgrades, for a total of 1,890 Moody's issuer-level watch actions between 1992 and 2011.

	Non-investment		Investment		Non-invest. - Invest.	
	Coefficient	*t*-Stat	Coefficient	*t*-Stat	Coefficient	*t*-Stat
Panel A: Dependent variable is change in long-term debt ratio						
$Before^4$ × Treatment	0.0805***	2.88	-0.001	-0.09	0.0815***	3.91
$Before^3$ × Treatment	0.0341	1.19	-0.0047	-0.52	0.0388*	1.86
$Before^2$ × Treatment	0.0219	0.89	-0.003	-0.37	0.0249	1.20
$Before^1$ × Treatment	0.0207	0.94	0.0027	0.38	0.0179	0.86
$After^1$ × Treatment	0.0632**	2.11	0.0124	1.32	0.0507**	2.44
$After^2$ × Treatment	0.0692**	2.09	0.0139	1.36	0.0553***	2.66
$After^3$ × Treatment	0.0793**	2.02	0.0187	1.59	0.0606***	2.91
$After^4$ × Treatment	0.0723**	2.52	0.0186*	1.70	0.0536***	2.58
N	3,555		10,584			
Adjusted R^2	0.138		0.081			
Difference tests						
$After^1$ × Treatment - $Before^1$ × Treatment	0.0425*	1.91	0.0097	0.99	0.0327	1.58
$After^2$ × Treatment - $Before^1$ × Treatment	0.0485**	2.18	0.0111	1.14	0.0373*	1.79
$After^3$ × Treatment - $Before^1$ × Treatment	0.0586***	2.64	0.0159	1.63	0.0426**	2.05
$After^4$ × Treatment - $Before^1$ × Treatment	0.0516**	2.31	0.0159	1.63	0.0357*	1.71
Panel B: Dependent variable is equity issuance ratio						

$\text{Before}^4 \times$ Treatment	-0.0049	-0.83	-0.0011	-0.38	-0.0038	-0.66
$\text{Before}^3 \times$ Treatment	-0.0075	-1.47	-0.0042***	-2.71	-0.0032	-0.56
$\text{Before}^2 \times$ Treatment	-0.0066*	-1.76	-0.0041***	-2.67	-0.0025	-0.45
$\text{Before}^1 \times$ Treatment	0.0027	1.01	-0.0016	-1.31	0.0044	0.77
$\text{After}^1 \times$ Treatment	0.0115*	1.81	0.0021	1.40	0.0094	1.63
$\text{After}^2 \times$ Treatment	0.0097	1.41	0.0025	1.56	0.0071	1.25
$\text{After}^3 \times$ Treatment	0.0118*	1.86	0.0008	0.53	0.0109*	1.90
$\text{After}^4 \times$ Treatment	0.0080	1.22	-0.0009	-0.62	0.0090	1.57
N	3,555		10,620			
Adjusted R^2	0.135		0.107			
Difference tests						
$\text{After}^1 \times$ Treatment - $\text{Before}^1 \times$ Treatment	0.0087	1.13	0.0037	1.73	0.0049	0.87
$\text{After}^2 \times$ Treatment - $\text{Before}^1 \times$ Treatment	0.0069	0.90	0.0042*	1.95	0.0027	0.48
$\text{After}^3 \times$ Treatment - $\text{Before}^1 \times$ Treatment	0.0090	1.17	0.0025	1.16	0.0065	1.13
$\text{After}^4 \times$ Treatment - $\text{Before}^1 \times$ Treatment	0.0053	0.68	0.0007	0.33	0.0045	0.79
Panel C: Dependent variable is change in long-term financing ratio						
$\text{Before}^4 \times$ Treatment	0.0770***	2.72	-0.0027	-0.21	0.0798***	3.49
$\text{Before}^3 \times$ Treatment	0.0256	0.89	-0.0094	-0.98	0.0351	1.53
$\text{Before}^2 \times$ Treatment	0.0146	0.62	-0.0072	-0.85	0.0219	0.96
$\text{Before}^1 \times$ Treatment	0.0224	1.04	0.0011	0.15	0.0213	0.93
$\text{After}^1 \times$ Treatment	0.0744**	2.47	0.0137	1.38	0.0606***	2.66
$\text{After}^2 \times$ Treatment	0.0789**	2.28	0.0154	1.44	0.0635***	2.78
$\text{After}^3 \times$ Treatment	0.0940**	2.32	0.0187	1.55	0.0753***	3.29
$\text{After}^4 \times$ Treatment	0.0817***	2.61	0.0176	1.55	0.0640***	2.80
N	3,555		10,584			
Adjusted R^2	0.145		0.080			
Difference tests						
$\text{After}^1 \times$ Treatment - $\text{Before}^1 \times$ Treatment	0.0519**	2.05	0.0126	1.20	0.0393*	1.72
$\text{After}^2 \times$ Treatment - $\text{Before}^1 \times$ Treatment	0.0565**	2.23	0.0143	1.36	0.0422*	1.84
$\text{After}^3 \times$ Treatment - $\text{Before}^1 \times$ Treatment	0.0716***	2.83	0.0175*	1.67	0.0540**	2.37
$\text{After}^4 \times$ Treatment - $\text{Before}^1 \times$ Treatment	0.0592**	2.32	0.0165	1.58	0.0427*	1.86
Panel D: Dependent variable is change in short-term debt ratio						
$\text{Before}^4 \times$ Treatment	-0.0139	-0.84	-0.0035	-0.55	-0.0104	-0.91
$\text{Before}^3 \times$ Treatment	0.0067	0.32	-0.0025	-0.45	0.0093	0.80
$\text{Before}^2 \times$ Treatment	0.0168	0.80	-0.0009	-0.20	0.0178	1.55
$\text{Before}^1 \times$ Treatment	0.0128	0.72	-0.0016	-0.42	0.0144	1.25
$\text{After}^1 \times$ Treatment	-0.0218	-1.05	-0.0009	-0.13	-0.0209*	-1.81
$\text{After}^2 \times$ Treatment	-0.0164	-0.80	-0.0015	-0.19	-0.0148	-1.28
$\text{After}^3 \times$ Treatment	-0.0152	-0.62	-0.0029	-0.43	-0.0122	-1.06
$\text{After}^4 \times$ Treatment	-0.0054	-0.35	0.0030	0.40	-0.0084	-0.73
N	3,519		10,557			

Adjusted R^2	0.109		0.062			
Difference tests						
$After^1$ × Treatment - $Before^1$ × Treatment	-0.0346***	-3.24	0.0007	0.13	-0.0354***	-3.06
$After^2$ × Treatment - $Before^1$ × Treatment	-0.0292***	-2.73	0.0000	0.02	-0.0293**	-2.53
$After^3$ × Treatment - $Before^1$ × Treatment	-0.0280***	-2.63	-0.0013	-0.23	-0.0267**	-2.31
$After^4$ × Treatment - $Before^1$ × Treatment	-0.0182*	-1.70	0.0046	0.79	-0.0228*	-1.97
Panel E: Dependent variable is change in cash holdings ratio						
$Before^4$ × Treatment	-0.0022	-0.24	0.0064	1.26	-0.0087	-1.16
$Before^3$ × Treatment	-0.0030	-0.49	0.0018	0.54	-0.0049	-0.65
$Before^2$ × Treatment	-0.0078	-1.26	0.0021	0.63	-0.0099	-1.33
$Before^1$ × Treatment	-0.0022	-0.44	0.0055**	2.12	-0.0078	-1.05
$After^1$ × Treatment	0.0143**	2.48	0.0070**	2.10	0.0073	0.98
$After^2$ × Treatment	0.0023	0.38	0.0001	0.04	0.0021	0.29
$After^3$ × Treatment	-0.0008	-0.10	-0.0044	-1.12	0.0035	0.48
$After^4$ × Treatment	-0.0032	-0.38	-0.0037	-0.83	0.0005	0.08
N	3,555		10,620			
Adjusted R^2	0.078		0.051			
Difference tests						
$After^1$ × Treatment - $Before^1$ × Treatment	0.0166**	2.06	0.0014	0.40	0.0152**	2.03
$After^2$ × Treatment - $Before^1$ × Treatment	0.0045	0.57	-0.0054	-1.55	0.0100	1.34
$After^3$ × Treatment - $Before^1$ × Treatment	0.0014	0.18	-0.0100***	-2.85	0.0114	1.53
$After^4$ × Treatment - $Before^1$ × Treatment	-0.0009	-0.12	-0.0093***	-2.66	0.0084	1.12
Panel F: Dependent variable is capital expenditures ratio						
$Before^4$ × Treatment	0.0043	0.55	-0.0034	-1.18	0.0078	1.08
$Before^3$ × Treatment	0.0024	0.52	-0.0050**	-2.06	0.0074	1.03
$Before^2$ × Treatment	0.0024	0.46	-0.0034	-1.42	0.0058	0.82
$Before^1$ × Treatment	0.0014	0.33	-0.0007	-0.28	0.0021	0.30
$After^1$ × Treatment	0.0074*	1.72	0.0025	0.68	0.0049	0.69
$After^2$ × Treatment	0.0124**	2.43	0.0043	1.15	0.0080	1.12
$After^3$ × Treatment	0.0178***	2.83	0.0080**	2.18	0.0098	1.36
$After^4$ × Treatment	0.0236***	3.59	0.0100**	2.46	0.0135*	1.89
N	3,312		10,215			
Adjusted R^2	0.497		0.419			
Difference tests						
$After^1$ × Treatment - $Before^1$ × Treatment	0.0060	0.83	0.0032	0.95	0.0028	0.39
$After^2$ × Treatment - $Before^1$ × Treatment	0.0110	1.50	0.0051	1.50	0.0058	0.82
$After^3$ × Treatment - $Before^1$ × Treatment	0.0164**	2.24	0.0087***	2.56	0.0076	1.07
$After^4$ × Treatment - $Before^1$ × Treatment	0.0221***	3.00	0.0107***	3.14	0.0114	1.58
Panel G: Dependent variable is PPE growth rate						
$Before^4$ × Treatment	0.0936**	2.04	-0.029	-0.96	0.1229***	2.69
$Before^3$ × Treatment	0.0421	1.16	-0.023	-0.83	0.0653	1.43

Before^2 × Treatment	0.0275	0.83	-0.012	-0.53	0.0399	0.88
Before^1 × Treatment	0.0209	0.62	-0.000	-0.01	0.0212	0.47
After^1 × Treatment	0.0649*	1.68	0.0050	0.21	0.0598	1.32
After^2 × Treatment	0.1411***	3.10	0.0382	1.58	0.1029**	2.26
After^3 × Treatment	0.1674***	3.36	0.0422*	1.72	0.1252***	2.74
After^4 × Treatment	0.1842***	3.46	0.0505*	1.68	0.1336***	2.94
N	3,546		10,620			
Adjusted R^2	0.177		0.091			
Difference tests						
After^1 × Treatment - Before^1 × Treatment	0.0439	0.96	0.0052	0.24	0.0386	0.85
After^2 × Treatment - Before^1 × Treatment	0.1202***	2.61	0.0384*	1.74	0.0817*	1.79
After^3 × Treatment - Before^1 × Treatment	0.1465***	3.19	0.0424*	1.92	0.1040**	2.29
After^4 × Treatment - Before^1 × Treatment	0.1632***	3.53	0.0507**	2.29	0.1124**	2.46
Panel H: Dependent variable is asset growth rate						
Before^4 × Treatment	0.1169***	3.12	-0.0584*	-1.94	0.1754***	3.65
Before^3 × Treatment	0.0764**	2.35	-0.0433	-1.40	0.1198**	2.49
Before^2 × Treatment	0.0568**	2.31	-0.0224	-0.84	0.0792*	1.65
Before^1 × Treatment	0.0579**	2.37	-0.0017	-0.08	0.0597	1.24
After^1 × Treatment	0.0925***	2.85	0.0253	0.85	0.0671	1.40
After^2 × Treatment	0.1534***	4.64	0.0360	1.11	0.1173**	2.44
After^3 × Treatment	0.1617***	4.11	0.0294	0.96	0.1322***	2.75
After^4 × Treatment	0.1729***	4.31	0.0297	0.84	0.1432***	2.99
N	3,555		10,620			
Adjusted R^2	0.220		0.122			
Difference tests						
After^1 × Treatment - Before^1 × Treatment	0.0346	0.81	0.0271	1.10	0.0074	0.15
After^2 × Treatment - Before^1 × Treatment	0.0954**	2.24	0.0378	1.53	0.0576	1.20
After^3 × Treatment - Before^1 × Treatment	0.1037**	2.44	0.0312	1.27	0.0724	1.51
After^4 × Treatment - Before^1 × Treatment	0.1149***	2.68	0.0315	1.28	0.0834*	1.73
Panel I: Dependent variable is operating income ratio						
Before^4 × Treatment	0.0071	0.73	-0.0068	-0.99	0.0140	1.32
Before^3 × Treatment	0.0042	0.41	-0.0014	-0.24	0.0057	0.54
Before^2 × Treatment	-0.0004	-0.06	0.0005	0.09	-0.0010	-0.10
Before^1 × Treatment	-0.0015	-0.19	0.0012	0.17	-0.0027	-0.26
After^1 × Treatment	0.0186**	2.35	0.0087*	1.73	0.0099	0.94
After^2 × Treatment	0.0229**	2.12	0.0150***	3.48	0.0078	0.74
After^3 × Treatment	0.0256**	2.23	0.0178***	3.48	0.0077	0.74
After^4 × Treatment	0.0249***	2.91	0.0155***	2.92	0.0094	0.89
N	2,961		8,217			
Adjusted R^2	0.315		0.548			
Difference tests						

$After^1 \times$ Treatment - $Before^1 \times$ Treatment	0.0202*	1.82	0.0074	1.46	0.0127	1.20
$After^2 \times$ Treatment - $Before^1 \times$ Treatment	0.0244**	2.21	0.0138***	2.71	0.0105	1.00
$After^3 \times$ Treatment - $Before^1 \times$ Treatment	0.0271**	2.46	0.0166***	3.25	0.0105	1.00
$After^4 \times$ Treatment - $Before^1 \times$ Treatment	0.0264**	2.38	0.0143***	2.79	0.0121	1.15
Panel J: Dependent variable is return on assets						
$Before^4 \times$ Treatment	-0.0039	-0.33	-0.0046	-0.87	0.0007	0.05
$Before^3 \times$ Treatment	-0.0052	-0.44	-0.0012	-0.24	-0.0040	-0.29
$Before^2 \times$ Treatment	-0.0112	-1.04	-0.0012	-0.20	-0.0100	-0.72
$Before^1 \times$ Treatment	-0.0056	-0.59	-0.0006	-0.13	-0.0049	-0.36
$After^1 \times$ Treatment	0.0430***	3.86	0.0139*	1.86	0.0290**	2.09
$After^2 \times$ Treatment	0.0547***	5.59	0.0233***	2.66	0.0313**	2.25
$After^3 \times$ Treatment	0.0457***	3.17	0.0231***	2.58	0.0225	1.62
$After^4 \times$ Treatment	0.0384**	2.49	0.0206**	2.37	0.0178	1.28
N	3,528		10,323			
Adjusted R^2	0.333		0.299			
Difference tests						
$After^1 \times$ Treatment - $Before^1 \times$ Treatment	0.0486***	3.25	0.0146**	2.23	0.0340**	2.44
$After^2 \times$ Treatment - $Before^1 \times$ Treatment	0.0603***	4.02	0.0239***	3.65	0.0363***	2.61
$After^3 \times$ Treatment - $Before^1 \times$ Treatment	0.0513***	3.44	0.0238***	3.63	0.0275*	1.98
$After^4 \times$ Treatment - $Before^1 \times$ Treatment	0.0440***	2.93	0.0212***	3.24	0.0228	1.63
Panel K: Dependent variable is return on equity						
$Before^4 \times$ Treatment	0.0877*	1.82	-0.0194	-1.21	0.1072***	2.61
$Before^3 \times$ Treatment	0.0752	1.61	-0.0076	-0.59	0.0829**	2.01
$Before^2 \times$ Treatment	0.0317	0.71	-0.0098	-0.63	0.0416	1.01
$Before^1 \times$ Treatment	0.0130	0.58	-0.0095	-0.75	0.0226	0.55
$After^1 \times$ Treatment	0.1415***	6.30	0.0361**	2.16	0.1054**	2.56
$After^2 \times$ Treatment	0.1849***	3.56	0.0608***	3.65	0.1241***	3.02
$After^3 \times$ Treatment	0.1968***	3.28	0.0661***	3.61	0.1306***	3.18
$After^4 \times$ Treatment	0.2166***	3.16	0.0533***	2.91	0.1632***	3.98
N	2,889		9,990			
Adjusted R^2	0.234		0.260			
Difference tests						
$After^1 \times$ Treatment - $Before^1 \times$ Treatment	0.1285**	2.42	0.0457***	2.74	0.0827**	2.01
$After^2 \times$ Treatment - $Before^1 \times$ Treatment	0.1719***	3.24	0.0704***	4.22	0.1015**	2.47
$After^3 \times$ Treatment - $Before^1 \times$ Treatment	0.1837***	3.47	0.0757***	4.55	0.1080***	2.63
$After^4 \times$ Treatment - $Before^1 \times$ Treatment	0.2035***	3.81	0.0629***	3.77	0.1406***	3.40

Table 2.6. The effects of treatment on the dynamic pattern of corporate financing conditional upon financial constraints.

Firms are sorted into financially constrained and unconstrained groups based on the median value of the cash flow-investment gap ratio (see Appendix A). This table presents for each of those groups the dynamic pattern of corporate financing from four quarters before to four quarters after the watch period. Results are for firms with confirmed ratings (treatment firms) benchmarked against firms with downgraded ratings (control firms). The dynamic pattern is estimated for the financially constrained and unconstrained firm groups separately (along with difference tests) based on the following regression specification

$$Y_{it} = \alpha + \sum_{q=1}^{4} \delta_q Before_{it}^{q} \times Treatment_i + \sum_{q=1}^{4} \mu_q After_{it}^{q} \times Treatment_i + \sum_{q=1}^{4} \varphi_q Before_{it}^{q}$$

$$+ \sum_{q=1}^{4} \omega_q After_{it}^{q} + \theta Treatment_i + \gamma X_{it} + Industry_i + Quarter_t + FiscalQuarter_{it} + \varepsilon_{it}, (1)$$

where Y_{it} is a financing measure; X_{it} is a set of control variables, which are: Ln(rating change + 1), Fallen angel, Ln(assets), Tobin's Q, and Tangibility; for brevity of presentation, only the coefficients on the variables of interest are reported here; all variables are defined in Appendix A; $Before_{it}^{q}$ is a dummy variable equal to 1 if the observation is q quarters prior to the watch period, where $q =$ 1, 2, 3, or 4 quarters; $After_{it}^{q}$ is a dummy variable equal to 1 if the observation is q quarters after the watch period, where $q =$ 1, 2, 3, or 4 quarters; $Treatment_i$ is a dummy variable equal to 1 if the credit watch is resolved with a rating confirmation; i indexes firms; t indexes time measured in calendar quarters; $Industry_i$ are 38 SIC-based industry fixed effects; $Quarter_t$ are calendar quarter fixed effects; and $FiscalQuarter_{it}$ are fiscal quarter fixed effects. Standard errors are clustered at the firm and calendar quarter levels. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The baseline sample consists of 509 rating confirmations and 1,381 rating downgrades, for a total of 1,890 Moody's issuer-level watch actions between 1992 and 2011.

	Constrained		Unconstrained		Const. - Unconst.	
	Coefficient	*t*-Stat	Coefficient	*t*-Stat	Coefficient	*t*-Stat
Panel A: Dependent variable is change in long-term debt ratio						
$Before^4$ × Treatment	0.0291	1.54	0.0022	0.17	0.0268	1.41
$Before^3$ × Treatment	0.0065	0.29	-0.0040	-0.31	0.0105	0.55
$Before^2$ × Treatment	0.0029	0.16	-0.0035	-0.30	0.0065	0.34
$Before^1$ × Treatment	0.0078	0.40	0.0069	0.79	0.0009	0.05
$After^1$ × Treatment	0.0487**	2.15	0.0031	0.28	0.0456**	2.40
$After^2$ × Treatment	0.0495**	2.06	0.0021	0.17	0.0473**	2.49
$After^3$ × Treatment	0.0600**	2.08	0.0023	0.17	0.0577***	3.04
$After^4$ × Treatment	0.0416**	2.22	0.0123	0.95	0.0292	1.54
N	6,624		6,624			
Adjusted R^2	0.112		0.104			
Difference tests						
$After^1$ × Treatment - $Before^1$ × Treatment	0.0409***	2.96	-0.0037	-0.29	0.0446**	2.35
$After^2$ × Treatment - $Before^1$ × Treatment	0.0416***	3.01	-0.0047	-0.37	0.0464**	2.44
$After^3$ × Treatment - $Before^1$ × Treatment	0.0522***	3.78	-0.0045	-0.35	0.0568***	2.99
$After^4$ × Treatment - $Before^1$ × Treatment	0.0338**	2.44	0.0054	0.42	0.0283	1.49
Panel B: Dependent variable is equity issuance ratio						

Before^4 × Treatment	-0.0049	-0.88	0.0014	0.74	-0.0064	-1.23
Before^3 × Treatment	-0.0091***	-2.80	-0.0002	-0.15	-0.0089*	-1.69
Before^2 × Treatment	-0.0068***	-2.91	-0.0016	-1.44	-0.0052	-0.99
Before^1 × Treatment	-0.0013	-0.69	-0.0007	-1.09	-0.0006	-0.12
After^1 × Treatment	0.0044	1.23	0.0046**	2.48	-0.0001	-0.02
After^2 × Treatment	0.0041	1.10	0.0044**	1.99	-0.0003	-0.06
After^3 × Treatment	0.0053	1.35	0.0029	1.28	0.0023	0.45
After^4 × Treatment	0.0059	1.64	0.0016	0.74	0.0043	0.82
N	6,642		6,642			
Adjusted R^2	0.108		0.075			
Difference tests						
After^1 × Treatment - Before^1 × Treatment	0.0058	1.18	0.0053**	2.35	0.0004	0.09
After^2 × Treatment - Before^1 × Treatment	0.0054	1.10	0.0051**	2.28	0.0002	0.06
After^3 × Treatment - Before^1 × Treatment	0.0067	1.36	0.0036	1.63	0.0030	0.57
After^4 × Treatment - Before^1 × Treatment	0.0073	1.48	0.0023	1.06	0.0049	0.94
Panel C: Dependent variable is change in long-term financing ratio						
Before^4 × Treatment	0.0243	1.17	0.0029	0.22	0.0213	1.02
Before^3 × Treatment	-0.0026	-0.12	-0.0052	-0.39	0.0025	0.12
Before^2 × Treatment	-0.0038	-0.22	-0.0056	-0.46	0.0017	0.08
Before^1 × Treatment	0.0063	0.30	0.0064	0.75	-0.0001	-0.01
After^1 × Treatment	0.0530**	2.25	0.0070	0.59	0.0460**	2.20
After^2 × Treatment	0.0549**	2.12	0.0044	0.32	0.0505**	2.42
After^3 × Treatment	0.0684**	2.28	0.0042	0.29	0.0642***	3.07
After^4 × Treatment	0.0503**	2.48	0.0130	0.96	0.0372*	1.78
N	6,624		6,624			
Adjusted R^2	0.119		0.102			
Difference tests						
After^1 × Treatment - Before^1 × Treatment	0.0467***	3.01	0.0005	0.04	0.0461**	2.21
After^2 × Treatment - Before^1 × Treatment	0.0486***	3.12	-0.0020	-0.15	0.0506**	2.42
After^3 × Treatment - Before^1 × Treatment	0.0621***	4.00	-0.0022	-0.16	0.0644***	3.08
After^4 × Treatment - Before^1 × Treatment	0.0439***	2.83	0.0065	0.47	0.0374*	1.79
Panel D: Dependent variable is change in short-term debt ratio						
Before^4 × Treatment	-0.0213	-1.63	0.0049	0.68	-0.0263**	-2.47
Before^3 × Treatment	-0.0109	-0.67	0.0080	1.11	-0.0190*	-1.78
Before^2 × Treatment	-0.0001	-0.01	0.0069	1.05	-0.0070	-0.67
Before^1 × Treatment	0.0018	0.12	0.0016	0.33	0.0001	0.02
After^1 × Treatment	-0.0214	-1.23	0.0018	0.22	-0.0232**	-2.19
After^2 × Treatment	-0.0201	-1.16	0.0038	0.48	-0.0240**	-2.25
After^3 × Treatment	-0.0202	-1.02	0.0019	0.24	-0.0222**	-2.09
After^4 × Treatment	-0.0011	-0.10	0.0002	0.03	-0.0014	-0.13
N	6,597		6,597			

Adjusted R^2	0.094		0.059			
Difference tests						
$After^1 \times$ Treatment - $Before^1 \times$ Treatment	-0.0232***	-2.77	0.0002	0.04	-0.0234**	-2.20
$After^2 \times$ Treatment - $Before^1 \times$ Treatment	-0.0219***	-2.61	0.0022	0.33	-0.0242**	-2.27
$After^3 \times$ Treatment - $Before^1 \times$ Treatment	-0.0220***	-2.63	0.0003	0.05	-0.0224**	-2.10
$After^4 \times$ Treatment - $Before^1 \times$ Treatment	-0.0029	-0.35	-0.0013	-0.20	-0.0015	-0.15
Panel E: Dependent variable is change in cash holdings ratio						
$Before^4 \times$ Treatment	0.0075	0.90	-0.0015	-0.34	0.0090	1.34
$Before^3 \times$ Treatment	0.0004	0.09	-0.0009	-0.22	0.0013	0.20
$Before^2 \times$ Treatment	-0.0033	-0.82	0.0005	0.12	-0.0038	-0.57
$Before^1 \times$ Treatment	-0.0013	-0.37	0.0041	1.05	-0.0055	-0.82
$After^1 \times$ Treatment	0.0154***	3.04	0.0042	1.12	0.0112*	1.67
$After^2 \times$ Treatment	0.0095*	1.66	-0.0054	-1.08	0.0150**	2.23
$After^3 \times$ Treatment	0.0010	0.19	-0.0067	-1.39	0.0077	1.15
$After^4 \times$ Treatment	-0.0001	-0.03	-0.0062	-1.11	0.0060	0.90
N	6,642		6,642			
Adjusted R^2	0.063		0.034			
Difference tests						
$After^1 \times$ Treatment - $Before^1 \times$ Treatment	0.0168***	3.28	0.0001	0.02	0.0167**	2.48
$After^2 \times$ Treatment - $Before^1 \times$ Treatment	0.0109**	2.14	-0.0095**	-2.17	0.0205***	3.05
$After^3 \times$ Treatment - $Before^1 \times$ Treatment	0.0024	0.47	-0.0108**	-2.47	0.0132**	1.97
$After^4 \times$ Treatment - $Before^1 \times$ Treatment	0.0011	0.23	-0.0104**	-2.37	0.0116*	1.72

Table 2.7. Pairwise comparison of the effects of treatment and corporate governance on profitability.

Firms are sorted into strong and weak governance groups based on the median value (equal to 10) of the G Index of Gompers, Ishii, and Metrick (2003). This table presents the dynamic pattern of corporate profitability for firms with confirmed ratings (treatment firms) and firms with downgraded ratings (control firms) within strong and weak governance firm groups. The dynamic pattern is from four quarters before to four quarters after the watch period. DiD^{SG} (DiD^{WG}) is the difference-in-differences test for treatment and control firms conditional on strong (weak) governance. DiDiD is a triple difference-in-differences test, where the last difference is computed as the difference-in-differences estimate for strong governance firms less that for weak governance firms. Variables are defined in Appendix A. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The baseline sample consists of 1,381 rating downgrades and 509 rating confirmations, for a total of 1,890 Moody's issuer-level watch actions between 1992 and 2011.

Quarters			Q_1 - Q_0		Q_2 - Q_0		Q_3 - Q_0		Q_4 - Q_0	
Firms classified by corporate governance	Firms classified by watch resolutions	*N*	Mean difference	*t*-Stat	Mean difference	*t*-Stat	Mean difference	*t*-Stat	Mean difference	*t*-Stat
Panel A: Profitability is measured by operating income ratio										
Strong governance	Treatment	177	0.0001	0.04	0.0034	1.11	0.0091**	2.53	0.0118***	3.02
	Control	474	-0.0204***	-9.81	-0.0229***	-7.68	-0.0220***	-6.73	-0.0146***	-4.28
	DiD^{SG}: Treatment - Control		0.0206***	5.88	0.0265***	6.12	0.0311***	6.39	0.0265***	5.09
Weak governance	Treatment	98	-0.0097***	-3.28	-0.0102***	-3.11	-0.0083*	-1.96	-0.0083*	-1.71
	Control	296	-0.0181***	-9.87	-0.0200***	-8.99	-0.0199***	-7.79	-0.0157***	-5.63
	DiD^{WG}: Treatment - Control		0.0083**	2.38	0.0098**	2.48	0.0116**	2.32	0.0073	1.31
	DiDiD: DiD^{SG} - DiD^{WG}		0.0121**	2.13	0.0165**	2.17	0.0195**	2.29	0.0191**	2.11
Panel B: Profitability is measured by return on assets										
Strong governance	Treatment	232	0.0018	0.50	0.0028	0.70	0.0061	1.29	0.0068	1.40
	Control	599	-0.0269***	-8.61	-0.0373***	-8.78	-0.0356***	-6.94	-0.0282***	-4.62
	DiD^{SG}: Treatment - Control		0.0288***	5.95	0.0402***	6.80	0.0418***	5.97	0.0350***	4.49
Weak governance	Treatment	133	-0.0059	-1.20	-0.0076	-1.28	-0.0099	-1.50	-0.0076	-1.12
	Control	388	-0.0140***	-4.43	-0.0208***	-5.54	-0.0216***	-5.15	-0.0188***	-3.55
	DiD^{WG}: Treatment - Control		0.0081	1.38	0.0132*	1.87	0.0116	1.48	0.0112	1.30
	DiDiD: DiD^{SG} - DiD^{WG}		0.0207**	2.41	0.0270**	2.46	0.0301**	2.33	0.0237	1.55
Panel C: Profitability is measured by return on equity										
Strong governance	Treatment	214	0.0053	0.42	0.0062	0.41	0.0146	0.82	0.0221	1.16
	Control	523	-0.0700***	-8.92	-0.0902***	-8.51	-0.1096***	-8.23	-0.1010***	-6.21
	DiD^{SG}: Treatment - Control		0.0754***	5.03	0.0966***	5.19	0.1242***	5.57	0.1232***	4.90

Weak governance	Treatment	131	-0.0114	-0.75	-0.0038	-0.21	-0.0112	-0.58	-0.0073	-0.36
	Control	374	-0.0426***	-4.26	-0.0650***	-5.38	-0.0720***	-5.15	-0.0648***	-4.39
	DiD^{WG}: Treatment - Control		0.0312*	1.72	0.0612***	2.81	0.0608**	2.54	0.0575**	2.28
	DiDiD: DiD^{SG} - DiD^{WG}		0.0442*	1.84	0.0353	1.17	0.0634*	1.75	0.0656	1.58

Table 2.8. The effects of treatment on the dynamic pattern of corporate profitability conditional upon corporate governance.

Firms are sorted into strong and weak governance groups based on the median value (equal to 10) of the G Index of Gompers, Ishii, and Metrick (2003). This table presents for each of those groups the dynamic pattern of corporate profitability from four quarters before to four quarters after the watch period. Results are for firms with confirmed ratings (treatment firms) benchmarked against firms with downgraded ratings (control firms). The dynamic pattern is estimated for the strong and weak governance firm groups separately (along with difference tests) based on the following regression specification

$$Y_{it} = \alpha + \sum_{q=1}^{4} \delta_q Before_{it}^{q} \times Treatment_i + \sum_{q=1}^{4} \mu_q After_{it}^{q} \times Treatment_i + \sum_{q=1}^{4} \varphi_q Before_{it}^{q}$$

$$+ \textstyle\sum_{q=1}^{4} \omega_q After_{it}^{q} + \theta Treatment_i + \gamma X_{it} + Industry_i + Quarter_t + FiscalQuarter_{it} + \varepsilon_{it}, \quad (1)$$

where Y_{it} is a profitability measure; X_{it} is a set of control variables, which are: Ln(rating change + 1), Fallen angel, Ln(assets), Tobin's Q, and Tangibility; for brevity of presentation, only the coefficients on the variables of interest are reported here; all variables are defined in Appendix A; $Before_{it}^{q}$ is a dummy variable equal to 1 if the observation is q quarters prior to the watch period, where q = 1, 2, 3, or 4 quarters; $After_{it}^{q}$ is a dummy variable equal to 1 if the observation is q quarters after the watch period, where q = 1, 2, 3, or 4 quarters; $Treatment_i$ is a dummy variable equal to 1 if the credit watch is resolved with a rating confirmation; i indexes firms; t indexes time measured in calendar quarters; $Industry_i$ are 38 SIC-based industry fixed effects; $Quarter_t$ are calendar quarter fixed effects; and $FiscalQuarter_{it}$ are fiscal quarter fixed effects. Standard errors are clustered at the firm and calendar quarter levels. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The baseline sample consists of 509 rating confirmations and 1,381 rating downgrades, for a total of 1,890 Moody's issuer-level watch actions between 1992 and 2011.

	Strong governance		Weak governance		Strong - Weak	
	Coefficient	*t*-Stat	Coefficient	*t*-Stat	Coefficient	*t*-Stat
Panel A: Dependent variable is operating income ratio						
$Before^4$ × Treatment	0.0014	0.15	-0.0051	-0.89	0.0066	0.60
$Before^3$ × Treatment	0.0050	0.73	-0.0011	-0.20	0.0068	0.62
$Before^2$ × Treatment	0.0022	0.22	0.0004	0.17	0.0018	0.17
$Before^1$ × Treatment	0.0028	0.32	0.0010	0.17	0.0017	0.16
$After^1$ × Treatment	0.0172**	2.48	0.0078	1.29	0.0093	0.84
$After^2$ × Treatment	0.0240***	3.00	0.0129**	2.18	0.0111	1.01
$After^3$ × Treatment	0.0280***	2.85	0.0158**	2.46	0.0121	1.10
$After^4$ × Treatment	0.0250***	2.70	0.0136**	2.06	0.0114	1.04
N	5,445		3,420			
Adjusted R^2	0.508		0.513			
Difference tests						
$After^1$ × Treatment - $Before^1$ × Treatment	0.0143*	1.94	0.0068	0.93	0.0075	0.68
$After^2$ × Treatment - $Before^1$ × Treatment	0.0212***	2.87	0.0118	1.62	0.0093	0.84
$After^3$ × Treatment - $Before^1$ × Treatment	0.0251***	3.41	0.0147**	2.02	0.0103	0.94
$After^4$ × Treatment - $Before^1$ × Treatment	0.0222***	3.01	0.0125*	1.71	0.0096	0.88
Panel B: Dependent variable is return on assets						

Before^4 × Treatment	0.0032	0.43	-0.0108	-1.37	0.0140	0.99
Before^3 × Treatment	0.0037	0.44	-0.0001	-0.04	0.0038	0.27
Before^2 × Treatment	0.0011	0.11	-0.0020	-0.49	0.0032	0.23
Before^1 × Treatment	0.0012	0.13	-0.0020	-0.78	0.0033	0.23
After^1 × Treatment	0.0231**	2.37	0.0081	1.32	0.0149	1.06
After^2 × Treatment	0.0392***	3.44	0.0134	1.60	0.0258*	1.82
After^3 × Treatment	0.0388***	2.76	0.0127	1.29	0.0261*	1.84
After^4 × Treatment	0.0353**	2.57	0.0144	1.63	0.0208	1.47
N	6,714		4,482			
Adjusted R^2	0.327		0.286			
Difference tests						
After^1 × Treatment - Before^1 × Treatment	0.0219**	2.18	0.0102	1.21	0.0116	0.82
After^2 × Treatment - Before^1 × Treatment	0.0380***	3.79	0.0154*	1.83	0.0225	1.59
After^3 × Treatment - Before^1 × Treatment	0.0376***	3.74	0.0147*	1.75	0.0228	1.61
After^4 × Treatment - Before^1 × Treatment	0.0340***	3.39	0.0165*	1.96	0.0175	1.24
Panel C: Dependent variable is return on equity						
Before^4 × Treatment	0.0187	0.69	-0.0205	-0.90	0.0393	1.00
Before^3 × Treatment	0.0260	1.01	0.0019	0.15	0.0241	0.61
Before^2 × Treatment	0.0081	0.27	-0.0085	-0.87	0.0166	0.43
Before^1 × Treatment	0.0051	0.24	-0.0065	-0.78	0.0116	0.30
After^1 × Treatment	0.0671**	2.34	0.0371**	2.54	0.0299	0.76
After^2 × Treatment	0.0872***	3.01	0.0684**	2.43	0.0188	0.48
After^3 × Treatment	0.1158***	2.75	0.0715**	2.46	0.0442	1.13
After^4 × Treatment	0.1143***	3.40	0.0696**	2.57	0.0446	1.14
N	6,138		4,356			
Adjusted R^2	0.293		0.221			
Difference tests						
After^1 × Treatment - Before^1 × Treatment	0.0620**	2.34	0.0436	1.60	0.0183	0.47
After^2 × Treatment - Before^1 × Treatment	0.0821***	3.10	0.0749***	2.73	0.0072	0.18
After^3 × Treatment - Before^1 × Treatment	0.1107***	4.18	0.0780***	2.85	0.0326	0.83
After^4 × Treatment - Before^1 × Treatment	0.1091***	4.12	0.0761***	2.79	0.0330	0.84

Table 2.9. Testing the parallel trend assumption.

I falsely assume that the onset of treatment occurs two years before it actually does, and define false watch assignment and resolution announcements as the two-year lagged actual assignment and resolution announcements, respectively. The table presents the dynamic pattern of corporate financing (Panel A), investment (Panel B), and profitability (Panel C) from four quarters before to four quarters after the false watch period. Results are for firms with confirmed ratings (treatment firms) benchmarked against firms with downgraded ratings (control firms). The dynamic pattern is estimated based on the following regression specification

$$Y_{it} = \alpha + \sum_{q=1}^{4} \delta_q Before_{it}^q \times Treatment_i + \sum_{q=1}^{4} \mu_q After_{it}^q \times Treatment_i + \sum_{q=1}^{4} \varphi_q Before_{it}^q + \sum_{q=1}^{4} \omega_q After_{it}^q + \theta Treatment_i$$

$$+\gamma X_{it} + Industry_i + Quarter_t + FiscalQuarter_{it} + \varepsilon_{it}, \qquad (1)$$

where Y_{it} is a financing, investment, or profitability measure; X_{it} is a set of control variables, which are: Ln(rating change + 1), Fallen angel, Ln(assets), Tobin's Q, and Tangibility; all variables are defined in Appendix A; $Before_{it}^q$ is a dummy variable equal to 1 if the observation is q quarters prior to the false watch period, where $q = 1$, 2, 3, or 4 quarters; $After_{it}^q$ is a dummy variable equal to 1 if the observation is q quarters after the false watch period, where $q = 1$, 2, 3, or 4 quarters; $Treatment$ is a dummy variable equal to 1 if the credit watch is resolved with a rating confirmation; i indexes firms; t indexes time measured in calendar quarters; $Industry_i$ are 38 SIC-based industry fixed effects; $Quarter_t$ are calendar quarter fixed effects; and $FiscalQuarter_{it}$ are fiscal quarter fixed effects. Standard errors are clustered at the firm and calendar quarter levels. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The baseline sample consists of 509 rating confirmations and 1,381 rating downgrades, for a total of 1,890 Moody's issuer-level watch actions between 1992 and 2011.

Panel A: Financing										
Dependent variable	Change in long-term debt ratio		Equity issuance ratio		Change in long-term financing ratio		Change in short-term debt ratio		Change in cash holdings ratio	
	[1]		[2]		[3]		[4]		[5]	
	Coefficient	*t*-Stat	Coefficient	*t*-Stat	Coefficient	*t*-Stat	Coefficient	*t*-Stat	Coefficient	*t*-Stat
$Before^4 \times$ Treatment	-0.0084	-0.93	-0.0011	-0.29	-0.0085	-0.78	0.0071	1.39	-0.0082**	-2.09
$Before^3 \times$ Treatment	-0.0019	-0.27	-0.0009	-0.23	-0.0025	-0.30	0.0041	0.93	0.0013	0.44
$Before^2 \times$ Treatment	-0.0022	-0.31	0.0005	0.12	-0.0021	-0.23	-0.0011	-0.33	-0.0018	-0.57
$Before^1 \times$ Treatment	-0.0010	-0.69	-0.0020	-0.81	-0.0031	-0.32	0.0020	0.61	-0.0006	-0.35
$After^1 \times$ Treatment	0.0030	0.79	-0.0036	-1.02	-0.0015	-0.28	-0.0015	-0.40	-0.0054**	-1.99
$After^2 \times$ Treatment	-0.0021	-0.34	-0.0038	-0.98	-0.0058	-0.72	-0.0042	-0.94	-0.0043	-1.32
$After^3 \times$ Treatment	-0.0089	-1.07	-0.0052	-1.30	-0.0129	-1.24	0.0038	0.59	-0.0068*	-1.68

After^4 × Treatment	-0.0158	-1.55	-0.0062	-1.51	-0.0214*	-1.74	0.0085	1.22	-0.0046	-1.37
N	15,525		15,543		15,525		15,399		15,543	
Adjusted R^2	0.081		0.127		0.110		0.063		0.055	
Difference tests										
After^1 × Treatment - Before^1 × Treatment	0.0040	0.50	-0.0016	-0.48	0.0016	0.16	-0.0035	-0.90	-0.0048	-1.30
After^2 × Treatment - Before^1 × Treatment	-0.0011	-0.15	-0.0018	-0.53	-0.0028	-0.29	-0.0062	-1.58	-0.0037	-1.00
After^3 × Treatment - Before^1 × Treatment	-0.0079	-1.00	-0.0031	-0.94	-0.0098	-1.01	0.0018	0.47	-0.0061*	-1.69
After^4 × Treatment - Before^1 × Treatment	-0.0148*	-1.87	-0.0042	-1.25	-0.0183*	-1.89	0.0064*	1.65	-0.0040	-1.08

Panel B: Investment

Dependent variable	Capital expenditures ratio		PPE growth rate		Asset growth rate	
	[6]		[7]		[8]	
	Coefficient	*t*-Stat	Coefficient	*t*-Stat	Coefficient	*t*-Stat
Before^4 × Treatment	-0.0014	-0.40	-0.0203	-0.92	-0.0239	-1.07
Before^3 × Treatment	-0.0016	-0.49	-0.0108	-0.75	-0.0124	-0.81
Before^2 × Treatment	0.0001	0.05	-0.0008	-0.06	-0.0040	-0.26
Before^1 × Treatment	-0.0007	-0.93	-0.0037	-0.44	0.0013	0.15
After^1 × Treatment	0.0002	0.05	-0.0077	-0.43	-0.0120	-0.74
After^2 × Treatment	-0.0013	-0.47	-0.0247	-1.32	-0.0305	-1.54
After^3 × Treatment	-0.0030	-0.78	-0.0236	-1.05	-0.0260	-1.16
After^4 × Treatment	-0.0052	-1.57	-0.0407*	-1.74	-0.0233	-1.12
N	14,868		15,534		15,543	
Adjusted R^2	0.463		0.093		0.126	
Difference tests						
After^1 × Treatment - Before^1 × Treatment	0.0009	0.25	-0.0040	-0.22	-0.0132	-0.72
After^2 × Treatment - Before^1 × Treatment	-0.0006	-0.18	-0.0210	-1.16	-0.0317	-1.72
After^3 × Treatment - Before^1 × Treatment	-0.0023	-0.66	-0.0199	-1.10	-0.0273	-1.48
After^4 × Treatment - Before^1 × Treatment	-0.0045	-1.28	-0.0369**	-2.04	-0.0245	-1.33

Panel C: Profitability

Dependent variable	Operating income ratio		Return on assets		Return on equity	
	[9]		[10]		[11]	
	Coefficient	*t*-Stat	Coefficient	*t*-Stat	Coefficient	*t*-Stat
Before^4 × Treatment	-0.0004	-0.11	0.0003	0.09	0.0082	0.80
Before^3 × Treatment	-0.0008	-0.28	-0.0001	-0.05	0.0108	1.28
Before^2 × Treatment	-0.0005	-0.15	-0.0021	-0.71	0.0024	0.30
Before^1 × Treatment	0.0003	0.06	-0.0016	-0.75	0.0038	0.42
After^1 × Treatment	0.0000	-0.01	0.0007	0.21	0.0022	0.17
After^2 × Treatment	-0.0014	-0.24	0.0020	0.62	0.0024	0.21
After^3 × Treatment	-0.0036	-0.74	0.0028	0.67	0.0010	0.06
After^4 × Treatment	-0.0017	-0.28	0.0032	0.63	0.0045	0.23
N	11,898		15,030		14,517	
Adjusted R^2	0.373		0.275		0.159	
Difference tests						
After^1 × Treatment - Before^1 × Treatment	-0.0003	-0.06	0.0023	0.53	-0.0017	-0.12
After^2 × Treatment - Before^1 × Treatment	-0.0017	-0.33	0.0036	0.83	-0.0015	-0.10
After^3 × Treatment - Before^1 × Treatment	-0.0039	-0.75	0.0044	1.02	-0.0029	-0.20
After^4 × Treatment - Before^1 × Treatment	-0.0020	-0.38	0.0048	1.10	0.0007	0.05

Table 2.10. Propensity score matching.

This table reports results based on a propensity score matching framework, which involves two stages. In the first stage, I run a probit regression to estimate propensity scores. The dependent variable is a dummy that equals one for firms with confirmed ratings (treatment firms) and zero for firms with downgraded ratings (control firms). The independent variables are: a dummy indicating non-investment grade status (Ba or lower), a dummy indicating the cash flow-investment gap ratio is below its median value, a dummy indicating the G Index of Gompers, Ishii, and Metrick (2003) is below its median value, Ln(assets), Tobin's Q, Tangibility, 38 SIC-based industry dummies, and calendar quarter dummies. In the second stage, I randomly match each treatment firm to a control firm with no replacement based on a 1% allowable absolute difference between propensity scores. The matching algorithm is optimized to maximize the number of propensity score matches. In this respect, the optimization algorithm retains the matches for treatment firms with the fewest possible number of matches first. The table compares the means of the changes in firm financing (Panel A), investment (Panel B), and profitability (Panel C) measures for matched treatment and control firms and reports difference tests. Changes in corporate fundamentals are computed from the quarter ending prior to the watch period (quarter 0) to up to four quarters subsequent to the watch period. Variables are defined in Appendix A. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The baseline sample consists of 1,381 rating downgrades and 509 rating confirmations, for a total of 1,890 Moody's issuer-level watch actions between 1992 and 2011.

	Quarters	Matched treatment firms (*N*=250)	Matched control firms (*N*=250)	Treat. - Cont.	*t*-Stat
		Panel A: Financing measures			
Change in long-term debt ratio	$Q_1 - Q_0$	0.0443	0.0139	0.0304**	2.39
	$Q_2 - Q_0$	0.0617	0.0266	0.0351**	2.34
	$Q_3 - Q_0$	0.0582	0.0191	0.0391**	2.52
	$Q_4 - Q_0$	0.0514	0.0127	0.0387***	2.61
Equity issuance ratio	$Q_1 - Q_0$	0.0042	0.0012	0.0030	0.75
	$Q_2 - Q_0$	0.0041	0.0013	0.0028	0.75
	$Q_3 - Q_0$	0.0043	-0.0004	0.0047	1.21
	$Q_4 - Q_0$	0.0039	-0.0007	0.0046	1.28
Change in long-term financing ratio	$Q_1 - Q_0$	0.0491	0.0154	0.0337**	2.44
	$Q_2 - Q_0$	0.0670	0.0296	0.0374**	2.29
	$Q_3 - Q_0$	0.0647	0.0207	0.0440**	2.58
	$Q_4 - Q_0$	0.0567	0.0137	0.0430***	2.66
Change in short-term debt ratio	$Q_1 - Q_0$	0.0018	0.0072	-0.0054	-0.70
	$Q_2 - Q_0$	-0.0053	0.0052	-0.0105	-1.21
	$Q_3 - Q_0$	-0.0124	0.0005	-0.0129	-1.44
	$Q_4 - Q_0$	-0.0156	-0.0100	-0.0056	-0.66
Change in cash holdings ratio	$Q_1 - Q_0$	0.0085	-0.0005	0.0090**	2.12
	$Q_2 - Q_0$	0.0026	0.0043	-0.0017	-0.38
	$Q_3 - Q_0$	0.0002	0.0058	-0.0056	-1.21
	$Q_4 - Q_0$	-0.0005	0.0042	-0.0047	-1.04
		Panel B: Investment measures			
Capital expenditures ratio	$Q_1 - Q_0$	-0.0009	-0.0051	0.0042*	1.77
	$Q_2 - Q_0$	-0.0020	-0.0083	0.0063**	2.28
	$Q_3 - Q_0$	-0.0020	-0.0108	0.0088***	2.80
	$Q_4 - Q_0$	-0.0021	-0.0129	0.0108***	3.12
PPE growth rate	$Q_1 - Q_0$	0.0486	0.0070	0.0416*	1.66

	Q_2 - Q_0	0.0896	0.0157	0.0739**	2.39
	Q_3 - Q_0	0.0931	0.0148	0.0783**	2.48
	Q_4 - Q_0	0.0791	0.0032	0.0759**	2.44
Asset growth rate	Q_1 - Q_0	0.0623	-0.0026	0.0649***	2.60
	Q_2 - Q_0	0.0970	0.0182	0.0788***	2.72
	Q_3 - Q_0	0.0894	0.0178	0.0716**	2.38
	Q_4 - Q_0	0.0747	0.0063	0.0684**	2.27
Panel C: Profitability measures					
Operating income ratio	Q_1 - Q_0	-0.0044	-0.0225	0.0181***	5.02
	Q_2 - Q_0	-0.0048	-0.0261	0.0213***	5.19
	Q_3 - Q_0	-0.0011	-0.0224	0.0213***	4.69
	Q_4 - Q_0	-0.0005	-0.0141	0.0136***	2.66
Return on assets	Q_1 - Q_0	-0.0040	-0.0203	0.0163***	3.26
	Q_2 - Q_0	-0.0038	-0.0234	0.0197***	3.50
	Q_3 - Q_0	-0.0029	-0.0236	0.0208***	3.36
	Q_4 - Q_0	-0.0039	-0.0149	0.0110	1.63
Return on equity	Q_1 - Q_0	-0.0070	-0.0662	0.0592***	3.61
	Q_2 - Q_0	-0.0039	-0.0759	0.0720***	3.83
	Q_3 - Q_0	-0.0009	-0.0883	0.0874***	3.58
	Q_4 - Q_0	-0.0071	-0.0538	0.0467*	1.69

Table 2.11. Switching regression with endogenous switching.

This table compares the means of the actual and hypothetical changes in firm financing (Panel A), investment (Panel B), and profitability (Panel C) measures and reports the differences in means computed as in Eq. (B.10). The estimated hypothetical change in a firm fundamentals measure reflects what the change would be if the firm's rating had been downgraded rather than confirmed. Changes in firm fundamentals are computed from the quarter ending prior to the watch period (quarter 0) to up to four quarters subsequent to the watch period. Variables are defined in Appendix A. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The baseline sample consists of 1,381 rating downgrades and 509 rating confirmations, for a total of 1,890 Moody's issuer-level watch actions between 1992 and 2011.

	Quarters	Actual change for treatment firms (*N*=234)	Hypothetical change for counterfactuals (*N*=234)	Act. - Hyp.	*t*-Stat
		Panel A: Financing measures			
Change in long-term debt ratio	$Q_1 - Q_0$	0.0480	0.0038	0.0442***	3.48
	$Q_2 - Q_0$	0.0646	0.0228	0.0418***	3.16
	$Q_3 - Q_0$	0.0600	0.0297	0.0303**	2.23
	$Q_4 - Q_0$	0.0512	0.0102	0.0411***	3.76
Equity issuance ratio	$Q_1 - Q_0$	0.0020	-0.0042	0.0062***	2.83
	$Q_2 - Q_0$	0.0035	-0.0008	0.0043*	1.89
	$Q_3 - Q_0$	0.0041	0.0020	0.0021	0.84
	$Q_4 - Q_0$	0.0040	0.0028	0.0012	0.52
Change in long-term financing ratio	$Q_1 - Q_0$	0.0503	-0.0001	0.0505***	3.76
	$Q_2 - Q_0$	0.0692	0.0231	0.0461***	3.23
	$Q_3 - Q_0$	0.0646	0.0337	0.0309**	2.17
	$Q_4 - Q_0$	0.0558	0.0159	0.0399***	3.49
Change in short-term debt ratio	$Q_1 - Q_0$	0.0007	0.0147	-0.0141*	-1.66
	$Q_2 - Q_0$	-0.0079	-0.0008	-0.0070	-0.79
	$Q_3 - Q_0$	-0.0149	-0.0128	-0.0020	-0.22
	$Q_4 - Q_0$	-0.0162	-0.0054	-0.0108	-1.40
Change in cash holdings ratio	$Q_1 - Q_0$	0.0095	-0.0007	0.0103***	2.70
	$Q_2 - Q_0$	0.0041	-0.0008	0.0049	1.38
	$Q_3 - Q_0$	-0.0017	-0.0039	0.0022	0.67
	$Q_4 - Q_0$	-0.0029	-0.0141	0.0111***	2.92
		Panel B: Investment measures			
Capital expenditures ratio	$Q_1 - Q_0$	-0.0051	-0.0067	0.0016	0.96
	$Q_2 - Q_0$	-0.0049	-0.0068	0.0018	1.11
	$Q_3 - Q_0$	-0.0033	-0.0069	0.0035*	1.88
	$Q_4 - Q_0$	-0.0022	-0.0068	0.0046**	2.24
PPE growth rate	$Q_1 - Q_0$	0.0456	-0.0759	0.1215***	5.64
	$Q_2 - Q_0$	0.0810	-0.0792	0.1603***	6.63
	$Q_3 - Q_0$	0.0814	-0.0655	0.1469***	6.06
	$Q_4 - Q_0$	0.0708	-0.0747	0.1456***	6.43
Asset growth rate	$Q_1 - Q_0$	0.0755	0.0319	0.0436**	2.17
	$Q_2 - Q_0$	0.1079	0.0444	0.0635***	2.97
	$Q_3 - Q_0$	0.0930	0.0586	0.0344	1.56

	$Q_4 - Q_0$	0.0820	0.0391	0.0430*	1.96
Panel C: Profitability measures					
Operating income ratio	$Q_1 - Q_0$	-0.0045	-0.0244	0.0201***	7.40
	$Q_2 - Q_0$	-0.0029	-0.0214	0.0185***	6.41
	$Q_3 - Q_0$	0.0016	-0.0185	0.0202***	6.09
	$Q_4 - Q_0$	0.0024	-0.0245	0.0269***	7.83
Return on assets	$Q_1 - Q_0$	0.0012	-0.0125	0.0139***	4.24
	$Q_2 - Q_0$	0.0034	-0.0173	0.0208***	5.39
	$Q_3 - Q_0$	0.0025	-0.0106	0.0132***	2.61
	$Q_4 - Q_0$	0.0026	-0.0102	0.0130***	2.71
Return on equity	$Q_1 - Q_0$	0.0086	-0.0402	0.0489***	4.37
	$Q_2 - Q_0$	0.0162	-0.0622	0.0785***	5.60
	$Q_3 - Q_0$	0.0221	-0.0313	0.0536**	2.58
	$Q_4 - Q_0$	0.0245	-0.0769	0.1015***	5.18

CHAPTER THREE: AMBIGUITY AVERSION AND THE INFLUENCE OF ANALYSTS

3.1. Introduction

Investors face a large amount of information yet the way they interpret and respond to such information in financial markets is poorly understood. In particular, there is substantial evidence of anomalous heterogeneity in investors' responses to the announcements of sell-side recommendation changes. Specifically, investors appear to be highly responsive only to select few recommendation changes. As documented by Loh and Stulz (2011), only 12% of recommendation changes are influential, implying that a typical recommendation change does not have a substantial identifiable impact on the stock price.[24] For example, on April 9, 2012, BTIG's Walter Piecyk downgraded Apple to neutral from buy rating, citing concerns about squeezing margins in the post-paid wireless industry; though the downgrade was widely covered in the media on that day, Apple's stock price actually closed up \$2.55, or 0.4%. In another example, however, when Barclays Capital's Jeff Kvaal cut his rating on Nokia to equal weight from overweight on April 13, 2012, Nokia's shares lost \$.21, or more than 5% of their market value on that day.[25]

A close look at the analysts' reports reveals key information that may well justify investors being highly responsive to the Nokia's downgrade and hardly responsive to the Apple's. Barclays Capital's Jeff Kvaal writes: "[…] *near term pressures are rising and are both clouding and lengthening this uncertain transition period. With this lack of visibility and what clearly will be another year of significant earnings declines, we downgrade Nokia to 2-Equal Weight/2-Neutral*

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[24] Loh and Stulz define a recommendation change as being influential if it satisfies two conditions. The first condition is that the event abnormal return has the same direction as the associated recommendation change. The second condition is that the recommendation change is statistically influential. That is, the absolute value of the associated event abnormal return is higher than $1.96 \times \sqrt{2} \times \sigma_\varepsilon$, where σ_ε is the residual standard deviation of stock daily returns against the three Fama–French factors in the three-month period prior to the recommendation change announcement.

[25] The above examples are from the following articles: "Even Apple Isn't Immune to a Case of the Mondays", by Paul Vigna, The Wall Street Journal, April 9, 2012; "Apple: BTIG Cuts to Hold; iPhone Subsidies Could Dry Up", by Tiernan Ray, Barron's, April 9, 2012; "Nokia: Barclays Cuts to Hold, \$4 Target, on 'Tougher Transition'", by Tiernan Ray, Barron's, April 13, 2012.

from a 1-Overweight […]” In contrast, BTIG’s Walter Piecyk writes: “*We continue to maintain our view that Apple is the primary beneficiary of an accelerating growth trend in the global adoption of smartphones, considering global penetration of smartphones has not even reached 30%*”. What is clear from these statements is that Apple, despite its downgrade, is still perceived as a stable, growing company, whereas Nokia is viewed as a company with high ambiguity and low visibility. Ambiguity in the firm environment may influence the way investors interpret and respond to information signaled by analyst recommendation changes.

This chapter investigates the implications of ambiguity in the firm environment for the influence of recommendation changes. Specifically, I present and empirically evaluate a theory of ambiguity that predicts a positive relation between ambiguity in the firm environment and the influence of recommendation changes. The main premise of the theory is that in ambiguous situations investors with ambiguity-averse preferences choose a worst-case scenario when they make decisions (Gilboa and Schmeidler, 1989). This assumption yields a range of empirical predictions, which I develop in Section 3.2.

Ambiguity in the firm environment, as I define it, refers to the difficulty investors face in processing and acting on firm incomplete information. The nature of deviation from complete information defines the specific aspect of ambiguity in the firm environment. This chapter focuses on two distinct aspects of ambiguity in the firm environment. First, when information distortion primarily relates to missing relevant information that otherwise could never be known, investors perceive ambiguity in firm fundamentals (hereafter ambiguity in fundamentals, or AIF). More specifically, by AIF I mean the difficulty investors face in formulating prior beliefs about firm fundamentals due to lack of knowledge of nonexistent relevant information. At the decision time, the information investors possess is insufficient to make reasonably precise projections of a firm’s future cash flows because the future is yet to be created. They face significant indeterminacy of the future, perhaps due to possibility of structural change in the firm’s business model or operating environment. In the examples above, Nokia's investors perceive AIF in that they lack visibility on the future as the firm undergoes restructuring, the outcome of which is difficult to predict given the insufficient information available at the time. In contrast, Apple's investors are more confident about the future because the information available at the decision time suggests that the firm is set to grow and capitalize on increasing global demand for smartphones. Second, when information distortion pertains to missing relevant information that otherwise could be known, investors perceive ambiguity in the firm information environment (hereafter ambiguity in information, or AII). The concept of AII pertains to cases in which a firm conveys information that is too sparse or imprecise for investors to correctly interpret, thereby facing difficulty in updating their prior beliefs in response to that information. In this case, the quality of information disclosed by the firm is at issue. Bad corporate governance, deficiencies in a firm's information disclosure process, lack of earnings guidance or operational updates, or the complex nature of a firm's business or operating environment are factors that may contribute to poor-quality information environments. In general, smaller firms, younger firms, or firms with lower analyst coverage release less information to the public, receive little media coverage, and attract less regulatory attention, thereby offering investors information environments of lower quality. [26]

Using a large sample of analyst recommendation changes between 1994 and 2010, I provide evidence that recommendation changes issued on higher AIF or higher AII firms are more influential (the "mean effect" of ambiguity). Empirical proxies for AIF and AII are introduced in Section 3.2. I use the buy-and-hold abnormal return over a two-day period, day 0 (announcement day) to 1, to measure the influence of a recommendation change. After controlling for various analyst attributes and firm characteristics, I find that upgrades issued on higher AIF firms are associated with a buy-and-hold abnormal return that is higher by an economically large and

[26] The terminologies AIF and AII are due to Caskey (2009). The definitions above are inspired from Camerer and Weber (1992) and Dequech (2000). Camerer and Weber define ambiguity (equivalent to AII here) as follows: "Ambiguity is uncertainty about probability, created by missing information that is relevant and could be known." Dequech defines fundamental uncertainty (equivalent to AIF here) as follows: "[…] some information does not exist at the decision time because the future is yet to be created."

statistically significant 117 bps. The effect of AII is also significant. I find that upgrades issued on higher AII firms are more influential by an amount ranging between 43 and 153 bps. In the subsample of downgrades, I find that the effect of AIF amounts to 211 bps and that of AII varies between 78 and 91 bps. All these effects are statistically significant at the 1% level.

I further show that the positive relation between AII and the influence of recommendation changes is stronger for firms for which investors perceive higher levels of AIF (the "interaction effect" of ambiguity). In the subsample of high AIF firms, I find that the effect of AII is substantial, ranging between 77 (116) and 220 (151) bps in case of upgrades (downgrades). In contrast, if I condition on low AIF firms the effect of AII is weaker, varying between 19 (24) and 109 (47) bps in case of upgrades (downgrades).

Overall, my findings are consistent with the view that in situations of incomplete information the opinions of experts, such as analysts, should be extremely valuable to investors. Compared to an average investor, analysts are typically better able to explain firm business complexities notably when firm fundamentals are more uncertain; they also invest more effort in idiosyncratic information searches when public information is of uncertain quality (Mohanram and Sunder, 2006). The average investor may view information signaled by analysts as resolving some of the ambiguity in the firm environment and thus could be highly responsive to that information.

Broadly speaking, this chapter contributes to the analyst literature by providing a novel behavioral explanation for anomalous heterogeneity in the influence of recommendation changes. After controlling for other relevant factors, I show that investors are much more responsive to recommendation changes when they perceive higher levels of ambiguity in the firm environment, a behavior consistent with ambiguity aversion in the equity market. Prior studies show that several analyst, firm, and recommendation characteristics affect the influence of recommendation changes. For instance, Stickel (1995) finds that recommendation changes are more informative if they are stronger, of higher magnitude, accompanied by earnings forecasts, from reputable analysts or larger brokerage houses, or issued on smaller firms. Ivković and Jegadeesh (2004) demonstrate that upgrades are most informative in the week before earnings announcements, but they fail to find similar evidence for downgrades. Asquith, Mikhail, and Au (2005) provide evidence that the content of analyst reports, such as target price revision, affects the influence of recommendation changes. Jegadeesh and Kim (2010) document that stock price reaction is stronger for recommendations issued away from consensus. Using a novel methodology, Loh and Stulz (2011) provide a comprehensive summary of prior findings and show that recommendation changes are more likely to be influential if they are from reputable analysts, issued away from consensus, or issued on growth, small, high institutional ownership, or high earnings forecast dispersion firms. None of these studies attempts to relate the influence of recommendation changes to behavioral biases on the part of investors.

Jiang, Lee, and Zhang (2005) and Zhang (2006) show that price continuation anomalies associated with earnings announcements and analyst earnings forecast revisions, respectively, are stronger among firms with higher information uncertainty, but they do not consider short-term returns or recommendation changes. Moreover, they do not make any distinction between AIF and AII, as their so-called information uncertainty simultaneously captures both aspects of ambiguity. In contrast, in this chapter I distinguish between AIF and AII and show that their interaction has a real effect on the influence of recommendation changes.

The remainder of this chapter is organized as follows. Section 3.2 presents a simple of model of information processing under ambiguity and introduces empirical proxies for variables of interest. Section 3.3 describes the sample formation process and presents descriptive statistics. Section 3.4 presents the main results. Section 3.5 conducts some robustness tests, and Section 3.6 concludes the chapter.

3.2. Model and empirical proxies

In this section, I present a simple model of information processing under both AIF and AII and discuss its empirical predictions. Next, I motivate and propose empirical proxies for both aspects of ambiguity.

3.2.1. Model

Ellsberg (1961) provides experimental evidence suggesting that agents face and dislike not only risk (known odds) but also ambiguity (unknown odds) in making decisions, implying that the observed preferences are inconsistent with the subjective expected utility (SEU) theory of Savage (1954). Specifically, SEU theory assumes that subjective probabilities are never unknown, whereas experimental evidence suggests that how much agents' know about a state's probability does influence their willingness to bet on the state (Camerer and Weber, 1992). Gilboa and Schmeidler (1989) propose an axiomatic foundation of the maxmin expected utility rule, which is compatible with the observed preferences emphasized by Ellsberg (1961). The rule implies that ambiguity-averse agents who perceive a set of priors in situations of incomplete information take into account the minimal expected utility over all priors when evaluating a bet. Put another way, they act as if they choose a worst-case prior when they make decisions. In what follows, I shall rely on this intuitive behavior and assume that ambiguity-averse investors make decisions based on the perception that a worst-case scenario reveals the true state of nature.

The concept of AIF can be viewed as a situation in which ambiguity-averse investors are endowed with a set of priors about firm fundamentals and face difficulty in identifying a unique prior from this set with which to value the firm. Following Gilboa and Schmeidler (1989), these investors typically act as if they select a worst-case prior from the perceived set of priors. It follows that they exhibit underconfidence in their prior beliefs about firm ambiguous fundamentals and thus act as if they underweight prior information about firm ambiguous fundamentals and overweight new information signaled by a recommendation change. As a result, they respond more strongly to recommendation changes issued on high-AIF firms than to recommendation changes issued on other firms.

Conceptually, one can view AII as a situation in which ambiguity-averse investors who observe a firm's signal of uncertain quality perceive the precision of the signal to lie in a certain range, thereby facing difficulty in updating their prior beliefs in response to that signal. Following Epstein and Schneider (2008), it is assumed that these investors act as if they choose a worst-case assessment of quality when processing the ambiguous signal. It follows that they are underconfident about the quality of the signal and thus act as if they underweight information implied by that signal and overweight new information from analysts. If investors exhibit such behavior, they will be more responsive to recommendation changes issued on high-AII firms than to recommendation changes issued on other firms.

Though AIF and AII refer, by definition, to distinct aspects of ambiguity, their implications for investors' beliefs may not be as independent as they first appear. Ambiguity-averse investors may well be more concerned about a firm's quality of information when the firm's fundamentals are perceived to be more ambiguous. For a firm with stable fundamentals, ambiguity-averse investors dislike information of uncertain quality, but not as much as they would if the firm's fundamentals were perceived to be unstable. Hence, ambiguity-averse investors who process poor-quality information from firms with more ambiguous fundamentals may exhibit stronger underconfidence in the quality of that information and thus act as if they place too little weight on that information and overweight more information from analysts. As a result, they are more responsive to analyst recommendation changes issued on high AII firms when their fundamentals are perceived to be more ambiguous. Below, I formalize these conceptual views and derive the associated empirical predictions.

There are four dates, $t = 0, 1, 2, 3$. The economy has a representative investor, a representative analyst, a firm (risky asset), and a riskless asset. The investor is assumed to be ambiguity-averse and risk-neutral (for tractability). The riskless interest rate is assumed to be zero. All random variables are independent and normally distributed. Each share of the firm is a claim to a dividend d, which is revealed at date $t = 3$. At date $t = 0$, the dividend is

$$d = \theta + \epsilon, \qquad (3.1)$$

where θ is the mean dividend, and ϵ is the noise associated with the dividend and is normally distributed with mean zero and variance σ^2, $\epsilon \sim N(0, \sigma^2)$. At date $t = 0$, the ambiguity-averse investor lacks knowledge of nonexistent relevant information and thus perceives AIF.[27] Formally, the mean dividend and the dividend volatility are perceived to lie in a set, $(\theta, \sigma) \in [\underline{\theta}, \overline{\theta}] \times [\underline{\sigma}, \overline{\sigma}]$, implying a perceived set of multiple priors over dividend outcomes. Following Gilboa and Schmeidler (1989), it is assumed that the investor acts as if he chooses a worst-case prior from the perceived set of priors. It follows that the selected prior has a worst-case mean dividend, $\underline{\theta}$, and a worst-case dividend volatility, $\overline{\sigma}$. Hence, the investor believes in the prior $d \sim N(\underline{\theta}, \overline{\sigma}^2)$. Intuitively, investor's perception of AIF has two key effects on his prior beliefs. First, the investor acts as if he chooses the lower bound of the mean dividend perceived range, implying a pessimistic view on firm's ambiguous fundamentals. Second, the investor is underconfident in his estimate of the mean dividend, as indicated by his choice of the upper bound of the dividend volatility perceived range (see Figure 3.1).

[27] For example, a structural change in a firm's business leaves more room for speculation and makes it difficult for investors to make reasonably precise projections of the firm's future cash flows. As a result, investors perceive the firm's fundamentals to be ambiguous.

At date $t = 1$, the firm discloses ambiguous news (e.g., a surprise announcement of an acquisition indicating a shift in the firm's strategy without disclosing further details). Formally, the news is represented by a signal

$$s_1 = d + \epsilon_1, \qquad (3.2)$$

where ϵ_1 is the noise associated with the signal and is normally distributed with mean zero and variance σ_1^2, $\epsilon_1 \sim N(0, \sigma_1^2)$. The investor perceives AII because he does not know how to interpret the surprise news due to lack of supporting information. He is unsure about the quality of the signal and thus perceives the precision of the signal, $1/\sigma_1^2$, to lie in some range. Formally, the investor perceives a set of likelihoods of the signal s_1, that is, $s_1 \sim N(\underline{\theta}, \overline{\sigma}^2 + \sigma_1^2)$, where $\sigma_1^2 \in [\underline{\sigma}_1^2, \overline{\sigma}_1^2]$, or $1/\sigma_1^2 \in [1/\overline{\sigma}_1^2, 1/\underline{\sigma}_1^2]$. Following Epstein and Schneider (2008), the investor acts as if he selects a worst-case assessment of quality, $1/\overline{\sigma}_1^2$, and thus believes in a worst-case likelihood, $s_1 \sim N(\underline{\theta}, \overline{\sigma}^2 + \overline{\sigma}_1^2)$.[28] This indicates that the investor is underconfident about the quality of the firm's ambiguous signal.

At date $t = 2$, the representative analyst issues a recommendation change on the firm. Formally, the recommendation change is represented by a signal

$$s_2 = d + \epsilon_2, \qquad (3.3)$$

where ϵ_2 is the noise associated with the signal and is normally distributed with mean zero and variance σ_2^2, $\epsilon_2 \sim N(0, \sigma_2^2)$. By assumption, there is no ambiguity about the quality of the signal s_2

[28] Epstein and Schneider (2008) assume that a worst-case likelihood depends on the value of the ambiguous signal. As a result, investors respond more strongly to bad news than to good news. In contrast, here I assume that a worst-case likelihood is independent of the value of the ambiguous signal, so that the ambiguous signal is always interpreted as of lowest quality. My assumption is more consistent with the empirical evidence on the underreaction to both good and bad news from firms with high information uncertainty (see for example, Zhang, 2006).

because ambiguity, as I define it, stems from the difficulty the investor faces in processing and acting on distorted information disclosed by the firm only. Hence, the variance of the noise associated with this signal, σ_2^2, is perceived to be constant. It follows that the investor believes in the likelihood $s_2 \sim N\left(\underline{\theta}, \overline{\sigma}^2 + \sigma_2^2\right)$.

Now I turn to deriving prices at dates $t = 0, 1, 2$. Because the ambiguity-averse investor is assumed to be risk-neutral and the riskless interest rate is assumed to be zero, the price of one share of the firm at each date satisfies

$$P_0 = E(d), \qquad (3.4.a)$$

$$P_1 = E(d|s_1), \qquad (3.4.b)$$

$$P_2 = E(d|s_1, s_2). \qquad (3.4.c)$$

At date $t = 3$, the dividend is revealed, and the firm's share price is equal to its true mean dividend, θ. By standard rules for updating normal random variables (Anderson, 1984),

$$P_0 = \underline{\theta}, \qquad (3.5.a)$$

$$P_1 = \frac{\overline{\sigma}_1^2}{\overline{\sigma}^2 + \overline{\sigma}_1^2}\underline{\theta} + \frac{\overline{\sigma}^2}{\overline{\sigma}^2 + \overline{\sigma}_1^2}s_1, \qquad (3.5.b)$$

$$P_2 = \frac{D - \overline{\sigma}^2\overline{\sigma}_1^2 - \overline{\sigma}^2\sigma_2^2}{D}\underline{\theta} + \frac{\overline{\sigma}^2\sigma_2^2}{D}s_1 + \frac{\overline{\sigma}^2\overline{\sigma}_1^2}{D}s_2, \qquad (3.5.c)$$

where $D = \overline{\sigma}^2\left(\overline{\sigma}_1^2 + \sigma_2^2\right) + \overline{\sigma}_1^2\sigma_2^2$.

At date $t = 0$, the firm's share price equals the lower bound of the price (mean dividend) perceived range, implying that the investor exhibits pessimistic views about firm's ambiguous fundamentals. This simple result emphasizes pessimism bias in investor's prior beliefs as a key implication of his aversion to lack of knowledge of nonexistent relevant information (in the next section, I shall use this intuition to motivate a proxy for AIF). At date $t = 1$, the firm's share price is a weighted average of the previous price, $\underline{\theta}$, and the firm's ambiguous signal, s_1. The weight associated with this signal is inversely related to investor's underconfidence in the quality of the signal, as measured by $\overline{\sigma}_1^2$ relative to σ_1^2. At date $t = 2$, the firm's share price is a weighted average of the previous price, $\underline{\theta}$, the firm's ambiguous signal, s_1, and the analyst's signal, s_2. The weight associated with the signal s_2 is of particular interest. It is straightforward to show that it is increasing in investor's underconfidence in his previous price estimate, $\underline{\theta}$, as measured by $\overline{\sigma}^2$ relative to σ^2, and increasing in his underconfidence in the quality of the firm's ambiguous signal, as measured by $\overline{\sigma}_1^2$ relative to σ_1^2.[29] It follows that the weight the investor places on the analyst's signal equals the upper bound of its perceived range.

Let $w_{s_2}^{AIF,AII}$, $w_{s_2}^{AIF}$, $w_{s_2}^{AII}$, and w_{s_2} be the weights associated with the analyst's signal, s_2, under various ambiguity scenarios. These are defined as follows

$$w_{s_2}^{AIF,AII} = \frac{\overline{\sigma}^2\overline{\sigma}_1^2}{\overline{\sigma}^2\left(\overline{\sigma}_1^2+\sigma_2^2\right)+\overline{\sigma}_1^2\sigma_2^2}, \qquad (3.6.a)$$

$$w_{s_2}^{AIF} = \frac{\overline{\sigma}^2\sigma_1^2}{\overline{\sigma}^2(\sigma_1^2+\sigma_2^2)+\sigma_1^2\sigma_2^2}, \qquad (3.6.b)$$

$$w_{s_2}^{AII} = \frac{\sigma^2\overline{\sigma}_1^2}{\sigma^2\left(\overline{\sigma}_1^2+\sigma_2^2\right)+\overline{\sigma}_1^2\sigma_2^2}, \qquad (3.6.c)$$

$$w_{s_2} = \frac{\sigma^2\sigma_1^2}{\sigma^2(\sigma_1^2+\sigma_2^2)+\sigma_1^2\sigma_2^2}. \qquad (3.6.d)$$

By comparing these weights, one can infer the cross-sectional implications of ambiguity for the influence of the analyst's signal. Tedious calculations (see Appendix C) show that

[29] This result holds provided that the analyst's signal is not noise-free. That is, if the analyst's signal is fully informative ($\sigma_2 = 0$) then its weight equals one.

$$w_{s_2}^{AIF} - w_{s_2} > 0, \qquad (3.7.a)$$

$$w_{s_2}^{AII} - w_{s_2} > 0, \qquad (3.7.b)$$

$$\left(w_{s_2}^{AIF,AII} - w_{s_2}^{AIF}\right) - \left(w_{s_2}^{AII} - w_{s_2}\right) > 0, \qquad (3.7.c)$$

and I thus have the following proposition.

Proposition 1:

1. ***The "mean" effect:*** *Ambiguity-averse investors overweight analyst signals when they perceive either AIF or AII* (3.7.a)-(3.7.b).
2. ***The "interaction" effect:*** *Ambiguity-averse investors who perceive AII overweight more analyst signals when firm fundamentals are perceived to be ambiguous* (3.7.c).

3.2.2. Empirical proxies

This chapter proposes a novel measure of AIF and follows prior work in measuring AII. Because the perception of AIF pertains to investors' beliefs, it cannot be directly observed and therefore can only be measured through its implications for the firm value. Based on the discussion from the previous section, investor's lack of knowledge of nonexistent relevant information at the decision time has two key effects that should be accounted for in any empirical proxy for AIF. First, the ambiguity-averse investor perceives the firm's share price, P_0, to lie in the range $[\underline{\theta}, \overline{\theta}]$. Intuitively, the more severe the deviation from complete information, the higher the uncertainty the investor faces and the more diffuse the price range he perceives. That is, with higher fundamental uncertainty, the investor perceives $\underline{\theta}$ and $\overline{\theta}$ to be lower and higher, respectively, indicating higher uncertainty with respect to the firm's value. In the cross-section of firms, the ratio $\frac{\overline{\theta}-\underline{\theta}}{\underline{\theta}}$ is likely to capture such an effect.[30] Second, the investor is pessimistic about firm's ambiguous fundamentals. This is a direct implication of the intuitive assumption that this investor acts as if he selects a worst-case prior when he perceives a set of multiple priors he has to choose from. Pessimism bias in investor's beliefs causes the firm's share price, P_0, to trade at a low level, $\underline{\theta}$. Empirically, one would expect that the more severe the investor's pessimism bias, the closer the price to this low level. To measure this effect in the cross-section of firms, I propose the ratio $\frac{\overline{\theta}-P_0}{\overline{\theta}-\underline{\theta}}$.[31] Because both ratios capture distinct, key implications of AIF that should be reflected in any successful empirical proxy, I multiply them to yield the measure $\frac{\overline{\theta}-P_0}{\underline{\theta}}$. This measure reaches its highest value when $P_0 = \underline{\theta}$ and attains its lowest value (zero) when $P_0 = \overline{\theta}$.

Obviously, the high and low prices, $\overline{\theta}$ and $\underline{\theta}$, pertain to investor's beliefs and thus can never be observed. Nevertheless, one can employ salient reference points from the price history as empirical proxies for these perceived prices. It is natural to consider a stock's 52-week high and low prices as empirical proxies for $\overline{\theta}$ and $\underline{\theta}$.[32] After all, these salient prices are widely publicized in the financial media and may well be used by investors as reference prices in formulating beliefs.[33] Formally, my empirical proxy for AIF is a stock price's nearness to its 52-week low, or *N52WL*, which is defined as

$$N52WL_{i,m} = \left(52WH_{i,m} - P_{i,m}\right)/52WL_{i,m}, \qquad (3.8)$$

[30] I scale the perceived price difference by $\underline{\theta}$ to make the final measure comparable across firms.

[31] The ratio equals one when $P_0 = \underline{\theta}$ and zero when $P_0 = \overline{\theta}$.

[32] Unreported results show that all findings hold, irrespective of the choice of either six-month or two-year high and low prices as empirical proxies for $\underline{\theta}$ and $\overline{\theta}$.

[33] Prior literature provides ample evidence on the importance of the 52-week high and low prices in investment decision making. For instance, George and Hwang (2004) show that a stock price's nearness to its 52-week high drives profits from momentum investing; Huddart, Lang, and Yetman (2009) find that trading volume increases when stock prices cross either their 52-week highs or lows; Baker, Pan, and Wurgler (2012) provide evidence that the target's 52-week high price acts as a psychological reference point in mergers and acquisitions.

where m is the calendar month prior to the announcement of a recommendation change; $P_{i,m}$ is stock i's price observed at the end of month m; and $52WH_{i,m}$ and $52WL_{i,m}$ are stock i's 52-week high and low prices, respectively, determined from the 52-week period prior to the end of month m.

I follow prior literature and employ the reciprocal of firm size, reciprocal of analyst coverage, and reciprocal of firm age as empirical proxies for AII (Hirshleifer, 2001; Jiang, Lee, and Zhang, 2005; Zhang, 2006; Autore, Billingsley, and Schneller, 2009). It seems plausible that smaller firms offer investors information environments of lower quality. Since information production costs are typically fixed, small firms are likely to produce less information than large firms. Firm size (*ME*) is measured as the market capitalization (in millions of dollars) at the end of month m. I also use the reciprocal of firm age as a proxy for AII. Compared to old firms, young firms release less information to the public, receive little media coverage, and attract less regulatory attention, thereby offering investors information environments of lower quality. Firm age (*AGE*) is defined as the number of quarters between the first time a firm's stock appears in CRSP and month m. I also employ the reciprocal of analyst coverage as a proxy for AII because analysts are typically reluctant to cover firms with poor-quality information environments. As a result, investors are likely to receive little information from firms with lower analyst coverage and thus face higher information uncertainty. Analyst coverage (*ACOV*) is defined as the total number of analysts following a firm during the 12-month period prior to the end of month m. As noted by Zhang (2006), each empirical proxy might also capture other effects, thereby confounding any inferences. Nonetheless, when these proxies are taken together, they have the common ability to capture information uncertainty.

3.3. Sampling procedure and descriptive statistics

This section describes the sample formation process, presents the features of sample recommendation changes, and discusses descriptive statistics of variables of interest.

3.3.1. Sample formation

The sample of analyst recommendations is from the Thompson Reuters Institutional Brokers' Estimate System (I/B/E/S) U.S. Detail Recommendations File. Thompson Reuters receives different wordings from different brokerage firms but then translates them into numerical scores on the following scale: strong buy = 1, buy = 2, hold = 3, sell = 4, and strong sell = 5. To construct my sample, I select all valid recommendations from 1993 (inception) to 2010. Valid recommendation data consist of recommendations issued on common equity for which the activation dates, ACTDATS, and review dates, REVDATS, are subsequent to the announcement dates, ANNDATS; the analyst identification code, AMASKCD, and the firm identification codes, TICKER and CUSIP, are not missing; and recommendation levels are between one and five.

To construct recommendation changes, I follow Ljungqvist, Malloy, and Marston (2009) and code ratings as follows: the first time an analyst issues a recommendation on a given firm in I/B/E/S is an initiation.[34] Following recommendations are categorized as either outstanding or non-outstanding. Outstanding recommendations are those that are not stopped by the brokerage firm and for which no more than 12 months have elapsed since the previous recommendation was confirmed.[35] Outstanding recommendations are further broken down into upgrades, downgrades, and reiterations. Non-outstanding recommendations are coded as reinitiations. Ultimately, only

[34] One key difference, though, between my sample construction procedure and that described in Ljungqvist, Malloy, and Marston (2009) is as follows: they define a recommendation change at the level of broker/firm pairs, whereas I define a recommendation change at the level of analyst/firm pairs. Since analysts may be employed by a new broker at any given point in time, it seems more appropriate to define a recommendation change at the level of analyst/firm rather than broker/firm pairs.

[35] I use the review date, REVDATS, of the previous recommendation for the confirmation status and the I/B/E/S stop file, RECDSTP, to check for broker scale changes and suspensions/terminations of broker coverage.

upgrades and downgrades announced between 1994 and 2010 are retained in the sample. By construction, an upgrade's (downgrade's) magnitude ranges between one and four (-4 and -1).

Following prior studies, I apply two screens to the sample of recommendation changes to minimize potential confounding effects. Bradley, Jordan, and Ritter (2008) attribute recommendation change clustering to firm-specific news. Welch (2000) argues that analysts herd based on little information. In either case, it is important to insulate the sample from data clustering biases. For a given firm, I ensure that any upgrade (downgrade) that is preceded by another upgrade (downgrade) in the two previous trading days is excluded from the sample. Altinkiliç and Hansen (2009) argue that analyst recommendation changes do not have a material market reaction once corporate announcements are removed. To minimize the effects of firm-specific news, I drop any recommendation change that is preceded by an earnings announcement in the two previous trading days. After both data screens are applied, the sample consists of 84,140 upgrades and 96,721 downgrades.

Finally, I merge the refined sample of recommendation changes with other variables (defined in Appendix D) constructed based on data from several sources. Stock data are from CRSP, accounting information is from Compustat, data on institutional ownership are from Thompson Reuters, and information on analyst affiliation is from the SDC database. The merged final sample retains only recommendation changes for which stock returns and the main empirical proxies for ambiguity are not missing. The merged final sample consists of 62,242 upgrades and 71,507 downgrades.

For a sample recommendation change j issued on a stock i, I compute a two-day buy-and-hold abnormal return as

$$BHAR_j(0,1) = \prod_{t=0}^{1}\left(1 + r_{i,t}\right) - \prod_{t=0}^{1}\left(1 + r_{i,t}^{DGTW}\right), \qquad (3.9)$$

where $r_{i,t}$ is the raw return on stock i on day t and $r_{i,t}^{DGTW}$ is the date-t raw return on a benchmark portfolio with comparable size, book-to-market, and momentum characteristics as stock i (Daniel, Grinblatt, Titman, and Wermers, 1997, hereafter DGTW).[36] Then, an average two-day buy-and-hold abnormal return is calculated as

$$ABHAR(0,1) = \frac{1}{n}\sum_{j=1}^{n} BHAR_j\,(0,1), \qquad (3.10)$$

where n equals the number of recommendation changes.

3.3.2. Sample features

Panel A of Table 3.1 shows the transition frequencies of recommendation changes. Analyst recommendations are highly optimistic, with strong buy, buy, and hold recommendations making up the largest percentage of sample recommendations (more than 90%).[37] Furthermore, transition frequencies are strikingly high within these three recommendation categories. For example, among prior hold recommendations, 34.8% are upgraded to current strong buy, 46% are upgraded to current buy recommendations, and less than 20% are downgraded to current sell or strong sell recommendations.

Panel B of Table 3.1 presents the distribution of recommendation changes according to their direction and magnitude. The distribution appears to be symmetric in that recommendation changes with higher magnitude are less frequent in both directions. The one-notch and two-notch recommendation changes make up more than 98% of the total number of sample recommendation

[36] In unreported results, I compute an average three-day buy-and-hold abnormal return, from day -1 to 1, and find that my conclusions about the effects of AIF and AII remain unchanged.

[37] (30,536+36,587+54,807)/133,749=91.16%.

changes, which is consistent with the view that analysts strongly favor gradual diffusion of information.[38]

3.3.3. Descriptive statistics

Table 3.2 presents descriptive statistics for variables of interest: an event two-day buy-and-hold abnormal return (*BHAR(0,1)*), a stock price's nearness to its 52-week low (*N52WL*), firm size (*ME*), firm analyst coverage (*ACOV*), and firm age (*AGE*). Panel A (B) shows results for the subsample of upgrades (downgrades). The results indicate that event *BHARs* are consistent with the direction of recommendation changes, but they are also highly asymmetric. For example, upgrade *BHARs* have a mean of 2.12% and a median of 1.28%, indicating a positive skewness in the distribution. They are also volatile as indicated by the standard deviation of 6.80%. By construction, the measure *N52WL* is bounded by zero from below and unbounded from above. Its mean is slightly higher in the subsample of downgrades (0.996 versus 0.783), implying that downgrades are on average more likely than upgrades to be issued on stocks whose prices are close to their 52-week lows. The mean *ME* in the subsample of upgrades (8,427 $m) is higher than that in the subsample of downgrades (7,348.4 $m), suggesting that analysts tend to upgrade rather than downgrade large firms. On average, about nine analysts cover both upgraded and downgraded firms. Not surprisingly, upgraded firms are on average older than downgraded firms (81.48 quarters versus 75.90 quarters).

Table 3.3 reports the Pearson (above the diagonal) and Spearman (below the diagonal) correlations for the main variables. Panels A and B show that the influence of both upgrades and downgrades, as measured by *BHARs*, is higher for smaller firms, younger firms, firms with lower analyst coverage, or firms whose stock prices are closer to their 52-week lows. Moreover, the three measures *ME*, *ACOV*, and *AGE* appear to be highly positively correlated. For example using

[38] (45,553+24,735+40,691+20,751)/133,749=98.49%.

Spearman correlations in the subsample of upgrades (Panel A), the correlations between *ME* on one hand and *ACOV* and *AGE* on the other hand are 0.636 and 0.475, respectively, implying that these measures capture common firm characteristics. I can also draw a similar conclusion from the subsample of downgrades in Panel B.

3.4. Ambiguity aversion and the influence of recommendation changes

The general idea of this chapter is that ambiguity-averse investors overweight analyst signals when they perceive higher levels of ambiguity in the firm environment. I focus on two aspects of ambiguity in the firm environment: the ambiguity aspect that emerges from lack of knowledge of nonexistent relevant information and that manifests itself through the difficulty investors face in formulating prior beliefs about firm fundamentals, or AIF; and the ambiguity aspect that pertains to the difficulty investors face in processing firm information of uncertain quality, or AII. I empirically evaluate this central idea using several tests and proxies for ambiguity. First, I test the effects of each aspect of ambiguity separately (the "mean" effect). Second, I investigate the implications of AII when firm fundamentals are perceived to be more or less ambiguous (the "interaction" effect).

3.4.1. The "mean" effect

Ambiguity-averse investors overweight analyst signals when they perceive higher levels of either AIF or AII. As a result, this chapter's first main hypothesis states that recommendation changes issued on higher AIF or higher AII firms are more influential. The two empirical predictions implied by this hypothesis are: (1) recommendation changes issued on firms whose stock prices are closer to their 52-week lows are associated with higher magnitude of event abnormal returns, and (2) recommendation changes issued on smaller firms, younger firms, or firms with lower analyst coverage are associated with higher magnitude of event abnormal returns. This section conducts several tests of these predictions. First, I present results of univariate tests. Second, I conduct regressions of event *BHARs* on ambiguity measures, allowing for the effects of other relevant factors.

Table 3.4 presents event *ABHARs* across ambiguity quintiles formed based on the measures *N52WL*, reciprocal of *ME*, reciprocal of *ACOV*, or reciprocal of *AGE*. This table provides consistent evidence that recommendation changes in the highest ambiguity quintile are much more influential than those in the lowest ambiguity quintile. In particular, recommendation changes issued on high-AIF stocks are about three times as influential as those issued on low-AIF stocks. For example, in Panel B of Table 3.4, downgrades issued on stocks whose prices are closest to their 52-week lows are associated with an event *ABHAR* equal to -4.06%, whereas downgrades issued on stocks whose prices are closest to their 52-week highs are associated with an event *ABHAR* equal to only -1.19%. Moreover, irrespective of the direction of recommendation changes and the choice of empirical proxies for AII, Table 3.4 shows that recommendation changes issued on high-AII stocks are more influential than those issued on low-AII stocks. For example, in Panel A of Table 3.4, upgrades issued on smallest firms are more influential than those issued on largest firms by about 2.38%.

In Table 3.5, I investigate the effects of ambiguity measures on event *BHARs* in a multivariate setting. To examine these effects while controlling for other relevant factors that could influence event *BHARs*, I employ the specification

$$BHAR_j(0,1) = \beta_0 + \beta_1 \times N52WL_j\ (> 70th)\ dummy + \beta_2 \times 1/ME_j\ (> 70th)\ dummy + \beta_3 \times 1/ACOV_j\ (> 70th)\ dummy + \beta_4 \times 1/AGE_j\ (> 70th)\ dummy + \beta X_j + \varepsilon_j. \quad (3.11)$$

The dependent variable is the *BHAR(0,1)* associated with a recommendation change j. The explanatory variables of interest are $N52WL_j\ (> 70th)\ dummy$, $1/ME_j\ (> 70th)\ dummy$,

$1/ACOV_j\ (> 70th)\ dummy$, and $1/AGE_j\ (> 70th)\ dummy$. These are dummy variables indicating that the measures $N52WL$, reciprocal of ME, reciprocal of $ACOV$, and reciprocal of AGE are above their respective 70th percentiles. Hence, $N52WL_j\ (> 70th)\ dummy$ is designed to capture high levels of AIF, whereas $1/ME_j\ (> 70th)\ dummy$, $1/ACOV_j\ (> 70th)\ dummy$, and $1/AGE_j\ (> 70th)\ dummy$ are constructed to measure high levels of AII. If investors are more responsive to recommendation changes when they perceive higher levels of both aspects of ambiguity, then one would expect the coefficients β_1, β_2, β_3, and β_4 to be significantly positive in the subsample of upgrades and negative in the subsample of downgrades. To put an accurate interpretation on these coefficients, I should control for moderate levels of ambiguity. The vector X in Equation (3.11) includes the control variables $N52WL_j\ (> 30th, < 70th)\ dummy$, $1/ME_j\ (> 30th, < 70th)\ dummy$, $1/ACOV_j\ (> 30th, < 70th)\ dummy$, and $1/AGE_j\ (> 30th, < 70th)\ dummy$. For example, $N52WL_j\ (> 30th, < 70th)\ dummy$ is a dummy variable indicating the measure *N52WL* is above its 30th and below its 70th percentile. Other dummy variables are constructed in the same way. Hence, by including these control variables the coefficients β_1, β_2, β_3, and β_4 can be interpreted as the incremental effects of high relative to low ambiguity on the influence of recommendation changes.

The vector X also includes a battery of control variables that may contribute to the influence of recommendation changes. These are: a book-to-market ratio (*BM*), price momentum (*MOM*), institutional ownership (*IO*), the natural logarithm of analyst experience (*Ln(AEXP)*), an analyst affiliated dummy (*AFF dummy*), an analyst independent dummy (*IND dummy*), recommendation deviation from consensus (*DC*), magnitude of a recommendation change (*MAG*), a Regulation Fair Disclosure dummy (*FD dummy*), and a Global Research Analyst Settlement dummy (*GS dummy*). These variables are defined in Appendix D. To draw correct statistical inferences, I address simultaneous residual correlations over time and across firms. Following

Petersen (2009), I include calendar year dummies in all regressions and estimate standard errors clustered at the firm level.[39]

Panel A of Table 3.5 shows the estimation results of several versions of Equation (3.11) in the subsample of upgrades. The results consistently show that high levels of ambiguity are associated with substantially greater influence of upgrades. In specification [1], upgrades issued on stocks whose prices are closest to their 52-week lows (high-AIF stocks) are more influential than those issued on stocks whose prices are closest to their 52-week highs (low-AIF stocks) by 179 bps $(t = 21.77)$. In specification [2], the results suggest that upgrades issued on high-AII stocks are more influential than those issued on low-AII stocks. For example, relative to upgrades issued on large firms, those issued on small firms are more influential by 194 bps $(t = 18.62)$. Specification [3] shows the estimation results for the full version of Equation (3.11). All coefficients of proxies for high ambiguity are both economically and statistically significant. For example, a stock price's nearness to its 52-week low increases the influence of upgrades by 117 bps $(t = 14.88)$. The effects of the three proxies for high AII are also significant, but the size-based proxy appears to have the strongest economic effect. Indeed, upgrades issued on small firms are more influential than those issued on large firms by 153 bps $(t = 15.18)$.

The coefficients of the control variables generally take on the expected signs, but their economic and statistical significance appears to be mitigated by the inclusion of proxies for high ambiguity. To illustrate, consider specification [3]. Although *BM, MOM, IO,* and *Ln(AEXP)* are positively correlated with event *BHARs*, their effects are neither statistically nor economically significant. The coefficients on analyst dummies suggest that upgrades from affiliated analysts increase event *BHARs* by 61 bps $(t = 6.58)$, whereas upgrades from independent analysts decrease event *BHARs* by 98 bps $(t = -12.08)$. The coefficients on *DC* and *MAG* imply that the more an upgrade deviates from consensus or the higher its magnitude, the more it is influential.

[39] I do not report the coefficients of the year dummies here.

Finally, the coefficient of *FD dummy* is insignificant, whereas the coefficient of *GS dummy* is 110 bps ($t = 3.86$).

Panel B of Table 3.5 shows similar evidence on the positive relation between high ambiguity and the influence of recommendation changes in the subsample of downgrades. Focusing on specification [3], downgrades issued on stocks whose prices are closest to their 52-week lows (high-AIF stocks) are associated with event *BHARs* that are relatively higher in magnitude by 211 bps ($t = -18.67$). Relative to downgrades issued on low-AII stocks, those issued on high-AII stocks are substantially more influential. For example, downgrades issued on stocks with lowest analyst coverage are more influential than those issued on stocks with highest analyst coverage by 81 bps ($t = -6.92$). The effects of size-based and age-based AII proxies are also in the same direction with comparable economic significance.

Focusing on the effects of control variables, the coefficient of *BM* is insignificant. The coefficients on *MOM*, *IO*, and *Ln(AEXP)* suggest that downgrades issued on high-momentum stocks, downgrades issued on higher institutional ownership stocks, or downgrades from more experienced analysts are more influential. As expected, the coefficients on analyst dummies imply that downgrades from affiliated analysts are more influential by 107 bps ($t = -8.73$), whereas downgrades from independent analysts are less influential by 143 bps ($t = 9.11$). The coefficients of recommendation characteristic variables take on the expected signs. Downgrades issued away from consensus or downgrades of higher magnitude are more influential. Downgrades issued after Regulation Fair Disclosure came into effect are less influential by 57 bps ($t = 2.07$), whereas those issued after the enactment of the Global Research Analyst Settlement are more influential by 103 bps ($t = -4.10$).

The general picture that emerges from these results is that investors are much more responsive to recommendation changes when they perceive higher levels of ambiguity in the firm environment. The evidence holds for both aspects of ambiguity irrespective of the direction of recommendation changes and persists even after controlling for other relevant factors, such as deviation from consensus and stock price momentum.

3.4.2. The "interaction" effect

I argue that the degree to which AII affects the influence of analyst signals will vary in a predictable manner across firms with differing levels of AIF. Ambiguity-averse investors may well be more concerned about the uncertain-quality of firm signals when they perceive firm fundamentals to be more ambiguous and thus place too little weight on firm signals and overweight more analyst signals. As a result, the second main hypothesis of this chapter emerges: recommendation changes issued on high-AII firms are more influential when their fundamentals are perceived to be more ambiguous. The empirical prediction that follows is that recommendation changes issued on small firms, young firms, or firms with low analyst coverage are associated with higher magnitude of event *ABHARs* when their stock prices are closer to their 52-week lows. In what follows, I conduct both univariate and multivariate analyzes testing this empirical prediction.

In Table 3.6, I use a double sort procedure and divide the sample into low-, moderate-, and high-AIF firm groups based on the 30th and 70th percentiles of *N52WL* and further divide each of these groups into low-, moderate-, and high-AII firm subgroups based on the 30th and 70th percentiles of either the reciprocal of *ME* (Panel A), reciprocal of *ACOV* (Panel B), or reciprocal of *AGE* (Panel C). I find that irrespective of the direction of recommendation changes and the choice of empirical proxies for AII, the difference in influence between recommendation changes issued on high-AII firms and those issued on low-AII firms generally increases with AIF. For example, when I use the reciprocal of *ACOV* as a proxy for AII (Panel B), the magnitude of this difference increases from 0.83% to 1.94% in the case of upgrades and from 0.66% to 1.57% in the case of downgrades as AIF increases from a low to high level. Moreover, Table 3.6 also shows that, in all cases, recommendation changes issued on high-AIF, high-AII firms are about *four* times as influential as those issued on low-AIF, low-AII firms. For example, when I use the reciprocal of *AGE* as a proxy for AII (Panel C), upgrades (downgrades) issued on high-AIF, high-AII firms are associated with an event *ABHAR* equal to 3.84% (-4.51%) compared with only 1% (-0.94%) for upgrades (downgrades) issued on low-AIF, low-AII firms.

Table 3.7 mainly answers how the relation between AII and the influence of recommendation changes varies with AIF after other relevant factors are controlled for. I divide the sample into low-, moderate-, and high-AIF firm subsamples based on the 30th and 70th percentiles of *N52WL*, and for each of these subsamples I estimate the specification

$$BHAR_j(0,1) = \beta_0 + \beta_1 \times 1/ME_j\ (> 70th)\ dummy + \beta_2 \times 1/ACOV_j\ (> 70th)\ dummy + \beta_3 \times 1/AGE_j\ (> 70th)\ dummy + \beta X_j + \varepsilon_j. \qquad (3.12)$$

This specification is similar to that in Equation (3.11), except for here I exclude the variables $N52WL_j\ (> 70th)\ dummy$ and $N52WL_j\ (> 30th, < 70th)\ dummy$. All variables included in Equation (3.12) are defined as before (see Appendix D). If investors are more responsive to recommendation changes issued on high-AII firms when their fundamentals are perceived to be more ambiguous, one would expect the coefficients β_1, β_2, and β_3 to increase in magnitude with AIF. Specifically, I expect these coefficients to be more positive in the subsample of upgrades and more negative in the subsample of downgrades among higher AIF firms.

Table 3.7 shows estimation results of the above specification and provides unequivocal evidence that the positive relation between AII and the influence of recommendation changes is weakest when AIF is perceived to be lowest and strongest when AIF is perceived to be highest. This main conclusion holds irrespective of the direction of recommendation changes and the choice of empirical proxies for AII. For example, in Panel B of Table 3.7, regression [1] (low-AIF firm subsample) suggests that downgrades issued on smaller firms, downgrades issued on firms with lower analyst coverage, and downgrades issued on younger firms are more influential by only 43 $(t = -2.71)$, 47 $(t = -4.07)$, and 24 bps $(t = -2.18)$, respectively. In contrast, regression [3] (high-AIF firm subsample) shows that downgrades issued on smaller firms, downgrades issued on firms with lower analyst coverage, and downgrades issued on young firms are more influential by 116 $(t = -3.17)$, 151 $(t = -5.12)$, and 147 bps $(t = -4.34)$, respectively.

These results have an intuitive interpretation: when ambiguity-averse investors are uncertain about the quality of firm information, their uncertainty scales with more ambiguous fundamentals. Because these investors dislike uncertainty, they exhibit stronger underconfidence in the quality of firm information and thus act as if they underweight more that information and overweight more analyst information. As a result, they are more responsive to recommendation changes issued on firms with poor-quality information environments when their fundamentals are perceived to be more ambiguous.

3.5. Robustness tests

This section presents results of several robustness tests. First, I consider an alternative definition for influence of recommendation changes. Second, I examine the implications of differences in investors' opinions for the influence of recommendation changes. Third, I investigate whether the measure *N52WL* is related to recommendation deviation from consensus or stock price prior performance.

3.5.1. Loh and Stulz's (2011) definition of influence of a recommendation change

Table 3.8 shows the proportions of influential recommendation changes across AIF/AII firm subgroups. These proportions are calculated as follows. First, following Loh and Stulz

(2011), I classify a recommendation change as influential if the associated event *BHAR(0,1)* is both in the correct direction and statistically significant. An event *BHAR(0,1)* is classified as statistically significant if it satisfies the condition $|BHAR(0,1)| > 1.96 \times \sqrt{2} \times \sigma_\varepsilon$, where σ_ε is the residual standard deviation of stock daily returns against the three Fama–French factors in the three-month period prior to the end of month m.[40] Second, I use a double sort procedure and divide the sample into low-, moderate-, and high-AIF firm groups based on the 30th and 70th percentiles of *N52WL* and further divide each of these groups into low-, moderate-, and high-AII firm subgroups based on the 30th and 70th percentiles of either the reciprocal of *ME* (Panel A), reciprocal of *ACOV* (Panel B), or reciprocal of *AGE* (Panel C). Third, I compute for each AIF/AII firm subgroup the proportion of influential recommendation changes as the number of influential recommendation changes scaled by the total number of recommendation changes within that subgroup.

Table 3.8 consistently shows that the proportion of influential recommendation changes among high-AIF, high-AII firms is substantially higher than that among low-AIF, low-AII firms. I can draw this conclusion regardless of the direction of recommendation changes and the choice of empirical proxies for AII. For example, using the reciprocal of *ACOV* as a proxy for AII (Panel B), the proportion of influential upgrades (downgrades) issued on high-AIF, high-AII firms is 17.51% (17.53%) compared with only 9.22% (8.97%) for upgrades (downgrades) issued on low-AIF, low-AII firms. I note that the proportions of influential recommendation changes across AIF/AII firm subgroups seem to be low, ranging roughly between 8% and 17%. Nevertheless, this range is quite comparable with Loh and Stulz's (2011) finding that only 12% of recommendation changes are influential. The influence test adopted here is somewhat stringent in that it requires a recommendation change to move the stock price by roughly 2.77 daily standard deviations in the

[40] To estimate idiosyncratic volatility, I require a minimum of 50 trading days. The daily idiosyncratic volatility estimate is scaled by $\sqrt{2}$ since the event *BHAR(0,1)* is computed over a two-day event window. Here 1.96 is the critical value from a normal distribution with a 5% significance level.

right direction over a two-day period. According to a normal distribution, however, such event can occur with only 2.5% probability. Hence, on a relative basis, an influence probability of 17% is quite large and suggests that recommendation changes issued on high-AIF, high-AII firms are likely to have to a large identifiable impact on their stock prices.

3.5.2. Divergence in investors' opinions and the influence of recommendation changes

Prior studies investigate the implications of differences of investors' opinions for stock prices and post-earnings announcement drifts (Diether, Malloy, and Scherbiba, 2002; Anderson, Harris, and So, 2007) as well as for takeover announcement characteristics (Moeller, Schlingemann, and Stulz, 2007; Chatterjee, John, and Yan, 2012). Diether, Malloy, and Scherbiba (2002) contend that dispersion in earnings forecasts is a reasonable proxy for differences of opinion among investors. Recently, Loh and Stulz (2011) find that recommendation changes issued on firms with higher dispersion in earnings forecasts are more influential. Unlike Loh and Stulz, I go one step further and investigate whether the positive relation between differences of investors' opinions and the influence of recommendation changes is stronger when public information is of lower quality.

I argue that for firms with information environments of uncertain quality differences in investors' opinions could exacerbate the influence of recommendation changes. When opinions diverge in a poor-quality information environment, optimists would overestimate the quality of good news and underestimate the quality of bad news. Pessimists, on the other hand, would do exactly the opposite. Hence, trading will likely generate high volume but also little returns as optimists do not take bad news seriously and pessimists tend to discount good news, resulting in underreaction to both good and bad news. As new information from analysts arrives, these investors update their beliefs in the direction implied by that information: optimists take less optimistic views following downgrade announcements, and pessimists become less pessimistic after upgrade announcements. As a result, one would expect investors with divergent views to be more responsive to both upgrades and downgrades as they update their beliefs in the right direction.

Like Diether, Malloy, and Scherbiba (2002), I employ dispersion in earnings forecasts (*FDISP*) to proxy for divergence in investors' opinions.[41] I estimate *FDISP* as the standard deviation of analyst annual EPS forecasts (a minimum of three forecasts) submitted any time during the three-month period prior to the end of month m, scaled by the stock price as of the end of this period. If an analyst makes more than one forecast during the three-month period, only the last forecast is used in my calculations.

In Table 3.9, I use a double sort procedure and divide the sample into low-, moderate-, and high-AII firm groups based on the 30th and 70th percentiles of either the reciprocal of *ME* (Panel A), reciprocal of *ACOV* (Panel B), or reciprocal of *AGE* (Panel C), and further divide each of these groups into low-, moderate-, and high-divergence firm subgroups based on the 30th and 70th percentiles of *FDISP*. I find that for any given level of AII divergence in investors' opinions increases the influence of recommendation changes. Moreover, the increase in influence is notably higher among high- than low-AII firms. For example, in Panel C of Table 3.9, the difference in influence between upgrades issued on young, high-divergence firms and those issued on young, low- divergence firms is equal 1.41% compared with only 0.62% as the difference in influence between upgrades issued on old, high-divergence firms and those issued on old, low-divergence firms. Hence, differences of investors' opinions raise the influence of recommendation changes, but such an effect is stronger among firms with information environments of lower quality.

[41] In unreported results, I also obtain similar evidence when I use idiosyncratic volatility as an alternative proxy for divergence in investors' opinions.

3.5.3. Is the measure N52WL related to deviation from consensus or momentum effects?

In this section, I investigate whether the measure *N52WL* captures other effects, such as those related to recommendation deviation from consensus or stock price prior performance. It is natural to ask whether the positive relation between *N52WL* and the influence of recommendation changes is driven by recommendation deviation from consensus: if investors recognize that certain recommendation changes do not add information and simply move toward consensus, they will rationally respond less to these recommendation changes than to those that deviate from consensus (Jegadeesh and Kim, 2010). In unreported results, I find that the correlation between *N52WL* and *DC* is positive in the subsample of upgrades and negative in the subsample of downgrades. This implies that upgrades (downgrades) moving toward consensus are more likely to be issued when the price is near its 52-week high (low). Thus, if deviation from consensus drives the positive relation between *N52WL* and the influence of recommendation changes, one would expect investors to respond less strongly to upgrades issued on stocks with prices near their 52-week highs and to downgrades issued on stocks with prices near their 52-week lows. However, the univariate evidence is at odds with this joint prediction since, in particular, downgrades issued on stocks with prices near their 52-week lows are the most influential when compared to other downgrades (Panel B of Table 3.4).

Alternatively, it is possible that *N52WL* simply captures momentum effects: a stock whose price increases (decreases) to levels near its 52-week high (low) is likely to be associated with a positive (negative) price trend. Based on the univariate evidence, however, it is unlikely that momentum drives the positive relation between *N52WL* and the influence of recommendation changes. This is because a momentum-based explanation predicts, in particular, that upgrades issued on stocks with prices near their 52-week highs will be more influential than what the evidence in Table 3.4 implies. Indeed, Panel A of Table 3.4 shows that upgrades issued on stocks with prices near their 52-week highs are the least influential when compared to other upgrades.

Table 3.10 provides further evidence against these concerns. If the measure *N52WL* is independent of deviation from consensus or stock price momentum effects, then its explanatory power for the influence of recommendation changes should persist after controlling for these effects. In Table 3.10, I use a double sort procedure and divide the sample into low-, moderate-, and high-deviation from consensus (Panel A) or momentum (Panel B) firm groups based on the 30th and 70th percentiles of *DC* or *MOM,* respectively, and further divide each of these groups into low-, moderate-, and high-AIF subgroups based on the 30th and 70th percentiles of *N52WL*. The results show consistent evidence that the influence of recommendation changes increases with *N52WL* within each firm group formed based on either deviation from consensus or momentum. For example, as shown in Panel A of Table 3.10, the magnitude of the event *ABHAR* of downgrades issued closer to consensus increases from 0.98% for stocks with prices closer to their 52-week highs to 3.05% for stocks with prices closer to their 52-week lows. Panel B of Table 3.10 shows that among upgrades issued high-momentum stocks, those issued on stocks with prices close to their 52-week lows are more influential than those issued on stocks with prices close to their 52-week highs by 1.38%. Moreover, among downgrades issued on low-momentum stocks, those issued on stocks with prices close to their 52-week lows are associated with an event *ABHAR* equal to -4% compared with only -2.32% for downgrades issued on stocks with prices close to their 52-week highs. Hence, I can conclude that neither deviation from consensus nor stock price momentum drives the positive relation between *N52WL* and the influence of recommendation changes.

3.6. Conclusion

There is substantial evidence of puzzling heterogeneity in investors' responses to the announcements of sell-side recommendation changes. As noted by Loh and Stulz (2011), few recommendation changes have a large identifiable impact on the stock price, implying that the effects of a typical recommendation change cannot be distinguished from noise. Motivated by recent, well-publicized analysts' reports on Apple and Nokia, I propose and empirically evaluate a theory of ambiguity that predicts stronger influence of recommendation changes issued on firms with more ambiguous environments. I focus on two aspects of ambiguity in the firm environment: the ambiguity aspect that emerges from lack of knowledge of nonexistent relevant information and that manifests itself through the difficulty investors face in formulating prior beliefs about firm fundamentals (ambiguity in fundamentals or AIF); and the ambiguity aspect that pertains to the difficulty investors face in updating their prior beliefs in response to firm information of uncertain quality, (ambiguity in information or AII).

I provide evidence that both aspects of ambiguity substantially increase the influence of recommendation changes (the "mean" effect). Moreover, I show that the influence of recommendation changes issued on higher AII firms substantially increases when their fundamentals are perceived to be more ambiguous (the "interaction" effect). One of the striking findings of this chapter is that recommendation changes issued on highest AIF, highest AII firms are about *four* times as influential as those issued on lowest AIF, lowest AII firms. In general, all findings are persistent irrespective of the direction of recommendation changes and the choice of empirical proxies for ambiguity.

To support my empirical findings, I propose a simple model of information processing in which a representative ambiguity-averse investor perceives ambiguity in the firm environment. When this investor perceives both aspects of ambiguity, he acts as if he underweights prior signals that imply fundamental uncertainty, underweights firm signals of uncertain quality, and overweights analyst signals.

This chapter focuses on a particular type of analyst information, that signaled by a recommendation change. It would be interesting to investigate whether the positive relation between ambiguity in the firm environment and the influence of analyst news holds beyond recommendations changes. For example, an interesting line of future research would be to investigate whether the effects of earnings forecast or target price revision announcements are stronger for firms with more ambiguous environments.

Figure 3.1. Conceptual view of AIF.
This figure depicts the conceptual implications of a situation in which an ambiguity-averse investor perceives AIF. The investor perceives a set of priors over dividend outcomes (the figure shows three of them) and acts as if he selects a worst-case prior from this set. The worst-case prior (selected) has a worst-case mean dividend, $\underline{\theta}$, implying investor's pessimistic views on firm's ambiguous fundamentals, and a worst-case dividend variance, $\overline{\sigma}^2$, indicating investor's underconfidence in his mean dividend estimate. The true prior (prior that would prevail with no perceived AIF) has a mean dividend equals θ and a dividend variance equals σ^2. The best-case prior has a best-case mean dividend, $\overline{\theta}$, and a best-case dividend variance, $\underline{\sigma}^2$.

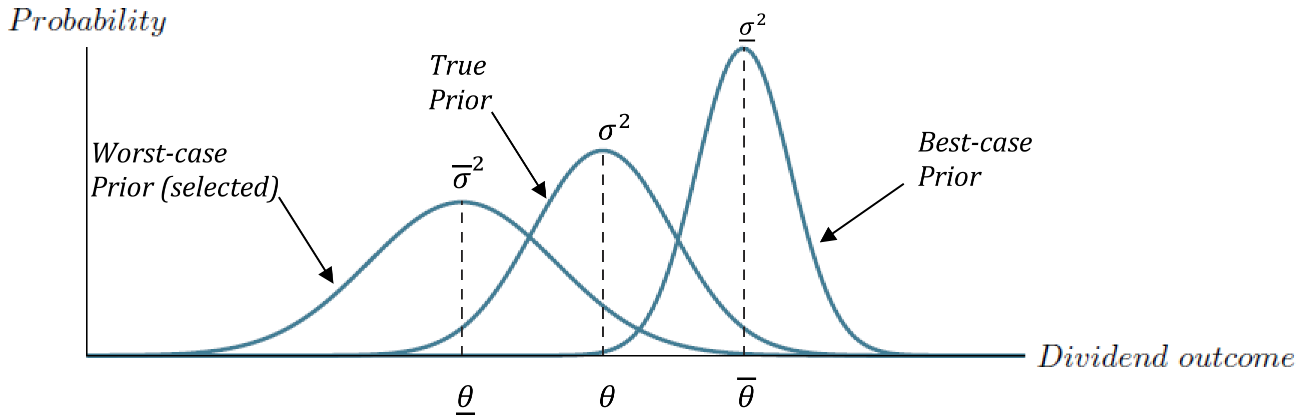

Table 3.1. Features of recommendation changes.
This table shows transition (Panel A) and magnitude frequencies (Panel B) of recommendation changes. The sample of recommendations is from the I/B/E/S U.S. Detail Recommendations File, from 1993 (inception) to 2010. Recommendations are associated with numerical scores on the following scale: strong buy = 1, buy = 2, hold = 3, sell = 4, and strong sell = 5. Upgrades and downgrades are constructed at the analyst/firm level from outstanding recommendations announced between 1994 and 2010. I identify outstanding recommendations as those recommendations not stopped by the brokerage firm and for which no more than 12 months have elapsed since the previous recommendation was confirmed. By construction, an upgrade's (downgrade's) magnitude ranges between one and four (-4 and -1). To minimize the effects of analyst herding or firm-specific news, I drop any recommendation change that is preceded by an earnings announcement or another recommendation change (issued in the same direction on the same firm) in the two previous trading days.

	Panel A: Transition frequency matrix of recommendation changes					
	Current recommendation					
Prior recommendation	1 (Strong buy)	2 (Buy)	3 (Hold)	4 (Sell)	5 (Strong sell)	Total
1 (Strong buy)	-	13,647	20,184	337	538	34,706
	-	39.3%	58.2%	1.0%	1.6%	100%
2 (Buy)	13,499	-	26,044	1,079	344	40,966
	33.0%	-	63.6%	2.6%	0.8%	100%
3 (Hold)	16,487	21,807	-	5,601	3,472	47,367
	34.8%	46.0%	-	11.8%	7.3%	100%
4 (Sell)	196	883	5,198	-	261	6,538
	3.0%	13.5%	79.5%	-	4.0%	100%
5 (Strong sell)	354	250	3,381	187	-	4,172
	8.5%	6.0%	81.0%	4.5%	-	100%
Total	30,536	36,587	54,807	7,204	4,615	133,749

	Panel B: Distribution of recommendation changes	
Magnitude	Frequency	Percentage
+4	354	0.3%
+3	446	0.3%
+2	20,751	15.5%
+1	40,691	30.4%
0	-	-
-1	45,553	34.1%
-2	24,735	18.5%
-3	681	0.5%
-4	538	0.4%
Total	133,749	100%

Table 3.2. Descriptive statistics.

This table shows descriptive statistics for variables of interest in the subsamples of upgrades (Panel A) and downgrades (Panel B). The variables are: an event two-day buy-and-hold abnormal return (*BHAR(0,1)*), a stock price's relative nearness to its 52-week low (*N52WL*), firm size (*ME*), analyst coverage (*ACOV*), and firm age (*AGE*). The event *BHAR(0,1)* is a two-day buy-and-hold return on a stock less a two-day buy-and-hold return on a DGTW portfolio with comparable size, B/M, and momentum characteristics. Other variables are defined in Appendix D. The sample of recommendation changes is from the I/B/E/S U.S. Detail Recommendations File, from 1994 to 2010.

						Percentiles				
Variable	*N*	*Mean*	*Std. Dev.*	*Skew*	*Kurt*	99%	75%	*Median*	25%	1%
					Panel A: Upgrades					
BHAR(0,1)	62,242	2.12%	6.80%	17.4	1466.0	23.35%	3.94%	1.28%	-0.73%	-10.25%
N52WL	62,242	0.783	2.425	30.0	2174.2	7.927	0.704	0.302	0.119	0.000
ME	62,242	8,427.0	24,728.0	7.9	89.3	123,979.0	5,710.1	1,578.7	484.8	35.8
ACOV	62,242	9.1	6.1	1.1	1.4	28	12	8	4	1
AGE	62,242	81.5	76.8	1.4	1.5	318	113	54	25	4
					Panel B: Downgrades					
BHAR(0,1)	71,507	-2.30%	9.47%	23.3	2236.2	16.40%	0.70%	-1.33%	-4.15%	-33.56%
N52WL	71,507	0.996	3.349	19.7	775.3	11.179	0.849	0.313	0.092	0.000
ME	71,507	7,348.4	22,909.0	8.7	108.5	107,480.0	4,712.4	1,274.7	386.1	27.8
ACOV	71,507	8.6	6.1	1.2	1.6	28	12	7	4	1
AGE	71,507	75.9	74.3	1.6	2.0	315	105	49	22	4

Table 3.3. Correlation matrix.

This table reports the Pearson (above the diagonal) and Spearman (below the diagonal) correlations for variables of interest. The variables are: an event two-day buy-and-hold abnormal return (*BHAR(0,1)*), a stock price's relative nearness to its 52-week low (*N52WL*), firm size (*ME*), analyst coverage (*ACOV*), and firm age (*AGE*). The event *BHAR(0,1)* is a two-day buy-and-hold return on a stock less a two-day buy-and-hold return on a DGTW portfolio with comparable size, B/M, and momentum characteristics. Other variables are defined in Appendix D. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The sample of recommendation changes is from the I/B/E/S U.S. Detail Recommendations File, from 1994 to 2010.

	BHAR(0,1)	*N52WL*	*ME*	*ACOV*	*AGE*
			Panel A: Upgrades		
BHAR(0,1)	1	0.101***	-0.057***	-0.074***	-0.072***
N52WL	0.111***	1	-0.058***	0.038***	-0.107***
ME	-0.103***	-0.321***	1	0.310***	0.283***
ACOV	-0.072***	-0.012***	0.636***	1	0.139***
AGE	-0.067***	-0.246***	0.475***	0.179***	1
			Panel B: Downgrades		
BHAR(0,1)	1	-0.024***	0.036***	0.046***	0.063***
N52WL	-0.126***	1	-0.057***	0.048***	-0.114***
ME	0.093***	-0.318***	1	0.304***	0.290***
ACOV	0.064***	0.015***	0.645***	1	0.156***
AGE	0.076***	-0.250***	0.497***	0.199***	1

Table 3.4. Event ABHARs sorted by ambiguity proxies: the "mean" effect.
This table shows event *ABHARs* across firm quintiles formed based on proxies for ambiguity. The proxies are: a stock price's nearness to its 52-week low (*N52WL*), reciprocal of firm size (1/*ME*), reciprocal of analyst coverage (1/*ACOV*), and reciprocal of firm age (1/*AGE*). The event *ABHAR(0,1)* is the average of two-day buy-and-hold returns on stocks less two-day buy-and-hold returns on DGTW portfolios with comparable size, B/M, and momentum characteristics. Other variables are defined in Appendix D. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Difference *t*-tests are based on the Satterthwaite method (of unequal variances). The sample of recommendation changes is from the I/B/E/S U.S. Detail Recommendations File, from 1994 to 2010.

	Sorted by *N52WL*	Sorted by 1/*ME*	Sorted by 1/*ACOV*	Sorted by 1/*AGE*
		Panel A: Upgrades		
Q1 (low)	1.30%	1.04%	1.36%	1.43%
Q2	1.42%	1.45%	1.81%	1.63%
Q3	1.78%	1.99%	2.36%	2.22%
Q4	2.28%	2.69%	2.46%	2.49%
Q5 (high)	3.83%	3.43%	2.85%	2.85%
Q5 - Q1	2.54%***	2.38%***	1.49%***	1.42***
		Panel B: Downgrades		
Q1 (low)	-1.19%	-1.31%	-1.60%	-1.41%
Q2	-1.37%	-1.64%	-1.96%	-1.83%
Q3	-1.95%	-2.37%	-2.45%	-2.42%
Q4	-2.95%	-3.20%	-2.93%	-2.57%
Q5 (high)	-4.06%	-3.00%	-2.61%	-3.34%
Q5 - Q1	-2.87%***	-1.68%***	-1.01%***	-1.93%***

Table 3.5. Effects of ambiguity proxies on event BHARs: the “mean” effect.

This table presents regression results of event *BHARs* on proxies for ambiguity and a number of control variables. The event *BHAR(0,1)* is a two-day buy-and-hold return on a stock less a two-day buy-and-hold return on a DGTW portfolio with comparable size, B/M, and momentum characteristics. Proxies for high ambiguity are constructed as dummy variables indicating ambiguity proxies are higher than their respective 70th percentiles. Ambiguity proxies are: a stock price’s nearness to its 52-week low (*N52WL*), reciprocal of firm size (1/*ME*), reciprocal of analyst coverage (1/*ACOV*), and reciprocal of firm age (1/*AGE*). Control variables include proxies for moderate ambiguity, which are constructed as dummy variables indicating ambiguity proxies are above their 30th and below their 70th percentiles, respectively. Other control variables are book-to-market equity ratio (*BM*), momentum (*MOM*), institutional ownership (*IO*), the natural logarithm of analyst experience (*Ln(AEXP)*), an affiliated analyst dummy (*AFF dummy*), an independent analyst dummy (*IND dummy*), a recommendation deviation from consensus (*DC*), the magnitude of a recommendation change (*MAG*), a dummy variable indicating a recommendation is issued after Regulation Fair Disclosure (*FD dummy*), and a dummy variable indicating a recommendation is issued after the Global Research Analyst Settlement (*GS dummy*). The variables are defined in Appendix D. For ease of presentation, the coefficients of the year dummies are suppressed from the table. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively, with *t*-statistics in parentheses. Standard errors are clustered at the firm level. The sample of recommendation changes is from the I/B/E/S U.S. Detail Recommendation File, from 1994 to 2010.

	Dependent variable: *BHAR(0,1)*					
	[1]		[2]		[3]	
Explanatory variable	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat
Panel A: Upgrades						
Intercept	-0.0000	(-0.03)	-0.0123***	(-8.07)	-0.0132***	(-8.71)
Ambiguity proxies						
N52WL (>70th) dummy	0.0179***	(21.77)			0.0117***	(14.88)
1/ME (>70th) dummy			0.0194***	(18.62)	0.0153***	(15.18)
1/ACOV (>70th) dummy			0.0021**	(2.23)	0.0043***	(4.48)
1/AGE (>70th) dummy			0.0061***	(7.15)	0.0044***	(5.24)
Ambiguity controls						
N52WL (>30th , <70th) dummy	0.0036***	(8.14)			0.0012***	(2.69)
1/ME (>30th , <70th) dummy			0.0077***	(12.80)	0.0060***	(10.19)
1/ACOV(>30th , <70th) dummy			0.0021***	(3.34)	0.0033***	(5.19)
1/AGE (>30th , <70th) dummy			0.0016***	(2.62)	0.0009*	(1.67)
Firm characteristics						
BM	0.0015	(1.57)	0.0014	(1.60)	0.0012	(1.58)
MOM	0.0012**	(2.35)	-0.0004	(-0.85)	0.0007	(1.40)
IO	-0.0071***	(-3.66)	0.0005	(0.26)	0.0012	(0.63)
Analyst characteristics						
Ln(AEXP)	0.0001	(0.37)	0.0004	(1.32)	0.0005	(1.53)
AFF dummy	0.0052***	(5.46)	0.0063***	(6.70)	0.0061***	(6.58)
IND dummy	-0.0102***	(-12.22)	-0.0097***	(-11.85)	-0.0098***	(-12.08)
Recommendation characteristics						
DC	0.0011***	(5.56)	0.0013***	(6.63)	0.0013***	(6.50)
MAG	0.0024***	(4.20)	0.0027***	(4.69)	0.0026***	(4.52)
FD dummy	0.0035	(1.18)	0.0032	(1.06)	0.0025	(0.86)
GS dummy	0.0113***	(3.91)	0.0119***	(4.17)	0.0110***	(3.86)
Year fixed effects	Yes		Yes		Yes	

Clustering by firm	Yes		Yes		Yes	
R^2	0.031		0.038		0.042	
F-test	58.77***		59.54***		61.60***	
N	58,494		58,494		58,494	
Panel B: Downgrades						
Intercept	0.0119***	(5.52)	0.0217***	(9.46)	0.0261***	(11.44)
Ambiguity proxies						
N52WL (>70th) dummy	-0.0262***	(-23.31)			-0.0211***	(-18.67)
1/ME (>70th) dummy			-0.0157***	(-10.35)	-0.0091***	(-5.98)
1/ACOV (>70th) dummy			-0.0043***	(-3.76)	-0.0081***	(-6.92)
1/AGE (>70th) dummy			-0.0107***	(-9.11)	-0.0078***	(-6.74)
Ambiguity controls						
N52WL (>30th , <70th) dummy	-0.0083***	(-13.39)			-0.0067***	(-10.85)
1/ME (>30th , <70th) dummy			-0.0071***	(-9.49)	-0.0043***	(-5.91)
1/ACOV(>30th , <70th) dummy			-0.0021***	(-2.71)	-0.0041***	(-5.26)
1/AGE (>30th , <70th) dummy			-0.0041***	(-4.69)	-0.0027***	(-3.25)
Firm characteristics						
BM	0.0018	(1.36)	0.0017	(1.30)	0.0021	(1.46)
MOM	-0.0025***	(-3.56)	0.0015**	(2.36)	-0.0016**	(-2.34)
IO	-0.0047**	(-2.28)	-0.0135***	(-6.39)	-0.0139***	(-6.68)
Analyst characteristics						
Ln(AEXP)	-0.0006	(-1.46)	-0.0007*	(-1.73)	-0.0009**	(-2.32)
AFF dummy	-0.0105***	(-8.48)	-0.0111***	(-8.94)	-0.0107***	(-8.73)
IND dummy	0.0145***	(9.19)	0.0140***	(8.92)	0.0143***	(9.11)
Recommendation characteristics						
DC	-0.0012***	(-5.75)	-0.0014***	(-6.39)	-0.0014***	(-6.44)
MAG	-0.0051***	(-6.75)	-0.0049***	(-6.57)	-0.0048***	(-6.45)
FD dummy	0.0050*	(1.83)	0.0051*	(1.86)	0.0057**	(2.07)
GS dummy	-0.0103***	(-4.11)	-0.0117***	(-4.65)	-0.0103***	(-4.10)
Year fixed effects	Yes		Yes		Yes	
Clustering by firm	Yes		Yes		Yes	
R^2	0.021		0.022		0.026	
F-test	55.56***		49.04***		53.46***	
N	67,106		67,106		67,106	

Table 3.6. Event ABHARs sorted by ambiguity proxies: the "interaction" effect.

This table shows event *ABHARs* across firm subgroups formed based on proxies for ambiguity. The proxies are: a stock price's nearness to its 52-week low (*N52WL*), reciprocal of firm size (1/*ME*), reciprocal of analyst coverage (1/*ACOV*), and reciprocal of firm age (1/*AGE*). I use a double sort procedure and divide the sample into low-, moderate-, and high-AIF groups based on the 30th and 70th percentiles of *N52WL* and further divide each of these groups into low-, moderate-, and high-AII subgroups based on the 30th and 70th percentiles of either 1/*ME* (Panel A), 1/*ACOV* (Panel B), or 1/*AGE* (Panel C). The event *ABHAR(0,1)* is the average of two-day buy-and-hold returns on stocks less two-day buy-and-hold returns on DGTW portfolios with comparable size, B/M, and momentum characteristics. Other variables are defined in Appendix D. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Difference *t*-tests are based on the Satterthwaite method (of unequal variances). The sample of recommendation changes is from the I/B/E/S U.S. Detail Recommendations File, from 1994 to 2010.

	Upgrades			Downgrades		
	AIF proxied by *N52WL*					
AII	Low (<30th)	Moderate	High (>70th)	Low (<30th)	Moderate	High (>70th)
Panel A: AII proxied by 1/*ME*						
Low (<30th)	0.79%	1.12%	2.06%	-0.86%	-1.39%	-2.94%
Moderate	1.34%	1.85%	3.35%	-1.23%	-2.03%	-4.46%
High (>70th)	1.87%	2.29%	4.75%	-1.61%	-2.58%	-3.69%
High - Low	1.07%***	1.17%***	2.70%***	-0.75%***	-1.19%***	-0.75%***
Panel B: AII proxied by 1/*ACOV*						
Low (<30th)	0.92%	1.17%	2.33%	-0.88%	-1.34%	-2.78%
Moderate	1.37%	1.84%	3.53%	-1.26%	-2.05%	-4.00%
High (>70th)	1.76%	2.29%	4.27%	-1.55%	-2.55%	-4.35%
High - Low	0.83%***	1.13%***	1.94%***	-0.66%***	-1.21%***	-1.57%***
Panel C: AII proxied by 1/*AGE*						
Low (<30th)	1.00%	1.40%	2.91%	-0.94%	-1.52%	-3.13%
Moderate	1.26%	1.83%	3.39%	-1.22%	-1.93%	-3.74%
High (>70th)	1.78%	2.04%	3.84%	-1.54%	-2.60%	-4.51%
High - Low	0.77%***	0.64%***	0.93%***	-0.59%***	-1.08%***	-1.38%***

Table 3.7. Effects of ambiguity proxies on event BHARs: the "interaction" effect.

This table presents the regression results of event *BHARs* on proxies for AII and a number of control variables in three AIF-level subsamples formed based on the 30th and 70th percentiles of *N52WL*. The event *BHAR(0,1)* is a two-day buy-and-hold return on a stock less a two-day buy-and-hold return on a DGTW portfolio with comparable size, B/M, and momentum characteristics. The term *N52WL* is a stock price's nearness to its 52-week low. Proxies for high AII are constructed as dummy variables indicating AII proxies are higher than their respective 70th percentiles. Proxies for AII are: the reciprocal of firm size (1/*ME*), reciprocal of analyst coverage (1/*ACOV*), and reciprocal of firm age (1/*AGE*). Control variables include proxies for moderate AII, which are constructed as dummy variables indicating AII proxies are above their 30th and below their 70th percentiles, respectively. Other control variables are book-to-market equity ratio (*BM*), momentum (*MOM*), institutional ownership (*IO*), the natural logarithm of analyst experience (*Ln(AEXP)*), an affiliated analyst dummy (*AFF dummy*), an independent analyst dummy (*IND dummy*), a recommendation deviation from consensus (*DC*), the magnitude of a recommendation change (*MAG*), a dummy variable indicating a recommendation is issued after Regulation Fair Disclosure (*FD dummy*), and a dummy variable indicating a recommendation is issued after the Global Research Analyst Settlement (*GS dummy*). The variables are defined in Appendix D. For ease of presentation, the coefficients of the year dummies are suppressed from the table. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively, with *t*-statistics in parentheses. Standard errors are clustered at the firm level. The sample of recommendation changes is from the I/B/E/S U.S. Detail Recommendation File, from 1994 to 2010.

	Dependent variable: *BHAR(0,1)*					
	[1]		[2]		[3]	
	AIF proxied by *N52WL*					
	Low (<30th)		Moderate		High (>70th)	
Explanatory variable	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat
Panel A: Upgrades						
Intercept	-0.0082***	(-4.93)	-0.0096***	(-6.20)	-0.0153***	(-3.29)
Ambiguity proxies						
1/ME (>70th) dummy	0.0109***	(7.46)	0.0121***	(10.41)	0.0220***	(9.38)
1/ACOV (>70th) dummy	0.0019*	(1.78)	0.0039***	(3.70)	0.0085***	(3.51)
1/AGE (>70th) dummy	0.0037***	(3.57)	0.0018**	(1.99)	0.0077***	(3.57)
Ambiguity controls						
1/ME (>30th , <70th) dummy	0.0058***	(8.32)	0.0070***	(9.34)	0.0087***	(5.02)
1/ACOV(>30th , <70th) dummy	0.0011*	(1.69)	0.0026***	(3.63)	0.0063***	(3.55)
1/AGE (>30th , <70th) dummy	0.0000	(0.14)	0.0011*	(1.76)	0.0022	(1.35)
Firm characteristics						
BM	-0.0006	(-1.52)	-0.0006***	(-2.86)	0.0021**	(2.07)
MOM	0.0022**	(2.33)	0.0033***	(4.71)	-0.0006	(-0.72)
IO	0.0062***	(3.34)	0.0025	(1.46)	-0.0071	(-1.50)
Analyst characteristics						
Ln(AEXP)	0.0006*	(1.76)	0.0009***	(2.72)	0.0000	(0.03)
AFF dummy	0.0035***	(3.40)	0.0053***	(4.47)	0.0107***	(4.41)
IND dummy	-0.0065***	(-7.28)	-0.0082***	(-8.75)	-0.0148***	(-6.90)
Recommendation characteristics						
DC	0.0005***	(3.33)	0.0005***	(3.33)	0.0030***	(5.04)
MAG	0.0009	(1.62)	0.0018***	(3.15)	0.0051***	(3.11)
FD dummy	0.0073*	(1.92)	0.0059*	(1.81)	0.0001	(0.02)
GS dummy	0.0026	(0.72)	0.0057*	(1.74)	0.0180***	(3.39)
Year fixed effects	Yes		Yes		Yes	

Clustering by firm	Yes		Yes		Yes	
R^2	0.051		0.041		0.029	
F-test	26.59***		32.61***		15.63***	
N	17,503		23,469		17,522	
Panel B: Downgrades						
Intercept	0.0078***	(4.12)	0.0176***	(7.36)	0.0388***	(6.80)
Ambiguity proxies						
1/ME (>70th) dummy	-0.0043***	(-2.71)	-0.0115***	(-6.68)	-0.0116***	(-3.17)
1/ACOV (>70th) dummy	-0.0047***	(-4.07)	-0.0069***	(-4.65)	-0.0151***	(-5.12)
1/AGE (>70th) dummy	-0.0024**	(-2.18)	-0.0078***	(-5.74)	-0.0147***	(-4.34)
Ambiguity controls						
1/ME (>30th , <70th) dummy	-0.0026***	(-3.21)	-0.0054***	(-5.49)	-0.0084***	(-3.77)
1/ACOV(>30th , <70th) dummy	-0.0021***	(-2.81)	-0.0027***	(-2.88)	-0.0082***	(-3.82)
1/AGE (>30th , <70th) dummy	-0.0011*	(-1.65)	-0.0021**	(-2.39)	-0.0089***	(-2.74)
Firm characteristics						
BM	0.0009*	(1.80)	0.0013*	(1.81)	0.0026	(1.37)
MOM	-0.004***	(-4.63)	-0.0011	(-1.32)	-0.0011	(-0.81)
IO	-0.0091***	(-4.96)	-0.0165***	(-6.54)	-0.0147***	(-2.91)
Analyst characteristics						
Ln(AEXP)	-0.0010**	(-2.55)	-0.0014***	(-2.62)	-0.0008	(-0.77)
AFF dummy	-0.0011	(-1.03)	-0.0092***	(-5.44)	-0.0217***	(-7.11)
IND dummy	0.0057***	(5.70)	0.0113***	(8.68)	0.0281***	(5.44)
Recommendation characteristics						
DC	-0.0009***	(-5.11)	-0.0009***	(-3.95)	-0.0026***	(-4.30)
MAG	-0.0014**	(-2.29)	-0.0032***	(-3.35)	-0.0097***	(-4.99)
FD dummy	0.0037	(1.21)	0.0108***	(3.35)	0.0027	(0.47)
GS dummy	-0.0057*	(-1.87)	-0.0102***	(-3.36)	-0.0124**	(-2.48)
Year fixed effects	Yes		Yes		Yes	
Clustering by firm	Yes		Yes		Yes	
R^2	0.024		0.025		0.019	
F-test	14.16***		19.06***		18.09***	
N	20,126		26,920		20,060	

Table 3.8. Using Loh and Stulz' (2011) definition of influence of a recommendation change.
This table shows the proportions of influential recommendation changes across AIF/AII firm subgroups. These proportions are calculated as follows. First, following Loh and Stulz (2011), I classify a recommendation change as influential if the associated event *BHAR(0,1)* is both in the correct direction and statistically significant. An event *BHAR(0,1)* is classified as statistically significant if it satisfies the condition $|BHAR(0,1)| > 1.96 \times \sqrt{2} \times \sigma_{\varepsilon}$, where σ_{ε} is the residual standard deviation of stock daily returns against the three Fama–French factors in the three-month period prior to the end of month m. Second, I use a double sort procedure and divide the sample into low-, moderate-, and high-AIF firm groups based on the 30th and 70th percentiles of *N52WL* and further divide each of these groups into low-, moderate-, and high-AII firm subgroups based on the 30th and 70th percentiles of either the reciprocal of *ME*, reciprocal of *ACOV*, or reciprocal of *AGE*. Third, I compute for each AIF/AII firm subgroup the proportion of influential recommendation changes as the number of influential recommendation changes scaled by the total number of recommendation changes within that subgroup. The event *BHAR(0,1)* is a two-day buy-and-hold return on a stock less a two-day buy-and-hold return on a DGTW portfolio with comparable size, B/M, and momentum characteristics. The sample of recommendation changes is from the I/B/E/S U.S. Detail Recommendations File, from 1994 to 2010.

	Upgrades			Downgrades		
	AIF proxied by *N52WL*					
AII	Low (<30th)	Moderate	High (>70th)	Low (<30th)	Moderate	High (>70th)
	Panel A: AII proxied by 1/*ME*					
Low (<30th)	10.59%	10.54%	10.36%	10.29%	11.43%	11.64%
Moderate	14.43%	14.98%	15.57%	12.60%	15.33%	17.79%
High (>70th)	14.84%	15.58%	16.96%	12.68%	16.36%	14.91%
	Panel B: AII proxied by 1/*ACOV*					
Low (<30th)	9.22%	9.21%	10.23%	8.97%	10.13%	10.63%
Moderate	14.12%	14.79%	15.27%	12.77%	14.96%	16.15%
High (>70th)	17.05%	17.23%	17.51%	13.75%	17.83%	17.53%
	Panel C: AII proxied by 1/*AGE*					
Low (<30th)	12.66%	13.12%	13.91%	12.16%	12.93%	13.85%
Moderate	13.07%	14.18%	14.98%	11.55%	14.89%	15.53%
High (>70th)	14.61%	14.06%	14.19%	12.22%	15.46%	15.72%

Table 3.9. Divergence in investors' opinions and the influence of recommendation changes.
This table compares event *ABHARs* across firm subgroups based on proxies for AII and divergence in investors' opinions. Proxies for AII are: the reciprocal of firm size (1/*ME*), reciprocal of analyst coverage (1/*ACOV*), and reciprocal of firm age (1/*AGE*). The proxy for divergence in investors' opinions is earnings' forecast dispersion (*FDISP*). I use a double sort procedure and divide the sample into low-, moderate-, and high-AII firm groups based on the 30th and 70th percentiles of either 1/*ME* (Panel A), 1/*ACOV* (Panel B), or 1/*AGE* (Panel C) and further divide each of these groups into low-, moderate-, and high-divergence firm subgroups based on the 30th and 70th percentiles of *FDISP*. The event *ABHAR(0,1)* is the average of two-day buy-and-hold returns on stocks less two-day buy-and-hold returns on DGTW portfolios with comparable size, B/M, and momentum characteristics. Other variables are defined in Appendix D. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Difference *t*-tests are based on the Satterthwaite method (of unequal variances). The sample of recommendation changes is from the I/B/E/S U.S. Detail Recommendations File, from 1994 to 2010.

	Upgrades			Downgrades		
	AII					
Opinions' divergence proxied by *FDISP*	Low (<30th)	Moderate	High (>70th)	Low (<30th)	Moderate	High (>70th)
Panel A: AII proxied by 1/*ME*						
Low (<30th)	1.05%	1.81%	2.33%	-1.38%	-2.50%	-3.41%
Moderate	1.05%	1.67%	3.08%	-1.30%	-2.09%	-3.24%
High (>70th)	1.25%	2.07%	4.05%	-1.43%	-2.42%	-4.07%
High - Low	0.20%***	0.26%***	1.71%***	-0.04%	0.08%	-0.66%***
Panel B: AII proxied by 1/*ACOV*						
Low (<30th)	1.28%	1.62%	1.98%	-1.59%	-2.23%	-2.75%
Moderate	1.21%	1.85%	2.45%	-1.42%	-2.20%	-2.91%
High (>70th)	1.71%	2.74%	3.58%	-2.02%	-2.75%	-3.81%
High - Low	0.42%***	1.11%***	1.61%***	-0.42%***	-0.51%***	-1.06%***
Panel C: AII proxied by 1/*AGE*						
Low (<30th)	1.23%	1.64%	1.99%	-1.45%	-2.18%	-3.03%
Moderate	1.26%	1.73%	2.50%	-1.39%	-2.15%	-3.04%
High (>70th)	1.85%	2.73%	3.40%	-1.79%	-2.98%	-3.73%
High - Low	0.62%***	1.09%***	1.41%***	-0.34%***	-0.79%***	-0.69%***

Table 3.10. Robustness of the measure N52WL as a proxy for AIF.

This table compares event *ABHARs* across firm subgroups. I use a double sort procedure and divide the sample into low-, moderate-, and high-deviation from consensus (Panel A) or momentum (Panel B) stock groups based on the 30th and 70th percentiles of *DC* or *MOM*, respectively, and further divide each of these groups into low-, moderate-, and high-AIF subgroups based on the 30th and 70th percentiles of *N52WL*. The event *ABHAR(0,1)* is the average of two-day buy-and-hold returns on stocks less two-day buy-and-hold returns on DGTW portfolios with comparable size, B/M, and momentum characteristics. Other variables are defined in Appendix D. The superscripts *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. Difference *t*-tests are based on the Satterthwaite method (of unequal variances). The sample of recommendation changes is from the I/B/E/S U.S. Detail Recommendations File, from 1994 to 2010.

	Upgrades			Downgrades		
	DC or *MOM*					
AIF proxied by *N52WL*	Low (<30th)	Moderate	High (>70th)	Low (<30th)	Moderate	High (>70th)
Panel A: Sorted by *DC* and *N52WL*						
Low (<30th)	1.21%	1.40%	1.28%	-0.98%	-1.16%	-1.55%
Moderate	1.69%	1.80%	1.75%	-1.84%	-1.84%	-2.42%
High (>70th)	2.82%	3.50%	3.70%	-3.05%	-3.64%	-4.60%
High - Low	1.61%***	2.11%***	2.42%***	-2.07%***	-2.48%***	-3.06%***
Panel B: Sorted by *MOM* and *N52WL*						
Low (<30th)	1.77%	1.16%	1.45%	-2.32%	-1.00%	-1.37%
Moderate	2.66%	1.41%	1.66%	-3.54%	-1.54%	-1.47%
High (>70th)	4.65%	2.27%	2.83%	-4.00%	-2.91%	-3.28%
High - Low	2.87%***	1.11%***	1.38%***	-1.68%***	-1.91%***	-1.91%***

CHAPTER FOUR: AFFILIATED ASSET MANAGERS AND INFORMED SELLING PRIOR TO ANALYSTS' DOWNGRADES: WHAT ARE THE IMPLICATIONS OF THE GLOBAL SETTLEMENT?

4.1. Introduction

The interdependence among the divisions of a full-service brokerage firm typically raises ethical issues with respect to the interests of investors. Anecdotal evidence suggests that sell-side analysts, those employed by a brokerage firm, may tip off their affiliated asset managers regarding the content of the soon to be released research reports. For example, on March 14, 2007 the Securities and Exchange Commission (SEC) fined Banc of America Securities LLC US$26 million over the claims that trading employees had access to research reports before they were published, and in at least two instances the bank traded in advance of their release.[42]

Informed trading prior to the announcement of recommendations by sell-side analysts is not new to the literature. Irvine, Lipson, and Puckett (2007) document abnormally high institutional buying beginning five days before initial buy and strong buy recommendations are released. More recently, Christophe, Ferri, and Hsieh (2010) present evidence of abnormally high short-selling prior to the release of analysts' downgrades. Both studies support the hypothesis of tipping by sell-side analysts to select institutional clients. It is important to note that the evidence on tipping presented in these two studies is specific to the 1998-2001 period. Following this period, however, the regulatory environment had seen substantial mutation with the adoption of the Global Research Analyst Settlement and related

[42] See the SEC's administrative proceeding file No. 3-12591 and Wall Street Journal (2007).

regulations in 2002-2003.[43] The purpose of the new regulations is to make analysts' research more meaningful and enhance its confidentiality. Although prior evidence suggests that these regulatory changes have altered several aspects of sell-side research, their potential effects on the practice of tipping by sell-side analysts have received little attention in the literature.[44]

This chapter builds on prior research and investigates evidence of informed selling by affiliated asset managers prior to downgrade releases for the period from 1996 to 2010.[45] Specifically, the two main objectives are (i) to study the extent of informed selling in response to prominent regulatory provisions that would limit the information flow from sell-side analysts to their affiliated asset managers, and (ii) to investigate the relation between informed selling and the downgrade characteristics that would imply early knowledge of forthcoming downgrades.

In this chapter, an affiliated asset manager is defined as any manager who discloses 13f security holdings under the name of a brokerage firm that employs a sell-side analyst. I use the CDA/Spectrum Institutional (13f) holdings data set, which collects institutional holdings from the SEC's Form 13f and records these holdings on a manager-quarter basis. To overcome the well-documented misclassification of managers in the Institutional (13f)

[43] Another important regulation, Regulation Fair Disclosure, was adopted in 2000. There is consensus in the literature that Regulation Fair Disclosure has had a negative effect on the precision and informativeness of analyst research (Bailey, Li, Mao, and Zhong, 2003; Agrawal, Chadha, and Chen, 2006; Gintschel and Markov, 2004).

[44] The documented effect of the Global Settlement and related regulations is that optimistic recommendations have become less frequent and more informative, whereas pessimistic recommendations have become more frequent and less informative (Kadan, Madureira, Wang, and Zach, 2009; Clarke, Khorana, Patel, and Rau, 2011).

[45] Throughout the chapter, I use the terminology "informed selling" to refer to the pre-downgrade selling activity of affiliated asset managers that is associated with significantly negative pre-announcement abnormal returns (and abnormally high share turnovers).

holdings data set, I disregard the specific type of managers, and for each quarter I aggregate the holdings of affiliated asset managers at the brokerage firm level.[46]

Who tips off whom? Brokerage firms and their affiliated asset managers have economic incentives to support each other. As documented by Gaspar, Massa, and Matos (2006), the mutual fund industry is concentrated into a number of mutual fund families, each of which is typically affiliated with a brokerage firm. The economic incentives faced by a brokerage firm and its affiliated mutual fund family may create a strong interdependence that would justify some brokerage analysts passing private information to their affiliated mutual fund managers. The cost of research is typically recovered, at least in part, by trading commissions paid by the affiliated fund family. Moreover, the fund family often pays sales commissions to its affiliated brokerage firm for promoting its shares. In exchange for these payments, the firm may provide early access to research to its affiliated mutual fund managers, who may exploit their early knowledge of research ahead of its public release.[47] It is clear that such interdependence is likely to persist, and the practice of tipping is likely to thrive, absent appropriate regulatory constraints.

In 2002-2003, however, a set of rules—NYSE Rule 472 ("Communications with the public"); NASD Rule 2711 ("Research analysts and research reports"); and the Global Research Analyst Settlement (collectively known as the GS)—were enacted in an effort to address conflicts of interests faced by sell-side analysts, whereby analysts come under pressure from their affiliated investment bankers to assign favorable ratings to current and

[46] See Moussawi and Palacios (2009) for evidence on the misclassification problem of managers in the institutional (13f) holdings data set.

[47] Similar arguments can be discussed to highlight the interdependence between brokerage firms and other affiliated asset managers, such as hedge fund managers.

prospective investment banking clients. Moreover, the new rules include provisions that require strict confidentiality of research reports until their public release. For example, NYSE Rule 472 (b) states that *"Research reports may not be subject to review or approval prior to publication by Investment Banking personnel or any other employee of the member or member organisation who is not directly responsible for investment research ("non-research personnel") other than Legal or Compliance personnel"*.[48] It follows that those asset managers who are affiliated with a brokerage firm may no longer be allowed to receive tips regarding the content of the soon to be released research reports from their brokerage analysts. The two main hypotheses of this chapter follow.

Hypothesis 1. (informed selling and the GS) If some affiliated asset managers are informed sellers, their pre-downgrade selling activity would be reflected in significantly negative pre-announcement abnormal returns and abnormally high share turnovers. Moreover, if informed selling is limited to the pre-GS period, one would imply that some of the informed selling is likely to be induced by tips received from brokerage analysts.

Hypothesis 2. (early knowledge of forthcoming downgrades) If pre-downgrade informed selling is related to the downgrade characteristics that would imply early knowledge of forthcoming downgrades, one would conclude that tipping is likely to be responsible for some of the selling.

Why does this chapter focus on downgrades only? Analysts are reluctant to assign unfavorable ratings to companies for many reasons (Mola and Guidolin, 2009). First, analysts

[48] See also NASD Rule 2711 (b).

have a preference to curry favor with companies' executives. Second, analysts tend to assign favorable ratings to companies in response to pressure stemming from their affiliated investment bankers. Third, analysts are biased toward the issuance of favorable recommendations to induce those institutional clients subject to short-selling constraints to trade on the research and generate trading commissions for their affiliated brokerage divisions. It follows that those affiliated asset managers who benefit from early access to overoptimistic research would not perceive a significant information advantage from the research and may be unwilling to trade on their early knowledge.[49] On the contrary, an analyst's downgrade of a company represents an unequivocal signal that the analyst's prior expectations about the company have not been satisfied and thus typically involves a reliable information advantage. Those managers with early access to negative research would be inclined to take immediate actions to cut their portfolio holdings of the soon to be downgraded stocks.

It would be ideal to analyze the short-selling activity of affiliated asset managers prior to downgrade releases. Abnormal short-selling of a soon to be downgraded stock would provide direct evidence of tipping. Because short-selling data is available only on an aggregate level, my analysis is limited to the sell trades that are reported by affiliated asset managers. Hence, it is clear that my research design runs against finding evidence of tipping. In other words, if evidence of tipping exists based on an analysis of sell trades before downgrades, one would expect this evidence to be stronger based on an analysis of short-selling before downgrades.

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[49] Consistent with this view, I find no clear empirical evidence of informed buying by affiliated asset managers prior to analysts' upgrades (unreported results).

I collect a sample of analysts' recommendations from Thompson Reuters Institutional Brokers' Estimates (I/B/E/S) U.S. Detail Recommendations File for the period from January 1996 to December 2010. Downgrades are constructed as in Ljungqvist, Malloy, and Marston (2009). Only those downgrades from the most active full-service brokerage firms are retained in the sample.[50] Since my analysis focuses on the holdings of affiliated asset managers that are reported during the month prior to a downgrade announcement, I ensure that any downgrade that is preceded by another downgrade or an earnings announcement in the prior month is excluded from the sample. Although this procedure significantly reduces the sample size, it ensures that the sample is insulated from analysts' herding and earnings announcement effects. The resulting sample of downgrades is then matched with sell trades from affiliated asset managers as reported during the month prior to downgrade announcements. The final sample consists of 2,306 downgrades issued on 1,414 unique companies by 1,259 unique analysts, who are employed by 43 unique full-service brokerage firms.

My findings support hypothesis 1. In the pre-GS period, I document significantly negative abnormal returns and abnormally high share turnovers in the month prior to downgrade announcements (Table 4.2). Compared with post-GS downgrades, pre-GS downgrades are associated with significantly more negative pre-announcement abnormal returns, after controlling for momentum effects (Table 4.3). It appears that the GS effect is stronger among downgrades that are from the sanctioned firms or affiliated analysts (Tables 4.4 and 4.5).[51] Overall, these findings are consistent with the view that some affiliated asset

[50] Specifically, the sample retains only those downgrades from full-service brokerage firms that contribute at least 0.5% (or 2,326 recommendations) of the total recommendations in I/B/E/S during the sample period.

[51] The sanctioned firms are those firms sanctioned by the GS. Affiliated analysts are those analysts with investment banking ties with the covered companies. Precise definitions are reported in Appendix E.

managers may receive tips from their brokerage analysts and engage in selling of the soon to be downgraded stocks.

My findings also support hypothesis 2. I show that informed selling is relatively more prevalent prior to strong downgrades, downgrades of high magnitude, downgrades that are issued before Regulation Fair Disclosure came into effect, or downgrades that are issued on high institutional ownership stocks (Table 4.6). Because informed selling is related to the downgrade characteristics that would imply early knowledge of forthcoming downgrades, it is likely that some of the selling is induced by tips received from brokerage analysts.

My contribution to the literature is twofold. First, I show that some affiliated asset managers may receive tips regarding the content of forthcoming downgrade reports, whereas prior research remains salient on the identity of those institutional investors who may benefit from tipping. Second, compared with prior research, my analysis relies on an up-to-date sample and emphasizes the role of regulatory changes in mitigating the practice of tipping.

The remainder of the chapter is as follows. Section 4.2 describes the sample formation process and presents the features of the sample. Section 4.3 investigates evidence of informed selling. Section 4.4 analyzes the relation between informed selling and the characteristics of forthcoming downgrades. Section 4.5 concludes.

4.2. Sampling procedure and features of the sample

In this section, I describe the formation of the sample of downgrades, the construction of sell trades from the quarterly holdings of affiliated asset managers, and the features of the matched sample.

4.2.1. I/B/E/S Sampling procedure

The sample of analysts' recommendations is from I/B/E/S U.S. Detail Recommendations File. I select a large sample of recommendations from 1996 to 2010. I/B/E/S receives different wordings from different brokerage firms, but then translates them into numerical scores on the following scale: strong buy = 1, buy = 2, hold = 3, sell = 4, and strong sell = 5.

Valid recommendation data consist of records for which activation dates, ACTDATS, and review dates, REVDATS, are subsequent to the announcement dates, ANNDATS; analyst identification number, AMASKCD, and CUSIP are not missing; recommendation levels are between 1 and 5; and shares are common equity.

To construct downgrades, I follow Ljungqvist, Malloy, and Marston (2009) and code ratings as follows: the first time a brokerage firm issues a recommendation on a given firm in I/B/E/S is an initiation. Following recommendations are categorized as either outstanding or non-outstanding recommendations. Outstanding recommendations are those recommendations that are not stopped by the brokerage firm and recommendations for which no more than 12 months have elapsed since the previous recommendation has been confirmed.[52] Outstanding recommendations are further broken down into upgrades, downgrades, and re-iterations. Non-outstanding recommendations are coded as re-initiations. Ultimately, only downgrades are retained in the sample.

To keep the sample size manageable, I limit my analysis to those downgrades issued by the most active full-service brokerage firms in I/B/E/S. First, I identify those firms that issue recommendations representing at least 0.5% (or 2,326 recommendations) of the total

[52] I use the review date of the previous recommendation for the confirmation status and I/B/E/S stop file to check for broker scale changes and suspensions / terminations of broker coverage.

number of recommendations in I/B/E/S (or 458,426 recommendations). For example, Goldman Sachs is the most active contributor with 16,555 recommendations (or 3.6%), and Adams Harkness Inc. is the least active contributor with 2,326 recommendations (or 0.5%). This procedure identifies 51 I/B/E/S contributors, 43 of which are full-service brokerage firms.[53]

I further refine the sample of selected downgrades to ensure my findings are not contaminated by the effects of concurrent observable events. I ensure that any downgrade that is preceded by another downgrade or an earnings announcement in the prior month is excluded from the sample. The resulting sample of downgrades is then matched with CRSP and COMPUSTAT data, a procedure that results in 29,164 downgrades.

4.2.2. 13f trades sampling procedure

The CDA/Spectrum Institutional (13f) holdings data set collects institutional holdings of asset managers from the SEC's Form 13f and records these holdings on a manager-quarter basis. Asset managers holding more than $100 million in equity are required to file a quarterly report of all equity holdings greater than 10,000 shares or $200,000 in market value. Using the CDA/Spectrum holdings quarterly data, I infer quarterly trades made by affiliated asset managers. First, for each quarter I aggregate the shares held by the asset managers who are affiliated with the same brokerage firm. Second, I construct quarterly trades and classify them

[53] An I/B/E/S contributor is classified as a full-service brokerage firm if it provides investment banking and asset management services anytime during the sample period. I use SDC to check for whether an I/B/E/S contributor has underwritten equity or debt securities anytime during the sample period and CDA/Spectrum Institutional (13f) to check whether an I/B/E/S contributor (through its affiliated asset managers) has invested in stocks anytime during the sample period. For example, Buckingham Research, which contributes 3,834 recommendations to I/B/E/S, is a pure equity research firm that does not provide underwriting services and thus is excluded from the sample.

into buy, hold, or sell. For example, in a given quarter, a buy trade is the case in which a brokerage firm reports a number of shares held in a stock at the end of a quarter that is higher than that at the end of the previous quarter. Only sell trades are retained in the sample. To ensure these trades are not driven by either liquidity needs or reporting inaccuracies, in a given quarter I exclude any sell trade on a stock that represents less than 5% of the manager's holdings in that stock in the previous quarter. In addition, there are cases in which holdings are reported discontinuously. Holdings that are reported with a one-quarter gap may be due to either a genuine share liquidating trade or an omission on the part of Thomson Reuters to record the holdings. Because of this uncertainty, I treat one-quarter gaps in quarterly holdings as missing.

Finally, sample downgrades are matched with sell trades from affiliated asset managers, provided that the managers' holdings are reported during the month prior to downgrade announcements. The final sample consists of 2,306 sell-preceded downgrades.

4.2.3. Features of the sample

In this chapter, firms that are sanctioned by the Global Research Analyst Settlement are referred to as the "sanctioned firms".[54] The Settlement required these brokerage firms to pay a total fine of $1.435 billion in an effort to promote independent research. Bear Stearns and Merrill Lynch, Pierce, Fenner & Smith are among the 12 sanctioned firms, but they do not report recommendations to I/B/E/S. Thus, my sample includes downgrades from only ten sanctioned firms.

[54] The original ten sanctioned firms according to the Global Research Analyst Settlement are: Bear Stearns; Credit Suisse First Boston; Goldman Sachs; Lehman Brothers; J. P. Morgan; Merrill Lynch, Pierce, Fenner & Smith; Morgan Stanley; Citigroup Global Markets; UBS Warburg; and U.S. Bancorp Piper Jaffray. In August 2004, Deutsche Bank and Thomas Weisel joined the settlement, bringing the total number of sanctioned firms to twelve.

I classify analysts into two groups: affiliated and unaffiliated analysts. An affiliated analyst is defined as an analyst who issues a downgrade at any point during the two-year period after the file date on a debt or equity issue in which the analyst's employer acts as the lead or co-lead underwriter.[55] In all other cases, analysts are categorized as unaffiliated analysts.[56]

Table 4.1 shows the sample distribution of downgrades, analysts, brokerage firms, and covered companies. During 1996-2010, the sampling procedure identifies 2,306 sell-preceded downgrades issued on 1,414 unique companies by 1,259 unique analysts, who are employed by 43 unique full-service brokerage firms. About 40% (913 downgrades) of sample downgrades are issued in the pre-GS period. The ten sanctioned brokerage firms are the most active contributors to the sample with about 59% (1,354 downgrades) of sample downgrades. Downgrades issued by affiliated analysts represent less than 15% (341 downgrades) of the total number of sample downgrades.

4.3. Analyzing affiliated asset managers' selling before analysts' downgrades

My first hypothesis states that if some affiliated asset managers are informed sellers, their pre-downgrade selling activity would be reflected in significantly negative pre-announcement abnormal returns and abnormally high share turnovers. Moreover, if informed selling is specific to the pre-GS period only, and given that the GS includes provisions that would limit the information flow from analysts to their affiliated asset managers, one would imply that some of the selling is likely to be induced by tips received from brokerage analysts.

[55] I use the "affiliation" terminology for asset managers and analysts to mean different concepts. Affiliated asset managers are those managers reporting 13f security holdings under the name of a brokerage firm. Affiliated analysts are those analysts covering investment banking clients.

[56] This identification uses data from the Securities Data Company (SDC).

In what follows, I employ several tests of this hypothesis. First, I present univariate evidence based on pre-announcement Average Buy-and-Hold Abnormal Returns (ABHARs) and Average Abnormal Share Turnovers (AASTs). Second, I conduct multivariate analyses measuring the effects of the regulatory changes on pre-announcement Buy-and-Hold Abnormal Returns (BHARs). Third, I investigate the extent of informed selling within sanctioned and non-sanctioned firms. Fourth, I study whether informed selling is more prevalent among downgrades from affiliated than unaffiliated analysts.

4.3.1. Hypothesis 1: univariate evidence

Table 4.2 reports percentage ABHARs and AASTs over several time periods measured relative to the downgrade announcement day, day 0. ABHAR is defined as the sample average of the buy-and-hold return on a stock less the buy-and-hold return on a size-B/M matched portfolio. A share turnover is the daily share volume scaled by concurrent total outstanding shares. An abnormal share turnover is measured as the daily average percentage change in share turnover relative to a normal level of share turnover, which I estimate as the median daily share turnover during the year in which the downgrade is issued.[57] AAST is defined as the sample average of abnormal share turnovers.

The results in Table 4.2 are stratified by whether a downgrade is issued in the pre- or post-GS period. Panel A shows that pre-GS downgrades are associated with significantly more negative pre-announcement ABHARs than post-GS downgrades. For example, the pre-announcement ABHAR from day -20 to day -1 of pre-GS downgrades is -2.718%, and that of post-GS downgrades is 1.551%. Whereas the former is negative and significant at the 1% level, the latter is positive and significant at the 1% level. The difference between these two

[57] This definition parallels the definition of normal daily short-selling in Christophe, Ferri, and Hsieh (2010).

numbers is -4.269%, which is significant at the 1% level. This difference remains negative and highly statistically significant over the sub-periods of the pre-event window. However, the largest difference in ABHARs occurs over the sub-period from day -5 to day -1 (-1.608%). Panel B shows that pre-announcement AASTs are much higher in the pre- than post-GS period. The difference in pre-announcement AASTs between the pre-GS and post-GS period is positive, economically large, and statistically significant at the 1% level for all pre-event sub-periods. For example, from day -5 to day -1, this difference is 52.4%.

In sum, pre-announcement abnormal returns are significantly negative, and pre-announcement share turnovers are abnormally high in the pre-GS period. After the GS came into effect, pre-announcement abnormal returns are no longer significantly negative, and pre-announcement abnormal share turnovers are significantly positive, but they are much lower than those of the pre-GS period. Collectively, the results provide evidence consistent with affiliated asset managers engaging in informed selling prior to downgrades from their brokerage analysts. Because this evidence is specific to the pre-GS period only, it is likely that some of the selling is induced by tips received from brokerage analysts.

4.3.2. Hypothesis 1: multivariate evidence

In this section, I investigate the effects of the regulatory changes on pre-announcement abnormal returns in a multivariate setting. To examine these effects while controlling for other relevant factors that could influence pre-announcement abnormal returns, I employ the specification

$$BHAR(-20,-1)_i = \beta_0 + \beta_1 \times PreGS\ dummy_i + \beta_2 \times \ln(Size)_i + \beta_3 \times \ln\left(1 + \frac{B}{M}\right)_i + \beta_4 \times BHAR(-86,-21)_i + \varepsilon_i. \tag{4.1}$$

The dependent variable is the BHAR from day -20 to day -1. The variable of interest is $PreGS\ dummy$, which is a dummy variable indicating a downgrade is issued prior to the GS came into force. If the GS has altered informed selling from affiliated asset managers prior to downgrades from their brokerage analysts, one would expect pre-announcement abnormal returns to be more negative before the GS came into effect, and thus β_1 to be significantly negative. Equation (4.1) also includes three control variables. $\ln(Size)$, the natural logarithm of equity measured in \$ million, controls for size effects. $\ln\left(1 + \frac{B}{M}\right)$, the natural logarithm of one plus Book-to-Market, controls for Book-to-Market effects. Finally, $BHAR(-86,-21)$, the BHAR from day -86 to day -21, is used as a factor controlling for momentum effects. This factor is added to the regression to address the concern that analysts may be more inclined to issue downgrades on stocks with stronger negative momentum.

Table 4.3 presents results from the estimation of Equation (4.1). To parsimoniously correct for biases that might arise from heteroskedasticity and serial correlation of the residuals, I employ the Generalized Method of Moments (GMM). It is well-known that GMM estimators are consistent, efficient, and asymptotically normal (Wooldridge, 2002). Specification [1] in Table 4.3 shows that the coefficient on the $PreGS\ dummy$ (-3.81%) is significantly negative at the 1% level, implying that pre-announcement abnormal returns associated with pre-GS downgrades are much more negative than those associated with post-GS downgrades. This is consistent with the view that the GS has altered informed selling before downgrades. The coefficients on the size and B/M factors imply that pre-announcement abnormal returns are more negative among small-cap or value stocks. Specification [2] augments specification [1] by a momentum factor, $BHAR(-86, -21)$. The coefficient on $PreGS\,dummy$ remains economically large and statistically significant, whereas the coefficient on the momentum factor takes the correct sign, but is statistically insignificant.

In sum, the results in Table 4.3 suggest that pre-announcement abnormal returns are substantially less negative after the GS came into effect, and this finding is independent of size, Book-to-Market, and momentum effects. It follows that sell trades from affiliated asset managers in the pre-GS period are likely to be informed trades, and those in the post-GS period are likely to be uninformed trades. Combining this interpretation with the confidentiality requirement of analysts' research that is introduced by the GS, one would imply that in the pre-GS period some affiliated asset managers may have received tips from their brokerage analysts and engaged in informed selling of the soon to be downgraded stocks.

4.3.3. Hypothesis 1: Sanctioned vs. non-sanctioned firms

In December 2002, the Global Research Analyst Settlement was formally announced and required ten firms to pay a total fine of $1.435 billion. In August 2004, two additional firms joined the Settlement, bringing the number of sanctioned firms to 12. The settlement targeted those firms that had engaged in inappropriate practices, whereby research analysts come under influence of their investment bankers to issue favorable ratings. It seems natural to investigate whether the sanctioned firms had also engaged more than others in tipping practices. First, I test whether the GS had a relatively stronger effect on pre-announcement abnormal returns of downgrades from the sanctioned firms. Second, I investigate whether pre-announcement abnormal returns are more negative for downgrades that are from the sanctioned than non-sanctioned firms.

Table 4.4 reports estimation results of Equation (4.1) for the subsamples of sanctioned (specifications [1] and [2]) and non-sanctioned (specifications [3] and [4]) firms. In specification [1], the coefficient on $PreGS\ dummy$ is significantly negative (-4.85%), implying a strong association between pre-GS downgrades from the sanctioned firms and negative pre-announcement abnormal returns. Specification [2] uses momentum as an additional control factor. The coefficient on momentum is insignificant, and the effect of the GS on pre-announcement abnormal returns remains virtually unchanged. The results in specifications [3] show that the relation between pre-GS downgrades from the non-sanctioned firms and pre-announcement abnormal returns is also significant. The coefficient on $PreGS\ dummy$ is significantly negative (-2.7%). Specification [4] considers momentum as an additional control factor and shows that the effect of the GS on pre-announcement abnormal returns remains strong. However, comparing the coefficients on $PreGS\ dummy$ in specifications [2] and [4] suggests that the GS has had a stronger effect on pre-announcement abnormal returns of downgrades from the sanctioned firms than those from the non-sanctioned firms.

To complete the analysis, I focus on the pre-GS period and investigate whether pre-announcement abnormal returns are more negative among those downgrades that are from the sanctioned than non-sanctioned firms. I consider the specification

$$BHAR(-20,-1)_i = \beta_0 + \beta_1 \times Sanctioned\ firm\ dummy_i + \beta_2 \times \ln(Size)_i + \beta_3 \times \ln\left(1 + \frac{B}{M}\right)_i + \beta_4 \times\ BHAR(-86,-21)_i + \varepsilon_i. \quad (4.2)$$

Equation (4.2) is similar to Equation (4.1), with the exception of the new variable of interest, $Sanctioned\ firm\ dummy$. This is a dummy variable indicating a downgrade is issued by a sanctioned firm. Specifications [5] and [6] in Table 4.4 report the estimation results of Equation (4.2). The coefficient on $Sanctioned\ firm\ dummy$ is negative and statistically significant at the 10% level in both specifications. The pre-announcement abnormal returns of downgrades from the sanctioned firms are 1.9% more negative than those from the non-sanctioned firms. This implies that informed selling before downgrades is more prevalent if these downgrades are from the sanctioned than non-sanctioned firms.

4.3.4. Hypothesis 1: Affiliated vs. unaffiliated analysts

Typically, brokerage firms acting as the lead or co-lead underwriters strive to develop and maintain the closest relationships with the corporate managers of the security issuing companies. Because affiliated analysts work for underwriters by definition, they are more likely than their unaffiliated peers to enjoy exclusive access to private information from companies' management. Whether affiliated analysts are more likely than their unaffiliated counterparts to tip off their affiliated asset managers is an interesting question to investigate.

The relation between analysts' affiliation and the practice of tipping is conceptually ambiguous. On the one hand, unaffiliated analysts, those who are presumably with little access to private information from companies' management, could produce research with little information advantage. It follows that affiliated asset managers who enjoy early access to this research may be relatively less willing to trade on their early knowledge. On the other hand, unaffiliated analysts have an incentive to increase their own private search for information, thereby creating an alternative information advantage. Affiliated asset managers with early access to this research would be inclined to trade on this information advantage. In sum, it is unclear whether affiliated asset managers who enjoy early access to research from their unaffiliated analysts are relatively less likely to trade on their early knowledge.

In what follows, I investigate the effect of the GS on the pre-announcement abnormal returns that are associated with downgrades from affiliated and unaffiliated analysts. Next, I examine whether abnormal returns before downgrades are relatively more negative if these downgrades are from affiliated analysts.

Table 4.5 reports estimation results of Equation (4.1) for the subsamples of affiliated (specifications [1] and [2]) and unaffiliated (specifications [3] and [4]) analysts. In specifications [1] and [2], the coefficient on $PreGS\ dummy$ is negative, economically large, and statistically significant at the 1% level. For example, after controlling for size, Book-to-Market, and momentum effects, pre-announcement abnormal returns are, on average, 8.86% more negative in the pre- than post-GS period (specification [2]). Turning to the unaffiliated analyst subsample, specifications [3] and [4] indicate that the effect of the GS on pre-announcement abnormal returns is also statistically significant, but is economically weaker when compared with the same effect in the affiliated analyst subsample (specifications [1] and [2]). These results indicate that the GS effect is not uniform across analysts. Informed selling before downgrades from affiliated analysts appears to be substantially altered by the GS.

A related question is whether informed selling before downgrades is more prevalent if these downgrades are from affiliated than unaffiliated analysts. To answer this question, I focus on the pre-GS period and propose the specification

$$BHAR(-20,-1)_i = \beta_0 + \beta_1 \times Affiliated\ analyst\ dummy_i + \beta_2 \times \ln(Size)_i + \beta_3 \times \ln\left(1+\frac{B}{M}\right)_i + \beta_4 \times\ BHAR(-86,-21)_i + \varepsilon_i. \quad (4.3)$$

The main variable in Equation (4.3) is $Affiliated\,analyst\,dummy$, which is a dummy variable indicating a downgrade is from an affiliated analyst. Specifications [5] and [6] in Table 4.5 show the estimation results of Equation (4.3). Downgrades from affiliated analysts are associated with significantly more negative pre-announcement abnormal returns than those from unaffiliated analysts. For example, in specification [6] the coefficient on $Affiliated\,analyst\,dummy$ is -3.62%, indicating a substantial difference in pre-announcement abnormal returns between the two groups of analysts. This result implies that informed selling is stronger among downgrades from affiliated than unaffiliated analysts. Because affiliated analysts are generally better informed than unaffiliated analysts, asset managers with early access to research would be more inclined to trade on this early knowledge if the research comes from affiliated than unaffiliated analysts.

4.3.5. Discussion

Combining the results of the previous sections, it is clear that informed selling is limited to the pre-GS period. Moreover, informed selling before downgrades is more prevalent if these downgrades are from affiliated analysts or the sanctioned firms. Based on these findings and the GS requirements in terms of confidentiality of analysts' research, I can imply that in the pre-GS period some affiliated asset managers may have received tips from their brokerage analysts and engaged in selling of the soon to be downgraded stocks.

Although my sample is free of analysts' herding and earnings announcement biases (see Section 4.2.1), one concern is that unobservable confounding events may induce managers to sell stocks and analysts to issue downgrades on these stocks. If so, one would expect downgrades to be dominated by sell-preceded rather than buy-preceded downgrades. To address this concern, I construct a secondary sample of downgrades that are matched with buy and sell trades from affiliated asset managers. In unreported results, I find that this sample contains only 2,306 (49%) sell-preceded downgrades. Hence, buy-preceded downgrades slightly outnumber sell-preceded downgrades. Moreover, the market reaction to sell-preceded downgrades, as measured by the ABHAR from day 0 to day 5, is negative, economically large, and statistically significant, indicating that the announcement of these downgrades signals substantial incremental information that would not otherwise exist if confounding events were already reflected into prices (Table 4.2). Thus, it appears that this concern is overstated.

Finally, my findings suggest that tipping appears not to be a widespread practice. First, only some affiliated asset managers may have early access to research. Based on my secondary sample, unreported results show that more than 50% of the sample downgrades are buy-preceded downgrades, which are presumably free of tipping. Second, it is unlikely that every analyst tips off his affiliated asset managers and other favored institutional investors before the announcement of downgrades. If tipping is a widespread practice, downgrade announcements would be merely a secondary dissemination. My results indicate that this is unlikely to be the case. Pre-GS downgrades are associated with a pre-announcement abnormal return that is equal to -2.718% and a post-announcement abnormal return that is equal to -4.873% (Table 4.2). Thus, it is clear that the largest price response occurs at the announcement of the downgrades, whereas the less influential price response before the announcement of the downgrades seems to be consistent with the view that tipping is limited to some affiliated asset managers. Third, tipping appears to be more prevalent before downgrades from affiliated analysts or the sanctioned firms (Tables 4.4 and 4.5). This provides further evidence that tipping is limited to select analysts or firms.

4.4. Hypothesis 2: early knowledge of forthcoming downgrades

My second hypothesis states that if pre-downgrade informed selling is related to the downgrade characteristics that would imply early knowledge of forthcoming downgrades, one would conclude that tipping is likely to be responsible for some the selling. To test this relation, I focus on the subsample of the pre-GS downgrades that are issued by the sanctioned firms and employ the binary logistic regression

$$\begin{aligned} Informed\ selling\ dummy_i = {} & \beta_0 + \beta_1 \times ln\big(1 + Institutional\ holdings(m)\big)_i \\ & + \beta_2 \times NASD\ dummy_i + \beta_3 \times Strong\ sell\ dummy_i \\ & + \beta_4 \times Downgrade\ magnitude_i + \beta_5 \times Below\ consensus\ dummy_i \\ & + \beta_6 \times ln\big(1 + Analyst\ experience(\#Q)\big)_i \\ & + \beta_7 \times Affiliated\ analyst\ dummy_i \\ & + \beta_8 \times PostFD\ dummy_i + \beta_9 \times \ln(Size)_i \\ & + \beta_{10} \times \ln\left(1 + \frac{B}{M}\right)_i + \beta_{11} \times BHAR(-86,-21)_i + \varepsilon_i, \qquad (4.4) \end{aligned}$$

The dependent variable is $Informed\ selling\ dummy$, which is a dummy variable indicating that the pre-announcement BHAR from day -20 to day -1 is negative. Thus, this dummy captures situations in which the selling activity of affiliated asset managers is reflected in negative pre-announcement abnormal returns. As explanatory variables, I include factors that proxy for the downgrade characteristics that would suggest early knowledge of forthcoming downgrades. I expect that the more institutions invest in a stock, the more likely they search for information about that stock and thus engage in informed selling. $Institutional\ holdings(m)$ is defined as the number of common shares, measured in millions, held by institutional investors at the end of the previous fiscal quarter. Christophe, Ferri, and Hsieh (2010) present evidence of informed short-selling prior to the release of downgrades in a sample of NASDAQ-listed stocks. To see whether informed selling is more pronounced among NASDAQ-listed stocks, I include a dummy variable, $NASDAQ\ dummy$, indicating a downgrade is issued on a stock that is listed on NASDAQ. I expect informed selling to be relatively more prevalent if the characteristics of a forthcoming downgrade would imply a greater loss in stock value. To test this prediction, I employ three variables. $Strong\ sell\ dummy$ is a dummy variable indicating a stock is downgraded to a strong sell recommendation (I/B/E/S numerical score = 5). $Downgrade\ magnitude$ is the difference in I/B/E/S numerical score between the current and previous recommendation. $Below\ consensus\ dummy$ is a dummy variable indicating a stock is downgraded to a recommendation that is below the consensus recommendation. Experienced analysts may be less likely than others to tip off their affiliated asset managers because of their career concerns. $Analyst\ experience(\#Q)$ is defined as the number of quarters since the analyst issued the first recommendation in I/B/E/S. Based on the evidence in Table 4.5, it appears that pre-announcement abnormal returns are relatively more negative if downgrades come from affiliated analysts. So, I expect a positive association between informed trading and affiliated analysts. $Affiliated\ analyst\ dummy$ is a dummy variable indicating a downgrade is issued by an analyst who is categorized as affiliated. After Regulation Fair Disclosure was enacted, the precision and informativeness of analysts' research have substantially decreased (Bailey, Li, Mao, and Zhong, 2003). It is interesting to investigate whether this regulation has also altered informed selling before downgrades. $PostFD\ dummy$is a dummy variable indicating a downgrade is issued after Regulation Fair Disclosure came into effect.[58] Equation (4.4) also includes control factors that proxy for size, Book-to-Market, and momentum effects.

Table 4.6 presents the estimation results of Equation (4.4). The results are generally consistent with my predictions. Informed selling is relatively more prevalent among high institutional ownership stocks. The coefficient on the NASDAQ dummy takes the expected sign, but is statistically insignificant. Informed selling before downgrades is relatively more likely if these downgrades are strong sell downgrades or downgrades of high magnitude. However, informed selling seems to be unrelated to whether a stock is downgraded to a recommendation that is below the consensus recommendation. The coefficients on the analyst experience and affiliation variables take the correct signs, but are statistically insignificant. As expected, informed selling before downgrades is relatively weaker if these downgrades are issued after Regulation Fair Disclosure took effect.

Now, I turn to discussing the effects of the control factors. The coefficients on the size and Book-to-Market factors indicate that informed selling is relatively more prevalent among small-cap or value stocks. Informed selling is relatively more likely among stocks with negative momentum; however, this relation is statistically insignificant.

The general picture that emerges from this analysis is that informed selling is related to the downgrade characteristics that would imply early knowledge of forthcoming

[58] I assume Regulation Fair Disclosure came into force on November 23, 2000.

downgrades. Specifically, informed selling is relatively more prevalent prior to strong downgrades, downgrades of high magnitude, downgrades that are issued before Regulation Fair Disclosure, or downgrades that are issued on high institutional ownership stocks. Thus, it seems reasonable to conclude that informed selling prior to downgrades is at least partly induced by tips received from brokerage analysts.

4.5. Conclusion

The economic interdependence between a full-service brokerage firm and its affiliated asset managers may result in unfair practices that are against the interests of investors. Anecdotal evidence suggests that affiliated asset managers may receive tips from their brokerage analysts regarding the content of the soon to be released research reports, while other investors remain uninformed. For example, on March 14, 2007 the SEC fined Banc of America Securities LLC US$26 million over the claims that trading employees had access to research reports before they were published, and in at least two instances the bank traded in advance of their release.

This chapter investigates whether affiliated asset managers engage in informed selling prior to downgrade releases from their brokerage analysts. I find that, in the period prior to the Global Settlement and related regulations, the selling activity of affiliated asset managers is associated with significantly negative pre-announcement abnormal returns and abnormally high share turnovers, implying pre-downgrade informed selling. Because these regulations include provisions that would limit the information flow from sell-side analysts to their affiliated asset managers, I can conclude that some of the selling activity is likely to be induced by tips received from those analysts.

Focusing on downgrades issued by the sanctioned firms before the Global Settlement, I show that pre-downgrade informed selling of affiliated asset managers is related to the downgrade characteristics that would imply early knowledge of the soon to be released downgrades. Specifically, I find that informed selling is relatively more prevalent prior to strong downgrades, downgrades of high magnitude, downgrades that are issued before Regulation Fair Disclosure, or downgrades that are issued on high institutional ownership stocks. This provides further evidence that some affiliated asset managers may have received tips from their brokerage analysts before downgrade releases and engaged in selling of the soon to be downgraded stocks.

However, my results also indicate that the extent of tipping practices is somewhat limited. First, tipping is limited to the period prior to the Global Settlement. Second, tipping appears to be stronger among downgrades from the sanctioned firms or affiliated analysts. Third, the substantial market impact of downgrades implies that the public announcement remains the primary dissemination of information. Fourth, an analysis of a secondary sample shows that buy-preceded downgrades dominate sell-preceded downgrades.

Table 4.1. Features of the sample.

This table reports the number and percentage of sample downgrades, analysts, brokerage firms, and covered companies. The results are stratified by whether a downgrade is issued before or after the Global Research Analyst Settlement (GS) (October 01, 2002), from a sanctioned or non-sanctioned firm, or from an affiliated or unaffiliated analyst. Appendix E provides the definitions of sanctioned firms and affiliated analysts. The sample consists of I/B/E/S downgrades from the most active full-service brokerage firms (contributing at least 0.5% of the total recommendations in I/B/E/S) between 1996 and 2010. Only those downgrades that are associated with a sell trade from affiliated asset managers in the preceding month are retained in the sample. To minimize the impact of confounding events, the sample includes only those downgrades for which the company did not experience another downgrade or make an earnings announcement in the preceding month.

Sample	Downgrades	Analysts	Firms	Companies
Pre-GS	913	617	36	735
	39.6%	49.0%	83.7%	52.0%
Post-GS	1,393	795	35	929
	60.4%	63.1%	81.4%	65.7%
Sanctioned firms	1,354	700	10	938
	58.7%	55.6%	23.3%	66.3%
Non-sanctioned firms	952	612	33	757
	41.3%	48.6%	76.7%	53.5%
Affiliated analysts	341	280	30	297
	14.8%	22.2%	69.8%	21.0%
Unaffiliated analysts	1,965	1,141	43	1,245
	85.2%	90.6%	100.0%	88.0%
Overall	2,306	1,259	43	1,414
	100.0%	100.0%	100.0%	100.0%

Table 4.2. Hypothesis 1: univariate evidence.
This table reports Average Buy-and-Hold Abnormal Returns (ABHARs) and Average Abnormal Share Turnovers (AASTs) over several sub-periods of the event window (-20, 20). The results are stratified by whether a downgrade is issued before or after the Global Research Analyst Settlement (GS) (October 01, 2002). ABHAR is defined as the sample average of the buy-and-hold return on a stock less the buy-and-hold return on a size-B/M matched portfolio. AAST is measured as the sample average of the daily average percentage change in share turnover relative to a normal level of share turnover, which I estimate as the median daily share turnover during the year in which the downgrade is issued. *, **, and *** denote significance levels of 10%, 5%, and 1%, respectively. Difference t-tests are based on the Satterthwaites's method (unequal variances).

	Pre-GS	Post-GS	Difference
Relative day	Panel A: ABHAR		
-20 to -1	-2.718***	1.551***	-4.269***
0 to 20	-4.873***	-3.300***	-1.573**
-20 to -11	-0.680*	0.847***	-1.527***
-10 to -6	-0.428*	0.271**	-0.699**
-5 to -1	-1.407***	0.201	-1.608***
0 to 5	-4.837***	-3.686***	-1.151**
6 to 10	0.480	0.161	0.319
11 to 20	-0.306	0.279	-0.585
	Panel B: AAST		
-20 to -1	50.2***	16.6***	33.6***
0 to 20	87.0***	51.6***	35.4***
-20 to -11	41.9***	14.4***	27.5***
-10 to -6	39.9***	13.1***	26.8***
-5 to -1	77.1***	24.7***	52.4***
0 to 5	159.7***	85.4***	74.3***
6 to 10	63.0***	38.3***	24.7***
11 to 20	55.2***	37.3***	17.9***

Table 4.3. Hypothesis 1: multivariate evidence.
This table reports GMM estimation results of a regression in which the dependent variable is the Buy-and-Hold Abnormal Return (BHAR) from day -20 to day -1, relative to the event day, day 0. BHAR is defined as the buy-and-hold return on a stock less the buy-and-hold return on a size-B/M matched portfolio. The explanatory variables are defined in Appendix E. *, **, and *** denote significance levels of 10%, 5%, and 1%, respectively.

Dependent variable	BHAR(-20,-1)	
	[1]	[2]
Number of observations = 2,306		
Intercept	-0.0328	-0.0324
	(0.112)	(0.115)
Pre-GS dummy	-0.0381***	-0.0372***
	(0.000)	(0.000)
Ln(Size (m$))	0.0087***	0.0084***
	(0.000)	(0.000)
Ln(1 + B/M)	-0.0503***	-0.0456**
	(0.006)	(0.014)
BHAR(-86,-21)		0.0239
		(0.208)
Adjusted R^2	0.043	0.044

Table 4.4. Hypothesis 1: sanctioned vs. non-sanctioned firms.

This table reports GMM estimation results of a regression in which the dependent variable is the Buy-and-Hold Abnormal Return (BHAR) from day -20 to day -1, relative to the event day, day 0. BHAR is defined as the buy-and-hold return on a stock less the buy-and-hold return on a size-B/M matched portfolio. The explanatory variables are defined in Appendix E. Specifications [1] and [2] ([3] and [4]) [[5] and [6]] report estimations results when the sample is restricted downgrades issued by the sanctioned firms (by the non-sanctioned firms) [before the Global Research Analyst Settlement (GS) (October 01, 2002)]. *, **, and *** denote significance levels of 10%, 5%, and 1%, respectively.

Dependent variable	BHAR(-20,-1)					
Subsample	Sanctioned firms		Non-sanctioned firms		Pre-GS	
	[1]	[2]	[3]	[4]	[5]	[6]
Number of observations	1,354	1,354	952	952	913	913
Intercept	-0.0625**	-0.0615**	0.0076	0.0072	-0.1033***	-0.1035***
	(0.026)	(0.028)	(0.795)	(0.805)	(0.002)	(0.002)
Pre-GS dummy	-0.0485***	-0.0476***	-0.0270***	-0.0263***		
	(0.000)	(0.000)	(0.006)	(0.008)		
Sanctioned firm dummy					-0.019*	-0.019*
					(0.092)	(0.091)
Ln(Size (m$))	0.0111***	0.0107***	0.0056*	0.0054*	0.016***	0.0157***
	(0.000)	(0.000)	(0.076)	(0.087)	(0.000)	(0.000)
Ln(1 + B/M)	-0.022	-0.0178	-0.0972***	-0.0924***	-0.0787***	-0.0717***
	(0.333)	(0.440)	(0.000)	(0.000)	(0.000)	(0.001)
BHAR(-86,-21)		0.0244		0.0193		0.0225
		(0.339)		(0.478)		(0.395)
Adjusted R^2	0.052	0.053	0.043	0.043	0.060	0.060

Table 4.5. Hypothesis 1: affiliated vs. unaffiliated firms.

This table reports GMM estimation results of a regression in which the dependent variable is the Buy-and-Hold Abnormal Return (BHAR) from day -20 to day -1, relative to the event day, day 0. BHAR is defined as the buy-and-hold return on a stock less the buy-and-hold return on a size-B/M matched portfolio. The explanatory variables are defined in Appendix E. Specifications [1] and [2] ([3] and [4]) [[5] and [6]] report estimations results when the sample is restricted to downgrades issued by affiliated analysts (by the unaffiliated analysts) [before the Global Research Analyst Settlement (GS) (October 01, 2002)]. *, **, and *** denote significance levels of 10%, 5%, and 1%, respectively.

Dependent variable	BHAR(-20,-1)					
Subsample	Affiliated analysts		Unaffiliated analysts		Pre-GS	
	[1]	[2]	[3]	[4]	[5]	[6]
Number of observations	341	341	1,965	1,965	913	913
Intercept	-0.0514	-0.0488	-0.0166	-0.0168	-0.0960***	-0.0964***
	(0.256)	(0.276)	(0.462)	(0.453)	(0.005)	(0.004)
Pre-GS dummy	-0.0896***	-0.0886***	-0.0294***	-0.0286***		
	(0.000)	(0.000)	(0.000)	(0.000)		
Affiliated analyst dummy					-0.0368**	-0.0362**
					(0.023)	(0.025)
Ln(Size (m$))	0.0112**	0.0106**	0.0069***	0.0066***	0.0146***	0.0143***
	(0.010)	(0.016)	(0.004)	(0.005)	(0.000)	(0.000)
Ln(1 + B/M)	-0.0172	-0.0134	-0.0607***	-0.0561***	-0.0800***	-0.0734***
	(0.748)	(0.805)	(0.000)	(0.000)	(0.000)	(0.001)
BHAR(-86,-21)		0.0227		0.0222		0.0212
		(0.686)		(0.257)		(0.422)
Adjusted R^2	0.104	0.103	0.035	0.036	0.063	0.063

Table 4.6. Hypothesis 2: early knowledge of forthcoming downgrades.
This table reports GMM estimation results of a logit regression in which the dependent variable, Informed selling dummy, is a dummy variable indicating the BHAR(-20,-1) is negative. BHAR(-20,-1) is the Buy-and-Hold Abnormal Return from day -20 to day -1, relative to the event day, day 0. BHAR is defined as the buy-and-hold return on a stock less the buy-and-hold return on a size-B/M matched portfolio. The explanatory variables are defined in Appendix E. Standard errors are clustered at the brokerage firm level. *, **, and *** denote significance levels of 10%, 5%, and 1%, respectively.

Dependent variable	Informed selling dummy			
	Normative direction	Actual direction	Regression coefficient	P-value
Number of observations = 453				
Intercept			1.737	(0.274)
Characteristics				
Ln(1 + Institutional ownership (m))	+	+	0.390**	(0.015)
NASDAQ dummy	+	?	0.597	(0.135)
Strong sell dummy	+	+	11.800***	(0.000)
Downgrade magnitude	+	+	0.456***	(0.004)
Below consensus dummy	+	?	-0.040	(0.840)
Ln(1 + Analyst experience (# of Q))	-	?	-0.307	(0.198)
Affiliated analyst dummy	+	?	0.117	(0.526)
Post-FD dummy	-	-	-0.611***	(0.000)
Controls				
Ln(Size (m$))			-0.361**	(0.041)
Ln(1 + B/M)			0.772*	(0.095)
BHAR(-86,-21)			-0.189	(0.433)
Wald test			321.6***	(0.000)

CHAPTER FIVE: CONCLUSION

This dissertation investigates novel issues related to information intermediaries in financial markets and is organized in three essays on the following distinct topics: (1) the monitoring role of credit rating agencies, (2) heterogeneity in the influence of sell-side analyst recommendation changes, and (3) the information flow from sell-side analysts to their affiliated asset managers.

The first essay (Chapter Two) investigates whether credit rating agencies act as third-party monitors through the mechanism of a credit watch with direction downgrade. The related empirical predictions follow from two main channels resulting from third-party rating agency monitoring. First, by exerting informal influence the agencies can induce firms to undertake safe projects, helping improve their profitability. Second, when the agencies reveal the true credit quality of firms following the monitoring episode, they can help reduce information asymmetry and thus facilitate access to capital markets. Accordingly, firms increase their long-term financing and ramp up their investment activities.

I provide evidence consistent with these predictions and document that firms with watch-preceded rating confirmations (treatment firms) experience an increase in long-term financing, investment, and profitability immediately following the credit watch period when benchmarked against firms with watch-preceded rating downgrades (control firms). Interestingly, these patterns are stronger for treatment firms of lower credit quality, for which external monitoring is more valuable. I further show that financially constrained treatment firms substantially increase their long-term financing immediately following the watch period, indicating that agency's monitoring can help alleviate capital constraints resulting from information asymmetry. Additional tests show that treatment firms are more profitable regardless of corporate governance, implying that agency's monitoring is independent of corporate governance. Collectively, these findings indicate that a credit watch with direction downgrade acts as an effective monitoring mechanism.

The second essay (Chapter Three) presents and empirically evaluates a theory of ambiguity that predicts stronger influence of recommendation changes issued on firms with more ambiguous environments. I focus on two aspects of ambiguity in the firm environment: the ambiguity aspect that emerges from lack of knowledge of nonexistent relevant information and that manifests itself through the difficulty investors face in formulating prior beliefs about firm fundamentals (ambiguity in fundamentals or AIF); and the ambiguity aspect that pertains to the difficulty investors face in updating their prior beliefs in response to firm information of uncertain quality, (ambiguity in information or AII).

I provide evidence that both aspects of ambiguity substantially increase the influence of recommendation changes (the "mean" effect). Moreover, I show that the influence of recommendation changes issued on higher AII firms substantially increases when their fundamentals are perceived to be more ambiguous (the "interaction" effect). This essay contributes to the analyst literature by providing a behavioral explanation for anomalous heterogeneity in the influence of recommendation changes.

The third essay (Chapter Four) documents evidence that affiliated asset managers engage in informed selling prior to downgrade releases from their brokerage analysts. I find that, in the period prior to the Global Settlement and related regulations, the selling activity of affiliated asset managers is associated with significantly negative pre-announcement abnormal returns and abnormally high share turnovers, implying pre-downgrade informed selling.

Because these regulations include provisions that would limit the information flow from sell-side analysts to their affiliated asset managers, I can conclude that some of the selling activity is likely to be induced by tips received from those analysts. Focusing on downgrades issued by the sanctioned firms before the Global Settlement, I also show that pre-downgrade informed selling of affiliated asset managers is related to the downgrade characteristics that would imply early knowledge of the soon to be released downgrades. Collectively, these findings point to evidence on tipping, but also highlight the effectiveness of the Global Settlement in mitigating this unfair practice.

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APPENDICES

Appendix A. Variable definitions (Chapter Two).

This appendix defines the variables employed in this study. Credit watch data are from Moody's, and accounting information is from Compustat. Firm characteristics are defined using Compustat data items. The variable ATQ_AVG is equal to (ATQ + ATQ_LAG) / 2, where ATQ_LAG is previous year's ATQ. The variable SEQQ_AVG is equal to (SEQQ + SEQQ_LAG) / 2, where SEQQ_LAG is previous year's SEQQ. The variable DLTTQ_LAG is previous year's DLTTQ. The variable DLCQ_LAG is previous year's DLCQ. The variable PPENTQ_LAG is previous year's PPENTQ.

Variable	Definition
Panel A: Watch characteristics	
1. Watch days	Number of days in the watch period, i.e., from the watch assignment date to the watch resolution date.
2. Non-investment grade (NIG)	Dummy variable indicating a rating placed under watch is of non-investment grade, i.e., a rating within the rating class range Ba-C.
3. Rating on watch	Numerical score associated with a rating on watch, where allocation of scores is based on the following scale: Aaa = 1; Aa1 = 2; Aa2 = 3; Aa3 = 4; …; and C = 21.
4. Rating off watch	Numerical score associated with a rating after watch, where allocation of scores is based on the following scale: Aaa = 1; Aa1 = 2; Aa2 = 3; Aa3 = 4; …; and C = 21.
5. Rating change	Magnitude of a rating change, measured as rating off watch minus rating on watch.
6. Fallen angel	Dummy variable indicating a downgraded rating has crossed the investment grade boundary, i.e. a rating moving from the rating class range Aaa-Baa to Ba-C.
7. Past confirmations	Number of watch-preceded rating confirmations issued on a given firm between sample inception (1992) and the current date.
Panel B: Firm characteristics	
Financing	
1. Change in long-term debt ratio	(DLTTQ - DLTTQ_LAG) / ATQ_AVG.
2. Equity issuance ratio	trailing-twelve-months SSTKY adjusted for fiscal quarter accumulation scaled by ATQ_AVG.
3. Change in long-term financing ratio	Change in long-term debt ratio plus equity issuance ratio.
4. Change in short-term debt ratio	(DLCQ - DLCQ_LAG) / ATQ_AVG.
5. Change in cash holdings ratio	Trailing-twelve-months CHECHY adjusted for fiscal quarter accumulation scaled by ATQ_AVG.
Investment	
6. Capital expenditures ratio	Trailing-twelve-months CAPXY adjusted for fiscal quarter accumulation scaled by ATQ_AVG.
7. PPE growth rate	Ln(PPENTQ / PPENTQ_LAG).
8. Asset growth rate	Ln(ATQ / ATQ_LAG).
Profitability	
9. Operating income ratio	Trailing-twelve-months OIBDPQ scaled by ATQ_AVG.
10. Return on assets	Trailing-twelve-months NIQ scaled by ATQ_AVG.
11. Return on equity	Trailing-twelve-months NIQ scaled by SEQQ_AVG.
Others	
12. Assets (million $)	ATQ.
13. Tobin's Q	(ATQ - CEQQ - TXDITCQ + CSHOQ * PRCCQ) / ATQ.

14. Tangibility	PPENTQ / ATQ.
15. Governance index	The G Index of Gompers, Ishii, and Metrick (2003).
16. Cash flow investment gap ratio	Trailing-twelve-months IBQ plus trailing-twelve-months DPQ minus trailing-twelve-months CAPXY adjusted for fiscal quarter accumulation all scaled by ATQ_AVG.
17. Reg FD	Regulation Fair Disclosure dummy variable, which indicates a credit watch is resolved after November 01, 2000.

Appendix B. Switching regression with endogenous switching (Chapter Two).

The model comprises a binary outcome equation along with a latent equation that matches watch resolution decisions with firm characteristics and other relevant factors, and a set of two equations that model changes in the firm fundamentals measure of interest in two different regimes. Formally, the model is described by the following set of equations

$$I_i = 1 \text{ iff } I_i^* > 0, \text{ and } I_i = 0 \text{ iff } I_i^* \leq 0, \text{ where} \tag{B.1}$$

$$I_i^* = \gamma Z_i + u_i; \text{ and} \tag{B.2}$$

$$\Delta Y_{1i} = \beta_1 X_i + u_{1i}, \text{ and} \tag{B.3}$$

$$\Delta Y_{2i} = \beta_2 X_i + u_{2i}. \tag{B.4}$$

Eq. (B.1) models the observed watch resolution outcomes, in which the binary variable I_i indicates whether a credit watch issued on firm i is resolved with a rating confirmation. Eq. (B.2) is the latent watch resolution equation, where the vector Z_i consists of variables that may help shape watch resolution decisions. Eq. (B.3) and (B.4) are the fundamentals equations for the rating confirmation and downgrade regimes, respectively. To capture the cumulative effects of watch resolution outcomes, I compute q-quarter changes in a fundamentals measure from quarter 0 to quarter q, where quarter 0 represents the quarter ending prior to the watch period, and $q = 1$, 2, 3, or 4 quarters. Accordingly, the variables ΔY_{1i} and ΔY_{2i} represent q-quarter future changes in a fundamentals measure of firm i in case of a rating confirmation and a rating downgrade, respectively. For a given watch observation either Eq. (B.3) or (B.4) is realized depending on the watch resolution outcome for firm i. For example, if a credit watch is resolved with a rating confirmation, we observe ΔY_{1i}, and never ΔY_{2i}, so that only Eq. (B.3) is realized. The vector X_i consists of variables that may affect the future pattern of firm fundamentals. To facilitate model estimation and inferences, I assume that the residuals u_{1i}, u_{2i}, and u_i have a trivariate normal distribution.[59]

A key distinctive feature of the endogenous switching regression model is the explicit modeling of endogeneity by allowing the residuals in the fundamentals equations, Eq. (B.3) and (B.4), to correlate with the residual in the watch resolution equation, Eq. (B.2). That is, unobserved or missing variables that influence watch resolution decisions are allowed to affect future changes in firm fundamentals. Formally, this implies that the residual vector has a nondiagonal covariance matrix with the following structure

$$cov(u_{1i}, u_{2i}, u_i) = \begin{bmatrix} \sigma_{11} & \sigma_{12} & \sigma_{1u} \\ \sigma_{21} & \sigma_{22} & \sigma_{2u} \\ \sigma_{u1} & \sigma_{u1} & 1 \end{bmatrix}. \tag{B.5}$$

To estimate the switching regression model, it is important to note that OLS estimation of Eq. (B.3) and (B.4) yields inconsistent estimates, given the above residual covariance structure. To see this, I take the expected

[59] In my empirical tests, Eq. (B.3) and (B.4) also include industry fixed effects, $Industry_i$, and calendar year fixed effects, $Year_t$, which are not shown here for brevity of presentation.

values of the future changes in the fundamentals measure in Eq. (B.3) and (B.4), where I apply standard rules for conditional normal variables (see, e.g., pp. 367 in Maddala, 1983). I define ΔY_i as the unconditional future change in the fundamentals measure of firm i and compute the expected values as follows

$$\begin{aligned} E[\Delta Y_{1i}] &= E[\Delta Y_i \mid I_i^* > 0] \\ &= E[\beta_1 X_i + u_{1i} \mid \gamma Z_i + u_i > 0] \\ &= \beta_1 X_i + E[u_{1i} \mid u_i > -\gamma Z_i] \\ &= \beta_1 X_i + \sigma_{1u} E[u_i \mid u_i > -\gamma Z_i] \\ &= \beta_1 X_i + \sigma_{1u} \underbrace{\left(\frac{\phi(\gamma Z_i)}{\Phi(\gamma Z_i)}\right)}_{=Inverse\ Mills_{1i}} ; \end{aligned} \tag{B.6}$$

$$\begin{aligned} E[\Delta Y_{2i}] &= E[\Delta Y_i \mid I_i^* \leq 0] \\ &= E[\beta_2 X_i + u_{2i} \mid \gamma Z_i + u_i \leq 0] \\ &= \beta_2 X_i + E[u_{2i} \mid u_i \leq -\gamma Z_i] \\ &= \beta_2 X_i + \sigma_{2u} E[u_i \mid u_i \leq -\gamma Z_i] \\ &= \beta_2 X_i - \sigma_{2u} \underbrace{\left(\frac{\phi(\gamma Z_i)}{1-\Phi(\gamma Z_i)}\right)}_{=Inverse\ Mills_{2i}} ; \end{aligned} \tag{B.7}$$

where σ_{1u} is the covariance between u_{1i} and u_i; σ_{2u} is the covariance between u_{2i} and u_i; $\phi(.)$ is the density function of the standard normal distribution; and $\Phi(.)$ is the cumulative distribution function of the standard normal distribution. The last terms in Eq. (B.6) and (B.7) are the associated inverse Mills ratios, which result from the nonzero conditional expectation of the residuals. Because of these additional terms, OLS estimation of Eq. (B.3) and (B.4) yields inconsistent estimates. However, if I augment Eq. (B.3) and (B.4) with the inverse Mills ratios as additional explanatory variables, OLS estimation will generate consistent estimates. The augmented fundamentals equations are

$$\Delta Y_{1i} = \beta_1 X_i + \sigma_{1u} Inverse\ Mills_{1i} + \varepsilon_{1i}, \text{ and} \tag{B.8}$$

$$\Delta Y_{2i} = \beta_2 X_i - \sigma_{2u} Inverse\ Mills_{2i} + \varepsilon_{2i}, \tag{B.9}$$

where ε_{1i} and ε_{2i} are the new residuals, each of which has a zero conditional mean. In light of the above results, the two-stage estimation of the switching regression model is now clear. First, I estimate Eq. (B.1) and (B.2) using a probit regression and obtain consistent estimates of γ, denoted by $\hat{\gamma}$. Next, I obtain estimates of the inverse Mills ratios for Eq. (B.8) and (B.9) by substituting $\hat{\gamma}$ for γ. I then estimate Eq. (B.8) and (B.9) by OLS, yielding consistent estimates of β_1, β_2, σ_{1u}, and σ_{2u}. This two-step estimation procedure was first proposed in Lee (1976) and later discussed in Maddala (1983).

To investigate the effects of watch resolution decisions on firm fundamentals while controlling for endogeneity, it is necessary to address the following question: for a firm with a rating confirmation, what would the alternative change in a fundamentals measure be had its rating been downgraded instead? To answer the question, I need an empirical methodology that evaluates for the same firm the effects of switching from an actual rating confirmation to a hypothetical rating downgrade. The above analysis lays the grounds for answering such a question. What I have to do next is compare the actual change in a fundamentals measure for a firm with a rating confirmation with the predicted change in that measure under a hypothetical rating downgrade scenario. Following Fang (2005), I compute the following difference

$$\begin{aligned}\underbrace{\Delta Y_{1i}}_{actual} - \underbrace{E[\Delta Y_{2i} \mid I_i^* > 0]}_{hypothetical} &= \Delta Y_{1i} - E[\beta_2 X_i + u_{2i} \mid \gamma Z_i + u_i > 0] \\ &= \Delta Y_{1i} - \beta_2 X_i - E[u_{2i} \mid u_i > -\gamma Z_i] \\ &= \Delta Y_{1i} - \beta_2 X_i - \sigma_{2u} E[u_i \mid u_i > -\gamma Z_i] \\ &= \Delta Y_{1i} - \beta_2 X_i - \sigma_{2u} \left(\frac{\phi(\gamma Z_i)}{\Phi(\gamma Z_i)}\right). \qquad \text{(B.10)}\end{aligned}$$

The difference in Eq. (B.10) has an intuitive interpretation. For example, if the firm fundamentals measure of interest is profitability, a positive difference can be interpreted as an improvement in profitability for the same firm i. From Eq. (B.10), note that X_i is the vector of characteristics for the firm with a rating confirmation and $\left(\frac{\phi(\gamma Z_i)}{\Phi(\gamma Z_i)}\right)$ is its associated inverse Mills ratio, and the parameters β_2 and σ_{2u} are the coefficients from the fundamentals equation for the rating downgrade regime. Therefore, the hypothetical future change in a firm fundamentals measure is the predicted value from evaluating the characteristics of a firm with a rating confirmation and its associated inverse Mills ratio in the fundamentals equation for the rating downgrade regime. My inferences with respect to the effects of watch resolution outcomes on future changes in firm fundamentals shall draw upon the key result in Eq. (B.10).

Empirically, I estimate a probit regression that models the decision of a rating confirmation, as described in Eq. (B.1) and (B.2). The vector Z_i includes the following variables: a non-investment grade dummy, firm size, Tobin's Q, and asset tangibility. I also consider other variables that may influence credit watch outcomes, but are unlikely to directly affect corporate fundamentals.[60] These are: a dummy variable indicating whether a watch is resolved after Regulation Fair Disclosure came into effect (Reg FD); the natural logarithm of the duration of the watch period (watch days); the natural logarithm of the number of instances a firm has been placed under watch in the past (Ln(1 + past watches)); and the natural logarithm of the number of instances a firm has received a watch-preceded rating confirmation in the past (Ln(1 + past confirmations)). Lastly, I also include dummy variables that indicate calendar years and 38 SIC-based industries. Next, I estimate the augmented fundamentals equations (B.8) and (B.9). The vector X_i consists of the following variables: a non-investment grade dummy, firm size, Tobin's Q, asset tangibility, cash flow investment gap, and the natural logarithm of the G Index. I also include dummy variables that indicate calendar years, fiscal quarters, and 38 SIC-based industries. Finally, I compute the empirical analogs of the difference in Eq. (B.10) and report the results in Table 2.11.

[60] Technically, these variables serve as identification restrictions in the endogenous switching model.

Appendix C. Derivation of Eq. (3.7.a)-(3.7.c) (Chapter Three)

$$w_{s_2}^{AIF} - w_{s_2} = \frac{\sigma_1^4\sigma_2^2\left(\overline{\sigma}^2-\sigma^2\right)}{\left(\overline{\sigma}^2(\sigma_1^2+\sigma_2^2)+\sigma_1^2\sigma_2^2\right)\left(\sigma^2(\sigma_1^2+\sigma_2^2)+\sigma_1^2\sigma_2^2\right)} > 0. \tag{C.1}$$

$$w_{s_2}^{AII} - w_{s_2} = \frac{\sigma^4\sigma_2^2\left(\overline{\sigma}_1^2-\sigma_1^2\right)}{\left(\sigma^2\left(\overline{\sigma}_1^2+\sigma_2^2\right)+\overline{\sigma}_1^2\sigma_2^2\right)\left(\sigma^2(\sigma_1^2+\sigma_2^2)+\sigma_1^2\sigma_2^2\right)} > 0. \tag{C.2}$$

$$w_{s_2}^{AIF,AII} - w_{s_2}^{AIF} = \frac{\overline{\sigma}^4\sigma_2^2\left(\overline{\sigma}_1^2-\sigma_1^2\right)}{\left(\overline{\sigma}^2\left(\overline{\sigma}_1^2+\sigma_2^2\right)+\overline{\sigma}_1^2\sigma_2^2\right)\left(\overline{\sigma}^2(\sigma_1^2+\sigma_2^2)+\sigma_1^2\sigma_2^2\right)} > 0. \tag{C.3}$$

$$\left(w_{s_2}^{AIF,AII} - w_{s_2}^{AIF}\right) - \left(w_{s_2}^{AII} - w_{s_2}\right) =$$

$$\frac{\left(\overline{\sigma}_1^2-\sigma_1^2\right)\left(\overline{\sigma}^2-\sigma^2\right)\sigma_2^4\left(\overline{\sigma}^2\sigma^2\left(\sigma_1^2\left(\overline{\sigma}_1^2+\sigma_2^2\right)+\overline{\sigma}_1^2(\sigma_1^2+\sigma_2^2)+\sigma_1^2\overline{\sigma}_1^2\sigma_2^2\left(\overline{\sigma}^2+\sigma^2\right)\right)\right)}{\left(\overline{\sigma}^2\left(\overline{\sigma}_1^2+\sigma_2^2\right)+\overline{\sigma}_1^2\sigma_2^2\right)\left(\overline{\sigma}^2(\sigma_1^2+\sigma_2^2)+\sigma_1^2\sigma_2^2\right)\left(\sigma^2\left(\overline{\sigma}_1^2+\sigma_2^2\right)+\overline{\sigma}_1^2\sigma_2^2\right)\left(\sigma^2(\sigma_1^2+\sigma_2^2)+\sigma_1^2\sigma_2^2\right)} > 0. \tag{C.4}$$

Appendix D. Variable definitions (Chapter Three).

This appendix defines the variables employed in this chapter. Most variables are constructed as of the end of the calendar month prior to the announcement of a recommendation change (denoted by month m). The sample of recommendations is from the I/B/E/S U.S. Detail Recommendations File. Stock data are from the Center for Research in Security Prices (CRSP), accounting information is from Compustat, data on institutional ownership are from Thompson Reuters, and information on analyst affiliation is from the Securities Data Company (SDC) database.

Variable	Description	Calculation detail
Firm characteristics		
1. *N52WL*	A stock price's nearness to its 52-week low	$N52WL_{i,m} = (52WH_{i,m} - P_{i,m})/52WL_{i,m}$, where $P_{i,m}$ = stock i's price observed at the end of month m, $52WH_{i,m}$ = stock i's 52-week high price determined from the 52-week period prior to the end of month m, $52WL_{i,m}$ = stock i's 52-week low price determined from the 52-week period prior to the end of month m.
2. *ME*	Market capitalization of common equity measured in millions of dollars	The price of a common share multiplied by the common shares outstanding observed at the end of month m.
3. *BM*	Book-to-Market equity ratio	The book value of equity (definition from Daniel and Titman (2006)) observed at the end of the previous fiscal year scaled by the market capitalization of common equity (*ME*) observed at the end of month m.
4. *MOM*	Stock return momentum	$MOM_{i,m} = \prod_{M=m-11}^{m}(1 + R_{i,M}) - \prod_{M=m-11}^{m}(1 + R_{i,M}^{DGTW})$, where $R_{i,M} =$ the month-M raw return on stock i, $R_{i,M}^{DGTW}$ = the month-M raw return on a benchmark portfolio with comparable size, book-to-market, and momentum characteristics as stock i (Daniel, Grinblatt, Titman, and Wermers, 1997, or DGTW).
5. *IO*	Institutional ownership of common shares measured in millions	The number of the common shares held by institutional investors scaled by the total number of outstanding common shares observed at the end of month m.
6. *AGE*	Firm age measured in quarters	The number of quarters between the first time a stock appears in CRSP and month m.
7. *ACOV*	Analyst coverage	The total number of analysts in I/B/E/S following the firm during the year prior to the end of month m.
8. *FDISP*	Earnings forecast dispersion	The standard deviation of analyst annual EPS forecasts (a minimum of three forecasts) submitted any time during the three-month period prior to the end of month m, scaled by the stock price as of the end of this period. If an analyst makes more than one forecast during the three-month period, only the last forecast is used in my calculations.
Analyst characteristics		
9. *AEXP*	Analyst experience	The number of quarters between the first time an analyst issued the first recommendation in I/B/E/S and month m.

10. *AFF dummy*	Affiliated analyst dummy	= 1 if the analyst issues a recommendation at any point during the period after the file date on an equity or debt issue in which the analyst employer acts as the lead or co-lead underwriter or after the file date on an acquisition in which the analyst employer acts as an advisor.
11. *IND dummy*	Independent analyst dummy	= 1 if the analyst is from an independent non-investment bank firm, i.e. a firm that has never been classified as a lead or co-lead on any equity or debt deal or has never advised either the target or acquirer in an acquisition at any point in the sample period.
Recommendation characteristics		
12. *DC*	Recommendation deviation from consensus	$= (Rec_{current} - Consensus)^2 - \left(Rec_{prior} - Consensus\right)^2$, where $Rec_{current}$ = a current recommendation, Rec_{prior} = a prior recommendation, $Consensus$ = a consensus recommendation.
13. *MAG*	Magnitude of a recommendation change	$= \left\|Rec_{current} - Rec_{prior}\right\|$, where $Rec_{current}$ = a current recommendation, Rec_{prior} = a prior recommendation.
14. *FD dummy*	Regulation Fair Disclosure dummy	= 1 after September 01, 2000.
15. *GS dummy*	The Global Research Analyst Settlement dummy	= 1 after August 01, 2002.

Appendix E. Variable definitions (Chapter Four).

This appendix defines variables. The data sources are: I/B/E/S, CRSP, COMPUSTAT, SDC, and CDA/Spectrum Institutional (13f) holdings data set.

Variable	Definition
Stock characteristics	
1. Size (m$)	is the company's market capitalization of equity (measured in millions of dollars) on day -1, relative to the event day, day 0
2. B/M	is measured as the company's book value at the end of the most recent fiscal year divided by the company's market capitalization of equity on day -1, relative to the event day, day 0
3. BHAR(-86,-21)	is a proxy for momentum and is measured as the BHAR from day -86 to day -21, relative to the event day, day 0
4. Institutional ownership (m)	is the number of the company’s common shares, measured in millions, held by institutional investors at the end of the most recent fiscal quarter.
5. NASDAQ dummy	is a dummy variable indicating a stock is listed on NASDAQ
Analyst/rec characteristics	
6. Analyst experience (# of Q)	is the number of quarters since the analyst issued the first recommendation in I/B/E/S.
7. Affiliated analyst dummy	is a dummy variable indicating an analyst issues a downgrade any time during the two-year period after the file date on a debt or equity issue in which the analyst’s employer acts as the lead or co-lead underwriter.
8. Strong sell dummy	is a dummy variable indicating a stock is downgraded to a strong sell recommendation (I/B/E/S numerical score = 5)
9. Downgrade magnitude	is the difference in I/B/E/S numerical score between the current and previous recommendation
10. Below consensus dummy	is a dummy variable indicating a stock is downgraded to a recommendation that is below the consensus recommendation
11. Sanctioned firm dummy	is a dummy variable indicating a downgrade is issued by a sanctioned firm. The original ten sanctioned firms according to the Global Research Analyst Settlement are: Bear Stearns; Credit Suisse First Boston; Goldman Sachs; Lehman Brothers; J. P. Morgan; Merrill Lynch, Pierce, Fenner & Smith; Morgan Stanley; Citigroup Global Markets; UBS Warburg; and U.S. Bancorp Piper Jaffray. In August 2004, Deutsche Bank and Thomas Weisel joined the settlement, bringing the total number of the sanctioned firms to twelve.
12. Informed selling dummy	is a dummy variable indicating that the pre-announcement BHAR from day -20 to day -1 is negative
Others	
13. Post-FD dummy	is a dummy variable indicating a downgrade is issued after Regulation Fair Disclosure came into effect (= 1 after November 23, 2000)

14. Pre-GS dummy	is a dummy variable indicating a downgrade is issued before the Global Research Analyst Settlement came into effect (= 1 before October 01, 2002)