Three Essays on Disclosures and Corporate Finance

by

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Declarations

I declare that any material contained in this thesis has not been submitted for a degree to any other university. I further declare that the current versions of Chapter 2 and Chapter 3 of this thesis are co-authored with Dr. Guanming He.

Abstract

This thesis is composed of three essays on corporate disclosures in the context of empirical corporate finance. It explores managerial incentives for corporate disclosures and examines how manager disclosure decisions affect stock prices as well as how manager behavior and information asymmetry can be affected by a specific disclosure regulation.

Chapter 2 investigates whether and how financial constraints on firms affect the risk of their stock prices crashing. We hypothesize that financial constraints increase future stock price crash risk via both bad-news-hoarding and default-risk channels. The results confirm our conjectures and are robust to using a dynamic panel generalized method of moments (GMM) estimator and two quasi-natural experiments to control for potential endogeneity. Cross-sectional analyses reveal that the positive relation between financial constraints and future crash risk is more prominent for firms with high abnormal accruals or with weak corporate governance and less pronounced for firms that commit tax avoidance or have a high credit rating.

Chapter 3 and Chapter 4 focus on market consequences of corporate disclosures about derivative instruments and hedging activities. Derivatives are increasingly used by managers not only to hedge risks but also to pursue non-hedging activities for fulfilling opportunistic incentives. Using the mandatory adoption of a financial reporting standard (SFAS 161) on derivative disclosures, we examine whether and how derivative disclosures influence managerial opportunistic behavior. We employ insider trades and stock price crash risk to capture managerial opportunism. Applying a difference-in-differences design with hand-collected data on derivative designations, we find that, after the implementation of SFAS 161, derivative users that comply with SFAS 161 experience a significantly greater decrease in both insider trades and stock price crash risk, compared with a matched control sample of non-derivative-users. Our cross-sectional analyses reveal that SFAS 161 has greater impact on firms in the case of high information opacity, high financial risk, or high business risk. We find no evidence that, compared to the non-derivative-users, derivative users not compliant with SFAS 161 have a greater reduction in either insider trades or stock price crash risk in the post-SFAS 161 period, implying the importance of enhancing the enforcement of the regulation.

Given that the transparency of firms' derivative disclosure improves after SFAS 161, in Chapter 4, we examine whether the enhanced derivative disclosures reduce the information asymmetry between informed and uninformed investors. Tension exists as the derivative information may not be comprehensible to relatively uninformed investors. We find that derivative users compliant with SFAS 161 experience significantly greater reduction in stock illiquidity and probability of informed trade in the post-SFAS 161 period, and such impact is more pronounced for firms with greater investor attention.

Chapter 1

Introduction

Corporate disclosure is crucial to an efficient capital market. Managers provide information of on their firms to the financial markets in various ways through financial statements, footnotes, management forecasts, conference calls and press releases, etc. The extent to which such information about the firm is reflected in firm value or stock prices indicates the level of efficiency of a market. Therefore, capital market research into the information content of corporate disclosures continues to be an important topic that straddles the accounting and finance literatures. This thesis aims to explore how managers' disclosing decisions can affect the stock prices of the firm and how managers' behavior can be affected by a specific disclosure regulation.

In Chapter 2, we explore managerial incentives to provide firm information and the impact of managers' disclosure decisions on stock performance. Previous literature (e.g., Kothari et al., 2009) suggests that managers disclose good news and bad news of a firm in different or asymmetric ways. For example, litigation risk and potential reputational costs can motivate managers to reveal bad news in a timely manner as good news (Baginski et al., 2002) whereas career and compensation concerns can incentivize managers to delay the release of bad news (Nagar et al., 2003; Graham et al., 2005). To extend this literature, we argue that the financial constraint status of a firm provides managers with the incentives to withhold bad news in order to secure external funds. When a firm has difficulty funding its desired investments, managers tend to conceal negative firm-specific information from outsiders. If a manager keeps withholding bad news for an extended period, the stock price will be severely overvalued. When the accumulated negative information reaches to a limit, it will burst out at once, leading to a sudden, large price drop, i.e., a stock price crash. Also, we posit the higher default risk of financially constrained firms as another channel through which financial constraints increase a firm's stock price crash risk. Because external funds become too costly for financially constrained firms, such firms rely more on their limited internal funds and hence are more susceptible to a stock price crash resulting from corporate failure. We find that financial constraints increase firms' future stock price crash risk, and financial constraints remain positively associated with crash risk as far as three years ahead.

The motivation to study the risk of stock price crashes relates to its importance in determining expected stock returns, return volatility and option pricing (Bates, 2000; Conrad et al., 2013). There has been a renewed interest in studying firm tail risk and stock price crash risk since the recent financial crisis 2007-2008. This chapter links financial constraints with future stock price crash risk and examines the role of financial constraints in information management.

In Chapter 2, the bad news that managers withhold refers to any negative information related to the firm that, if released, will stop potential investors from funding the firm's projects. Chapter 3 and Chapter 4 study a specific type of corporate disclosure about firms' derivative usage and hedging activities. The reason we choose this particular disclosure include the exponential increase in use of financial derivatives over the last few decades, as well as the complex nature of derivative instruments that creates challenges for both reporting entities and users of financial statements. Also, according to Leuz and Verrecchia (2000), the disclosure environment in the U.S. is already rich, thus any increased levels of disclosure are largely incremental but not fundamental. However, the disclosure requirements on derivatives are relatively under-developed compared with most other disclosure requirements.

Existing literature provides mixed evidence on the effect of derivatives on firm value and risk (Guay, 1999; Allayannis and Weston, 2001; Adam and Fernando, 2006; Bartram et al., 2011; Gilje and Taillard, 2017). Two reasons are conjectured for the mixed findings. First, firms' objectives for using derivatives cannot be easily disentangled due to insufficient disclosures. On the one side, derivatives can be used to hedge firm risk. Effective hedging can reduce cash flow volatility (Froot et al., 1993), improve the predictability of earnings (DeMarzo and Duffie, 1995), and hence reduce financial distress costs and lower expected tax liabilities (Smith and Stulz, 1985). On the other hand, derivatives can be used for non-hedging purposes such as earnings management and speculation (Brown, 2001; Géczy et al., 2007; Chernenko and Faulkender, 2011). Second, the endogeneity involved in managers' decisions about using derivatives can result in unreliable estimates. In order to mitigate this endogeneity concern, we use a specific disclosure regulation, namely, FASB Statement No. 161 (SFAS 161) as a setting to examine the impact of enhanced derivative disclosures. SFAS 161, as issued in 2008, seeks to improve the transparency of derivative disclosures and requires greater effort by firms to distinguish between derivatives used for risk management and those used for other purposes. Considering that managers use derivatives not only to hedge risks but also to pursue non-hedging activities to fulfil their opportunistic incentives, Chapter 3 examines whether and how the enhanced derivative disclosures, as prescribed by SFAS 161, can effectively reduce managerial opportunism.

Before SFAS 161, information about how and why a firm uses derivatives was not disclosed clearly to the outsiders. Following SFAS 161, the enhanced disclosures required as to the purposes of, and strategies in, using derivatives are expected to reduce the information asymmetry between managers and outside investors after SFAS 161. Investors should be able to better assess the impact of derivative use on stock prices, reducing the probability that managers make use of investors' uncertainty about stock performance to behave opportunistically. In addition, more transparent derivative disclosures can discipline managers and encourage more active risk management via hedging. Using insider trades and stock price crash risk as two proxies for managerial opportunism and a hand-collected sample, we find that firms using derivatives and complying with SFAS 161 are less likely to pursue insider trading or encounter a stock price crash.

In summary, Chapter 3 shows that insider trading and managerial bad news hoarding behavior can be affected by firms' derivative disclosures. In this way, it echoes Kanodia and Sapra (2016)'s call for future research on the real economic consequences of specific accounting standards. Despite SFAS 161 applying to all entities, we find that not all firms using derivatives make real changes in their disclosures in response to the requirements of this new standard and hence we expect no improvement in non-compliers' derivative disclosures. Consistent with our expectation, results show that managerial opportunism is only reduced in derivative-using firms that comply with SFAS 161. Chapter 3 is the first to consider the compliance issue in empirical analysis of firms' derivative disclosures, making another contribution to the literature.

Given the reduced information asymmetry between insiders and outsiders after SFAS 161 documented in Chapter 3, Chapter 4 takes a step further to investigate how the enhanced derivative disclosures impact information asymmetry and how it varies among different investors. While Chapter 3 focuses on the real effects of mandatory disclosures on firm management, Chapter 4 examines the effects of the enhanced derivative disclosures on investors. In Chapter 4, tension exists as any enhanced derivative information may not be comprehensible to relatively unsophisticated or uninformed investors. As one of the most complex financial contracts in the markets, derivatives create challenges for both financial statement users and reporting firms. Before SFAS 161, being only subject to SFAS 133, firms did not provide adequate and consistent disclosures on their use of derivatives, leaving it "*next to impossible*" (Kawaller, 2004, pp.29) for stakeholders to assess the effectiveness of firms' hedging activities and its impact on firm value (Chang et al., 2016). Related research (Campbell et al., 2018) finds that the mispricing of derivative-using firms by investors has disappeared after the implementation of SFAS 161. We further examine whether the information asymmetry between informed and uninformed investors decreases, increases or remains unchanged after SFAS 161, which crucially depends on differential abilities of investors to interpret the derivative information.

Unlike insider trades and stock price crash risk used in Chapter 3 to gauge two different forms of managerial opportunism, the stock liquidity and probability of informed trades (PIN) variables, used in Chapter 4, both measure information asymmetry. In Chapter 4, we find that for firms that comply with SFAS 161, the enhanced derivative disclosures reduce the information asymmetry between informed and uninformed investors in terms of increased stock liquidity and lower probability of informed trades. Previously less uninformed investors become more confident to trade on firms using derivatives. We also find that this effect is more pronounced for firms with greater investor attention because investors are more likely to be aware of the improved derivative information provided by such firms.

Chapter 5 summarizes the main findings and contributions of the thesis and discusses the limitations and implications for future research.

Chapter 2

Are Financially Constrained Firms Susceptible to a Stock Price Crash?

2.1 Introduction

Financial crises and corporate scandals such as those involving Enron, Worldcom, and Fannie Mae have triggered increased academic research into the probability of stock price crashes, which are normally observed in the far-left tail of firm-specific return distributions (e.g., Chen et al., 2001; Jin and Myers, 2006; Hutton et al., 2009). The motivation to study the risk of extreme negative residual returns lies in its importance in determining expected stock returns (Conrad et al., 2013), return volatility, and option pricing (Merton, 1976). The objective of our study is to examine whether and how firm financial constraints affect future stock price crash risk. As with Lamont et al. (2001), we define financial constraints as frictions that prevent firms from funding their desired investments. Previous studies (e.g., Fazzari et al., 1988; Lamont et al., 2001; Livdan et al., 2009; Denis and Sibilkov, 2010) have examined the association of financial constraints with capital investments, firm value, risk, and expected returns, but none has evaluated the stock price crash risk of financially constrained firms. We seek to fill this gap in the literature. Given that stock price crashes have material impacts on investor welfare, our study on financially constrained firms' crash risk should be of interest to investors making portfolio investment decisions, and relevant to creditors, suppliers, customers, and other stakeholders concerned about corporate creditworthiness and viability.

Difficulties in raising external funds induce managers in financially constrained firms to withhold bad news. The accumulated bad news and resultant inflation of stock prices increase the likelihood of future stock price crashes. Moreover, financially constrained firms are subject to a higher probability of corporate failure and are more likely to experience stock price crashes at the point of default. However, if investors can decipher these implications and discount the financially constrained stocks promptly, stock prices will be likely to decline timely over time without triggering a crash, thereby lowering future stock price crash risk. Therefore, the relation between financial constraints and future stock price crash risk remains ambiguous, which constitutes another motivation for our study.

We posit that bad news hoarding and default risk are two mechanisms that make financially constrained firms susceptible to stock price crashes.[1] First, the literature (e.g., Chen et al. 2001; Jin and Myers, 2006; Hutton et al., 2009; Kim et al., 2011a, b; Andreou et al., 2017) regards withholding bad news as a fundamental cause of stock price crashes. Because bad news might increase the costs of issuing equity and debt, managers in financially troubled firms are particularly prone to hide bad news for an extended period to secure external funds. However, though the amount of bad news that managers are able to hide is limited (Jin and Myers, 2006), managers often cannot *anticipate*, and thus *control*, when such a limit is reached (He, 2015), given constant and unforeseeable changes in the business environments. Once that limit is reached, all the bad news will become uncontainable, resulting in a sudden, dramatic price drop, that is, a stock price crash. In essence, with strong incentives to secure external finance, firms in financial constraints are more likely to withhold bad news and thus have higher future crash risk, compared with unconstrained firms.

Second, financially constrained firms need more cash to cover necessary investments and avoid default. However, because external financing is often too expensive for such firms, they have to rely on limited internal funds and hence are more susceptible to default and a stock price crash resulting from corporate failure. Therefore, it follows that financially constrained firms have a high risk of stock price crashes. Furthermore, firms facing financial constraints have an incentive to forego positive net-present-value projects; such underinvestment and debt overhang problem would further exacerbate their potential default risk and associated crash risk.

A counter-argument plausibly holds when taking into consideration the investor's ability to decipher the implications of financial constraints for future crash risk. Financial constraint, by definition, is determined by whether and to what extent the funds available/accessible for a firm exceed the funds needed by the firm for its desired investments. Therefore, to appreciate a firm's financial constraint, investors

[1] We refer to default risk as the probability of default, financial distress, economic distress, or bankruptcy, which are often used interchangeably in the literature (Campbell et al., 2008).

need to gauge the firm's ability to acquire external finance, the amount of internal funds available, and the amount of funds required for its desired investments (Dechow et al., 1996); this is a difficult, challenging task for investors, who generally do not have access to private information. Even if investors detect financial constraints, they might not be able to infer the implications of financial constraints for future crash risk, because the amount of hidden bad news and the probability of default can hardly be appraised by outsiders (Dye, 1985; Jung and Kwon,1988; Dichev, 1998; Griffin and Lemmon, 2002; Campbell et al., 2008). Therefore, we refute the view that investors tend to promptly discount financially constrained stocks in a way that makes future stock price crash risk lower. We expect the association between financial constraints and future crash risk to be positive.

We also explore financially constrained firms' crash risk in more detail. Earnings management can facilitate bad news hoarding behavior (Hutton et al., 2009; Zhu, 2016). Zhu (2016) argues that managers seeking to withhold bad news are inclined to make aggressive income-increasing accruals estimates; these make it more difficult for outside investors to discern any related hidden bad news, providing managers with stronger incentives to manage accruals upwards to conceal bad news. Hence, we expect that earnings management, as a powerful tool to disguise bad news, strengthens the positive relation between financial constraints and future crash risk.

In the presence of agency conflict between shareholders and management, managers tend to withhold bad news associated with rent extraction or with adverse firm performance. Strong corporate governance reduces such agency conflict, curbs opportunistic bad-news-hoarding behavior, and thereby reduces stock price crash risk. Hence, we expect that the positive association between financial constraint and future crash risk is stronger for firms with weak corporate governance.

When firms face financial constraints, equity and debt financing becomes more costly and less accessible (Edward et al., 2016), and consequently, the firms become more reliant on internal funds to meet their investment needs. To make more internal funds available, managers may resort to corporate tax avoidance. The cash savings attributed to tax avoidance help lower the default risk of a financially constrained firm and thereby decrease its future crash risk. Accordingly, we expect that the relationship between financial constraints and future crash risk is weaker for firms that avoid income taxes aggressively. Although some tax avoidance transactions might obfuscate financial reporting and facilitate bad news hoarding (Desai and Dharmapala, 2006, 2009; Kim et al., 2011a), tax avoidance itself is used by a financially constrained firm as a tool primarily to generate cash flow and mitigate default risk, rather than to conceal bad news. Consistent with this notion, Edward et al. (2016) and Law and Mills (2015) predict and find that financial constraints have a positive impact on cash tax savings. Our prediction is in line with Edward et al. (2016) and Law and Mills (2015).

Credit rating measures a firm's default probability. A high credit rating implies a greater distance to default, facilitating external financing for a financially constrained firm. In contrast, a low credit rating limits a financially constrained firm's ability to raise external funds for investments and repayments of debt; as a result, default risk will be heightened, and crash risk will increase. Therefore, we predict that the association between financial constraints and future crash risk is more pronounced when firms have lower credit ratings.

As with previous studies (e.g., Chen et al., 2001; Jin and Myers, 2006; Hutton et al., 2009), we focus solely on firm-level stock price crashes; crash risk that is attributed to market-wide factors is not within the scope of our study. Following Hutton et al. (2009), we measure crash risk based on the likelihood of extreme negative firm-specific weekly stock returns for a fiscal year. As robustness checks, we use four other proxies for crash risk as well: (i) the number of crash weeks with negative extreme weekly returns, (ii) the negative skewness of firm-specific weekly stock returns, (iii) the "down-to-up" volatility of firm-specific weekly returns, and (iv) the minimum value of firm-specific weekly returns, as per prior research (e.g., Chen et al., 2001; Hutton et al., 2009; Kim et al., 2011a, b; Callen and Fang, 2013, 2015; Andreou et al., 2017; Chang et al., 2017; Lobo et al., 2017). We measure financial constraints by the SA index developed by Hadlock and Pierce (2010). Using a sample of 28,331 firm-year observations from U.S. listed firms for the period of 1995-2016, we find that financial constraints are positively associated with one-year-ahead stock price crash risk. This association is both economically and statistically significant, suggesting that investors are incapable of appreciating the prospects of financial-constraint firms. In the cross-sectional analyses, we find that this positive relationship is more pronounced for firms with high abnormal accruals or weak corporate governance and is attenuated when firms commit tax avoidance or have high credit ratings.

The past or current crash risk may affect firm financial constraints and thereby influence future crash risk; this engenders a dynamic type of endogeneity. To remediate this concern, we follow Wintoki et al. (2012) to conduct a dynamic panel GMM analysis, in which two lags of crash risk are included in the dynamic model, and all the independent variables lagged three and four periods are employed as instruments. Our GMM results suggest that our evidence of a positive association between financial constraints and future crash risk is immune from the dynamic endogeneity bias.

There are two other sources of endogeneity. One is potential measurement errors in our financial-constraint proxy, and the other is correlated omitted variables, either of which might bias our results and inferences. Such endogeneity is addressed in two quasi-natural experimental settings. First, following Kim (2018), we use the collapse of the junk bond market in 1989 as an exogenous shock to firm financial constraints and conduct a difference-in-differences (DiD) regression analysis. Regulations were enforced to restrict the supply of credit to speculative-grade firms, thereby considerably tightening up the financial constraints of those companies (Lemmon and Roberts, 2010). Accordingly, we define the treatment firms as those that receive a speculative grade from the S&P credit rating agency, and the control firms as those without an S&P credit rating.[2] Our DiD estimator amounts to 0.7587 and is statistically significant at the 5% level, suggesting that an increase in crash risk for the treatment firms, which are subject to tightened financial constraints during the post-collapse period, is significantly higher than that for the unrated control firms, of which the financial constraint statuses are much less affected by the junk-bond-market collapse. This result elicits a causal inference that financial constraints lead to higher future crash risk.

The second quasi-experimental setting involves the Internet bubble, which exogenously relaxed financial constraints for non-technology (henceforth, non-tech) firms (Campello and Graham, 2013). With the rapidly increasing use of the Internet for commerce in the 1990s, the technology (hereafters, tech) profession thrived; tech firms rose up, with their stock prices increasingly overvalued by the market. This overvaluation had significant spillover effects on the non-tech stocks, making their

[2] In this study, we use the S&P's long-term domestic issuer credit ratings to classify firms into investment-grade versus speculative-grade firms.

prices generally inflated as well (Caballero et al., 2006; Anderson et al., 2010). The market optimism and excess supply of capital in the U.S. stock market gave rise to a stock price bubble, which started in 1995 and persisted until 2000. A firm's financial constraint status hinges critically on the supply of funds to the firm vis-à-vis its demand for funds, the latter of which is determined by firm investment needs. Conditional on the investment needs being unaffected by the bubble, such a bubble would exogenously decrease the financial constraint, if any, of a firm, because the firm can ease the financial constraint by raising more funds from equity issuances in the bubble period. Whereas tech firms have significantly increasing investment opportunities during the bubble, non-tech firms do not (Jorgenson and Stiroh, 1999; Gordon, 2000; Stiroh, 2002) and hence are well suited for use in our natural-experimental setting. Consistent with Campello and Graham (2013), non-tech firms that are (are not) in financial constraints during the pre-bubble period are used as our treatment (control) firms. We implement a coarsened-exact-matching approach, per Iacus et al. (2012), to match the treatment firms with the control firms based on the determinants of financial constraints. Using a difference-in-differences design, we find that non-tech firms that face financial constraints in the pre-bubble period experience significantly larger decreases in crash risk, as a result of the ease of financial constraints, during the bubble period, compared with the control firms. This again corroborates the causal, positive relationship between financial constraints and future crash risk. In the last test, we examine the association between financial constraints and longer-term future crash risk. Our results show that financial constraints remain positively correlated with future crash risk on the two-year and three-year horizons, respectively.

Our paper makes three main contributions to the literature. First, the prior research largely focuses on the impact of financial constraints on firm performance, cost of capital, corporate policies, and real business activities (e.g., Fazzari et al., 1988; Lamont et al., 2001; Gomes et al., 2006; Livdan et al., 2009; Campello et al., 2010; Denis and Sibilkov, 2010; Li, 2011; Campbell et al., 2012); evidence on the association between financial constraints and stock returns is mixed (Lamont et al., 2001; Whited and Wu, 2006; Livdan et al., 2009). Our study investigates the impact of financial constraints from a different angel by examining the role of financial constraints in information management and focusing on the extreme future returns of financially constrained firms. We contribute to the literature by employing more rigorous identification strategies; the quasi-experimental designs allow us to establish a causal effect of financial constraints on future stock price crash risk. We also extend the literature on managerial incentives by showing that securing external finance to alleviate financial constraints forms managerial incentives for hiding negative firm-specific information.

Second, there are three key drivers of firm-specific stock price crash risk: (i) managerial bad news hoarding; (ii) firms' fundamental risk profiles, which generate unexpected, egregious bad news impossible for managers to withhold once it occurs; (iii) market frictions that hinder investors' abilities to discern the bad news hoarding or a high risk of the egregious bad news. The vast literature on crash risk (e.g., Kim et al., 2011a, b; He, 2015; Kim et al., 2016; Andreou et al., 2017; Chang et al., 2017; Hong et al., 2017) focuses predominantly on the first driver of crash risk. Our study complements this literature by shedding light on the other two drivers as well; specifically, we offer insight that financially constrained firms' high crash risk is also attributable to their high risk of corporate failure, and that investors are unlikely to infer the implications of financial constraints for future crash risk.

Third, in developing our hypothesis, our study adds to the crash risk literature by providing insights into the tension between the benefits and costs associated with bad news hoarding. By showing that financially constraints are positively associated with future stock price crash risk, we demonstrate that firms tend to withhold bad news when facing with financial constraints since the benefits of accessing external financing are likely to be greater than the costs of potential litigation and reputational risk.

The remainder of this chapter is structured as follows. Section 2.2 reviews the related literature and develops our hypotheses. Section 2.3 describes our sample, measurements of key variables, and research design. Section 2.4 presents our empirical results. Section 2.5 conducts the additional tests, and Section 2.6 concludes.

2.2 Literature review and hypothesis development

2.2.1 The association between financial constraints and future stock price crash

risk

Bad news hoarding

Prior research has proposed a number of explanations for firm-level stock price crashes, among which managerial bad news hoarding is considered as a fundamental cause of stock price crashes (e.g., Jin and Myers, 2006; Bleck and Liu, 2007; Hutton et al., 2009; Benmelech et al., 2010; Kim et al., 2011a, b; Kim and Zhang, 2014, 2016; Chang et al., 2017; Hong et al., 2017). Withholding one piece of bad news entails a low risk of detection by outsiders, because it is difficult for them to discern whether managers are withholding the bad news or unaware of it. However, as withheld bad news accumulates, it would become increasingly hard for insiders to continually hoard it. The occurrence of a stock price crash is attributed to a sudden overrun of a bad-news-hoarding limit, a threshold point at which managers can no longer withhold any unfavorable information. At that point, all the hidden news would come out at once, resulting in a sudden stock price plunge. The maximum amount of bad news that managers can withhold varies unforeseeably and constantly with a firm's changing environments, making it difficult for managers to *anticipate* by themselves when the threshold point will be reached and to prevent a stock price crash from occurring (He, 2015). As such, the incidence of a stock price crash depends on how much bad news managers withhold. The extent to which managers camouflage their firm's unfavorable information, the higher the future crash risk. Given the limited amount of internal funds available for investments, financially constrained firms need more external funds. To facilitate external financing, they are more likely to withhold bad news and have a high risk of future stock price crashes.

Default risk

The potentially high default risk of financially constrained firms provides yet another explanation for their high future crash risk. Default risk (or distress risk) refers to the probability that a firm fails to meet its financial obligations (Vassalou and Xing, 2004; Campbell et al., 2008; Garlappi et al., 2008) and is conceptually different from financial constraints. Kaplan and Zingales (2000, *p*710) argue that financially constrained firms share similar characteristics as financially distressed firms and note that "*financial distress is a form of being financially constrained*". This implies that financial constraint is an important aspect in determining a firm's default risk but not necessarily vice versa.

Fazzari et al. (1988), Almeida et al. (2004), and Acharya et al. (2007) document that the investment spending by financially constrained firms is more sensitive to cash flow than that by unconstrained firms; this is primarily because constrained firms are subject to restrictions in accessing external finance. Whereas cash adequacy helps financially healthy firms avoid default, cash shortages that often beset financial-constraint firms are likely to induce their corporate default (Davydenko, 2012). Thus, a financially constrained firm is more likely to default than an unconstrained firm. Consistent with this notion, the survey research of Campello et al. (2010) suggests that a firm's inability to fund investments, which manifests itself in high financial constraints, would lead to higher distress risk. Because firms with high default risk are more likely to fail and experience crashes at the point of default (Zhu, 2016), it follows that financially constrained firms are more prone to stock price crashes.

Furthermore, to avoid, or delay the realization of, a default, financially constrained firms have incentives to bypass some positive net-present-value projects; this gives rise to the debt overhang problem (Smith and Warner, 1979), aggravating future default risk and associated crash risk.

Financial constraints and lower crash risk

As discussed in the previous sections, both the bad-news-hoarding and default-risk mechanisms predict that financial constraints are positively associated with future crash risk. This section further considers conditions under which financial constraints might lower future crash risk.

First, managers may decide not to withhold bad news so that stocks are less likely to be overpriced and crash in the future. Tension exists as managers' decisions to withhold bad news depend on their trade-offs between the benefits of accessing external financing and the costs associated with threat of litigation and reputational risk. Prior studies suggest that early revelation of bad news reduces the likelihood of being sued and the expected costs of litigation (Skinner, 1994; Skinner, 1997; Field et al., 2005; Donelson et al., 2012). Managers may choose not to hide bad news considering the high litigation risk and reputational risk in the long run. However, without access to private information, managerial bad news hoarding behavior is unlikely to be detected by outsiders. Therefore, we posit that managers in financially constrained firms are inclined to withhold bad news as the associated detection risk is relatively low.

Second, if investors are able to discover financial constraint and infer its implications for bad news hoarding and default probability, then financially constrained stocks will be discounted by investors promptly, such that the stock price will not be inflated in a way that likely plunge significantly at a particular point in time. However, these conditions are unlikely to hold. There is no prior evidence showing that investors are able to observe the financial-constraint status of a firm. Farre-Mensa and Ljungqvist (2016) note that financial constraints are not directly observable. To measure financial constraints, the previous literature has to rely on indirect proxies or indices based on firm characteristics. Conceptually, a firm's financial constraints depend on whether its available funds can meet its demand for desired investments (Povel and Raith, 2002). Investors might be able to assess the adequacy of a firm's internal funds based on its cash flow statement. However, in the absence of access to private corporate information, it is difficult for investors to appraise the firm's investment opportunities as well as the amount of funds needed for the investments. As such, it is most unlikely that investors can fully evaluate a firm's financial constraint status.

Even if investors are able to observe financial constraint, it is still difficult for them to decipher its implications for associated risk and future payoffs. If investors can perceive financial constraint and infer its association with heightened risk, they will require a higher risk premium, i.e., a higher return from such a stock to compensate for the higher risk they bear. In such a case, we should observe a positive association between financial constraints and equity returns. However, empirical evidence (e.g., Lamont et al., 2001; Whited and Wu, 2006; Livdan et al., 2009) shows that financially constrained stocks do not earn significantly higher returns than unconstrained stocks, suggesting that investors are incapable of evaluating the valuation impact of financial constraints. Furthermore, Lamont et al. (2001) find that financially constrained firms earn even lower average returns than unconstrained firms, which implies the mispricing of financially constrained stocks and the irrationality of market participants. If, as evidenced by Lamont et al. (2001), financially constrained stocks are overpriced for the current period, their future crash risk should be higher.

Even if the market were efficient in pricing constrained stocks based on public information, it might not follow that market participants can decipher the implications of financial constraints for future crash risk because the extent of crash risk hinges critically on the amount of bad news hoarded by managers. Without access to private information, it is unlikely that outside investors will be able to appraise the amount of hidden bad news and adjust stock prices for the bad news hoarding (e.g., Dye, 1985; Jung and Kwon, 1988). When the bad news remains withheld and stockpiles within financially constrained firms, their future crash risk will be higher.

From the perspective of the default risk mechanism, investors are probably able to link financial constraint with higher distress risk, but they are possibly not able to extrapolate future stock price crash risk from current default risk. Prior evidence (Dichev, 1998; Campbell et al., 2008; George and Hwang, 2010) shows a negative relation between default risk and stock returns, suggesting that investors are not capable of evaluating the potential default probability of a firm and fail to demand a sufficient premium to compensate for their exposure to default risk. Based on the above discussion, we refute the possibility that investors can infer the implications of financial constraints for future crash risk. We posit that financially constrained firms are more likely to encounter future stock price crashes. Therefore, our first hypothesis is as follows:

H1: *Financial constraints and future stock price crash risk are positively associated.*

2.2.2 Cross-sectional analyses of the association between financial constraints and future crash risk

Earnings management

Under an accrual accounting system, a firm's performance is based on earnings, which comprise accruals and cash flow. Firm management is responsible for giving shareholders earnings estimates, and the subjectivity of these estimates provides managers with a tool to hide bad news. Prior studies (Hutton et al., 2009; Zhu, 2016) find evidence that earnings management is associated with a larger extent of bad news hoarding and with higher future crash risk, which supports the notion that managers tend to make aggressive accruals estimates to withhold bad news.

One type of accruals that managers can use to disguise bad news is working capital accruals, which involve balance sheet items such as inventory, accounts receivable, accounts payable, and provisions for contingent liabilities. For example, by understating the *provision for bad debt* or *allowance for doubtful accounts*, managers can withhold customer-related bad news, which arises from deteriorating financial health of customers or from worsening customer relationship. Other bad-news-hoarding strategies include understating the *provisions for contingent liabilities*, such as an obligation to clean up polluted production sites or to provide warranty coverage for low-quality products sold, both of which would lead to a future outflow of cash for a firm. Appendix 2.7.2 shows more examples of managers using accruals to withhold bad news. In essence, aggressive recognition of accruals makes it difficult for outside investors to discern related corporate bad news. Earnings management thereby serves as a device for managers to conceal bad news.

In addition, financial opacity resulting from accruals inflation hampers shareholders from discriminating good projects from bad ones at an early stage. As a result, shareholders cannot abandon bad projects in a timely manner, thereby leading to potentially higher crash risk (Bleck and Liu, 2007). Based on the above discussion, we expect that earnings management will aggravate the future crash risk of a financially constrained firm. Accordingly, we establish the second hypothesis as follows:

H2: *The positive association between financial constraints and future stock price crash risk is more pronounced for firms that have high abnormal accruals.*

Corporate governance

Bad news is more likely to arise when there is an agency conflict between shareholders and firm management. Such bad news might be attributed to managerial rent extraction or other managers' self-interested behaviors. Concerns about job prospects, personal reputation, the value of option grants, and bonus plans (Graham et al., 2005;

Kothari et al., 2009; Jiang et al., 2013; Baginski et al., 2018) give managers an incentive to withhold the bad news. Strong corporate governance puts management under intense monitoring (Ashbaugh-Skaife et al., 2006) and reduces its ability to hoard bad news (Ajinkya et al., 2005; Karamanou and Vafeas, 2005; Andreou et al., 2016), thereby mitigating future crash risk (Kim et al., 2011a, b; Callen and Fang, 2013; Andreou et al., 2016; Chang et al., 2017). On this basis, we expect that managers in a well-governed, financially constrained firm are less likely to withhold bad news, and hence, their firm's future crash risk tends to be lower. This leads to our third hypothesis stated in an alternative form as follows:

H3: *The positive association between financial constraints and future stock price crash risk is weaker for firms with strong corporate governance.*

Corporate tax avoidance

In an imperfect capital market, external finance is not a perfect substitute for internal capital and is particularly costly and difficult for financially constrained firms to access. Firms that face high costs of external financing have to rely more on their own cash holdings (Fazzari et al., 1988; Almeida et al., 2004; Acharya et al., 2007; Denis and Sibilkov, 2010). However, current cash holdings often do not meet financially constrained firms' demand for investments. In such a case, the firms might resort to tax avoidance to generate additional internal funds. Edwards et al. (2016) and Law and Mills (2015) find that an increase in financial constraints incentivizes firms to increase tax avoidance activities to obtain cash tax savings. They argue that reducing tax payments has less adverse impact on firm operations than other cost-cutting strategies that are aimed at building cash reserves.

Some complex tax-avoidance transactions might obfuscate financial reporting, facilitating managers' bad news hoarding and resource diversion (Kim et al., 2011a, b; He et al., 2019). Nonetheless, the main intent of a financially constrained firm avoiding taxes is to obtain additional internal funds and mitigate default risk. When facing financial constraints, firms need to seek alternative funds for investments, since traditional sources of financing (i.e., debt and equity financing) become more costly and less accessible. Edward et al. (2016) and Law and Mills (2015) argue that cash tax savings achieved via tax avoidance is a potential source of financing and that managers can implement various tax planning strategies to reduce tax payments. In this sense, tax avoidance increases internal funds for a financially constrained firm, enhances its ability to fulfill financial obligations and to resist potential default, and thereby reduces its future crash risk. Therefore, we have our fourth hypothesis:

H4: *The positive association between financial constraints and future stock price crash risk is less pronounced for firms that commit tax avoidance.*

Credit rating

A firm's credit rating reflects a credit rating agency's opinion about the firm's creditworthiness and its ability to meet financial obligations (Standard & Poor's, 2009). A low credit rating implies a shorter distance to default. Therefore, financially constrained firms with low credit ratings should be more likely to default and to encounter stock price crashes. Moreover, low-credit-rating firms often find it difficult and costly to access external funds (Kisgen, 2006; Manso, 2013). As a result, they tend to face high risk of default and of stock price crashes. Thus, we have the fifth hypothesis.

H5: *The positive association between financial constraints and future stock price crash risk is stronger for firms with low credit ratings.*

2.3 Data and research design

2.3.1 Data sources and sample selection

We obtain data primarily from four sources, the Center for Research in Security Prices (CRSP), Compustat, Factset, and Institutional Shareholder Services (ISS). The crash risk variables are constructed using stock returns data from the CRSP database. Firms' financial and stock information is collected from the merged Compustat/CRSP database. The institutional ownership data are taken from Factset. Given that our crash risk measure is one-year lagged by the financial-constraint index and control variables in our regressions, the sample period for our crash risk variables (financial constraint variable) ranges from 1996 (1995) to 2016 (2015). We require that firms have necessary data available for constructing the variables of interest for our empirical analyses. In dealing with potential outliers, we winsorize the variables for book-to-market ratios and book-tax differences at the top and bottom 1% levels, respectively. Our final sample comprises 28,331 firm-year observations corresponding with 6,557 unique firms. Table 2.1 shows descriptive statistics of all the key variables used in our main multivariate tests. Our corporate governance variables are constructed using data mainly from the ISS database, where the data are available only for the period of 2007-2015. The summary statistics of all the corporate governance measures used in our cross-sectional analysis are shown in Panel A of Table 2.5. We report in Table 2.2 the Spearman correlations among the independent variables used in our baseline regression. We also conduct the variance inflation factors (VIF) test; the results, not tabulated for simplicity, reveal that VIF values are less than 5 for all the explanatory variables, indicating that multicollinearity is unlikely to be an issue with our regression analysis.

2.3.2 Crash risk measures

In line with prior literature (Chen et al., 2001; Hutton et al., 2009; Kim et al., 2011a, b; Callen and Fang, 2013, 2015; Kim and Zhang, 2016; Andreou et al., 2017; Chang et al., 2017; Lobo et al., 2017), we employ five measures of firm-specific stock price crash risk: (i) the likelihood of negative extreme firm-specific weekly returns over a fiscal year (*crashrisk*); (ii) the number of crash weeks with negative extreme firm-specific weekly returns (*ncrash*); (iii) the negative of the third-moment of firm-specific weekly returns (*ncskew*); (iv) the down-to-up volatility of firm-specific weekly returns (*duvol*); and (v) the negative of the minimum weekly returns over a fiscal year (*minreturn*). The weekly stock returns are all adjusted for market-wide factors.

As per Hutton et al. (2009) and Kim et al. (2011a, b), a stock price crash is defined as a situation in which a firm experiences a firm-specific weekly return falling 3.2 standard deviations below the mean firm-specific weekly return for a fiscal year. *crashrisk* equals 1 if a firm experiences one or more stock price crashes in a fiscal year and 0 otherwise. *ncrash* is equal to the number of crash weeks, in which a firm experiences a negative extreme weekly return, over a fiscal year. *ncskew* is defined as the third-moment of firm-specific weekly returns for a stock and is expressed as follows:

$$ncskew_{it} = -\left(n(n-1)^{\frac{3}{2}}\sum R_{it}^{3}\right) \Big/ \left((n-1)(n-2)\left(\sum R_{it}^{2}\right)^{\frac{3}{2}}\right) \tag{2.1}$$

duvol is calculated based on the standard deviation of "down"-week firm-specific weekly returns relative to the standard deviation of "up"-week firm-specific weekly returns and is expressed as follows:

$$duvol_{it} = \left(n_u - 1\right)\sum_{DOWN} R_{it}^{2} \Big/ \left(\left(n_d - 1\right)\sum_{UP} R_{it}^{2}\right) \tag{2.2}$$

where the standard deviation of "down" ("up")-week firm-specific weekly returns is scaled by the number of "down" ("up") weeks ($n_d(n_u)$) minus one. A "down" ("up") week is defined as a week in which firm-specific weekly stock return is below (above) the mean weekly return for a fiscal year. The last crash risk variable, *minreturn*, is computed as -1 times the minimum value of firm-specific weekly returns, less the mean firm-specific weekly return, and divided by the standard deviation of firm-specific weekly returns, for a fiscal year.

Our empirical analysis is based mainly on the *crashrisk* variable, which is consistent with Hutton et al. (2009); the four other crash risk variables (i.e., *ncrash*, *ncskew*, *duvol*, *minreturn*) are used for robustness checks.[3] 15.70% of our sample observations (corresponding with 4,447 firm-years) experience one crash (*ncrash*=1), 5.82% (corresponding with 1,649 firm-years) have two crashes (*ncrash*=2), and 1.96% (corresponding with 556 firm-years) undergo more than two crashes. These statistics are close to those reported by Hutton et al. (2009). As reported in Table 2.1, the mean of *crashrisk* in our sample is 0.2348, indicating that the firm-specific stock price crash

[3] *ncskew*, *duvol*, and *minreturn* might be less powerful in measuring a stock price crash. Suppose that stock price decreases slowly to a considerably low level in response to a firm's gradual releases of bad news and then is maintained continually low for an extended period. In this case, the stock price decline features large negative skewness (*ncskew*), high down-to-up return volatility (*duvol*), and extreme low returns (*minreturn*) but should not be regarded as a stock price crash. *ncrash* does not proportionally reflect the distinction in crash risk across different levels. For instance, the differential in crash risk, as indicated by the difference between *ncrash*=1 and *ncrash*=2, is far smaller than the differential in crash risk, as indicated by the difference between *ncrash*=0 and *ncrash*=1. Moreover, conceptually speaking, the *ncrash* variable measures more of the frequency, rather than the incidence, of stock price crashes, and hence is a relatively weak measure of crash risk.

risk is, on average, 23.48% for a fiscal year. This is in line with the figures reported in prior research (e.g., Hutton et al., 2009; Kim et al., 2011a, b).

2.3.3 Financial constraint index

The SA index constructed by Hadlock and Pierce (2010) is used as our primary measure of financial constraints and is defined as follows:

$$SA = -0.737 \times size + 0.043 \times size^2 - 0.040 \times age \quad (2.3)$$

where *size* is the natural logarithm of the book value of total assets, and *age* is the number of years since the year of a firm's incorporation or founding. More financially constrained firms have higher SA indices (*SA*). Hadlock and Pierce (2010) hand collected qualitative information that is closely related to firm financial constraints, categorize firms' financial constraint statuses based on the qualitative information, and estimate the ordered logit regressions of the financial-constraint category on the determinants of two commonly used financial-constraint measures (namely, Kaplan and Zingales' (1997) (KZ) index, Whited and Wu's (2006) (WW) index), respectively. Their ordered logit regression results show that only two out of five determinants of the KZ index and three out of six determinants of the WW index have significant coefficients with predicted signs; this casts doubt on the validity of using the KZ and WW indices as proxies for financial constraints. In developing a more valid measure of financial constraint, Hadlock and Pierce (2010) sort firms by firm characteristics that are arguably associated with financial constraints and test the association between the sorting variables and the aforementioned financial-constraint category. They find evidence that only firm size and firm age are powerful in predicting a firm's financial constraint status. They further argue that firm size and firm age are relatively exogenous to a firm's financial choices compared to other firm characteristics and therein use these two variables to construct a new financial-constraint measure, that is, the SA index. Although the SA index is arguably more advantageous than the KZ index and WW index in measuring financial constraints, the SA index might still be subject to measurement errors, thereby inducing an endogeneity problem to our multivariate analysis. We address this concern in Section 2.5 by conducting two natural experiments in which the collapse of the junk bond market in 1989 and the Internet bubble in the late 1990s, respectively, are used as exogenous shocks to firms' financial-constraint statuses.

2.3.4 Test of H1

We estimate the following pooled logit regression model to test H1:

$$\begin{aligned} Crashrisk_{i,t+1} = \alpha_0 + \alpha_1 SA_{i,t} + \sum_k \alpha_k Controls_{i,t}^k \\ + Industry\text{-}fixed\text{-}effects + Year\text{-}fixed\text{-}effects + \varepsilon_{i,t} \end{aligned} \tag{2.4}$$

crashrisk and *SA* are defined as previously. If H1 holds, the coefficient on *SA* should be positive and statistically significant at conventional levels. Following prior literature (e.g., Chen et al., 2001; Jin and Myers, 2006; Hutton et al., 2009; Kim et al., 2011a, b; Callen and Fang, 2013), we include a broad set of control variables to ensure that our results are not driven by correlated omitted variables.

We control for firm size (*size*) because Chen et al. (2001) and Hutton et al. (2009) show that stock price crashes are more likely to occur among large firms.[4] We control for the book-to-market ratio (*btm*), a proxy for a firm's growth opportunities, since Harvey and Siddique (2000) and Chen et al. (2001) find that growth firms are more prone to future stock price crashes. As per Kim et al. (2011a), we include return on assets (*roa*) to control for the effect of firm performance on crash risk. Previous studies (Chen et al., 2001; Callen and Fang, 2013, 2015) document that analyst coverage might pressure managers into meeting and beating analyst forecasts, thereby exacerbating managerial myopia and increasing stock price crash risk. Hence, we control for analyst coverage (*lanacov*) and expect it to be positively correlated with future crash risk. Callen and Fang (2013) find supportive evidence that high institutional ownership curbs bad news hoarding and reduces future crash risk.

[4] Our main results are qualitatively the same if we include firm age (*firmage*), in lieu of firm size (*size*), as a control variable in the regressions.

Therefore, we also include institutional stock holdings (*insti*) as a control for crash risk.

Hutton et al. (2009) find that firms with high financial opacity are more likely to experience future stock price crashes. Therefore, we control for financial opacity (*opacity*) and predict that it is positively correlated with future crash risk. Chen et al. (2001) find that highly volatile stocks are more likely to crash in their stock prices; hence, we include return volatility (*stdret*) in the regression.[5] High trading volume is associated with high stock liquidity and hence with a higher likelihood of stock price crashes (Chang et al., 2017). Thus, we control for trading volume (*tradevol*) and predict a positive association with future crash risk. Prior literature (Chen et al., 2001; Jin and Myers, 2006; Kim and Zhang, 2014) finds that firms with high negative skewness in their weekly stock return distributions are more likely to have stock price crashes in the future periods. Therefore, we control for the negative weekly return skewness (*ncskew*).[6] All the control variables are defined in detail in Appendix 2.7.1. Lastly, we include industry-fixed effects and year-fixed effects in the regression.

2.3.5 Tests of H2-H5

To ease the interpretation of the results, we undertake subsample analyses to test H2-H5. We construct the moderator variables of abnormal accruals, corporate governance, corporate tax avoidance, and credit ratings, and divide the full sample into two subsamples based on the four moderator variables, respectively.

We employ the balance sheet approach, per Dechow et al. (1995) and Sloan (1996), to estimate abnormal accruals (*da*). The variable definition is presented in Appendix 2.7.1. We partition our sample into two groups based on the sample median of the abnormal accruals (*da*), and estimate model (2.4) separately for the two

[5] Our results remain valid when including average firm-specific weekly returns over a fiscal year as a control variable in our baseline regression. Stocks with high returns are more likely to crash in the future (Chen et al., 2001; Kim et al., 2011a).

[6] As a robustness check, we include corporate governance as an additional control in our regression. We obtain qualitatively the same results after controlling for any one of the corporate governance variables that are to be covered in Section 2.3.5. The data used to construct the corporate governance variables are available only for the period of 2007-2015 in the Institutional Shareholder Services (ISS) database. Our sample size reduces substantially once a corporate governance variable is included for the regression estimation. For example, 23,293 firm-years are dropped when outside directors' equity ownership is controlled.

subsamples. If the coefficient on *SA* is significantly more positive for the high-accruals firms than for the low-accruals firms, H2 holds.

Building on previous studies (e.g., Byrd and Hickman, 1992; Petra, 2005; Callen and Fang, 2013; Andreou et al., 2016), we employ sixteen corporate governance measures for our analysis. These measures are outside directors' stock ownership (*directorownership*) (e.g., Ayers et al., 2011), the proportion of independent directors on board (*indp*) (e.g., Laksmana, 2008; Hoitash et al., 2009; Li and Srinivasan, 2011; Hazarika et al., 2012; Masulis et al., 2012; Morellec et al., 2012; Wintoki et al., 2012), board size (*boardsize*) (e.g., Core et al., 1999; Laksmana, 2008; Hoitash et al., 2009; Li and Srinivasan, 2011; Chen et al., 2012; Hazarika et al., 2012; Hoechle et al., 2012; Masulis et al., 2012; Wintoki et al., 2012; Andreou et al., 2016), the percentage of independent directors who sit on the compensation committee (*indpComp*), nominating committee (*indpNomi*), auditing committee (*indpAudit*), and corporate governance committee (*idpCG*) (e.g., Klein, 2002; Xie et al., 2003; Ashbaugh-Skaife et al., 2006), CEO-chair duality (*CEOduality*) (e.g., Hazarika et al., 2012; Masulis et al., 2012; Andreou et al., 2016), the percentage of busy independent directors (*indpbusy*) (e.g., Laksmana, 2008; Hoitash et al., 2009; Hoechle et al., 2012; Masulis et al., 2012; Andreou et al., 2016), the percentage of directors who age over 64 (*olddirector*) (e.g., Armstrong et al., 2012; Hoechle et al., 2012), the percentage of female independent directors (*indpfemale*) (e.g., Shrader et al., 1997; Carter et al., 2003; Erhardt et al., 2003; Adams and Ferreira, 2009), the independence of the chairman of board (*directorchair*) (e.g., Armstrong et al., 2012), the voting power possessed by independent directors (*indpvotingpower*) (e.g., Ashbaugh-Skaife et al., 2006), the percentage of directors appointed before the current CEO took office (*directorpredate*) (Coles et al., 2014), staggered board (*staggered*) (e.g., Zhao and Chen, 2008), and the percentage of independent directors who have continuously served the board for ten years or more (*longtenuredindp*) (Bonini et al., 2017). Detailed definitions of the corporate governance variables are provided in Appendix 2.7.1. Low (high) values of *directorownership*, *indp, indpComp, indpNomi, indpAudit, indpCG, indpvotingpower, olddirector, directorchair*, *directorpredate*, *boardsize*, *longtenuredindp*, and *staggered* (*indpbusy*, *indpfemale*, and *CEOduality*) indicate weak corporate governance.

For the corporate governance variables that are non-indicators, we use their sample medians as the cut-off point to divide the full sample into two groups. For the corporate governance variables that are binary, we partition our sample based on whether the binary variables are equal to 1 or 0. Based on H3, we expect that the positive relation between financial constraints and future crash risk is statistically more evident for the weak-corporate-governance group than for the strong-corporate-governance group.

We use cash effective tax rate (*cashetr*) (Dyreng et al., 2008; Lisowsky et al., 2013) to proxy for corporate tax avoidance, as it may capture the extent of cash tax savings that mitigate default risk of a financially constrained firm. *cashetr* is calculated as cash taxes paid, divided by pretax book income, over a fiscal year. Firm-year observations with negative pretax book income are excluded from our sample. Following Desai and Dharmapala (2006, 2009), we also use the residual book-tax difference (*ddmpbtd*) to measure corporate tax avoidance. Book-tax differences may result from either upwards accruals management or tax avoidance. Desai and Dharmapala's (2006, 2009) residual book-tax difference measure removes the effect of book-tax differences that is attributed to accruals inflation. A lower (higher) value of *cashetr* (*ddmpbtd*) indicates a larger degree of corporate tax avoidance. We split our sample into high- and low-tax-avoidance subsamples, based on the sample medians of *cashetr* and *ddmpbtd*, respectively. H4 holds if the coefficient on *SA* is less positive for the high-tax-avoidance firms than for the low-tax-avoidance firms.

To test H5, we use credit rating as a measure for default probability and construct two subsamples consisting of investment- and speculative-grade firms. We then estimate model (2.4) separately for these two subsamples. The investment-grade firms, which are rated with a BBB-grade or above, are believed to have a stronger capacity for meeting financial obligations and be less likely to default, compared with the speculative-grade firms that are rated at BB+ or below. It is predicted that financially constrained firms with higher default risk have higher future crash risk. Therefore, in supporting H5, *SA* should take on a more positive coefficient in the speculative-grade subsample than in the investment-grade subsample. In addition, to see whether the effect of financial constraints on future crash risk is subsumed by the effect of default risk for the speculative-grade firms, we include credit rating as an additional control variable in the subsample analysis.

2.4 Empirical results

Table 2.3 presents the regression results for H1. Column (1) reports the results for model (2.4), where $crashrisk_{t+1}$ is the dependent variable. The coefficient for SA_t is positive and statistically significant at the 0.1% level. An increase of one standard deviation in SA_t leads to an increase in the probability of a stock price crash ($crashrisk_{t+1}$) by 2.89 percentage points, which is equivalent to 12.31% of the mean value of $crashrisk_{t+1}$ in our sample and is thus economically significant. This result supports H1, indicating that financial constraint is positively associated with one-year-ahead stock price crash risk and is consistent with our argument that outside investors are not able to deduce the implications of financial constraints for bad news hoarding and default risk. We check the robustness of this result using alternative measures of crash risk, namely, $ncrash_{t+1}$, $ncskew_{t+1}$, $duvol_{t+1}$ and $minreturn_{t+1}$. Columns (2-5) report the results. The coefficients for SA_t remain statistically positive at the 1% level across all the columns, enhancing the robustness of our inferences.

Table 2.4 reports the regression results for H2. The coefficient for SA_t is positive and statistically significant (p=0.001) in the high-abnormal-accruals firms. By contrast, the coefficient for SA_t in the low-abnormal-accruals subsample, albeit positive, is not statistically significant (p=0.205). The positive association between financial constraint and future crash risk is evident only in firms with high abnormal accruals. This evidence is consistent with H2 and offers support to our view that earnings management provides managers with a tool to withhold bad news and increases future crash risk of a financially constrained firm.

The results for H3 are shown in Table 2.5. Panel A presents descriptive statistics for all the corporate governance variables used in our subsample analyses. Panel B presents the regression results for the subsample test in which *directorownership* is used as a proxy for corporate governance. As expected, the positive relation between financial constraints and future crash risk is statistically significant (p=0.031) only in the low-*directorownership* subsample, which features weak corporate governance. Panel C shows the results for the subsample tests, in which fifteen other alternative proxies for corporate governance are used. The intercepts and the coefficients on the control variables are not reported for the sake of brevity. The coefficients for the SA index (SA_t) are statistically significant at the 5% level across the weak-corporate-governance groups, except that the coefficient on SA_t is marginally significant for the low-*indpComp*, low-*indpNomi*, and low-*indpCG* groups. By contrast, the coefficients for SA_t are not statistically significant in any of the strong-corporate-governance groups. Together, these results support H3 that the positive link between financial constraints and future crash risk is more pronounced for firms with weak corporate governance.

Table 2.6 shows the regression results for H4. Column (1) shows that the coefficient on SA_t is only statistically significant (p<0.001) in the low-tax-avoidance (high-*cashetr*) subsample but not significant (p=0.114) in the high-tax-avoidance (low-*cashetr*) subsample. In Column (2), the coefficient for SA_t is significantly positive at the 5% level in the low-tax-avoidance (low-*ddmpbtd*) subsample but is not significant in the high-tax-avoidance (high-*ddmpbtd*) subsample. These results support the proposition for H4 that tax avoidance helps financially constrained firms generate internal funds from cash tax savings, thereby mitigating their default risk and associated future crash risk. This finding is also in line with Edward et al. (2016) and Law and Mills (2015), suggesting that tax avoidance is used by a financially constrained firm as a device mainly to generate cash flows, not to withhold bad news.

Table 2.7 reports the regression results for H5. The coefficient for SA_t is positive and significant at the 5% level for the speculative-grade subsample, whereas the coefficient for SA_t is not statistically significant for the investment-grade subsample. This result is consistent with H5 that the positive association between financial constraints and future crash risk is more salient for low-credit-rating firms. In addition, *rating* does not have a statistically significant coefficient for the speculative-grade subsample, suggesting that the association between distress risk and future crash risk is subsumed by the effect of financial constraints.

2.5 Additional tests

2.5.1 A dynamic panel generalized method of moments (GMM) estimator

A potential source of endogeneity is the possibility that the current value of the financial constraint index is a function of current and/or past crash risk. In such a scenario, the crash risk in the past and/or current periods affects current financial constraints and in turn influences crash risk in the future period. To address this dynamic type of endogeneity, we follow Wintoki et al. (2012) in applying the dynamic GMM estimator to model (2.4) to re-estimate the relation between financial constraints and future crash risk. The dynamic panel GMM model is specified as follows:

$$\begin{aligned} Crashrisk_{i,t+1} &= \alpha_0 + \alpha_1 Crashrisk_{i,t} + \alpha_2 Crashrisk_{i,t-1} + \alpha_3 SA_{i,t} + \sum_k \alpha_k Controls_{i,t}^k \\ &+ \left(YearDummies\right) + \left(IndustryDummies\right) + \eta_i + u_{i,t} \end{aligned} \tag{2.5}$$

where $Controls_{i,t}$ captures the same set of control variables as that included in model (2.4) and η_i represents unobserved firm-fixed effects. Two lags of the dependent variable, namely, $Crashrisk_{i,t}$ and $Crashrisk_{i,t-1}$, are included to control for the dynamic aspect of the relationship between crash risk and financial constraints. [7] The estimation procedure consists of two steps that make the dynamic GMM estimator superior to OLS and fixed-effects estimates. First, the first-differencing eliminates potential bias that arises from time-invariant unobserved heterogeneity. Second, we follow Wintoki et al. (2012) and include lagged values of crash risk, and of all the explanatory variables lagged three and four periods, as instruments for the differenced equations.[8] Because our dependent variable is one-year-ahead crash risk, the dynamic GMM model controls for the influences of current and one-year lagged crash risk on future crash risk. To ensure that we have included proper lags to control for dynamic endogeneity, we employ the Arellano-Bond (1991) (*AR*) tests of first-order and second-order serial correlations. By construction, there should be serial correlations among the residuals in first differences (*AR*(1)) but not in second differences (*AR*(2)). Accordingly, we expect to reject the null hypothesis in *AR*(1) but not in *AR*(2). Given

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[7] We augment model (2.4) with $Crashrisk_{i,t-2}$ and $Crashrisk_{i,t-3}$, and run the logistic regression. In results not reported, the coefficients on $Crashrisk_{i,t}$ and $Crashrisk_{i,t-1}$ ($Crashrisk_{i,t-2}$ and $Crashrisk_{i,t-3}$) are (are not) statistically significant. This finding suggests that two lags of crash risk are sufficient to ensure dynamic completeness. Accordingly, crash risk, as well as other explanatory variables, that are lagged beyond two periods can be regarded as exogenous and hence as valid instruments for use in our GMM model.

[8] The instruments used in the GMM estimation include $Crashrisk_{i,t-2}$, $Crashrisk_{i,t-3}$, $SA_{i,t-3}$, $SA_{i,t-4}$, $Controls_{i,t-3}$, $Controls_{i,t-4}$, $\Delta\, YearDummies$, and $\Delta\, IndustryDummies$ ($\Delta\, Crashrisk_{i,t-2}$, $\Delta\, SA_{i,t-2}$ $\Delta Controls_{i,t-2}$, $YearDummies_{i,t}$, and $IndustryDummies_{i,t}$) in the differenced (level) equations. The assumption underlying such a choice of instruments is that all the regressors, except year dummies and industry dummies, are endogenous. The industry dummies used in the GMM specification are based on the Fama-French's twelve industries, rather than the first two digits of SIC codes, because the inclusion of too many industry dummies as instruments might weaken the power of the Hansen test of over-identification.

that we use multiple lags as instruments, we also conduct a Hansen test of over-identification to check the validity of our instruments.

Table 2.8 reports the regression results from our dynamic GMM estimation for H1. It shows that the coefficient for SA_t is positive and statistically significant, supporting H1.[9] Our $AR(1)$ ($AR(2)$) test yields a p-value less than 0.001 (0.180), indicating that we can (cannot) reject the null hypothesis of no serial correlation in first (second) differences; thus, it is consistent with the assumptions of the GMM specification (Wintoki et al., 2012). The Hansen J test yields a p-value of 0.458, which implies that we cannot reject the null hypothesis of valid instruments used in our GMM model. Overall, our results suggest that dynamic endogeneity does not plague our empirical analysis of H1.

2.5.2 Control for endogeneity – A collapse of the junk bond market (1989) and crash risk

As we discussed in Section 2.2.1, outside investors, who generally do not have access to private information, are unlikely to appraise the amount of bad news withheld in a firm or extrapolate future crash risk from current default risk. Therefore, it is hard for investors to predict a firm's future stock price crash risk. On this basis, reverse causality is less of a concern in our study. That said, it is possible that either correlated omitted variables or measurement errors in the financial-constraint index bias the coefficient estimates in our hypothesis tests. To mitigate this concern, we follow Kim (2018) and conduct a quasi-experiment in which the collapse of bond market in 1989 is used as an exogenous shock that increased financial constraints of speculative-grade firms. Lemmon and Roberts (2010) argue that three unexpected events in 1989 led to a substantial decline in the supply of credit to speculative-grade firms. These events include (i) the collapse of Drexel Burnham Lambert, Inc., which caused a substantial reduction in funds available to speculative-grade firms; (ii) the passage of the Financial Institutions Reform, Recovery, and Enforcement Act of 1989 (FIRREA), which resulted in a forced sell-off of all junk bonds by Savings and Loans (S&Ls),

[9] Similar to our main test, we use alternative crash risk measures to check the robustness of our results. Consistent with H1, $ncskew_{t+1}$ and $minreturn_{t+1}$ have positive coefficients (4.0101 and 0.3438) that are both statistically significant at the 5% level (p-value=0.047 and 0.012, respectively).

which previously held a large fraction of junk bonds; and (iii) a change in the National Association of Insurance Companies (NAIC) credit rating guideline, which led to a sharp decrease in the life-insurance companies' commitments to purchase bonds from speculative-grade issuers. As a result of these events, speculative-grade firms, which used to rely heavily on junk bond issuances to secure external funds, became more financially constrained. Therefore, the junk-bond-market collapse offers a nice experimental setting to examine the causal effect of financial constraints on crash risk. If the casual effect is positive as implied by H1, the increase in financial constraints of speculative-grade firms following the junk-bond-market collapse should lead to a more significant increase in crash risk, compared with nonrated firms that do not rely on bond financing.

Using the collapse of the junk bond markets as an exogenous event, we conduct a difference-in-differences (DiD) test for the period of 1987-1992, in which 1987-1989 (1990-1992) is designated as the pre- (post-) collapse period. The treatment firms are defined as those rated with a speculative grade (i.e., a grade of BB+ or lower) by the S&P credit rating agency; the control firms are defined as those without an S&P credit rating.[10] The DiD regression is specified below.

$$\begin{aligned} Crashrisk_{i,t+1} = {} & \alpha_0 + \alpha_1 PostCollapse + \alpha_2 Junk + \alpha_3 PostCollapse \times Junk \\ & + \sum_k \alpha_k Controls_{i,t}^k + Industry\text{-}fixed\text{-}effects + Year\text{-}fixed\text{-}effects + \varepsilon_{i,t} \end{aligned} \tag{2.6}$$

PostCollapse equals 1 if a firm is in the post-collapse period and 0 otherwise. *Junk* equals 1 (0) if a firm pertains to a treatment (control) firm. The interaction term, *PostCollapse*×*Junk*, captures the change in crash risk from the pre-collapse period to the post-collapse period for the treatment firms, relative to the control firms. The control variables included in model (2.6) are similar to those in model (2.4). The sample size decreases to 2,360 firm-years after clearing missing values for the control

[10] To reduce potential multivariate imbalance in covariates between the treatment and control groups, we apply coarsened exact matching (CEM, the same approach used in Section 2.5.3), a monotonic imbalance bounding approach. Specifically, an automated coarsening *k*-to-*k* match is done between the treatment firms and control firms. We then repeat our DiD analysis using the matched data, and obtain qualitatively the same results. However, the number of observations after the matching drops to 191 firm-years, reducing the power of the test; hence, the results from the test need to be interpreted with caution. Likewise, when we include firm-fixed effects in model (2.6), firms that have no time-series variation are removed from the regression estimation, reducing our sample to only 772 firm-years. Due to the lack of power of the test, we do not provide our firm-fixed-effects regression analysis.

variables. To ensure sufficient observations for the test, the *opacity* variable, which has many missing values, is not included in model (2.6). *insti* is not included either, because none of the control firms in the period of 1987-1992 have an institutional ownership greater than zero.

Table 2.9 reports the DiD results for model (2.6). The coefficient on the interaction term, *PostCollapse*×*Junk*, is positive (0.7587) and statistically significant at the 5% level, indicating that the treatment firms, which suffered from tightened financial constraints after the collapse of the junk bond markets, experienced higher crash risk than the control firms, which were not affected by the collapse event.[11] The parallel trends assumption underlying our difference-in-differences analysis requires similar trends of crash risk for both treatment and control firms during the pre-collapse period. To test the validity of this assumption, we follow Robert and Whited (2013) and rerun our DiD regression model by using 1988 and 1989 (as well as 1987 and 1988), respectively, as the pre- and post-"event" periods. We find no evidence of a substantive change in crash risk for the treatment firms relative to the control firms. This suggests that our DiD results reported in Table 2.9 are not biased by potential violation of the parallel trends assumption.

2.5.3 Control for endogeneity – The Internet bubble and crash risk

The Internet bubble of the late 1990s, which generated exogenous variation in firms' financial constraints, is employed as our second quasi-experimental setting to examine the causal effect of financial constraint on crash risk. In the late 1990s, due to the prevalent use of computers, investors were keen on investing in tech firms, making tech stocks highly priced and yield over 1,000-percent returns (Ofek and Richardson, 2003). The rise in technology stocks also fueled a run-up in non-tech firms' equity prices, thereby leading to a stock price bubble in the whole equity market. This bubble was argued to be driven by irrational euphoria among retail investors (Shiller, 2000), speculative trading by hedge funds (Brunnermeier and Nagel, 2004; Griffin et al., 2011), and limits of arbitrages (Morck et al., 1990; Shleifer and Vishny, 1997; Ofek

[11] We also use alternative crash risk measures to run our DiD regression. The results show that when using *ncrash* and *duvol* as the dependent variable, the coefficients on *PostCollapse*×*Junk* are positive (0.7730 and 0.0696) and statistically significant at the 5% (*p*-value=0.014) and 10% levels (*p*-value=0.083), respectively.

and Richardson, 2003). Financially constrained firms could take advantage of the stock price bubble by issuing equities to ease their financial constraints; in this sense, the bubble exogenously decreased firms' financial constraints. Nonetheless, the technological innovations that triggered the Internet bubble also brought a good deal of investment opportunities to tech firms, raising such firms' demand for funds and thereby engendering and/or amplifying their financial constraints; this offset the foregoing, attenuating effect that the bubble per se exerted on the tech firms' financial constraints. Therefore, we expect that only financially constrained *non-tech* firms experienced a substantial decrease in constraints during the bubble, when external funds became cheaper for the non-tech firms but their investment opportunities and demand for funds remained largely unchanged (Jorgenson and Stiroh, 1999; Gordon, 2000; Stiroh, 2002).

On the above basis and in line with Campello and Graham (2013), our treatment (control) firms are defined as non-tech firms that faced high (low) financial constraints during the pre-bubble period of 1990-1994; the bubble period is defined to cover the years of 1995-1999.[12] The pre-bubble financial constraint statuses of non-tech firms are measured by the standardized mean of the SA indices over the five-year pre-bubble period.

We implement Coarsened Exact Matching (CEM) to reduce the imbalance in pre-treatment covariates between the treatment and control groups (Blackwell et al., 2009). The idea of CEM is to temporarily coarsen each covariate into meaningful strata, exactly match on these coarsened data, and retain only the un-coarsened values of the matched data. Specifically, we match the treatment firms with the control firms based on the pre-bubble firm characteristics as to firm size (*size*), the book-to-market ratio (*btm*), the leverage ratio (*debt*), return on assets (*roa*), earnings volatility (*stdearnings*), and financial opacity (*opacity*), which are arguably related to firms' financial constraints. Unlike commonly used matching techniques such as propensity score matching (PSM), CEM does not require checking *ex post* the covariate balances, as the coarsening levels are chosen *ex ante* (Iacus et al., 2012; King and Nielsen, 2019).

[12] We obtain qualitatively identical results, when using a bubble period of 1996-1999 and a pre-bubble period of 1992-1995 for the DiD test. We do not include the year 2000 in our bubble period, because the bubble burst, with stock price crashes occurring for a large number of firms, occurred during that year.

After an automated coarsening k-to-k match, our matched data contain the same number of treated and control units in all strata.

The following DiD regression model is specified to carry out the experimental test.

$$\begin{aligned} Crashrisk_{i,t+1} = {} & \alpha_0 + \alpha_1 Bubble + \alpha_2 FC + \alpha_3 Bubble \times FC + \sum_k \alpha_k Controls_{i,t}^k \\ & + Firm\text{-}fixed\text{-}effects + Industry\text{-}fixed\text{-}effects + Year\text{-}fixed\text{-}effects + \varepsilon_{i,t} \end{aligned} \quad (2.7)$$

Bubble equals 1 (0) if a firm is in the Internet bubble (pre-bubble) period of 1995-1999 (1990-1994). *FC* is equal to 1 (0) if a firm is a treatment (control) firm, defined as having a pre-bubble standardized mean of the SA indices that is higher (lower) than the sample median.[13] The interaction term, *Bubble*×*FC*, captures the DiD estimate of crash risk between the treatment and matched control firms across the pre-bubble and bubble periods. We maintain the same control variables as those included in model (2.4). It is possible that the Internet bubble also caused exogenous changes in some unobserved firm-specific factors that influence crash risk. Accounting for this possibility, we also include firm-fixed effects in the regression. If the causal effect implied by H1 holds, the coefficient on *Bubble*×*FC* will be negative and statistically significant.

Table 2.10 reports the DiD regression results. As expected, the coefficient on the interaction term, *Bubble*×*FC*, is significantly negative at the 1% level.[14] This indicates that non-tech firms faced with high financial constraints have significantly larger declines in crash risk during the Internet bubble when compared with non-tech firms that are less subject to financial constraints. Generally, inflated stock prices during the bubble imply higher crash risk for our treatment firms, but we still find the significantly lower crash risk of such firms; this reinforces the causal inference that

[13] Following previous literature (e.g., Bond and Cummins, 2000; Campello and Graham, 2013), we classify tech firms as those with the first three digits of SIC codes of 355, 357, 366, 367, 369, 381, 382, and 384. These codes correspond to special industry machinery, computer and office equipment, communications equipment, electric components and accessories, electric transmission and distribution equipment, electric industrial apparatus, miscellaneous electrical equipment, search and navigation equipment, measuring and controlling devices, and medical instruments, respectively. The non-tech firms refer to those not in these sectors.

[14] Using the alternative crash risk measures, *ncrash* and *minreturn*, respectively, to repeat our DiD test, we obtain similar results: the coefficients on *Bubble*×*FC* are negative (-0.3732 and -0.0863) and statistically significant at the 5% level (p-value=0.026 and 0.020).

eases in financial constraints lead to lower stock price crash risk. In addition, we conduct a multivariate test of the parallel trends assumption for our DiD analysis, as per Roberts and Whited (2013). Specifically, we rerun model (2.7) by using 1990 and 1991 (as well as 1991 and 1992, 1992 and 1993, 1993 and 1994, or 1994 and 1995), respectively, as the pre-"event" and "event" periods. In the results (not tabulated), none of the DiD estimators are statistically significant, which signifies that the parallel trends assumption is tenable. By and large, the results for our second quasi-experiment speak strongly to the positive, causal relationship between financial constraints and future crash risk.

2.5.4 The association between financial constraints and longer-term future crash risk

Our main test concerns the association between financial constraints and one-year-ahead crash risk. However, if the difficulty in raising external funds induces financially constrained firms to withhold bad news for an extended period (say, two to three years), financial constraints might have an impact on longer-term future crash risk. To test this conjecture, we extend the measurement windows of crash risk to two years and three years ahead of our financial constraint measure (SA_t) and re-estimate model (2.4). Specifically, we replace the one-year-ahead crash risk, $crashrisk_{t+1},$ with the two-year and three-year lead measures of crash risk, $crashrisk_{t+2}$ and $crashrisk_{t+3}$, respectively, as the dependent variable for our regression estimations. Column (1) ((2)) of Table 2.11 reports the results as to the association between financial constraints and the two-year-ahead (three-year-ahead) crash risk. The coefficients on SA_t are positive and statistically significant at the 1% and 5% levels, respectively, which suggests that financial constraints can predict crash risk as far as two years and three years ahead, respectively. A one-standard-deviation increase in SA_t leads to an increase in $crashrisk_{t+2}$ ($crashrisk_{t+3}$) by 2.60 (1.57) percentage points, which accounts for 10.59% (6.30%) of its mean value; thus, it is economically significant. In results not tabulated for brevity, SA_t is also positively associated with the alternative crash risk variables, *ncrash*, *duvol*, and *minreturn*, which are measured on the two-year-ahead and three-year-ahead horizons, respectively; this finding is both statistically and economically significant. Overall, our results imply that financial constraints are strongly predictive of future crash risk as far as three years ahead.

2.6 Conclusion

This study examines whether financial constraints are associated with future stock price crash risk. On one hand, financially constrained firms have stronger incentives to withhold bad news for an extended period to secure external funds. As withheld bad news accumulates, stock prices become increasingly overvalued, leading to a higher risk of future stock price crashes. On the other hand, financially constrained firms are subject to higher default risk and are more likely to undergo a stock price crash when they default. Consistent with these rationales, we find strong evidence that financial constraints are positively correlated with the one-year-ahead stock price crash risk. This finding is robust to controlling for potential endogeneity in a dynamic panel generalized method of moments (GMM) analysis and in two quasi-experimental settings including the collapse of the junk bond market in 1989 and the Internet bubble in the late 1990s. In the quasi-natural experiments, crash risk was significantly higher (lower) in periods when firms' financial constraints were exogenously exacerbated (eased) by the collapse of the junk bond market (by the Internet bubble); this corroborates our causal inference that financial constraints lead to high future stock price crash risk, suggesting that outside investors are unlikely to extrapolate the implication of financial constraints for future stock price crash risk.

In the cross-sectional analyses, we find that the positive relation between financial constraints and future crash risk is more pronounced for firms with earnings management activities or with weak corporate governance and is less pronounced for firms that commit tax avoidance or have high credit ratings. Additional analysis reveals that financial constraints are associated with future crash risk as far as three years ahead. Overall, our results shed light on the stock price crash risk of financially constrained firms and should have important implications for not only companies per se but also their stakeholders, including investors, creditors, suppliers, and customers concerned about the companies' creditworthiness, viability, and prospects. On the other hand, to mitigate crash risk, it is important for a financially constrained firm to build up strong corporate governance and increase creditworthiness as well as information transparency to the public.

2.7 Appendices

2.7.1 Summary of variable definitions

Variables	Definitions
crashrisk	1 if a firm experiences one or more firm-specific weekly returns falling 3.2 standard deviations below the mean firm-specific weekly return over a fiscal year, and 0 otherwise. The firm-specific weekly returns measure follows Kim et al. (2011a).
ncrash	The number of firm-specific weekly returns that fall 3.2 standard deviations below the mean firm-specific weekly return over a fiscal year.
duvol	The standard deviation of "down"-week firm-specific weekly returns (scaled by the number of "down"-weeks minus one), divided by the standard deviation of "up"-week firm-specific weekly returns (scaled by the number of "up"-weeks minus one) over a fiscal year. The firm-specific weekly returns measure follows Kim et al. (2011a).
minreturn	The minimum value of firm-specific weekly returns over a fiscal year, times -1, less the mean firm-specific weekly return, divided by the standard deviation of firm-specific weekly returns over a fiscal year. The firm-specific weekly returns measure follows Kim et al. (2011a).
ncskew	The negative of the third moment of firm-specific weekly returns. The firm- specific weekly returns measure follows Kim et al. (2011a).
SA	A financial constraint index (*SA*) developed by Hadlock and Pierce (2010). SA=-0.737*$size$+0.043*$size^2$-0.040*age, where *size* is the natural logarithm of total assets capped at $4.5 billion, and *age* is the number of years for which a firm has been listed. *SA* index is re-scaled by dividing 1,000.
size	The natural logarithm of the market value of a firm's equity at the end of a fiscal year.
btm	The book value of firm equity divided by the market value of firm equity at the end of a fiscal year, winsorized at the 1% and 99% levels, respectively.
insti	Institutional investors' stock ownership as a percentage of the outstanding shares for a firm at the end of a fiscal year.
lanacov	The natural logarithm of 1 plus the number of analysts that make at least one annual EPS forecast for a firm over a fiscal year.
roa	Return on assets at the end of a fiscal year.
stdret	The standard deviation of firm-specific weekly returns for a fiscal year. The firm-specific weekly returns measure follows Kim et al. (2011a).
stdearnings	The standard deviation of income before extraordinary items in the current and previous four fiscal years.
tradevol	The average of monthly trading volume for a firm over a fiscal year, scaled by shares outstanding at the end of the year.
opacity	The three-year moving sum of the absolute value of annual discretionary accruals, a measure of financial opacity developed by Hutton et al. (2009).
da	Abnormal accruals of a firm for a fiscal year, which is estimated using industry-specific modified Jones model per Dechow et al. (1995).
ddmpbtd	The residual domestic book-tax difference based on Desai and Dharmapala (2006), which equals the residuals obtained from the following firm-fixed-effects regression model: $MPBTD_{i,t}=\beta_1 TA_{i,t}+u_i+\varepsilon_{i,t}$. *MPBTD* is domestic book-tax difference based on Manzon and Plesko (2002), which is calculated as: (domestic pre-tax income - (current

	federal income tax expense/statutory tax rate) - state income tax expense - other income tax expense - equity income)/ lagged total assets. *TA* is total accruals measured using the cash flow method of Hribar and Collins (2002). Both *MPBTD* and *TA* are scaled by lagged total assets and are winsorized at the 1% and 99% levels, respectively, for the fixed-effects regression estimation.
cashetr	Cash effective tax rate, calculated as cash taxes paid, divided by pre-tax income net of special items. Observations for *cashetr* are excluded if its denominator is 0 or negative.
firmage	The number of years a firm has been listed.
PostCollapse	1 if a firm is in the three-year period (i.e., 1990-1992) after the collapse of junk bond market in 1989, and 0 if a firm is in the three-year period (i.e., 1987- 1989) as of the 1989 junk bond collapse.
Junk	1 if a firm is rated at BB+ or lower by the S&P credit rating agency, and 0 if a firm does not have an S&P credit rating, in a year over the period of 1987-1992. Credit ratings used in this study are the Standard & Poor's long-term domestic issuer credit ratings reported by Compustat.
FC	1 (0) if a firm is a financially constrained (unconstrained) non-tech firm that has the standardized mean of the SA indices higher (lower) than the sample median. The standardized mean of the SA indices is calculated based on the pre-bubble period of 1990-1994.
Bubble	1 if a firm is in the Internet bubble period of 1995-1999, and 0 if a firm is in the pre-bubble period of 1990-1994.
rating	Standard & Poor's long-term domestic issuer credit ratings, which range from AAA to D/SD and are transformed into conventional numerical scores ranging from 22 to 0.
directorownership	The outside directors' equity ownership as a percentage of total shares outstanding of a firm at the end of a fiscal year.
indp	The number of the independent outside directors on the board of a firm, divided by the number of all the directors on the board, at the end of a fiscal year.
boardsize	The number of directors on the board of a firm at the end of a fiscal year.
indpComp	The number of the independent outside directors who sit on the compensation committee, divided by the number of all the directors on the board, at the end of a fiscal year.
indpNomi	The number of the independent outside directors who sit on the nominating committee, divided by the number of all the directors on the board, at the end of a fiscal year.
indpAudit	The number of the independent outside directors who sit on the auditing committee, divided by the number of all the directors on the board, at the end of a fiscal year.
indpCG	The number of the independent outside directors who sit on the corporate governance committee, divided by the number of all the directors on the board, at the end of a fiscal year.
CEOduality	1 if the CEO and the chairman of the board are the same person for a firm for a fiscal year and 0 otherwise.
indpbusy	The number of the independent outside directors who hold two or more board directorships, divided by the number of the independent outside directors, for a firm as of the end of a fiscal year.
olddirector	The number of directors who are older than 64, divided by the number of all the directors on the board of a firm, at the end of a fiscal year.
directorchair	1 if the chairman of the board is an independent outside director for a firm for a fiscal year and 0 otherwise.
indpfemale	The number of the female independent outside directors, divided by the number of all the directors on the board of a firm, at the end of a fiscal

	year.
indpvotingpower	The average percentage of a firm's voting power controlled by an independent outside director at the end of a fiscal year.
directorpredate	The number of directors appointed before the current CEO took office, divided by the number of all the directors on the board, for a firm at the end of a fiscal year.
staggered	1 if a firm's board is staggered board for a fiscal year and 0 otherwise.
longtenuredindp	The number of the independent outside directors who have continuously served the board for ten years or more, divided by the number of the independent outside directors, for a firm at the end of a fiscal year.

2.7.2 Examples of using accruals to withhold bad news

Strategies	**Examples of Corporate Bad News**
Understating bad debt provisions	Deteriorating financial health of customers; Uncollectable payments due to bankruptcy or other cash-inadequacy issues of customers.
Understating contingent liabilities	Obligations to clean up polluted production sites; Obligations to provide warranty coverage for products sold due to malfunction of operating appliances; Obligations to pay expenses incurred from a lawsuit.
Understating impairment loss on inventories	Obsolescence or physical damage of products; Significant decline in some major customers' demand for products due to worsening customer relationship, deteriorating financial health of customers, or changes in customers' tastes, preferences, and needs on products; Emergence and increase in substitute products made by a competitor, which undermine the potential sales outlet and market value of existing products in stock.
Delaying or underestimating write-off of assets	A warehouse fire that impaired assets such as inventories, building, equipment, and machinery; Discontinued operations or disposals of a subsidiary, which reduce the values of currently operated assets; Changes in technologies, markets, or regulations which engendered adverse impacts that reduce the value of brands, goodwill, and other intangible assets.

Table 2.1: Descriptive statistics

Variables	No. of firm-years	No. of firms	Mean	Std. dev.	25th	Median	75th
$crashrisk_{t+1}$	28,331	6,557	0.2348	0.4239	0	0	0
$ncrash_{t+1}$	28,331	6,557	0.3928	1.6109	0	0	0
$duvol_{t+1}$	28,088	6,501	-0.2069	0.5215	-0.4502	-0.1597	0.0944
$minreturn_{t+1}$	28,281	6,544	2.4195	0.7528	1.9331	2.3128	2.8040
$ncskew_{t+1}$	28,331	6,557	-4.8025	16.6795	-12.1256	-4.4138	3.1539
SA_t	28,331	6,557	-1.0519	1.2116	-1.7477	-0.4375	-0.1074
$size_t$	28,331	6,557	6.2898	2.0483	4.9008	6.3410	7.6526
btm_t	28,331	6,557	0.8824	9.4426	0.2827	0.5068	0.8520
$lanacov_t$	28,331	6,557	2.6478	1.6100	1.6094	2.9957	3.8712
$insti_t$	28,331	6,557	0.4691	0.3579	0.1191	0.4884	0.7713
roa_t	28,331	6,557	-0.0249	0.2251	-0.0212	0.0305	0.0690
$stdret_t$	28,331	6,557	0.0676	0.0448	0.0384	0.0562	0.0834
$tradevol_t$	28,331	6,557	1.5485	2.6914	0.4873	1.0064	1.9353
$opacity_t$	28,331	6,557	229.9549	27,826.010	0.0510	0.1914	1.4147
da_t	21,301	5,781	23.7001	556.3483	-0.0843	0.0009	0.1402
$cashetr_t$	18,195	4,288	0.3419	5.3355	0.0936	0.2261	0.3397
$ddmpbtd_t$	20,418	5,705	0.0657	3.1288	-0.0158	0.0319	0.0761
$rating_t$	9,513	1,996	12.9565	3.3837	10	13	15

Notes: This table presents descriptive statistics for the variables used in the multivariate tests. The sample contains firm-year observations for the period of 1995-2016. All the variables are defined in Appendix 2.7.1.

Table 2.2: Spearman correlations

Variables	SA_t	$size_t$	btm_t	$lanacov_t$	$insti_t$	roa_t	$stdret_t$	$tradevol_t$	$opacity_t$	$ncskew_{t+1}$
SA_t	1									
$size_t$	-0.8612***	1								
	(<0.001)									
btm_t	-0.0419***	-0.3451***	1							
	(<0.001)	(<0.001)								
$lanacov_t$	-0.6626***	0.7405***	-0.2316***	1						
	(<0.001)	(<0.001)	(<0.001)							
$insti_t$	-0.3857***	0.4537***	-0.1543***	0.4991***	1					
	(<0.001)	(<0.001)	(<0.001)	(<0.001)						
roa_t	-0.2650***	0.3671***	-0.1969***	0.2098***	0.2293***	1				
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)					
$stdret_t$	0.4932***	-0.4545***	-0.0004	-0.1804***	-0.1935***	-0.3831***	1			
	(<0.001)	(<0.001)	-0.943	(<0.001)	(<0.001)	(<0.001)				
$tradevol_t$	-0.2163***	0.3520***	-0.2421***	0.5389***	0.4497***	0.0193***	0.2516***	1		
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	-0.001	(<0.001)			
$opacity_t$	0.1642***	-0.0593***	-0.1326***	-0.0596***	-0.0515***	-0.1222***	0.1682***	0.0804***	1	
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)		
$ncskew_{t+1}$	-0.1579***	0.2147***	-0.1113***	0.1870***	0.1466***	0.0974***	-0.0432***	0.1735***	0.0198***	1
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	

Notes: This table reports the results for the Spearman correlations among the variables used in model (2.4). The sample consists of 28,331 firm-year observations and covers the years of 1995-2016. All the variables are defined in Appendix 2.7.1. *, **, and *** indicate the two-tailed statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 2.3: Tests of H1: the association between financial constraints and future stock price crash risk

Variables		Dependent Variable =
	Predicted Sign	(1) $crashrisk_{t+1}$
Intercept	?	2.8037***
		(<0.001)
$\boldsymbol{SA_t}$	+	**0.1401*** **
		(<0.001)
$size_t$	+	0.0630***
		(<0.001)
btm_t	-	-0.0005
		(0.708)
$lanacov_t$	+	0.0834***
		(<0.001)
$insti_t$	-	-0.0842
		(0.218)
roa_t	-	0.0075***
		(<0.001)
$stdret_t$	+	-0.7930*
		(0.100)
$tradevol_t$	+	0.0097*
		(0.063)
$opacity_t$	+	3.83E-05
		(0.296)
$ncskew_{t+1}$	?	0.0044***
		(<0.001)
Industry-fixed effects		included
Year-fixed effects		included
No. of observations		28,331
Pseudo R-squared		0.1739

Table 2.3: ((Continued)

Variables	Dependent Variable =			
	(2) *ncrash*$_{t+1}$	(3) *ncskew*$_{t+1}$	(4) *duvol*$_{t+1}$	(5) *minreturn*$_{t+1}$
Intercept		-9.5747***	-0.4839***	1.7863***
		(<0.001)	(<0.001)	(<0.001)
***SA*$_t$**	**0.1330*** **	**0.5628*** **	**0.0155*** **	**0.0432*** **
	(<0.001)	**(<0.001)**	**(<0.001)**	**(<0.001)**
size$_t$	0.0637***	1.4317***	0.0540***	0.0508***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)
btm$_t$	-0.0004	-0.0413***	0.0004	0.0002
	(0.752)	(<0.001)	(0.227)	(0.547)
lanacov$_t$	0.0790***	0.4304***	0.0052*	0.0069
	(<0.001)	(<0.001)	(0.083)	(0.128)
insti$_t$	-0.0708	0.4053	0.0059	-0.0363*
	(0.266)	(0.278)	(0.586)	(0.069)
roa$_t$	0.0076***	0.0516***	0.0014***	0.0030***
	(<0.001)	(0.002)	(<0.001)	(<0.001)
stdret$_t$	-0.5406	8.2449***	-0.8457***	-1.6254***
	(0.227)	(0.002)	(<0.001)	(<0.001)
tradevol$_t$	0.0089*	0.0452	0.0017*	0.0028*
	(0.063)	(0.149)	(0.073)	(0.077)
opacity$_t$	2.10E-06***	3.63E-04	-2.62E-08***	-4.17E-08***
	(<0.001)	(0.327)	(<0.001)	(<0.001)
ncskew$_{t+1/t}$	0.1330***	0.5628***	0.0155***	0.0432***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)
Industry-fixed effects	included	included	included	included
Year-fixed effects	included	included	included	included
No. of observations	28,331	26,624	28,088	28,281
Pseudo R-squared	0.2294			
Adjusted R-squared		0.0602	0.0723	0.0490

Notes: This table presents the logistic regression (Column (1)), ordered logistic regression (Column (2)), and OLS regression (Columns (3), (4) and (5)) results for the tests of the association between financial constraints and future crash risk. The sample period covers the years of 1995-2016. In column (1), the dependent variable, *crashrisk*$_{t+1}$, equals 1 if a firm experiences one or more firm-specific weekly returns falling 3.2 standard deviations below the mean firm-specific weekly return over the fiscal year t+1, and 0 otherwise. *ncrash, ncskew, duvol* and *minreturn* are the alternative proxies for stock price crash risk. The treatment variable is *SA*$_t$. In Columns (1), (2), (4), and (5), *ncskew*$_{t+1}$, the negative return skewness in year t+1, is controlled. In Column (3) where *ncskew*$_{t+1}$ is the dependent variable, *ncskew*$_t$, the negative return skewness in year t is controlled. All the variables are defined in Appendix 2.7.1. Industry dummies (constructed based on the first two digits of SIC codes) and year dummies are included in all the regressions but are not reported for simplicity. The *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 2.4: Test of H2: the moderating effect of abnormal accruals

Variables	Dependent Variable = *crashrisk*$_{t+1}$	
	Abnormal accruals (*da*)	
	Low	High
Intercept	2.8450***	2.2229***
	(<0.001)	(0.005)
***SA**$_t$*	**0.0472**	**0.1326*** **
	(0.205)	**(0.001)**
size$_t$	0.0643**	0.0362
	(0.019)	(0.222)
btm$_t$	8.37E-07	0.0216
	(1.000)	(0.137)
lanacov$_t$	0.0601**	0.0940***
	(0.032)	(0.002)
insti$_t$	0.0306	-0.1073
	(0.729)	(0.280)
roa$_t$	0.0097	-0.0017
	(0.128)	(0.847)
stdret$_t$	0.6728	-1.4854*
	(0.415)	(0.075)
tradevol$_t$	0.0040	0.0202
	(0.718)	(0.149)
opacity$_t$	-7.69E-06	0.0002**
	(0.858)	(0.013)
ncskew$_{t+1}$	0.0047***	0.0045***
	(0.004)	(0.005)
Industry-fixed effects	included	included
Year-fixed effects	included	included
No. of observations	10,644	10,651
Pseudo R-squared	0.1816	0.2298

Notes: This table presents the logistic regression results for the test of H2 as to the moderating effect of abnormal accruals (*da*) on the association between financial constraints and future crash risk. The sample period covers the years of 1995-2016. The dependent variable, *crashrisk*$_{t+1}$, equals 1 if a firm experiences one or more firm-specific weekly returns falling 3.2 standard deviations below the mean firm-specific weekly return over the fiscal year t+1, and 0 otherwise. The treatment variable is the SA index (*SA*$_t$). Our sample are partitioned, based on the sample median of *da*, into the high-abnormal-accruals subsample and low-abnormal-accruals subsample. All the variables are defined in Appendix 2.7.1. Industry dummies (constructed based on the first two digits of SIC codes) and year dummies are included in both regressions but are not reported for simplicity. The *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 2.5: Tests of H3: the moderating effect of corporate governance

Panel A. Descriptive statistics of corporate governance measures (2007-2015)

Corporate governance Variables	Obs.	Mean	Std. dev.	25th	Median	75th
directorownership	5,038	0.0146	0.0509	0.0015	0.0038	0.0086
indp	5,038	0.7972	0.1050	0.7273	0.8284	0.8889
boardsize	5,038	9.0389	2.2540	8	9	10
indpComp	2,473	0.4195	0.1217	0.3333	0.4000	0.5000
indpNomi	2,435	0.4274	0.1454	0.3333	0.4000	0.5000
indpAudit	2,474	0.4274	0.1052	0.3636	0.4286	0.5000
indpCG	2,370	0.4289	0.1465	0.3333	0.4000	0.5000
CEOduality	5,038	0.5123	0.4999	0	1	1
indpbusy	4,089	0.2396	0.1410	0.1250	0.2222	0.3333
olddirector	4,867	0.4356	0.1891	0.2857	0.4286	0.5714
directorchair	5,038	0.0981	0.2974	0	0	0
indpfemale	5,038	0.1151	0.0982	0	0.1111	0.1818
indpvotingpower	1,989	1.4062	4.1106	0	0	1
directorpredate	4,205	0.5785	0.2718	0.3571	0.6000	0.8000
staggered	5,038	0.4460	0.4971	0	0	1
longtenuredindp	4,087	0.3922	0.1878	0.2500	0.3750	0.5000

Panel B. Subsample test using outside directors' equity ownership (*directorownership*) as a measure of corporate governance

	Dependent Variable = $crashrisk_{t+1}$	
	Outside Directors' Equity Ownership	
Variables	Low	High
Intercept	1.9256	2.8167***
	(0.139)	(0.007)
$\boldsymbol{SA_t}$	**0.1824****	**0.1647**
	(0.031)	**(0.101)**
$size_t$	0.0337	0.1104
	(0.672)	(0.295)
btm_t	-0.0242	0.0495
	(0.883)	(0.777)
$lanacov_t$	0.1031	0.0722
	(0.369)	(0.482)
$insti_t$	0.0170	0.0685
	(0.898)	(0.701)
roa_t	0.7392	1.2133*
	(0.320)	(0.064)
$stdret_t$	5.3223	-1.4472
	(0.156)	(0.681)
$tradevol_t$	-0.0600	-0.0109
	(0.157)	(0.816)
$opacity_t$	0.0001	-0.0002
	(0.437)	(0.476)
$ncskew_{t+1}$	0.0035	0.0030
	(0.226)	(0.308)
Industry-fixed effects	included	included
Year-fixed effects	included	included
No. of observations	2,463	2,513
Pseudo R-squared	0.2518	0.2284

Panel C. Subsample tests using alternative measures for corporate governance

	Dependent Variable = *crashrisk*$_{t+1}$					
Corporate governance variables	*% of independent directors on board (indp)*		*Board size (boardsize)*		*% of independent directors on compensation committee (indpComp)*	
	Low	High	Small	Large	Low	High
SA$_t$	**0.2087****	**0.1247**	**0.2791*****	**0.1104**	**0.2449***	**0.2112**
	(0.018)	**(0.156)**	**(0.010)**	**(0.168)**	**(0.053)**	**(0.120)**
Controls	included	included	included	included	included	included
Industry-fixed effects	included	included	included	included	included	included
Year-fixed effects	included	included	included	included	included	included
No. of observations	2,515	2,511	2,155	2,876	1,252	1,161
Pseudo R-squared	0.2404	0.2777	0.2112	0.2826	0.0747	0.0563

	Dependent Variable = *crashrisk*$_{t+1}$					
Corporate governance variables	*% of independent directors on nominating committee (indpNomi)*		*% of independent directors on auditing committee (indpAudit)*		*% of independent directors on corporate governance committee (indpCG)*	
	Low	High	Low	High	Low	High
SA$_t$	**0.2178***	**0.2129**	**0.3932*****	**0.0706**	**0.2476***	**0.1579**
	(0.091)	**(0.118)**	**(0.002)**	**(0.595)**	**(0.086)**	**(0.230)**
Controls	included	included	included	included	included	included
Industry-fixed effects	included	included	included	included	included	included
Year-fixed effects	included	included	included	included	included	included
No. of observations	1,223	1,154	1,132	1,256	1,062	1,099
Pseudo R-squared	0.0792	0.0572	0.0646	0.0513	0.0784	0.0481

Panel C. (Continued)

	Dependent Variable = $crashrisk_{t+1}$					
Corporate governance variables	*CEO serving as chairman of the board (CEOduality)*		*% of busy independent directors (indpbusy)*		*% of directors over age 64 (olddirector)*	
	No	Yes	Low	High	Low	High
SA_t	**0.1503**	**0.1711****	**0.0619**	**0.3039*****	**0.3469*****	**0.0098**
	(0.108)	**(0.046)**	**(0.530)**	**(0.005)**	**(<0.001)**	**(0.910)**
Controls	included	included	included	included	included	included
Industry-fixed effects	included	included	included	included	included	included
Year-fixed effects	included	included	included	included	included	included
No. of observations	2,457	2,538	1,945	2,141	2,299	2,555
Pseudo R-squared	0.2592	0.2218	0.2957	0.2437	0.2375	0.2804

	Dependent Variable = $crashrisk_{t+1}$					
Corporate governance variables	*Chairman of board being independent director (directorchair)*		*% of female independent directors (indpfemale)*		*% of voting power by independent directors (indpvotingpower)*	
	No	Yes	Low	High	Low	High
SA_t	**0.2389*****	**-0.2588**	**0.0647**	**0.2365*****	**0.3315*****	**0.1829**
	(<0.001)	**(0.217)**	**(0.511)**	**(0.004)**	**(0.006)**	**(0.419)**
Controls	included	included	included	included	included	included
Industry-fixed effects	included	included	included	included	included	included
Year-fixed effects	included	included	included	included	included	included
No. of observations	4,544	434	2,220	2,767	1,401	537
Pseudo R-squared	0.2549	0.0798	0.2151	0.2686	0.2811	0.2478

Panel C. (Continued)

	Dependent Variable = $crashrisk_{t+1}$					
Corporate governance variables	*% of directors appointed before the current CEO took office (directorpredate)*		*Staggered board (staggered)*		*% of independent directors continuously serving the board for 10 years or more (longtenuredindp)*	
	Low	High	No	Yes	Low	High
SA_t	**0.2088****	**0.0986**	**0.2791*****	**0.0531**	**0.2199****	**0.0788**
	(0.028)	**(0.314)**	**(0.001)**	**(0.599)**	**(0.025)**	**(0.407)**
Controls	included	included	included	included	included	included
Industry-fixed effects	included	included	included	included	included	included
Year-fixed effects	included	included	included	included	included	included
No. of observations	2,133	2,059	2,788	2,209	1,956	2,113
Pseudo R-squared	0.2603	0.2663	0.2714	0.2029	0.2370	0.2709

Notes: Panel A presents descriptive statistics for the corporate governance variables used in the tests of H3 as to the moderating effect of corporate governance on the relation between financial constraints and future stock price crash risk. The corporate governance variables are constructed using the data from Institutional Shareholder Services (ISS) database, where the data cover the period starting from 2007. The sample period for the financial constraints (crash risk) variable ranges from 2007 (2008) to 2015 (2016). Panels B and C present the logistic regression results for the tests of H3. The dependent variable is the indicator variable, $crashrisk_{t+1}$. The treatment variable is the SA index (SA_t). The moderator variable used in Panel B is *directorownership*, which is measured by outside directors' equity ownership as a percentage of total shares outstanding. Our sample is separated into two subsamples based on whether an observation has value of *directorownership* higher than the sample median of *directorownership*. The high (low) *directorownership* subsample represents strong (weak) corporate governance group. The moderator variables used in Panel C are 15 alternative measures of corporate governance. Our sample is partitioned based on whether an observation has a value of the alternative, continuous measures of corporate governance higher than their sample medians, respectively. If the corporate governance measures are indicator variables, the sample is split based on the indicators. Observations that have low (high) values of *indp, boardsize, indpComp, indpNomi, indpAudit, indpCG, olddirector, directorchair, indpvotingpower, directorpredate*, *longtenuredindp*, *staggered* (*indpbusy, indpfemale*, *CEOduality*) are classified as having weak corporate governance. All the variables are defined in Appendix 2.7.1. The control variables included in all the regressions are the same as those included in model (2.4), but are not reported for brevity in Panel C. Industry dummies (constructed based on the first two digits of SIC codes) and year dummies are included in all the regressions but are not reported for simplicity. The *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 2.6: Test of H4: the moderating effect of corporate tax avoidance

Variables	Dependent Variable = $crashrisk_{t+1}$			
	Corporate Tax Avoidance			
	(1) *cashetr*		(2) *ddmpbtd*	
	Low	High	Low	High
Intercept	3.0398**	4.0049***	1.9288***	-1.0999**
	(0.014)	(0.001)	(0.005)	(0.029)
$\boldsymbol{SA_t}$	**0.0651**	**0.2372*** **	**0.0823** **	**0.0358**
	(0.114)	**(<0.001)**	**(0.047)**	**(0.329)**
$size_t$	0.0310	0.1533***	0.0788***	0.0740**
	(0.357)	(<0.001)	(0.008)	(0.012)
btm_t	-0.0374	0.0748**	0.0237*	0.0259
	(0.139)	(0.015)	(0.052)	(0.155)
$lanacov_t$	0.0654*	0.0541	0.0506*	0.0618**
	(0.053)	(0.124)	(0.077)	(0.027)
$insti_t$	-0.2059**	0.0318	-0.1704*	-0.1039
	(0.041)	(0.737)	(0.100)	(0.206)
roa_t	0.0157	-0.1619	-0.0470	0.0059
	(0.287)	(0.679)	(0.582)	(0.201)
$stdret_t$	0.1104	-0.4960	-1.4777**	-4.0657***
	(0.931)	(0.709)	(0.049)	(<0.001)
$tradevol_t$	0.0348*	0.0202	0.0149	-0.0022
	(0.082)	(0.214)	(0.218)	(0.827)
$opacity_t$	0.0001	0.0001	0.0001	0.0005***
	(0.131)	(0.315)	(0.126)	(0.000)
$ncskew_{t+1}$	0.0024	0.0058***	0.0063***	0.0012
	(0.159)	(<0.001)	(<0.001)	(0.375)
Industry-fixed effects	included	included	included	included
Year-fixed effects	included	included	included	included
No. of observations	9,093	9,083	10,205	10,209
Pseudo R-squared	0.1862	0.1798	0.2038	0.0742

Notes: This table presents the logistic regression results for the tests of H4 as to the moderating effect of corporate tax avoidance on the association between financial constraints and future crash risk. The sample period covers the years of 1995-2016. The dependent variable is the indicator variable, $crashrisk_{t+1}$. The treatment variable is the SA index (SA_t). The moderator variable is corporate tax avoidance, which is measured by cash effective tax rate (*cashetr*) and the Desai and Dharmapala's (2006) residual domestic book-tax difference (*ddmpbtd*) in Column (1) and (2). A lower (higher) value of *cashetr* (*ddmpbtd*) indicates a larger extent of corporate tax avoidance. Our sample is split into the high-tax-avoidance subsample and low-tax-avoidance subsample, based on the sample median of *cashetr* and *ddmpbtd*, respecitvely. All the variables are defined in Appendix 2.7.1. Industry dummies (constructed based on the first two digits of SIC codes) and year dummies are included in all the regressions but are not reported for simplicity. The *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 2.7: Test of H5: the moderating effect of credit ratings

Variables	Dependent Variable = *crashrisk*$_{t+1}$	
	Speculative-grade	Investment-grade
Intercept	3.2331**	3.2751***
	(0.017)	(<0.001)
SA$_t$	**0.1104****	**0.0441**
	(0.033)	**(0.542)**
size$_t$	0.0727	0.0484
	(0.151)	(0.408)
btm$_t$	-0.0001	0.0104
	(0.981)	(0.213)
lanacov$_t$	0.0442	0.0118
	(0.317)	(0.836)
insti$_t$	-0.2591**	-0.0198
	(0.043)	(0.882)
roa$_t$	-0.0591	0.0798
	(0.635)	(0.593)
stdret$_t$	1.8136	-1.0464
	(0.313)	(0.775)
tradevol$_t$	-0.0010	-0.0741
	(0.970)	(0.186)
opacity$_t$	2.10E-05	0.0003*
	(0.867)	(0.092)
ncskew$_{t+1}$	0.0023	0.0011
	(0.343)	(0.621)
rating$_t$	0.0107	-0.0639**
	(0.697)	(0.031)
Industry-fixed effects	included	included
Year-fixed effects	included	included
No. of observations	5,179	4,298
Pseudo R-squared	0.2204	0.1907

Notes: This table reports the logistic regression results for the test of H5 as to the moderating effect of credit ratings on the association between financial constraints and future crash risk. The sample period covers the years of 1995-2016. The dependent variable is the indicator variable, *crashrisk*$_{t+1}$. The treatment variable is the SA index (*SA*$_t$). Our sample is separated into low-credit-rating subsample and high-credit-rating subsample, based on whether a firm receive an investment grade or speculative grade from the S&P's credit rating agency in a year. Investment-grade firms are those rated at BBB- or higher; Speculative-grade firms are rated at BB+ or lower. All the variables are defined in Appendix 2.7.1. Industry dummies (constructed based on the first two digits of SIC codes) and year dummies are included in both regressions but are not reported for simplicity. The *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 2.8: A dynamic panel generalized method of moments (GMM) estimation

Variables	Dependent Variable = $crashrisk_{t+1}$
SA_t	**0.0882****
	(0.039)
$size_t$	0.0944**
	(0.042)
btm_t	0.0019
	(0.803)
$lanacov_t$	0.0166
	(0.685)
$insti_t$	0.3379**
	(0.024)
roa_t	0.0027
	(0.114)
$stdret_t$	3.2094
	(0.166)
$tradevol_t$	0.0061
	(0.550)
$opacity_t$	-0.0004**
	(0.016)
$ncskew_{t+1}$	-0.0146***
	(<0.001)
$crashrisk_t$	-0.1174
	(0.653)
$crashrisk_{t-1}$	-0.7339*
	(0.082)
No. of observations	13,633
AR(1) test (*p*-value)	<0.001
AR(2) test (*p*-value)	0.180
Hansen test of over-identification (*p*-value)	0.458

Notes: This table presents the GMM regression results for the test of H1. The sample period covers the years of 1995-2016. The dependent variable is $crashrisk_{t+1}$, as defined previously. The treatment variable is SA_t. All the variables are defined in Appendix 2.7.1. The instruments used in the GMM estimation include $Crashrisk_{i,t-2}$, $Crashrisk_{i,t-3}$, $SA_{i,t-3}$, $SA_{i,t-4}$, $Controls_{i,t-3}$, $Controls_{i,t-4}$, $\Delta YearDummies$, and Δ *IndustryDummie*s (Δ $Crashrisk_{i,t-2}$, Δ $SA_{i,t-2}$ Δ $Controls_{i,t-2}$, $YearDummies_{i,t}$, and $IndustryDummies_{i,t}$) in the differenced (level) equations. The industry dummies used in the GMM specification are based on the Fama-French's twelve industries. *AR(1)* and *AR(2)* are tests for first-order and second-order serial correlation in the first-differenced residuals in the model, under the null hypothesis of no serial correlation. The Hansen test of over-identification has a null hypothesis that all the instruments are valid. The *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 2.9: The effect of the junk-bond-market collapse (1989) on stock price crash risk

Variables	Dependent Variable = $crashrisk_{t+1}$
Intercept	-0.2774
	(0.653)
PostCollapse	0.0643
	(0.763)
Junk	-0.5150*
	(0.052)
PostCollapse×Junk	**0.7587****
	(0.018)
$size_t$	-0.1644***
	(0.005)
btm_t	0.0001
	(0.574)
$lanacov_t$	0.1239*
	(0.057)
roa_t	0.2515*
	(0.091)
$stdret_t$	0.0214
	(0.990)
$tradevol_t$	0.0274
	(0.150)
$ncskew_{t+1}$	-0.0015
	(0.720)
Industry-fixed effects	included
Year-fixed effects	included
No. of observations	2,360
Pseudo R-squared	0.0534

Notes: This table reports the logistic regression results of the difference-in-differences test for the effect of the junk-bond-market collapse on stock price crash risk. The dependent variable is $crashrisk_{t+1}$, as defined previously. The indicator variable, *PostCollapse*, equals 1 (0) if a sample firm is in the period of 1990-1992 (1987-1989). The indicator variable, *Junk*, equals 1 if a sample firm is rated with a speculative grade (BB+ or lower) by the S&P credit rating agency in a year, and 0 if a firm does not receive an S&P credit rating in a year. The interaction term, *PostCollapse*×*Junk*, is the DiD estimator. All the variables are defined in Appendix 2.7.1. Industry dummies (constructed based on the first two digits of SIC codes) and year dummies are included in the regression but are not reported for simplicity. The *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 2.10: The effect of the Internet bubble (1995-1999) on stock price crash risk

Variables	Dependent Variable = $crashrisk_{t+1}$
Intercept	-4.3543***
	(0.001)
Bubble	1.5550***
	(0.001)
FC	2.0047
	(0.294)
Bubble×FC	**-0.4882****
	(0.037)
$size_t$	0.5880***
	(<0.001)
btm_t	0.1170
	(0.235)
$lanacov_t$	-0.0986
	(0.382)
$insti_t$	-0.4845
	(0.367)
roa_t	0.5622
	(0.501)
$stdret_t$	-11.7588***
	(0.007)
$tradevol_t$	0.1165
	(0.541)
$opacity_t$	0.0359**
	(0.014)
$ncskew_t$	-0.0138***
	(<0.001)
Year-fixed effects	included
Industry-fixed effects	included
Firm-fixed effects	included
No. of observations	2,487
Pseudo R-squared	0.1207

Notes: This table reports the logit regression results of the difference-in-differences tests for the effect of the Internet bubble on stock price crash risk. The sample period for the DiD test is 1990-1999. Non-tech firms are those that do not have the first three digits of SICs of 355, 357, 366, 367, 369, 381, 382, or 384. The dependent variable is $crashrisk_{t+1}$, as defined previously. The indicator variable, *FC*, equals 1 (0) if a firm is a financially constrained (unconstrainted) non-tech firm that has the pre-bubble standardized mean of SA indices higher (lower) than the sample median. The indicator variable, *Bubble*, equals 1 (0) if a sample firm is in the Internet bubble (pre-bubble) period (1995-1999 (1990-1994)). The interaction term, *Bubble×FC*, is the DiD estimator. All the variables are defined in Appendix 2.7.1. Firm-fixed effects, alongside with industry dummies (constructed based on the first two digits of SIC codes) and year dummies, are included in the regression but are not reported for simplicity. The *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 2.11: The association between financial constraints and two-year- and three-year-ahead stock price crash risk

Variables	(1) Dependent Variable = *crashrisk*$_{t+2}$	(2) Dependent Variable = *crashrisk*$_{t+3}$
Intercept	2.4832***	2.6107***
	(<0.001)	(<0.001)
SA_t	**0.1223*** **	**0.0740** **
	(<0.001)	**(0.011)**
$size_t$	0.0743***	0.0411*
	(<0.001)	(0.066)
btm_t	-0.0034**	-0.0017
	(0.027)	(0.278)
$lanacov_t$	0.0691***	0.0701***
	(<0.001)	(0.001)
$insti_t$	-0.1544**	-0.1246
	(0.013)	(0.142)
roa_t	0.0030**	0.0007
	(0.039)	(0.921)
$stdret_t$	-0.9242*	-1.8951***
	(0.089)	(0.003)
$tradevol_t$	0.0180***	0.0076
	(0.004)	(0.324)
$opacity_t$	-6.54E-07**	-0.0001
	(0.014)	(0.422)
$ncskew_{t+2/t+3}$	0.0039***	0.0035***
	(<0.001)	(0.003)
Year-fixed effects	included	included
Industry-fixed effects	included	included
No. of observations	23,278	17,624
Pseudo R-squared	0.1848	0.1899

Notes: Column (1) ((2)) of this table reports the logistic regression results for the test of the association between financial constraints and two-year-(three-year-) ahead stock price crash risk. For the results in Column (1) ((2)), the sample period covers the years of 1995-2015 (1995-2014), and the dependent variable is $crashrisk_{t+2}$ ($crashrisk_{t+3}$). The treatment variable is the SA index (SA_t). $ncskew_{t+2}$ ($ncskew_{t+3}$) is the control for the same period negative weekly return skewness for $crashrisk_{t+2}$ ($crashrisk_{t+3}$). All the variables are defined in Appendix 2.7.1. Industry dummies (constructed based on the first two digits of SIC codes) and year dummies are included in both regressions but are not reported for simplicity. The *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Chapter 3

The Impact of Derivative Disclosures on Managerial Opportunism: Evidence from SFAS 161

3.1 Introduction

Financial derivatives have undergone significant development and been used increasingly by a wide array of firms over the last two decades. According to the Bank for International Settlements (BIS), the notional amount of outstanding over-the-counter (OTC) derivatives increased from $94 trillion at the end of June 2000 to $595 trillion at the end of June 2018. Nonetheless, managers use derivatives not only to hedge risks but also to pursue non-hedging activities such as speculation and earnings manipulation (Brown, 2001; Géczy et al., 2007; Chernenko and Faulkender, 2011). For example, Enron once used derivatives excessively to hide losses and inflate the value of its troubled business and continued to pay substantial amounts of bonus to its key executives in subsequent years (Bratton, 2002). Managers' incentives for opportunistic activities at the expense of outside investors induce the use of derivatives for non-hedging, opportunistic purposes. One possible way to restrain the use of derivatives for non-hedging purposes is requirements of firms to publicly disclose the purposes and strategies of their derivative use. The aim of our study is to examine whether such derivative disclosures, mandated by the Statement of Financial Accounting Standards No. 161 (henceforth, SFAS 161), [15] reduce managerial opportunism. We define managerial opportunism as managers' opportunistic behavior

[15] The Statement of Financial Accounting Standards No.161, *Disclosures about Derivative Instruments and Hedging Activities – An Amendment of FASB Statement No.133*, was issued by the Financial Accounting Standards Board (FASB) in year 2008. The Statement of Financial Accounting Standards No.133 (hereafter, SFAS 133), *Accounting for Derivative Instruments and Hedging Activities*, was issued by FASB in year 1998. SFAS 133 and SFAS 161 were codified under the Accounting Standards Codification Topic 815 (ASC 815) *Derivatives and Hedging* in year 2014.

that is detrimental to outside investors.

Previous literature documents that derivatives used for hedging reduce cash flow volatility (Froot et al., 1993), heighten earnings predictability (DeMarzo and Duffie, 1995), alleviate financial distress, and lower expected tax liabilities (Smith and Stulz, 1985). However, derivatives also serve non-hedging purposes such as earnings management and speculation (Brown, 2001; Faulkender, 2005; Géczy et al., 2007; Chernenko and Faulkender, 2011; Manchiraju et al., 2016, 2018), giving rise to a source of information uncertainty and/or asymmetry. Unfortunately, different managerial incentives for using derivatives cannot be easily distinguished, especially absent associated disclosures made in an adequate manner.

Before SFAS 161 was issued in March 2008, subject to the SFAS 133, firms were not transparent in disclosure as to their objectives and strategies of using derivatives. Inconsistent accounting treatments associated with the reasons for and ways of using derivatives leave financial professionals and investors a difficult task of interpreting the purposes of derivative use and its impact on firm valuation. Accordingly, SFAS 161 sought to enhance the transparency of firms' derivative disclosures. This standard requires firms to distinguish between derivatives *designated* as hedging instruments and derivatives *not designated* as hedging instruments, and provide tabular disclosures about the fair value of derivative assets and liabilities in the balance sheet and derivative-related gains and losses in the income statement; these are further classified into primary risk exposure categories such as interest rate, commodity, and foreign currency. Such accounting designation and disclosures are informative about whether firms use derivatives for hedging or for non-hedging purposes (Manchiraju et al., 2018), and can "*better convey the purpose of derivative use in terms of the risks that the entity is intending to manage*" (FASB, 2008). We use SFAS 161 to investigate whether and how the derivative disclosures affect managerial opportunistic behavior.

We put forward two arguments for the impact of SFAS 161 on managerial opportunism. First, asymmetry of the information about the purposes and strategies of using derivatives, and associated impacts on stock prices, exists between managers and outside investors before SFAS 161 was implemented. After its implementation, the enhanced derivative disclosures plausibly reduce such an information asymmetry, thereby making it less likely for managers to exploit investors' misperception, or uncertainty, about stock performance to act opportunistically at the expense of outside investors.

Second, previous literature suggests that derivatives generally reduce risk if used as hedging instruments and increase risk if used for speculation or other non-hedging purposes (Guay, 1999; Bartram et al., 2011), and that investors react positively to firms that use derivatives for hedging but not to firms that speculate (Koonce et al., 2008). The derivative disclosures, as prescribed by SFAS 161, may make managers discipline themselves by using derivatives more for hedging purpose than for opportunistic purposes. Thus, we expect SFAS 161 to induce firms to use derivatives more to hedge, reducing risk exposures and the associated probability of bad news events, and thereby preventing managerial opportunistic behavior.

We use two proxies for the managerial opportunism that is to the detriment of outside investors: (i) insider trades and (ii) firm-specific stock price crash risk (hereafter, crash risk).[16] First, we expect that a lower degree of information asymmetry and more efficient risk management in the post-SFAS 161 era reduce managerial incentives for insider trades. Insiders who previously have better knowledge about how derivative usage affects stock performance and trade on such information may not be able to do so anymore. Second, the reduced information asymmetry increases the costs and difficulty for managers to withhold bad news from outsiders and hence reduces the associated probability of a stock price crash. Since derivatives can serve non-hedging purposes such as earnings management which can be used as a means for managers to withhold bad news, more derivatives used for

[16] A vast literature (e.g., Jin and Myers, 2006; Hutton et al., 2009; Kim et al., 2011a) documents that firm-specific stock price crash risk is primarily attributed to managers hoarding bad news about their firms, which is detrimental to outside investors holding stocks of the firms. As with the literature, market-wide factors triggering stock price crashes are not within the scope of this study.

hedging purpose following SFAS 161 would also lessen the bad-news-hoarding behavior and associated stock price crash risk.

A plausible countervailing argument is that managers may falsify their purposes and strategies of derivative usage in the tabular disclosures that are made in compliance with SFAS 161. As such, SFAS 161 would not restrain managerial opportunism. Nonetheless, we surmise that such a case is less likely to take place, to the extent that a misrepresentation of information in a financial statement would attract substantive legal and reputational risks to managers and their firms.

Our empirical analysis is based on our hand-collected data on derivative disclosures by 1,191 U.S. listed firms in the non-financial and non-utility industries from 2006 to 2011. We employ a difference-in-differences regression model, in which treatment firms are defined as derivative users that make changes to their derivative disclosures to comply with SFAS 161, and control firms are defined as non-derivative-users which are unaffected by SFAS 161. We find that, after the adoption of SFAS 161, the reduction in insider trades and stock price crash risk is significantly greater for the derivative-using compliers than for the matched control sample of non-derivative-users. This finding supports our conjecture that the derivative disclosures prescribed by SFAS 161 reduce managerial opportunism.

We also conduct three cross-sectional tests to examine how the impacts of SFAS 161 differ by information opacity, financial risk, and business risk. Information opacity reflects lack of transparency of financial statements, which enables managers to continually conceal negative corporate information from outside investors (Jin and Myers, 2006; Hutton et al., 2009; Kothari et al., 2009). If SFAS 161 is effective in reducing the information asymmetry between insiders and outsiders, the extent to which SFAS 161 curbs managers' opportunistic activities should be greater for firms with high information opacity. On the other hand, SFAS 161 might encourage prudent risk management and prompt more derivative usage for hedging purposes, which would lead to lower firm risk. If SFAS achieves this end, we would expect that, in the case of firms facing with high financial risk and high business risk, SFAS 161 suppresses the use of non-hedging derivatives, thereby mitigating such risks and associated managerial opportunism, to a larger extent. Together, we expect that the impact of SFAS 161 is more pronounced for firms with high information opacity, high financial risk, and high business risk. Our empirical results are consistent with this expectation.

Compared with misstatements of information in a financial statement, insufficient disclosure therein entails relatively low litigation risk for a firm. Thus, managers may not comply with SFAS 161 by disclosing their objectives for using derivatives. Indeed, around 45% of the derivative-using companies in our sample do not comply with SFAS 161. We find no evidence that these non-compliers experience a greater reduction in either insider trades or stock price crash risk post SFAS 161, compared with a matched control sample of non-derivative-users. This suggests that SFAS 161 does not reduce managerial opportunism in the non-compliant firms.

This study makes four main contributions to the extant literature. First, a large body of derivative literature documents the determinants and consequences of derivative usage. Far less research attention has been paid to managerial incentives behind derivative usage and to the real consequences of derivative disclosures. Our study sheds light on these issues and is the first to provide evidence that disclosures of firms' objectives and strategies of using derivatives curb managerial opportunism.

Second, this study is the first to examine whether a derivative-related regulation helps curb managers' opportunistic behavior. Prior research (Kanodia and Sapra, 2016; Jayaraman and Wu, 2019) on the real effects of mandatory disclosures is limited. Kanodia and Sapra (2016) call for future research on the real economic consequences of accounting standards, in specific, that "*future research should focus on specific disclosure/accounting measurement rules and specific corporate decisions that are predicted to be affected*" (p.671). We respond to Kanodia and Sapra's call by showing that SFAS 161 suppresses insider trades and reduces bad news hoarding and associated crash risk.

Third, while previous studies on regulations focus on examining whether a particular regulation achieves its regulatory objectives (Leuz and Wysocki, 2016), our study complements this literature by shedding light on the side benefits of a regulation. Specifically, SFAS 161 has a side benefit of mitigating managerial opportunism, which goes beyond the regulatory objectives set by the regulators.

Fourth, our paper is the first corporate-level empirical study to account for issues about firms' compliance with a disclosure regulation. We find no evidence of a decrease in either insider trades or crash risk for the derivative users that do not comply with SFAS 161. Our study therefore calls for greater scrutiny on compliance with SFAS 161 so as to improve the transparency of firms' disclosures about their hedging decisions and to encourage more effective use of derivatives. External authorities and regulators should take stronger enforcement actions to ensure firms' compliance with the disclosure requirements to achieve positive regulatory outcomes. Such inferences and practical implications are generalizable to other financial reporting standards, and echo Leuz and Wysocki's (2016) call for research on the role of enforcement in disclosure regulations.

The remainder of this study is organized as follows. In Section 3.2, we develop our main hypotheses. Section 3.3 provides details of the data resources, sample selection, and variables. Section 3.4 explains research design. Section 3.5 discusses our empirical results. Section 3.6 conducts further analyses, and Section 3.7 concludes.

3.2 Hypothesis development

Hedge accounting allows companies, which use derivatives for hedging, to secure their income statements from the effect of adverse changes in interest rates, commodity prices, and foreign exchange rates, etc. One common example of cash flow hedges is a derivative contract that protects firms from potentially rising oil prices in the future. Derivatives are recorded at fair values at the reporting date in the balance sheet, and unrealized gains/losses from the derivative contract are reported as a component of other comprehensive income. Subsequently, any gain from buying oil at lower contracted prices are reclassified into earnings after the hedge expires. When any gain/loss in the fair value of derivatives cannot be completely offset by the loss/gain in the fair value of hedged items, the ineffective portion is reported in earnings immediately (FASB, 2008). If derivatives are not designated as hedges, the changes in fair values of these non-designated hedges are also recognized in earnings immediately. Considering the impact of hedge accounting on earnings, we argue that managers' choice of approaches to estimate the fair value of derivatives can be influential, and that managers may use derivatives to inflate earnings and conceal bad news.

Despite investors' common perception of derivatives is that derivatives are used as hedging instruments (Koonce et al., 2008), corporate scandals, such as Enron's extensive use of derivatives to boost revenues and managerial pay, suggest that it may not be the case. Nevertheless, Manchiraju et al. (2018) conjecture that some oil and gas companies possess private information about prospective development trends in their industry and engage in trading activities using derivatives for the purposes of generating profits from market price changes of commodities, and such derivatives are often not designated as hedging instruments.

SFAS 161 aims to enhance disclosures about (i) how and why a firm uses derivative instruments; (ii) how derivative instruments are accounted for; and (iii) how derivative instruments affect a firm's financial position, financial performance, and cash flow (FASB, 2008). This standard should increase the attention paid by investors to corporate derivative disclosures. SFAS 161 requires firms to distinguish derivatives designated as hedges and derivatives not designated as hedges in the tabular format. Manchiraju et al. (2018) argue that the accounting designation of derivatives is informative about the purposes and strategies of derivative use. They find that, while derivatives designated as hedges are negatively associated with firm risk, firms tend to use derivatives not designated as hedges to achieve or beat performance benchmarks, leading to higher firm risk. If SFAS 161 achieves its objectives, information asymmetry should be reduced, helping investors better evaluate the effect of derivative use on firm valuation and stock price volatility. As a consequence, the probability of managers exploiting investors' misperception and/or uncertainty about stock performance to behave opportunistically should be lowered.

As SFAS 161 provides useful information for assessing the effectiveness of derivative use for hedging, another argument about SFAS 161 is that it should encourage more active risk management by firms. Prior research documents mixed evidence on the effect of derivatives on firm value and risk (Guay, 1999; Adam and Fernando, 2006; Bartram et al., 2011; Gilje and Taillard, 2017). In general, derivatives, if used effectively for hedging purpose, reduce firm risk and increase firm value, however, they may increase risk if used for speculation and other non-hedging purposes. Thus, more active risk management via hedging in the post-SFAS 161 period would reduce firm risk and the associated likelihood of bad news. We expect that the improved derivative disclosures set forth in SFAS 161 will restrain managers from pursuing non-hedging activities and associated opportunistic behavior.

Managers may misrepresent their objectives and/or strategies of derivative use in their tabular derivative disclosures when complying with SFAS 161. However, this would subject managers and their firms to a substantially high risk of litigation and reputational losses, and is thus less likely to take place. On the premise that the disclosure mandate of SFAS 161 is effective in increasing the transparency about managers' derivative usage and in prompting more hedging activities via efficient and effective use of derivatives, we hypothesize that SFAS 161 would reduce managerial opportunistic behavior that is at the cost of external investors.

To investigate our general hypothesis, we use insider trades and stock price crash risk as two specific proxies for the managerial opportunism. Firstly, as previous literature (e.g., Ke et al., 2003; Huddart and Ke, 2007; Huddart et al., 2007; Skaife et al., 2013) suggests, insiders have an incentive to exploit their informational advantage to generate abnormal gains from trading the securities of their firms. The profitability from insider trades increases with the degree of information asymmetry between insiders and outsiders (Huddart and Ke, 2007). The enhanced derivative disclosures required by SFAS 161 should help investors better understand the effect of firms' derivative use on stock price movements, leading to fewer opportunities for insiders to gain from their privileged information.

In addition, more transparent disclosure as to the objectives and strategies of derivative usage would likely induce managers to use derivatives more for hedging and less for non-hedging purposes, leading to more effective risk management. If so, firms' risk exposures will decrease and firm value will increase (Bartram et al., 2011; Gilje and Taillard, 2017). The opportunity costs (i.e., reputational costs and compensation losses) for managers to engage in insider trades are likely to be higher for better-performing firms whose risk exposures are lowered by derivative hedging. Therefore, we expect that the enhanced disclosures of derivatives after SFAS 161 will lead to fewer insider trades, and accordingly, establish our first hypothesis as follows:

H1: *Firms that follow SFAS 161 to provide tabular disclosures of derivative usage experience a decrease in insider trades.*

Second, more transparent and informative disclosures of derivative usage are likely to reduce information asymmetry and help investors better correct for mispricing, thereby lowering the probability of a stock price crash. Also, the reduced information asymmetry increases the difficulty managers have in withholding bad news of a firm. As documented in the crash risk literature (e.g., Chen et al., 2001; Hutton et al., 2009; Kim et al., 2011a, b; He, 2015; Zhu, 2016), the probability of stock price crashes would become high for the sake of bad-news-hoarding behavior. The more bad news withheld, the larger degree of stock overvaluation, and the higher likelihood of a stock price crash for firms. Thus, we predict that the reduced information asymmetry in the post-SFAS 161 period leads to lower stock price crash risk.

The complexity of derivative use and associated higher level of information asymmetry also create agency tension between managers and shareholders. Managers that possess private information about their firm tend to hide bad news from outside investors for an extended period (Kothari et al., 2009). Previous research (e.g., Pincus and Rajgopal, 2002; Chernenko and Faulkender, 2011; Manchiraju et al., 2018; He and Ren, 2019) suggests that derivatives can serve as earnings manipulation devices to facilitate managers' withholding bad news. For instance, using interest rate swaps, firms can manage earnings via interest expense, specifically, by altering their interest rate exposures when there is a large difference in current interest payment between the fixed interest rate and the floating interest rate (Faulkender, 2005). Firms can inflate earnings and hide losses by lowering the interest expense via a favored (lower) interest rate. In contrast, if derivatives are used for hedging, and downside risks are hedged away (Gilje and Taillard, 2017), bad news and associated hoarding malpractices will be lessened, thereby leading to lower stock price crash risk. Therefore, to the extent that SFAS 161 helps outside investors better understand the purposes and strategies of derivative usage, and increases (decreases) firms' use of derivatives for hedging (non-hedging), stock price crash risk should decrease following the passage of SFAS 161. This leads to our second hypothesis:

H2: *Firms that follow SFAS 161 to provide tabular disclosures of derivative usage experience a reduction in stock price crash risk.*

3.3 Sample construction

3.3.1 Data and sample selection

Our empirical analysis is based on a sample of U.S. firms in non-financial and non-utility industries. As with some previous studies (e.g., Donohoe, 2015; Chang et al., 2016), we exclude firms from financial industries (two-digit SIC codes 60-69) and utility industry (two-digit SIC code 49), because these firms often act as derivative dealers and are subject to different financial reporting requirements. Since SFAS 161 was issued in 2008 and is effective for annual reporting periods starting after 15 November 2008, companies generally started applying this standard from the fiscal year 2009. Accordingly, our sample period spans years 2006-2011, covering the three-year pre-SFAS 161 period (i.e., 2006-2008) and post-SFAS 161 period (i.e., 2009-2011).

Insider trading data are obtained from Thomson Financial Insider Research Services Historical Files and include stock transactions by directors and officers only. Financial statement data and stock information come from Compustat and *Center for Research in Security Prices (*CRSP). To constitute our sample, we begin with all non-financial and non-utility firms with available data on Compustat for the fiscal years 2006-2011. A company is included in our sample if it has data for at least three consecutive years including years 2008 and 2009. We exclude firm-year observations with negative values of total assets or with missing data on the market value of firm equity. We also exclude cases for which stock return (analyst forecast) data are not available on CRSP (Institutional Brokers Estimate System).

The tabular disclosures of whether derivatives are designated as hedging instruments are hand-collected from 10-K filings in the Securities and Exchange Commission's EDGAR files (see tabular disclosures in the Kadant Inc.'s 2010 annual report in Appendix 3.8.2, for example). Keywords such as "designated", "derivative", "hedge", "risk", "SFAS No. 133", "SFAS No. 161" are used for our screen search. One of the most apparent changes made per SFAS 161 is requirements of derivative users to provide tabular disclosures on derivatives under two broad titles, "derivatives *designated* as hedges" and "derivatives *not designated* as hedges", in the notes to financial statements.[17]

3.3.2 Construction of treatment and control groups

From a close look at the derivative disclosures in firms' 10-K reports, we find that not every firm using derivatives provides tabular disclosures on derivative instruments segregated by types of risk exposures as required by SFAS 161, although this standard

[17] The titles can also be "designated hedges" and "non-designated hedges".

is mandatory and applies to all derivative-using entities. In line with Drakopoulou's (2014) finding that "*most companies failed with the requirements of SFAS No. 161 to disclose required information*", approximately 45% of the derivative-using companies in our hand-collected sample do not provide tabular disclosures distinguishing between designated and non-designated hedges in the three-year post-SFAS 161 period (2009-2011). Thus, we categorize our sample firms into three groups: compliers (395 firms), non-compliers (332 firms), and non-users (464 firms).[18]

Compliers are defined as derivative using firms that follow SFAS 161 to provide tabular disclosures distinguishing between derivatives *designated* and *not designated* as hedging instruments. For a derivative-using firm to be classified as a complier in our treatment sample, designation of derivatives use must be made in the tabular disclosures in the three-year post-SFAS 161 period. Firms that do not use derivatives in any year during our sample period, either before or after SFAS 161, are named non-users. They are not affected by the standard, thus satisfying the condition of being classified into a control group for a difference-in-differences analysis. Following previous literature (e.g., Donohoe, 2015; Chang et al., 2016), we define our control sample as consisting of non-users, as opposed to our treatment sample of compliers.

To capture the treatment effect of SFAS 161 on managers' opportunistic behavior, we need to compare firms, which use derivatives *and* apply SFAS 161, with firms that are completely unaffected by the regulation, i.e., the non-users who do not use derivatives in any year during our sample period. The non-compliers identified in our sample cannot be used as control firms, because the comparison between the compliers and non-compliers relates to managers' decision to comply or not comply with SFAS 161, which would induce self-selection bias. Or rather, if the noncompliers

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[18] Both compliers and noncompliers pertain to firms that use derivatives in at least one year in both the pre-SFAS 161 period (i.e., 2006-2008) and the post-SFAS 161 period (2009-2011). In our initially collected data, there are 17 out of 1,208 (i.e., 1.4%) unique firms used derivatives prior to SFAS 161 (2005-2008) but not after (2009-2011). Any firm, which stops (or starts) using derivatives as a result of SFAS 161 implemented in 2008, is excluded from our sample. As such, the effect of SFAS 161 on a firm's choice of whether to use derivatives would not confound our analysis and results.

are used for the control group, firms which tend to be opportunistic are less likely to adopt the standard, thereby self-selecting to the control group. As such, the decision to not comply is mechanically correlated with our dependent variables. To avoid this problem, we define compliers as our treatment sample and non-users as our control sample. After excluding missing data, we obtain 2,762 firm-year observations for insider trades and 2,949 firm-year observations for crash risk, corresponding with 712 (373 compliers and 339 non-users) and 725 (379 compliers and 346 non-users) unique firms, respectively. The summary statistics for variables are presented in Table 3.1. Although the decision to use or not use derivatives is unrelated to the changes in the disclosure requirements, we further eliminate potential selection bias by applying a propensity-score-matching approach, which is covered in Section 3.4.1.

3.3.3 Measures of managerial opportunism

We employ two proxies for managerial opportunism in our hypothesis tests. The first is insider trading. We measure insider trades (*INSITRADE*) as the natural logarithm of the total dollar volume of insider sales and insider purchases made by all directors and officers of a firm over a fiscal year.[19] Missing values of insider trading are set as zero.

Our second measure of managerial opportunism is stock price crash risk. The crash risk literature (e.g., Jin and Myers, 2006; Hutton et al., 2009) argues that managers' bad news hoarding is the fundamental cause of stock price crashes. Managers can conceal bad news from outside investors for an extended period. But when the accumulated bad news eventually exceeds a limit, a sudden crash in stock prices will occur. Following Hutton et al. (2009) and Kim et al. (2011a), we use an indicator variable (*CRASH*) to capture the likelihood of extremely low firm-specific

[19] We also use insider trading profitability as an alternative measure of managerial opportunism. It is calculated as per Skaife et al. (2013). All our main results are robust to using this measure in the analysis. We do not report these results because insider trading profitability is a noisy measure. Insiders' transactions are largely insider sales. When calculating the profitability of insider sales, we cannot distinguish whether the shares sold are originally granted by the firms or bought by insiders from open market.

weekly returns in the one-year-ahead measurement window. Firm-specific weekly return is defined as the natural logarithm of one plus the residual return, $\varepsilon_{i,\tau}$, from the following regression model, adjusted for market-wide factors:

$$r_{i,\tau} = \alpha_i + \beta_{1i} r_{m,\tau-2} + \beta_{2i} r_{m,\tau-1} + \beta_{3i} r_{m,\tau} + \beta_{4i} r_{m,\tau+1} + \beta_{5i} r_{m,\tau+2} + \varepsilon_{i,t} \tag{3.1}$$

where $r_{i,\tau}$ is the return on stock i, and $r_{m,\tau}$ is the return on the CRSP value-weighted market index, in week τ. Accordingly, *CRASH* equals 1 for a firm that experiences one or more firm-specific weekly returns falling 3.2 standard deviations below the mean firm-specific weekly return over a fiscal year, and 0 otherwise.

3.4 Research design

3.4.1 Matching of sample between treatment and control groups

Our main research specification is a difference-in-differences (hereafter, DID) regression model. DID analysis is a common approach to get around time trends or structure changes that may affect all treatment companies. To this end, we contrast the changes in our outcome variables (i.e., insider trades and stock price crash risk) observed in our treatment firms after the adoption of SFAS 161 with those observed in our control firms which are unaffected by the standard. The treatment and control samples are defined as in Section 3.3.2.

To mitigate potential selection bias, we use a propensity-score-matching approach (e.g., Irani and Oesch, 2013, 2016; Hasan et al., 2014; Ke et al., 2019) to match a complier with a non-user. We estimate propensity scores from a logistic regression of derivative usage on its determinant variables measured prior to SFAS 161. Prevailing literature shows that derivatives are more likely to be used by large firms (e.g., Nance et al., 1993; Mian, 1996; Géczy et al., 1997; Haushalter, 2000; Graham and Rogers, 2002), high-growth firms (e.g., Géczy et al., 1997), financially constrained firms (Acharya et al., 2007), firms with high financial leverage (Tufano, 1996; Haushalter, 2000; Graham and Rogers, 2002), and firms that have high cash flow volatility (Froot et al., 1993; Minton and Schrand, 1999; Bartram et al., 2011) or high dedicated institutional stock holdings (e.g., Bodnar et al., 2003; Chang et al., 2016). Hence, we use market value of equity (*SIZE*), book-to-market ratio (*BTM*), financial leverage (*LEV*), financial constraints (*SA*), cash flow volatility (*STDCFO*), and dedicated institutional stock holdings (*DEDI*) as our matching covariates. All the covariates are measured in years before the implementation of SFAS 161 (i.e., 2006-2008) to avoid the matching being affected by the event. Panel A of Table 3.2 presents the results from the logistic regression on the six matching covariates. For both insider trades sample and crash risk sample, there are five covariates that have statistically significant coefficients.

We match each treatment firm with a control firm by using the closest propensity score within a caliper of 1%. Because we have a relatively small sample with treatment firms more than control firms, we allow replacement in the matching so that a control firm can be matched more than once with a treatment firm. Matching with replacement in this case can improve the quality of matching, ensure the statistical power, and reduce bias (Caliendo and Kopeinig, 2008; Shipman et al., 2017). After applying our propensity-score matching, we check the balance of covariates between the treatment and control groups by conducting standard t-tests and calculating standardized bias.

Panel B of Table 3.2 reports the results for our covariate balance check. The t-statistics from the two-sample t-test of mean differences show that the covariates in the treatment group in general do not differ significantly from those in the control group. Another way to evaluate covariate balance is to examine the standardized bias for each covariate using Rosenbaum and Rubin's (1985) formula. The last column in Panel B shows that none of the covariates has standardized bias greater than 10%, suggesting that the matching procedure effectively reduces the covariate imbalance between the treatment and control groups in our sample. After the matching, we end up with 3,040 and 3,294 firm-year observations for the insider trading sample and the crash risk sample, corresponding to 746 and 758 firms, respectively.

3.4.2 Difference-in-differences regression specification

To test H1 and H2, we use the following difference-in-differences regression models:

$$\begin{aligned} INSITRADE_{i,t} = {} & \alpha_0 + \alpha_1 TREAT_i + \alpha_2 POST_t + \alpha_3 TREAT_i \times POST_t \\ & + \sum_k \alpha_k CONTROLS_{i,t}^k + \sum_z \alpha_z IND_i^z + \sum_t \alpha_t YR_{i,t}^t + \varepsilon_{i,t} \end{aligned} \tag{3.2}$$

$$\begin{aligned} CRASH_{i,t+1} = {} & \beta_0 + \beta_1 TREAT_i + \beta_2 POST_t + \beta_3 TREAT_i \times POST_t \\ & + \sum_k \beta_k CONTROLS_{i,t}^k + \sum_z \beta_z IND_i^z + \sum_t \beta_t YR_{i,t}^t + u_{i,t} \end{aligned} \tag{3.3}$$

Models (3.2) and (3.3) specify insider trades and one-year-ahead stock price crash risk,[20] respectively, as the dependent variable. The treatment indicator variable, *TREAT*, equals 1 for a treatment firm and 0 for a control firm. Because SFAS 161 was effective for annual reporting periods commencing after November 15th, 2008, all our treatment firms start applying this standard from the fiscal year 2009. Accordingly, the time indicator variable, *POST*, is equal to 1 if a firm is in a fiscal year during the post-SFAS 161 period (i.e., 2009-2011), and 0 if it is in the pre-SFAS 161 period (i.e., 2006-2008). The variable of interest to our hypothesis tests is the interaction term, $TREAT_i \times POST_t$. Its coefficient captures the impact of SFAS 161 on insider trades and stock price crash risk for the compliers relative to the non-users. Larger difference-in-differences estimators (α_3 in model (3.2) and β_3 in model (3.3)) indicate greater impacts of SFAS 161 in reducing insider trades and crash risk. Hence, to support H1 and H2, the coefficients for the interaction terms should be negative and statistically significant at conventional levels.

We include a range of control variables in models (3.2) and (3.3) based on previous literature. Regarding the control variables for insider trades, we consider firm size (*SIZE*) because corporate insiders trade more actively in large firms (Lakonishok and Lee, 2001). Piotroski and Roulstone (2005) find that insider trading is positively associated with future firm performance and growth prospect. Thus, we include return

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[20] Following the crash risk literature (e.g., Kim et al., 2011a, b; Callen and Fang, 2013; Zhu, 2016), we measure the likelihood of future stock price crashes in a one-year-ahead forecast window.

on assets (*ROA*) and book-to-market ratio (*BTM*) as controls. Because it is easier for insiders to trade on stocks with low transaction costs, insider trades should increase with a decrease in transaction costs, which are measured by trading volume (*TRADEVOL*) (e.g., Mendenhall, 2004). *TRADEVOL* is also a proxy for stock liquidity, which is expected to be positively related to insider trades. We also include analyst coverage (*LANACOV*) and dedicated institutional ownership (*DEDI*) as controls for external monitoring on insiders' opportunistic trading behavior. Insiders are expected to trade less in firms with more analyst following (Frankel and Li, 2004) or higher dedicated institutional ownership (Chen et al., 2007; Skaife et al., 2013). We also include cash flow volatility (*STDCFO*) and firm age (*FIRMAGE*) to further control for the impact of information asymmetry on insider trades (Huddart and Ke, 2007); *STDCFO* (*FIRMAGE*) is expected to be positively (negatively) related to insider trades.

As regards the control variables for crash risk in model (3.3), we include firm size (*SIZE*), book-to-market ratio (*BTM*), analyst coverage (*LANACOV*), dedicated institutional ownership (*DEDI*), return on assets (*ROA*), trading volume (*TRADEVOL*), cash flow volatility (*STDCFO*), corporate tax avoidance (*CETR*), and negative skewness of firm-specific weekly returns (*NCSKEW*). Large firms and high-growth firms are more likely to experience stock price crashes (Harvey and Siddique, 2000; Chen et al., 2001; Hutton et al., 2009), hence *SIZE* (*BTM*) should be positively (negatively) correlated with crash risk. Previous studies (e.g., Kothari et al., 2009; Kim et al., 2011a; He and Tian, 2013) show that financial analysts may pressure firm management into concealing bad news in order to meet their earnings forecasts, and that institutional investors seek to monitor management in a way that prevents it from hoarding bad news about firms. Therefore, we expect that *LANACOV* (*DEDI*) is positively (negatively) associated with stock price crash risk. Profitable firms are less prone to a stock price crash (e.g., Hutton et al., 2009). So, we control for return on assets (*ROA*) and expect it to have a negative association with crash risk. We include trading volume (*TRADEVOL*), an inverse measure of stock liquidity, in the regression because Chang et al. (2017) find that liquid stocks are more likely to collapse in stock prices. Kim et al. (2011a) provide evidence to suggest that corporate tax avoidance facilitates managerial rent extraction and bad news hoarding. Thus, we also control for tax avoidance (*CETR*), which is measured by the cash effective tax rate as per Dyreng et al. (2010) and Hanlon and Heitzman (2010). A lower value of *CETR* represents a higher degree of tax avoidance and thus should be associated with higher crash risk. Chen et al. (2001) find that firms with high return skewness in year t-1 are more likely to have high crash risk in year t. Thus, we also control for negative skewness of weekly returns (*NCSKEW*). We further include year dummies (*YR*) and industry dummies (*IND*) in the regressions since insider trades and crash risk are likely to vary systematically across years and industries. Definitions of all the foregoing control variables are detailed in Appendix 3.8.1.[21]

The parallel trends assumption behind the DID research design requires similar trends in the outcome variable for both treatment and control groups prior to the treatment event (Roberts and Whited, 2013); this assumption denotes that, in the absence of treatment, the average change in the outcome variable would have been the same for both treatment and control groups. To test the validity of the assumption, we first compare annual growth rates in insider trades and crash risk of the treatment firms with those of the control firms for our pre-event sample period (i.e., 2006-2008). The growth rate is computed as: a change in insider trades (crash risk) from the previous year to the current year, divided by insider trades (crash risk) in the previous year. Results from standard t-tests (un-tabulated) show that the growth rates in insider trades (crash risk) of the treatment firms are statistically indifferent from those of the control firms in 2006, 2007, and 2008, respectively. Furthermore, we re-run our DID regression models (3.2) and (3.3) by using 2005 and 2006 (as well as 2006 and 2007 or 2007 and 2008) as the pre- and post-treatment periods, respectively. In our results (un-tabulated), we do not find any significant change in insider trading or crash risk

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[21] Our results remain qualitatively the same if financial constraint (*SA*) is included as a control variable in our baseline regression model (3.3). We expect financial constraints to be positively associated with crash risk (He and Ren, 2019).

for the treatment firms relative to the control firms. The foregoing results are all supportive of the parallel trends assumption not being violated in our DID regression analysis.

3.5 Empirical results

3.5.1 Main results for hypotheses 1 and 2

Table 3.3 presents the main results for our hypotheses. Column (1) shows that the coefficient on the interaction term, $TREAT_i \times POST_t$, is significantly negative at the 1% level (p-value <0.001). The coefficient for $TREAT_i \times POST_t$ amounts to 1.5940, which accounts for 32% of the mean of *INSITRADE* for the treatment sample and thus is economically significant. These results indicate that insider trades in the compliers decline more significantly after the adoption of SFAS 161, relative to the non-users that are not affected by the standard. Thus, H1 is supported.

Column (2) shows a similar result of a statistically significant coefficient on $TREAT \times POST$ with a negative sign (p-value=0.020), indicating that, compared with the non-users, the compliers experience a greater reduction in the one-year-ahead stock price crash risk post SFAS 161. This result supports H2 and is consistent with our conjecture that SFAS 161 improved the information transparency for outsiders and encouraged prudent risk management with a greater use of derivatives for hedging purposes, thereby restraining insider trades and lowering stock price crash risk. The marginal effect of $TREAT_i \times POST_t$ for crash risk amounts to 5.99 percentage points, which is equivalent to 30% of the mean of *CRASH* for the treatment sample and hence is economically significant.

We also conduct the variance-inflation-factors (VIF) test to check the potential multicollinearity concern on our regression estimations. The un-tabulated results show that the VIF value is less than 5 for all the explanatory variables, indicating that multicollinearity is unlikely to be an issue in our regression analysis. Overall, our results corroborate that the enhanced derivative disclosures, as prescribed by SFAS

161, are effective in reducing managerial opportunism.

3.5.2 Check of robustness of main results

Anticipation effects

Before SFAS 161 took effect, it is possible that some derivative users anticipated the regulatory change and disclosed the purposes of their derivative usage voluntarily. With such an anticipation, managers in these firms might refrain from behaving opportunistically in advance of the regulatory event. This might alternatively explain our main findings. To mitigate this concern, we first look through the 10-K reports of all treatment firms and ensure that none of them provides the tabular disclosures pursuant to SFAS 161 before it was implemented for the fiscal year 2009. Second, we re-run the DID regression models (3.2) and (3.3), using 2005-2007 and 2008-2010 as pre- and post-event periods, respectively, in order to test whether there is a foregoing anticipation effect in 2008, the year before SFAS 161 was adopted. In our regression results (un-tabulated), we find no statistically significant result for the DID estimators, suggesting that the anticipation effect is unlikely to be at play to drive our main results.

Financial crisis

A potential countervailing force that might weaken the inference from our main results is the impact of the recent financial crisis, which, as documented in Chang (2011) and Boyallian and Ruiz-Verdú (2018), lasts from 2007 to 2010. Nevertheless, since the SFAS 161 event stands at the midpoint of the crisis period of 2007-2010 (i.e., the end of 2008), the effect of the crisis should not confound our results. To further address the concern, we conduct placebo tests. Specifically, we use 2009-2010 as the crisis period and 2011-2012 as the post-crisis period to re-run our DID regression models and then analyze the treatment effects of the financial crisis on our managerial opportunism variables. Provided that the effect of financial crisis is more evident during 2007-2008 than in 2009-2010, the same would be true for 2009-2010 relative to 2011-2012. On this basis, if we get statistically significant results for the DID estimators in this placebo test, financial crisis would play a role in explaining the reduction in managerial opportunism post SFAS 161. However, our results in Columns (1) and (2) of Table 3.5 show that the coefficients on the interaction terms of our re-run DID regressions are statistically insignificant.

We conduct another placebo test by using 2005-2006 as the pre-crisis period and 2007-2008 as the crisis period to re-run our DID regressions. If financial crisis explains higher managerial opportunism prior to the implementation of SFAS 161, we should find positive and statistically significant results on the DID estimators. Nonetheless, we do not find such evidence: the coefficients on the interaction term, $TREAT_i \times POST_t$, are statistically insignificant in Columns (3) and (4) of Table 3.5. Collectively, the results of our placebo analysis suggest that our earlier finding of the reduced managerial opportunism is attributed to SFAS 161 rather than financial crisis.

Exogeneity of SFAS 161

When studying accounting regulations, some might argue that some regulatory changes are responses to the capital market's demand for the changes and are not genuinely exogenous. Nevertheless, the implementation of SFAS 161 is not a result of public pressure from outside investors knowing and concerning that managers use derivatives for insider trades or for bad news hoarding that leads to stock price crashes. Or rather, SFAS 161 is not issued due to managers using derivatives for opportunistic reasons, and hence is exogenous to managerial opportunism.

To confute the possibility that the promulgation of SFAS 161 is a response to a significant increase in managerial opportunism, we re-do both the univariate and multivariate tests of parallel trends assumptions, as in Section 3.4.2, by extending our pre-SFAS 161 period to years 2003-2008. The un-tabulated results show that, in this pre-SFAS 161 period, there is no evidence of a statistically significant increase in either insider trades (*INSITRADE*) or stock price crash risk (*CRASH*); insider trades and crash risk do not increase substantially or peak prior to SFAS 161. This thus implies that the implementation of SFAS 161 is not endogenous to managers' opportunistic behavior in our study.

Firm-fixed effects

Although our baseline regression models (3.2) and (3.3) control for an extensive list of the determinants of insider trades and stock price crash risk, alongside with industry-fixed effects, we cannot exclude the possibility that our regressions might still omit some unobserved firm characteristics that also affect our outcome variables. To ease this concern, we re-estimate our DID models by including firm-fixed effects therein.[22] Table 3.4 presents the results. In Columns (1) and (2), the coefficients for the interaction terms are significant at the 1% level with the negative sign, suggesting that our previous finding of the negative impact of SFAS 161 on managerial opportunism is unlikely to be driven by omitted time-invariant factors. We also run a firm-fixed effects model that includes only $TREAT_i$, $POST_t$, $TREAT_i \times POST_t$, and year dummies. Results are shown in Columns (3) and (4) and elicit the same inferences as do the results in Columns (1) and (2).

3.6 Further tests

3.6.1 Cross-sectional analyses of the effect of enhanced derivative disclosures on managerial opportunism

This section further investigates whether SFAS 161 reduces managerial opportunism

[22] One key assumption underlying the firm-fixed-effects regression model is sufficient time-series variation in the dependent variable. When including firm-fixed effects in our models, observations that have no time-series variance in the dependent variable are omitted from the regression estimation. For this reason, the sample involving the firm-fixed-effects regression for model (3.3), where the dependent variable is an indicator variable of crash risk (*CRASH*), drops to 1,739 observations.

to a higher degree for firms with high information opacity, high financial risk, and high business risk. The opacity is referred to as the degree of low transparency about a firm's financial reporting and disclosures. A lack of such information transparency enables managers to conceal bad news or malpractices from outside investors for an extended period (Jin and Myers, 2006), hence the probability of stock price crashes for these firms will be higher. The likelihood and extent of insider trading are also higher because the profits managers can obtain from insider trading are greater when information opacity is high (Huddart and Ke, 2007). Provided that SFAS 161 alleviates the information asymmetry and helps investors better assess the implications of derivative usage in their stock valuations, the influences of SFAS 161 on insider trades and crash risk should be greater for firms with high information opacity.

Risk management theory suggests that firms use derivatives with an aim to reduce financial risk and/or business risk. The higher the financial risk (the probability of default on debt and the probability of bankruptcy) of a firm, the higher the benefits it can get from hedging. As discussed previously, SFAS 161 is likely to direct managers to use derivatives more for hedging than for non-hedging purposes. Active risk management via hedges decreases financial risk, lessens associated bad news, and reduces investor uncertainty about stock performance. Thus, if SFAS 161 is effective in inducing firms to use derivatives to hedge against financial risk, we expect that the attenuating impacts of SFAS 161 on insider trades and crash risk are more pronounced for firms with high financial risk.

Business risk is the overall risk inherent in a firm and is independent of the way the firm is financed (Gabriel and Baker, 1980). Firms with high business risk are typically characterized by high volatility of net operating income, and tend to have high financial risk because of great variability in the capacity to repay. In a similar vein, we expect that effective hedging reduces business risk and the associated probability of bad news and decreases investor uncertainty about firm prospects. Therefore, if SFAS 161 is effective in directing firms to use derivatives to hedge against business risk, the mitigating effects of SFAS 161 on insider trades and crash risk should be stronger for firms with high business risk.

To test our predictions, we split our full samples into two subsamples based on the medians of variables for information opacity, financial risk, and business risk, respectively. We then match each treatment firm with a control firm in each subsample using the same propensity-score-matching approach as in Section 3.4.1, and estimate models (3.2) and (3.3) for each post-matching subsample. Following Hutton et al. (2009), we measure information opacity (*OPACITY*) as the three-year moving sum of absolute discretionary accruals, which capture the multi-year effects of potential earnings management. We use financial leverage (*LEV*) as the proxy for financial risk (e.g., Smith and Stulz, 1985; Tufano, 1996; Haushalter, 2000) and earnings volatility (*STDEARN*) as the measure of business risk (e.g., Abdel-khalik and Chen, 2015); both variables are defined in Appendix 3.8.1.

Table 3.6 reports the results for the test of the moderating effect of information opacity. Neither of the coefficients on the interaction term, $TREAT_i \times POST_t$, is statistically significant for the low-opacity subsamples, whereas the coefficients on $TREAT_i \times POST_t$ are significant for the high-opacity subsamples. The effects of SFAS 161 in reducing insider trades and stock price crash risk are more evident for firms with high financial opacity. This thus strengthens our earlier proposition that SFAS 161 is effective in reducing the information asymmetry and thereby deterring managerial opportunism.

Table 3.7 and Table 3.8 present the results from testing the moderating effects of financial risk and business risk. Table 3.7 shows that the coefficients on $TREAT_i \times POST_t$ are statistically significant and negative in the high-financial-risk subsamples, whereas $TREAT_i \times POST_t$ does not have a statistically significant coefficient for either of the low-financial-risk subsamples. In Table 3.8, the coefficients on $TREAT_i \times POST_t$ are statistically significant and negative in the high-business-risk subsamples, but not statistically significant in the low-business-risk subsamples.[23] Together, these results indicate that the attenuating effects of SFAS 161 on insider trades and crash risk are stronger for firms with high financial risk and high business risk. This reinforces our earlier argument that SFAS 161 is effective in inducing more risk management via hedges and thereby curbing managerial opportunism.

3.6.2 Is managerial opportunism reduced in the non-compliers post SFAS 161?

In this section, we explore whether managerial opportunism is reduced post SFAS 161 if derivative users do not comply with the standard. In our initial sample, we identify 332 derivative-using firms, which are not in compliance with SFAS 161 to provide tabular derivative disclosures, as opposed to 395 compliers. The non-compliance pertains to an issue relating to the enforcement of FASB's reporting standards. As an independent and private standard-setting organization, FASB claims to have no authority over the enforcement of its standards. The responsibility for ensuring compliance with its standards rests with the reporting entity, its auditors, and the Securities and Exchange Commission (SEC). SEC and/or auditors would require a firm to restate its financial reporting and disclosures when any error therein is discovered and considered material enough to lead to inaccurate conclusions drawn by financial statement users. In such a case, companies would face an increased risk of SEC enforcement and litigation and a higher possibility of civil penalties, injunctions, clawback remedies, and sanctions by SEC and firm stakeholders (Pecht et al., 2014). Nonetheless, SEC, auditors, and lawyers are often more concerned about material errors than others. The legal risks associated with insufficient disclosure of derivative usage are relatively low. In general, there is no substantial penalty for non-compliance with SFAS 161 which aims at enhancing the transparency of derivative

[23] Because the propensity-score matching is conducted separately for each subsample, the number of observations in the high-opacity subsample in Table 3.6 (and also the high-leverage subsample in Table 3.7 and the high-earnings-volatility subsample in Table 3.8) is different from that of the corresponding low-opacity subsample (and the low-leverage subsample and the low-earnings-volatility subsample) due to the difference in efficiency of matchings.

disclosures.

To examine whether SFAS 161 affects managerial opportunism of derivative users that do not comply with the standard, we re-define our treatment firms to be the non-compliers, and re-estimate models (3.2) and (3.3). As such, the treatment effects of compliance with SFAS 161 are removed from our baseline regression estimations. The new DID estimator is expected to be statistically insignificant, if our main DID results are attributed to the treatment effects of the enhanced derivative disclosures pursuant to SFAS 161, rather than to other omitted factors. Such a placebo analysis using the alternative treatment group not only mitigates potential correlated-omitted-variable(s) concern but can also provide important practical implications regarding regulatory compliance and enforcement.

Our placebo difference-in-differences regression models are the same as models (3.2) and (3.3), except that the treatment indicator variable is replaced with $NONCOMPLIER_i$. It equals 1 for a derivative-using firm that is not compliant with SFAS 161, and equals 0 for a non-derivative-user. Each treatment firm is matched with a control firm using the same propensity-score-matching approach as in Section 3.4.1. Table 3.9 reports the regression results. The coefficients on the interaction term, $NONCOMPLIER_i \times POST_t$, are not statistically significant in Columns (1) and (2), suggesting that SFAS 161 does not have an attenuating impact on insider trades and crash risk of non-complying derivative-users. Thus, SFAS 161 is effective in reducing managerial opportunism only when a derivative user complies with the standard. This highlights the importance of enforcement in achieving the regulatory outcome of reduced managerial opportunism. In addition, the results for our placebo test provide support for our main DID results being free from potential omitted-variable(s) bias.

3.7 Conclusion

SFAS 161 mandates derivative-using firms to disclose their purposes and strategies of using derivatives. We employ SFAS 161 as a setting to examine whether such derivative disclosures deter managerial opportunism that is at the expense of outside investors. We use insider trades and stock price crash risk as proxies for the opportunism. Using difference-in-differences research design and our hand-collected data on the derivative disclosures, we find that firms using derivatives *and* complying with SFAS 161 are less likely to pursue insider trades or encounter a stock price crash. This suggests that the derivative disclosures mandated by SFAS 161 curb managerial opportunism. We also find that the mitigating impact of SFAS 161 on managerial opportunism is stronger for firms with high information opacity, high financial risk, and high business risk. Nevertheless, we do not find evidence to suggest that derivative users that do not comply with SFAS 161 exhibit less managerial opportunism after the implementation of this standard. This calls for stronger monitoring of compliance with SFAS 161 to maximize its impacts and benefits in the public interest.

3.8 Appendices

3.8.1 Summary of variable definitions

Variables	Definitions
CRASH	1 if a firm experiences one or more firm-specific weekly returns falling 3.2 standard deviations below the mean firm-specific weekly return over a fiscal year, and 0 otherwise. The firm-specific weekly returns measure follows Kim et al. (2011a).
INSITRADE	The natural logarithm of 1 plus the total of the dollar volume of insider sales and the dollar volume of insider purchases made by all directors and officers of a firm over a fiscal year.
POST	1 if a firm is in the three fiscal years (i.e., 2009-2011) after SFAS 161 was implemented in 2008, and 0 if a firm is in the three fiscal years (i.e., 2006-2008) predating the implementation of SFAS 161.
TREAT	1 for a treatment firm that follows SFAS 161 to provide tabular disclosures distinguishing between derivatives *designated* and *not designated* as hedging instruments in the three-year post-SFAS 161 period (i.e., 2009-2011), and 0 for a control firm that does not use derivatives in any year during our sample period, either before or after SFAS 161.
NONCOMPLIER	1 for a treatment firm that does not comply with SFAS 161 (i.e., a firm that does not provide tabular disclosures distinguishing between derivatives designated as hedges and those not designated as hedges), and 0 for a non-user of derivatives.
SIZE	The natural logarithm of the market value of a firm's equity at the end of a fiscal year.
BTM	The book value of firm equity divided by the market value of firm equity at the end of a fiscal year.
DEDI	Dedicated institutional investors' stock ownership as a percentage of a firm's outstanding shares at the end of a fiscal year.
LANACOV	The natural logarithm of 1 plus the number of analysts that make at least one annual earnings per share (EPS) forecast for a firm over a fiscal year.
ROA	Return on assets, calculated as income before extraordinary items divided by total assets at the beginning of a fiscal year.
LEV	The sum of short-term and long-term debt divided by total assets for a firm over a fiscal year. We set missing values of short-term debt equal to zero and drop the observations for which long-term debt values are missing.
FIRMAGE	The number of years for which a firm has been listed.
TRADEVOL	The average of monthly trading volume for a firm over a fiscal year, scaled by the number of shares outstanding at the end of the year.
STDCFO	The standard deviation of cash flow of a firm for the current and previous four fiscal years.
OPACITY	The three-year moving sum of the absolute value of annual discretionary accruals, a measure of financial opacity developed by Hutton et al. (2009).
STDEARN	The standard deviation of income before extraordinary items for the current and previous four fiscal years.
CETR	The cash effective tax rate, calculated as cash taxes paid (TXPD) divided by pretax income (PI) net of special items (SPI). We set missing values of TXPD to be zero, and exclude observations for which the

	denominator of *CETR* is zero or negative.
NCSKEW	The negative of the third moment of firm-specific weekly returns. The firm-specific weekly returns measure follows Kim et al. (2011a).
SA	A financial constraint index (*SA*) developed by Hadlock and Pierce (2010). SA=-0.737*$size$+0.043*$size^2$-0.040*age, where *size* is the natural logarithm of total assets capped at \$4.5 billion, and *age* is the number of years for which a firm has been listed. *SA* index is re-scaled by dividing 1,000.

3.8.2 Examples of Derivative Disclosures Before and After SFAS 161

1. An excerpt from notes to Consolidated Financial Statements of Kadant Inc. for the fiscal year ended on December 31, 2007

"The Company uses derivative instruments primarily to reduce its exposure to changes in currency exchange rates and interest rates. When the Company enters into a derivative contract, the Company makes a determination as to whether the transaction is deemed to be a hedge for accounting purposes. For contracts deemed to be a hedge, the Company formally documents the relationship between the derivative instrument and the risk being hedged. In this documentation, the Company specifically identifies the asset, liability, forecasted transaction, cash flow, or net investment that has been designated as the hedged item, and evaluates whether the derivative instrument is expected to reduce the risks associated with the hedged item. To the extent these criteria are not met, the Company does not use hedge accounting for the derivative.

SFAS No. 133 (SFAS 133), "Accounting for Derivative Instruments and Hedging Activities," as amended, requires that all derivatives be recognized on the balance sheet at fair value. For derivatives designated as cash flow hedges, the related gains or losses on these contracts are deferred as a component of accumulated other comprehensive items. These deferred gains and losses are recognized in the period in which the underlying anticipated transaction occurs. For derivatives designated as fair value hedges, the unrealized gains and losses resulting from the impact of currency exchange rate movements are recognized in earnings in the period in which the exchange rates change and offset the currency gains and losses on the underlying exposures being hedged. The Company performs an evaluation of the effectiveness of the hedge both at inception and on an ongoing basis. The ineffective portion of a hedge, if any, and changes in the fair value of a derivative not deemed to be a hedge, are recorded in the consolidated statement of income.

The Company entered into interest rate swap agreements in 2007 and 2006 to hedge a portion of its variable rate debt and has designated these agreements as cash flow hedges of the underlying obligations. The fair values of the interest rate swap agreements are included in other assets for unrecognized gains and in other liabilities for unrecognized losses with an offset in accumulated other comprehensive items (net of tax). The Company has structured these interest rate swap agreements to be 100% effective and as a result, there is no current impact to earnings resulting from hedge ineffectiveness.

The Company uses forward currency exchange contracts primarily to hedge certain operational ("cash flow" hedges) and balance sheet ("fair value" hedges) exposures resulting from fluctuations in currency exchange rates. Such exposures primarily result from portions of the Company's operations and assets that are denominated in currencies other than the functional currencies of the businesses conducting the operations or holding the assets. The Company enters into forward currency exchange contracts to hedge anticipated product sales and recorded accounts receivable made in the normal course of business, and accordingly, the hedges are not speculative in nature."

2. An excerpt from notes to Consolidated Financial Statements of Kadant Inc. for the fiscal year ended on December 31, 2010

"The Company uses derivative instruments primarily to reduce its exposure to changes in currency exchange rates and interest rates. When the Company enters into a derivative contract, the Company makes a determination as to whether the transaction is deemed to be a hedge for accounting purposes. For a contract deemed to be a hedge, the Company formally documents the relationship between the derivative instrument and the risk being hedged. In this documentation, the Company specifically identifies the asset, liability, forecasted transaction, cash flow, or net investment that has been designated as the hedged item, and evaluates whether the derivative instrument is expected to reduce the risks associated with the hedged item. To the extent these criteria are not met, the Company does not use hedge accounting for the derivative. The changes in the fair value of a derivative not deemed to be a hedge are recorded currently in earnings. The Company does not hold or engage in transactions involving derivative instruments for purposes other than risk management.

ASC 815, "Derivatives and Hedging," requires that all derivatives be recognized on the balance sheet at fair value. For derivatives designated as cash flow hedges, the related gains or losses on these contracts are deferred as a component of accumulated other comprehensive items. These deferred gains and losses are recognized in the period in which the underlying anticipated transaction occurs. For derivatives designated as fair value hedges, the unrealized gains and losses resulting from the impact of currency exchange rate movements are recognized in earnings in the period in which the exchange rates change and offset the currency gains and losses on the underlying exposures being hedged. The Company performs an evaluation of the effectiveness of the hedge both at inception and on an ongoing basis. The ineffective portion of a hedge, if any, and changes in the fair value of a derivative not deemed to be a hedge, are recorded in the consolidated statement of operations.

Interest Rate Swaps

The Company entered into interest rate swap agreements in 2008 and 2006 to hedge its exposure to variable-rate debt and has designated these agreements as cash flow hedges. On February 13, 2008, the Company entered into a swap agreement (2008 Swap Agreement) to hedge the exposure to movements in the 3-month LIBOR rate on future outstanding debt. The 2008 Swap Agreement has a five-year term and a $15,000,000 notional value, which decreased to $10,000,000 on December 31, 2010, and will decrease to $5,000,000 on December 30, 2011. Under the 2008 Swap Agreement, on a quarterly basis the Company receives a 3-month LIBOR rate and pays a fixed rate of interest of 3.265% plus the applicable margin. The Company entered into a swap agreement in 2006 (the 2006 Swap Agreement) to convert a portion of the Company's outstanding debt from floating to fixed rates of interest. The swap agreement has the same terms and quarterly payment dates as the corresponding debt, and reduces proportionately in line with the amortization of the debt. Under the 2006 Swap Agreement, the Company receives a three-month LIBOR rate and pays a fixed rate of interest of 5.63%. The fair values for these instruments as of year-end 2010 are included in other liabilities, with an offset to accumulated other comprehensive items (net of tax) in the accompanying consolidated balance sheet. The Company *has structured these interest rate swap agreements to be 100% effective and as a result, there is no current impact to earnings resulting from hedge ineffectiveness. Management believes that any credit risk associated with the swap agreements is remote based on the Company's financial position and the creditworthiness of the financial institution issuing the swap agreements.*

The counterparty to the swap agreement could demand an early termination of the swap agreement if the Company is in default under the 2008 Credit Agreement, or any agreement that amends or replaces the 2008 Credit Agreement in which the counterparty is a member, and the Company is unable to cure the default. An event of default under the 2008 Credit Agreement includes customary events of default and failure to comply with financial covenants, including a maximum consolidated leverage ratio of 3.5 and a minimum consolidated fixed charge coverage ratio of 1.2. The unrealized loss of $1,595,000 as of year-end 2010 represents the estimated amount that the Company would pay to the counterparty in the event of an early termination.

Forward Currency-Exchange Contracts

The Company uses forward currency-exchange contracts primarily to hedge exposures resulting from fluctuations in currency exchange rates. Such exposures result primarily from portions of the Company's operations and assets and liabilities that are denominated in currencies other than the functional currencies of the businesses conducting the operations or holding the assets and liabilities. The Company typically manages its level of exposure to the risk of currency-exchange fluctuations by hedging a portion of its currency exposures anticipated over the ensuing 12-month period, using forward currency-exchange contracts that have maturities of 12 months or less.

Forward currency-exchange contracts that hedge forecasted accounts receivable or accounts payable are designated as cash flow hedges. The fair values for these instruments are included in other current assets for unrecognized gains and in other current liabilities for unrecognized losses, with an offset in accumulated other comprehensive items (net of tax). For forward currency-exchange contracts that are designated as fair value hedges, the gain or loss on the derivative, as well as the offsetting loss or gain on the hedged item are recognized currently in earnings. The fair values of forward currency-exchange contracts that are not designated as hedges are recorded currently in earnings. The Company recognized a loss of $34,000 and $699,000 in 2010 and 2009, respectively, and a gain of $896,000 in 2008 included in selling, general, and administrative expenses associated with forward currency-exchange contracts that were not designated as hedges. Management believes that any credit risk associated with forward currency-exchange contracts is remote based on the Company's financial position and the creditworthiness of the financial institutions issuing the contracts.

The following table summarizes the fair value of the Company's derivative instruments designated and not designated as hedging instruments, the notional values of the associated derivative contracts, and the location of these instruments in the consolidated balance sheet:

(In thousands)	Balance Sheet Location	2010 Asset (Liability) (a)	2010 Notional Amount (b)	2009 Asset (Liability) (a)	2009 Notional Amount (b)
Derivatives Designated as Hedging Instruments:					
Derivatives in an Asset					
Forward currency-exchange contracts	Other Current Assets	$ 131	$ 1,794	$ 207	$ 7,856
Derivatives in a Liability Position:					
Forward currency-exchange contracts	Other Current Liabilities	$ (59)	$ 1,056	$ –	$ –
Interest rate swap agreements	Other Long-Term Liabilities	$ (1,595)	$ 17,750	$ (1,517)	$ 23,250
Derivatives Not Designated as Hedging Instruments:					
Derivatives in a Liability					
Forward currency-exchange contracts	Other Current Liabilities	$ (48)	$ 1,816	$ (98)	$ 1,728

(a) See Note 11 for the fair value measurements relating to these financial instruments.
(b) The total notional amount is indicative of the level of the Company's derivative activity during 2010 and 2009.

The following table summarizes the activity in accumulated other comprehensive items (OCI) associated with the Company's derivative instruments designated as cash flow hedges as of and for the period ended January 1, 2011:

(In thousands)	Interest Rate Swap Agreements	Forward Currency-Exchange Contracts	Total
Unrealized loss (gain), net of tax, at January 2, 2010	$ 1,212	$ (138)	$ 1,074
(Loss) gain reclassified to earnings (a)	(710)	138	(572)
Loss (gain) recognized in OCI	788	(50)	738
Unrealized loss (gain), net of tax, at January 1, 2011	$ 1,290	$ (50)	$ 1,240

(a) Included in interest expense for interest rate swap agreements and in revenues for forward currency-exchange contracts in the accompanying consolidated statement of operations.

As of January 1, 2011, $552,000 of the net unrealized loss included in OCI is expected to be reclassified to earnings over the next twelve months."

Table 3.1: Descriptive statistics

Panel A. Insider trades (*INSITRADE*) sample

Variables	No. of firm-years	No. of firms	Mean	Std. dev.	25th	Median	75th
INSITRADE	2,762	712	4.6479	6.3358	0	0	11.9837
SIZE	2,762	712	7.1450	1.7263	6.0880	7.1197	8.1458
BTM	2,762	712	0.5787	0.8088	0.2673	0.4236	0.6727
SA	2,762	712	-1.2502	1.2236	-2.2649	-0.6912	-0.2156
LANACOV	2,762	712	3.4697	1.2244	2.8904	3.6889	4.3041
DEDI	2,762	712	0.0792	0.0930	0.0113	0.0573	0.1202
ROA	2,762	712	0.1248	1.5040	0.0400	0.0711	0.1122
LEV	2,762	712	0.1489	0.1627	0.0002	0.1018	0.2462
TRADEVOL	2,762	712	2.3167	1.7974	1.1420	1.8795	2.9895
FIRMAGE	2,762	712	20.9066	18.9084	9	15	26
STDCFO	2,762	712	112.598	357.4053	8.3166	23.9519	75.1656
OPACITY	2,480	700	10.0161	67.4258	0.0401	0.1472	0.9973
STDEARN	2,762	712	127.951	595.8608	6.0127	17.6636	69.0112

Panel B. Stock price crash risk (*CRASH*) sample

Variables	No. of firm-years	No. of firms	Mean	Std. dev.	25th	Median	75th
CRASH	2,949	725	0.1987	0.3991	0	0	0
SIZE	2,949	725	7.0848	1.7399	5.9827	7.0675	8.0870
BTM	2,949	725	0.6424	1.3068	0.2752	0.4435	0.7077
SA	2,949	725	-1.2554	1.2198	-2.2617	-0.6968	-0.2252
LANACOV	2,949	725	3.4751	1.2149	2.8904	3.6889	4.3041
DEDI	2,949	725	0.0803	0.0953	0.0105	0.0581	0.1225
ROA	2,949	725	0.1078	1.4572	0.0311	0.0652	0.1067
LEV	2,949	725	0.1546	0.1663	0.0005	0.1098	0.2561
TRADEVOL	2,949	725	2.3246	1.7968	1.1438	1.9014	2.9894
STDCFO	2,949	725	111.7992	352.9264	8.6216	24.5243	75.2657
RETR	2,949	725	0.2799	1.8343	0.0940	0.2247	0.3301
NCSKEW	2,949	725	-2.6395	33.2068	-8.6045	-1.9687	4.5735
OPACITY	2,658	715	9.8189	65.9171	0.0399	0.1461	0.9508
STDEARN	2,949	725	127.9418	583.0573	6.3188	18.8912	70.6109

Notes: The tables present descriptive statistics for the variables which are used in the multivariate tests and based on the samples before the propensity-score matching. Panel A reports the statistics for the insider trades (*INSITRADE*) sample, and Panel B reports those for the stock price crash risk (*CRASH*) sample. The period for both samples covers six years from 2006 to 2011. All the variables are defined in Appendix 3.8.1.

Table 3.2: Propensity-score-matching specification

Panel A. A logistic regression on the determinants of derivative usage

Variables	(1) *INSITRADE* Sample Dependent Variable = $TREAT_i$	(2) *CRASH* Sample Dependent Variable = $TREAT_i$
$SIZE_t$	0.3380***	0.3101***
	(<0.001)	(<0.001)
BTM_t	0.1545**	0.1001*
	(0.020)	(0.062)
LEV_t	-0.4575***	-0.5065***
	(<0.001)	(<0.001)
ROA_t	3.9340***	3.8460***
	(<0.001)	(<0.001)
$DEDI_t$	0.9985*	1.0092*
	(0.067)	(0.053)
$STDCFO_t$	3.96E-05	4.75E-05
	(0.875)	(0.850)
Intercept	-3.3758***	-3.1587***
	(<0.001)	(<0.001)
No. of observations	1,263	1,357
Pseudo R-squared	0.2344	0.2343

Panel B. Checking covariate balance between treatment and control groups

Insider trades (*INSITRADE*) sample

Variables	Mean *TREAT*=0 (N=1,520)	Mean *TREAT*=1 (N=1,520)	Mean Difference (t-stat)	Standardized Bias (%)
SIZE	7.7784	7.9016	-0.1232*	-7.8
			(-1.98)	
BTM	0.5862	0.5550	0.0311	3.9
			(1.00)	
SA	-1.7775	-1.7562	-0.0213	-2.0
			(-0.47)	
LEV	0.2012	0.1941	0.0071	4.8
			(1.16)	
DEDI	0.0886	0.0871	0.0015	1.7
			(0.40)	
STDCFO	174.7400	177.4900	-2.7500	-0.8
			(-0.19)	

Stock price crash risk (*CRASH*) sample

Variables	Mean *TREAT*=0 (N=1,647)	Mean *TREAT*=1 (N=1,647)	Mean Difference (t-stat)	Standardized Bias (%)
SIZE	7.6987	7.7164	-0.0177	-1.1
			(-0.28)	
BTM	0.6262	0.6487	-0.0225	-1.8
			(-0.61)	
SA	-1.7710	-1.6972	-0.0738*	-7.1
			(-1.70)	
LEV	0.2063	0.2140	-0.0077	-5.0
			(-1.25)	
DEDI	0.0901	0.0881	0.0020	2.1
			(0.54)	
STDCFO	171.4600	163.8800	7.5800	2.3
			(0.56)	

Notes: Panel A presents the results for the regressions of derivative usage on its determinants. The sample period spans years 2006-2008.The dependent variable is the indicator variable, *TREAT*, which equals 1 for a derivative-using firm that complies with SFAS 161, and 0 for a non-derivative-user. *p*-values in parentheses are based on robust standard errors clustered by firm. Propensity scores are estimated from the regressions for each firm-year observation in the insider trades (*INSITRADE*) sample and stock price crash risk (*CRASH*) sample, respectively. Each treatment firm is then matched with a control firm that has the closest propensity score, with replacement and within the caliper of 1%. Panel B reports the descriptive statistics of matching covariates between the complier (*TREAT*=1) group and the non-user (*TREAT*=0) group post propensity-score matching. t-statistics from the two-sample t-test for equal means, alongside with standardized bias, are calculated for checking the post-matching covariate balance. All the variables in the tables are defined in Appendix 3.8.1. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 3.3: Difference-in-differences regression analysis: the impact of SFAS 161 on managerial opportunism

Variables	(1) Dependent Variable = $INSITRADE_t$	(2) Dependent Variable = $CRASH_{t+1}$
Intercept	-5.5062*	-2.9451**
	(0.052)	(0.012)
$TREAT_i$	1.0119***	0.2803*
	(0.004)	(0.062)
$POST_t$	2.2746***	0.6746***
	(<0.001)	(<0.001)
$\boldsymbol{TREAT_i \times POST_t}$	**-1.5940*** **	**-0.4329** **
	(<0.001)	**(0.020)**
$SIZE_t$	0.9316***	0.2156***
	(<0.001)	(<0.001)
BTM_t	0.0630	-0.0136
	(0.697)	(0.840)
$LANACOV_t$	-0.0309	0.0549
	(0.862)	(0.471)
$DEDI_t$	-1.9778*	-0.5057
	(0.091)	(0.315)
ROA_t	-0.0911	-0.0475
	(0.241)	(0.627)
$TRADEVOL_t$	0.0744	-0.0108
	(0.353)	(0.745)
$STDCFO_t$	0.0004	-0.0005***
	(0.275)	(0.006)
$FIRMAGE_t$	-0.0413***	
	(<0.001)	
$CETR_t$		-0.0139
		(0.699)
$NCSKEW_t$		-0.0024
		(0.188)
Year-fixed effects	included	included
Industry-fixed effects	included	included
No. of observations	3,040	3,294
Adjusted/Pseudo R-squared	0.1196	0.0707

Notes: This table reports the results of the difference-in-differences regressions for the impact of SFAS 161 on managerial opportunism. The sample period covers six years from 2006 to 2011. The dependent variable is insider trades ($INSITRADE_t$) in Column (1) and stock price crash risk ($CRASH_{t+1}$) in Column (2). The treatment indicator variable, $TREAT_i$, equals 1 for a derivative-using firm that complies with SFAS 161, and 0 for a non-derivative-user. The time indicator variable, $POST_t$, equals 1 (0) if a firm is in the post-SFAS 161 (pre-SFAS 161) period (i.e., 2009-2011 (2006-2008)). The interaction term, $TREAT_i \times POST_t$, is the variable of interest which captures the effect of SFAS 161 on insider trading and stock price crash risk for the compliers (*TREAT*=1) relative to the non-users of derivatives (*TREAT*=0). All the variables are defined in Appendix 3.8.1. Industry dummies (constructed from the first two digits of SIC codes) and year dummies are included in all the regression but are not reported for simplicity. *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 3.4: Firm-fixed-effects difference-in-differences regression analysis: the impact of SFAS 161 on managerial opportunism

	Dependent Variable =			
Variables	(1) $INSITRADE_t$	(2) $CRASH_{t+1}$	(3) $INSITRADE_t$	(4) $CRASH_{t+1}$
Intercept	-90.4639**	-5.6015	-2.0112	-1.3358
	(0.050)	(0.014)	(0.803)	(0.568)
$TREAT_i$	132.0049*	-0.0826	0.6653	-0.0689
	(0.056)	(0.969)	(0.926)	(0.973)
$POST_t$	10.7533**	0.4930*	0.8139*	0.9383***
	(0.010)	(0.064)	(0.065)	(<0.001)
$TREAT_i \times POST_t$	**-1.4053*** **	**-0.7262*** **	**-1.4445*** **	**-0.5039** **
	(0.001)	**(0.005)**	**(0.001)**	**(0.039)**
$SIZE_t$	0.5497	0.7159***		
	(0.126)	(0.003)		
BTM_t	-0.2129	-0.0429		
	(0.595)	(0.848)		
$LANACOV_t$	0.3510	0.4362*		
	(0.281)	(0.059)		
$DEDI_t$	-3.6970*	-0.1623		
	(0.096)	(0.895)		
ROA_t	-0.1190	2.9464***		
	(0.118)	(0.010)		
$TRADEVOL_t$	0.2349*	-0.0475		
	(0.094)	(0.605)		
$STDCFO_t$	0.0009*	-0.0020***		
	(0.085)	(<0.001)		
$FIRMAGE_t$	-1.7671**			
	(0.035)			
$CETR_t$		0.0009		
		(0.979)		
$NCSKEW_t$		-0.0090**		
		(0.041)		
Year-fixed effects	included	included	included	included
Firm-fixed effects	included	included	included	included
No. of observations	3,040	1,739	3,040	1,739
Adjusted/Pseudo R-squared	0.3899	0.1611	0.3865	0.1279

Notes: This table reports the results of the difference-in-differences tests for the impact of SFAS 161 on managerial opportunism after including firm-fixed effects in the regressions. The sample period spans years 2006-2011. The dependent variable is insider trades ($INSITRADE_t$) in Columns (1) and (3) and stock price crash risk ($CRASH_{t+1}$) in Columns (2) and (4). The treatment indicator variable, $TREAT_i$, equals 1 for a derivative-using firm that complies with SFAS 161, and 0 for a non-derivative-user. The time indicator variable, $POST_t$, equals 1 (0) if a firm is in the post-SFAS 161 (pre-SFAS 161) period (i.e., 2009-2011 (2006-2008)). The interaction term, $TREAT_i \times POST_t$, is the variable of interest which captures the effects of SFAS 161 on insider trading and crash risk for the compliers (*TREAT*=1) relative to the non-derivative-users (*TREAT*=0). All the variables are defined in Appendix 3.8.1. Firm dummies, and year dummies are included in all the regression but are not reported for simplicity. *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 3.5: Placebo tests – the potential confounding effect of financial crisis

Variables	2009-2010 vs. 2011-2012		2005-2006 vs. 2007-2008	
	(1) $INSITRADE_t$	(2) $CRASH_{t+1}$	(3) $INSITRADE_t$	(4) $CRASH_{t+1}$
Intercept	-6.2942***	-3.4569***	-6.9411***	-1.5200
	(<0.001)	(0.005)	(<0.001)	(0.186)
$TREAT_i$	-1.1191	0.4424	1.5152**	0.3260
	(0.107)	(0.174)	(0.027)	(0.502)
$POST_t$	-0.1794	0.5813	1.5182**	0.6866
	(0.826)	(0.154)	(0.045)	(0.269)
$\boldsymbol{TREAT_i \times POST_t}$	**0.6971**	**-0.5982**	**-1.2127**	**-0.2575**
	(0.370)	**(0.138)**	**(0.105)**	**(0.663)**
$SIZE_t$	1.1277***	0.3074**	0.6543**	0.0537
	(<0.001)	(0.036)	(0.012)	(0.614)
BTM_t	0.1605	-0.1906	0.2220	-0.0361
	(0.660)	(0.416)	(0.261)	(0.817)
$LANACOV_t$	0.0302	-0.0033	0.3354	-0.1121
	(0.930)	(0.984)	(0.166)	(0.378)
$DEDI_t$	1.9991	0.0878	-0.9372	-0.6299
	(0.365)	(0.925)	(0.637)	(0.549)
ROA_t	-0.0364	-0.0052	-0.0951***	-0.0876
	(0.232)	(0.861)	(0.001)	(0.488)
$TRADEVOL_t$	-0.1612	-0.0485	0.2028	0.0645
	(0.344)	(0.554)	(0.109)	(0.308)
$STDCFO_t$	-0.0009	-0.0014**	0.0005	-0.0002
	(0.298)	(0.036)	(0.671)	(0.564)
$FIRMAGE_t$	-0.0076		-0.0346*	
	(0.587)		(0.064)	
$CETR_t$		0.0714		0.3073
		(0.233)		(0.419)
$NCSKEW_t$		0.0032		-0.0144*
		(0.693)		(0.059)
Year-fixed effects	included	included	included	included
Firm-fixed effects	included	included	included	included
No. of observations	2,314	2,083	2,114	1,386
Adjusted/Pseudo R-squared	0.1510	0.1032	0.1021	0.0842

Notes: This table reports the results from the placebo tests, which examine the potential confounding effect of financial crisis on managerial opportunism. The dependent variable is insider trades ($INSITRADE_t$) in Columns (1) and (3) and stock price crash risk ($CRASH_{t+1}$) in Columns (2) and (4). The treatment indicator variable, $TREAT_i$, equals 1 for a derivative-using firm that complies with SFAS 161, and 0 for a non-derivative-user. The time indicator variable, $POST_t$, equals 1 (0) if a firm is in the post-crisis (crisis) period (i.e., 2011-2012 (2009-2010)) in Column (1) and Column (2); $POST_t$, equals 1 (0) if a firm is in the crisis (pre-crisis) period (i.e., 2007-2008 (2005-2006)) in Column (3) and Column (4). The interaction term, $TREAT_i \times POST_t$, is the variable of interest which captures the effects of events on insider trading and crash risk for the compliers (*TREAT*=1) relative to the non-derivative-users (*TREAT*=0). All the variables are defined in Appendix 3.8.1. Industry dummies (constructed from the first two digits of SIC codes) and year dummies are included in all the regression but are not reported for simplicity. *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 3.6: The moderating effect of information opacity

Variables	(1) Dependent Variable = *INSITRADE*$_t$		(2) Dependent Variable = *CRASH*$_{t+1}$	
Information Opacity (*OPACITY*)	**Low**	**High**	**Low**	**High**
Intercept	11.1847*	-2.9309	-1.1750	-1.1601
	(0.058)	(0.644)	(0.191)	(0.380)
TREAT$_i$	-0.1426	0.3210	-0.4436	0.3533
	(0.797)	(0.586)	(0.292)	(0.153)
POST$_t$	-2.2083***	0.6891	-0.9358	0.8430***
	(0.008)	(0.356)	(0.197)	(0.003)
***TREAT*$_i$×*POST*$_t$**	**0.7449**	**-1.2058***	**0.9001**	**-0.7383****
	(0.268)	**(0.099)**	**(0.100)**	**(0.014)**
SIZE$_t$	0.8503***	0.5911***	0.2667**	-0.0648
	(<0.001)	(0.005)	(0.043)	(0.401)
BTM$_t$	-0.8438*	0.2071	0.0412	-0.4945**
	(0.059)	(0.302)	(0.331)	(0.013)
LANACOV$_t$	-0.6148**	0.9846***	-0.1505	0.3265***
	(0.037)	(0.001)	(0.383)	(0.010)
DEDI$_t$	-0.6268	1.4109	0.6455	-0.5218
	(0.779)	(0.461)	(0.616)	(0.503)
ROA$_t$	-0.1243	-0.0961	-0.0379*	-0.0238
	(0.172)	(0.468)	(0.053)	(0.811)
TRADEVOL$_t$	0.5241***	-0.2362*	-0.0134	-0.0294
	(<0.001)	(0.100)	(0.864)	(0.575)
STDCFO$_t$	-0.0008	-0.0001	-0.0002	-0.0003
	(0.205)	(0.856)	(0.563)	(0.368)
FIRMAGE$_t$	-0.0454***	-0.0376***		
	(<0.001)	(<0.001)		
CETR$_t$			-0.2525	0.0613
			(0.217)	(0.365)
NCSKEW$_t$			-0.0027**	-0.0073
			(0.047)	(0.160)
Year-fixed effects	included	included	included	included
Industry-fixed effects	included	included	included	included
No. of observations	1,296	1,362	1,331	1,427
Adjusted/Pseudo R-squared	0.1736	0.1374	0.1129	0.1256

Notes: This table reports the results from testing the moderating effect of information opacity on the regression estimations of models (3.2) and (3.3). The sample period spans years 2006-2011. The moderator variable is information opacity (*OPACITY*), which is measured as the three-year moving sum of the absolute value of annual discretionary accruals as per Hutton et al. (2009). A higher value of *OPACITY* indicates a larger extent of information opacity. The difference-in-differences regressions are run separately in the low-opacity subsample and the high-opacity subsample, which are split based on the full-sample median of *OPACITY*. The dependent variable is insider trading (*INSITRADE*$_t$) in Column (1) and stock price crash risk (*CRASH*$_{t+1}$) in Column (2). The treatment indicator variable, *TREAT*$_i$, equals 1 for a derivative-using firm that complies with SFAS 161, and 0 for a non-derivative-user. The time indicator variable, *POST*$_t$, equals 1 (0) if a firm is in the post-SFAS 161 (pre-SFAS 161) period (i.e., 2009-2011 (2006-2008)). The interaction term, *TREAT*$_i$×*POST*$_t$, is the variable of interest to the analysis. All the variables are defined in Appendix 3.8.1. Industry dummies (constructed from the first two digits of SIC codes) and year dummies are included in all the regression but are not reported for simplicity. *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 3.7: The moderating effect of financial risk

Variables	(1) Dependent Variable = *INSITRADE*$_t$		(2) Dependent Variable = *CRASH*$_{t+1}$	
Leverage (*LEV*)	**Low**	**High**	**Low**	**High**
Intercept	-12.6289**	-3.7272	-0.9141	-3.3728**
	(0.050)	(0.542)	(0.254)	(0.012)
TREAT$_i$	0.8012	0.9698**	-0.0444	0.2927
	(0.187)	(0.041)	(0.860)	(0.223)
POST$_t$	0.6481	2.5844***	0.3746	0.8350***
	(0.451)	(<0.001)	(0.244)	(0.004)
***TREAT*$_i$×*POST*$_t$**	**-0.8281**	**-1.6891*** **	**0.2999**	**-1.1411*** **
	(0.282)	**(0.004)**	**(0.337)**	**(<0.001)**
SIZE$_t$	1.0227***	1.0744***	-0.0681	0.2434***
	(<0.001)	(<0.001)	(0.455)	(0.001)
BTM$_t$	1.7860***	0.1842	-0.1814	-0.1291
	(0.002)	(0.455)	(0.451)	(0.463)
LANACOV$_t$	0.2809	-0.2965	0.0503	0.1092
	(0.352)	(0.260)	(0.682)	(0.349)
DEDI$_t$	-0.8999	-1.3317	-0.4617	0.3017
	(0.652)	(0.452)	(0.579)	(0.687)
ROA$_t$	2.9890	-0.1089	1.3824	-1.1114
	(0.353)	(0.161)	(0.189)	(0.327)
TRADEVOL$_t$	0.2148	-0.0057	0.1372***	-0.1949***
	(0.120)	(0.959)	(0.006)	(0.002)
STDCFO$_t$	-0.0017*	-0.0011**	-0.0009	-0.0003
	(0.059)	(0.034)	(0.200)	(0.192)
FIRMAGE$_t$	-0.0625***	-0.0376***		
	(<0.001)	(<0.001)		
CETR$_t$			-0.2028	-0.0784
			(0.501)	(0.602)
NCSKEW$_t$			0.0025	-0.0079*
			(0.678)	(0.078)
Year-fixed effects	included	included	included	included
Industry-fixed effects	included	included	included	included
No. of observations	1,100	1,906	1,211	1,876
Adjusted/Pseudo R-squared	0.1178	0.1401	0.0947	0.1159

Notes: This table reports the results from testing the moderating effect of financial risk on the regression estimations of models (3.2) and (3.3). The sample period covers years 2006-2011. The moderator variable is financial leverage (*LEV*), which is the proxy for a firm's financial risk and is measured as the sum of short-term and long-term debt divided by total assets of a firm for a fiscal year. The difference-in-differences regressions are run separately in the low-leverage subsample and the high-leverage subsample, which are split based on the full-sample median of *LEV*. The dependent variable is insider trading (*INSITRADE*$_t$) in Column (1) and stock price crash risk (*CRASH*$_{t+1}$) in Column (2). The treatment indicator variable, *TREAT*$_i$, equals 1 for a derivative-using firm that complies with SFAS 161, and 0 for a non-derivative-user. The time indicator variable, *POST*$_t$, equals 1 (0) if a firm is in the post-SFAS 161 (pre-SFAS 161) period (i.e., 2009-2011 (2006-2008)). The interaction term, *TREAT*$_i$×*POST*$_t$, is the variable of interest to the analysis. All the variables are defined in Appendix 3.8.1. Industry dummies (constructed from the first two digits of SIC codes) and year dummies are included in all the regression but are not reported for simplicity. *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 3.8: The moderating effect of business risk

Variables	(1) Dependent Variable = *INSITRADE*$_t$		(2) Dependent Variable = *CRASH*$_{t+1}$	
Earnings Volatility (*STDEARN*)	**Low**	**High**	**Low**	**High**
Intercept	-12.7641**	3.6242	-3.1537***	-2.1964
	(0.035)	(0.403)	(0.001)	(0.123)
TREAT$_i$	-0.6007	1.3734***	0.0643	0.3282
	(0.245)	(0.007)	(0.769)	(0.177)
POST$_t$	-2.2602***	1.2084**	0.1770	0.7932***
	(0.001)	(0.049)	(0.530)	(0.006)
***TREAT*$_i$×*POST*$_t$**	**1.0670**	**-1.4393****	**0.1089**	**-0.6307****
	(0.118)	**(0.018)**	**(0.702)**	**(0.036)**
SIZE$_t$	1.3278***	1.0744***	0.0465	0.2490***
	(<0.001)	(<0.001)	(0.611)	(0.006)
BTM$_t$	1.0874***	-0.1612	0.0729	-0.3339**
	(<0.001)	(0.502)	(0.485)	(0.032)
LANACOV$_t$	0.9077***	-0.0313	0.1417	0.2220
	(<0.001)	(0.905)	(0.186)	(0.131)
DEDI$_t$	2.1350	-3.6362**	-0.1222	-1.5630*
	(0.277)	(0.014)	(0.870)	(0.078)
ROA$_t$	-0.1352	-0.1077	-0.0409	-0.0105
	(0.146)	(0.385)	(0.699)	(0.903)
TRADEVOL$_t$	0.0207	0.1076	0.1403***	-0.0581
	(0.884)	(0.315)	(0.005)	(0.283)
STDCFO$_t$	-0.0144**	0.0001	-0.0032	-0.0010***
	(0.036)	(0.819)	(0.253)	(0.001)
FIRMAGE$_t$	0.0027	-0.0216***		
	(0.858)	(0.006)		
CETR$_t$			-0.2243	0.0425
			(0.307)	(0.518)
NCSKEW$_t$			0.0063	-0.0100*
			(0.245)	(0.050)
Year-fixed effects	included	included	included	included
Industry-fixed effects	included	included	included	included
No. of observations	1,250	1,664	1,387	1,601
Adjusted/Pseudo R-squared	0.1834	0.2270	0.0687	0.1369

Notes: This table reports the results for testing the moderating effect of business risk on the regression estimations of models (3.2) and (3.3). The sample period covers years 2006-2011. The moderator variable is earnings volatility (*STDEARN*), which is the proxy for a firm's business risk and is measured as the standard deviation of income before extraordinary items for the current and previous four fiscal years. The difference-in-differences regressions are run separately in the low-earnings-volatility subsample and the high-earnings-volatility subsample, which are split based on the full-sample median of *STDEARN*. The dependent variable is insider trading (*INSITRADE*$_t$) in Column (1) and stock price crash risk (*CRASH*$_{t+1}$) in Column (2). The treatment indicator variable, *TREAT*$_i$, equals 1 for a derivative-using firm that complies with SFAS 161, and 0 for a non-derivative-user. The time indicator variable, *POST*$_t$, equals 1 (0) if a firm is in the post-SFAS 161 (pre-SFAS 161) period (i.e., 2009-2011 (2006-2008)). The interaction term, *TREAT*$_i$×*POST*$_t$, is the variable of interest to the analysis. All the variables are defined in Appendix 3.8.1. Industry dummies (constructed from the first two digits of SIC codes) and year dummies are included in all the regression but are not reported for simplicity. *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 3.9: Is managerial opportunism reduced in the non-compliers post SFAS 161?

Variables	(1) Dependent Variable = $INSITRADE_t$	(2) Dependent Variable = $CRASH_{t+1}$
Intercept	-1.5475	-2.2837***
	(0.566)	(<0.001)
$NONCOMPLIER_i$	-1.0419***	0.3849**
	(0.004)	(0.024)
$POST_t$	-0.9353*	0.5560**
	(0.070)	(0.012)
$NONCOMPLIER_i \times POST_t$	**0.7452**	**-0.2100**
	(0.120)	**(0.353)**
$SIZE_t$	0.5236***	0.0783
	(<0.001)	(0.225)
BTM_t	0.2275**	-0.1225
	(0.014)	(0.201)
$LANACOV_t$	0.2451	0.0718
	(0.151)	(0.402)
$DEDI_t$	3.8943***	0.3438
	(0.004)	(0.587)
ROA_t	1.5304	1.3047*
	(0.110)	(0.054)
$TRADEVOL_t$	0.2220***	0.0015
	(0.009)	(0.970)
$STDCFO_t$	0.0010*	-0.0010**
	(0.061)	(0.034)
$FIRMAGE_t$	0.0009	
	(0.921)	
$CETR_t$		-0.0356
		(0.531)
$NCSKEW_t$		0.0024
		(0.560)
Year-fixed effects	included	included
Industry-fixed effects	included	included
No. of observations	2,388	2,302
Adjusted/Pseudo R-squared	0.1061	0.0748

Notes: This table reports the results of the placebo tests of whether managerial opportunism is reduced for non-compliers post SFAS 161. The sample period covers years 2006-2011. The dependent variable is insider trading ($INSITRADE_t$) in Column (1) and stock price crash risk ($CRASH_{t+1}$) in Column (2). The treatment indicator variable, $NONCOMPLIER_i$, equals 1 for a derivative-using firm that complies with SFAS 161, and 0 for a non-derivative-user. The time indicator variable, $POST_t$, equals 1 (0) if a firm is in the post-SFAS 161 (pre-SFAS 161) period (i.e., 2009-2011 (2006-2008)). The interaction term, $TREAT_i \times POST_t$, is the variable of interest which captures the effects of SFAS 161 on insider trading and stock price crash risk for the non-compliers ($NONCOMPLIER_i$=1) relative to the non-derivative-users ($NONCOMPLIER_i$=0). All the variables are defined in Appendix 3.8.1. Industry dummies (constructed from the first two digits of SIC codes) and year dummies are included in all the regression but are not reported for simplicity. *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Chapter 4

How Do Enhanced Derivative Disclosures Affect Information Asymmetry Between Informed and Uninformed Investors?

4.1 Introduction

In response to financial statement users' assertion that the derivative disclosures required by SFAS 133[24] did not provide adequate information about the effects of a firm's derivative usage and hedging activities on firm performance, the Financial Accounting Standards Board (FASB) issued Statement No. 161 *Disclosures about Derivative Instruments and Hedging Activities* (SFAS 161) in 2008 to "*better convey the purpose of derivative use in terms of the risks that the entity is intending to manage*" (FASB, 2008). Given that investors tend to assign a higher value to firms that use derivatives to address risks than those that use derivatives for speculation and other purposes (Koonce et al., 2008), information about the objectives of firms' derivative use can facilitate investors' trading decisions. While it is expected that SFAS 161 renders a firm's derivative disclosures more transparent, we examine how the enhanced derivative disclosures affect the information gap between informed and uninformed investors. Whether SFAS 161 increases or decreases information asymmetry among different investors depends crucially on the differential abilities of investors to digest the derivative information disclosed under SFAS 161. Tension exists as the enhanced derivative disclosures may not be comprehensible to relatively uninformed investors, which constitutes the motivation for this study. Also, this study responds to the increasing trend in research on the use of derivatives by non-financial firms (e.g., Bartram et al., 2011; Campbell, 2015; Choi et al., 2015; Chang et al., 2016;

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[24] The Statement of Financial Accounting Standards No. 133 (SAFS 133) *Accounting for Derivative Instruments and Hedging Activities* was issued by FASB in 1998. SFAS 133 and SFAS 161 were codified under the Accounting Standards Codification Topic 815 (ASC 815) *Derivatives and Hedging* in year 2014. The pre-codification nomenclature is used in this study to discuss the changes made in disclosure requirements for derivatives over time.

Chen et al., 2018).

Economic theories suggest that increased level of disclosure reduces information asymmetry and hence increases the liquidity of a stock (Diamond and Verrecchia, 1991; Baiman and Verrecchia, 1996), whereas empirical studies on the economic consequences of specific reporting standards provide mixed evidence. Leuz and Verrecchia (2000) argue that the difficulty of substantiating the economic consequences of corporate disclosures is attributed to the fact that the disclosure environment in the U.S. is already rich and that any increase in quantity and quality of disclosures would be largely incremental rather than fundamental. However, unlike other mandatory disclosures, firms' disclosures of their derivatives usage remained insufficient. Before SFAS 161 was issued in 2008, SFAS 133 was deemed the first step toward fair value accounting in that it started to recognize derivative instruments that affect the earnings in firms' financial statements (Kawaller, 2004; Barth et al., 2013). Lack of guidance and disclosure requirements distinguishing between derivative instruments used for hedging purposes and those used for non-hedging purposes in SFAS 133 led to inconsistent and inadequate disclosures by derivative users. Given that SFAS 161 aims to improve disclosures about (i) how and why a firm uses derivative instruments; (ii) how derivative instruments are accounted for; and (iii) how derivative instruments affect a firm's financial position, financial performance, and cash flows (FASB, 2008), information asymmetry is expected to reduce after the adoption of SFAS 161.

Improved transparency of firms' disclosures indicates a reduction in information asymmetry between managers and outsiders, but not necessarily an improved understanding of the information by all investors. The complexity and expanded use of derivatives by firms create significant financial reporting challenges, both to the reporting entities themselves and to the users of financial statements. Chang et al. (2016) argue that even financial experts such as analysts routinely misjudge the earnings implications of firms' derivative activities. According to Chang et al. (2016), "*complexity [in the reporting-user context] refers to the difficulty that a user may have* *in understanding the mapping of economic transactions and reporting standards into financial statements*" (pp. 585), which therefore crucially depends on the abilities of investors to understand such information. Previous research argues that transient or short-term institutional investors are relatively better informed compared with other investors (e.g., Chakravarty, 2001; Ke and Ramalingegowda, 2005; Sias et al., 2006); and that institutional investors can better comprehend the information disclosed by firms compared with individual investors (Kumar, 2009).

If SFAS 161 serves to enhance previously uninformed investors' understanding of firm derivative usage, the information gap between informed and uninformed investors will be reduced. Tension exists when enhanced disclosures have substantially different effects on informed investors versus uninformed investors. The required tabular disclosures distinguishing the purposes of derivatives usage and enhanced quantitative disclosures about fair value and derivative gains and losses after SFAS 161 may not be comprehensible to unsophisticated investors. If SFAS 161 improves informed investors' understanding of firm derivative use more, relative to uninformed investors, the information gap between informed and uninformed investors will be greater.

Our empirical analysis is conducted on a hand-collected sample of U.S. non-financial and non-utility firms using a difference-in-differences regression model. We use two proxies for information asymmetry between informed and uninformed investors: stock liquidity and probability of informed trade. First, we measure stock (il)liquidity as the natural logarithm of relative effective spreads. Second, we apply Brown and Hillegeist (2007)'s measure of probability of informed trade (PIN) that is extended on Easley, Kiefer, and O'Hara (1997)'s model. In our initial stock liquidity sample (PIN sample), there are 1,036 (1,177) unique firms, 374 (404) of which are identified as compliers that provide tabular disclosures of the purposes of derivative use as complied with SFAS 161, whereas 366 (456) firms are non-users that do not use derivatives in any year over the sample period. The remaining 296 (317) firms are recognized as non- compliers whose derivative disclosures remain qualitatively the same after SFAS 161, with no clear sign of applying the new standard or a clear tabular disclosure that distinguishes derivatives *designated* and *not designated* as hedges. The assignment of compliers and non-users, or compliers and non-compliers as treatment and control groups, pertains to decisions by managers whereas the outcome variable – stock liquidity or PIN is determined by outside investors. To further assure random assignment and mitigate endogeneity in a firm's decision to use derivatives, we apply a propensity score matching approach. Our results show that compliers experience a significantly greater decrease in information asymmetry between informed and uninformed investors following the implementation of SFAS 161, compared with a matched control sample of non-users or non-compliers.

In additional analysis, we evaluate whether the impact of SFAS 161 on information asymmetry is moderated by firm visibility and investor attention. Previous literature suggests that disclosure regulation has a greater impact on firm with higher visibility. Larger, more visible firms tend to have a larger investor base, which implies greater investor attention to firms' derivative disclosures due to the passage of SFAS 161. Using firm size and an abnormal search volume variable constructed based on Google Trends' SVI data as the proxies for firm visibility, we find that the effect of SFAS 161 in reducing information asymmetry between informed and uninformed investors is more pronounced for larger and more visible firms with greater investor attention.

This study contributes to the literature in several ways. First, it extends the mandatory disclosure literature by showing that more transparent derivative disclosures as to the purposes of firms' derivative use after SFAS 161 improve investors', especially relatively uninformed investors', understanding of the economic effects of derivatives on firm performance, improving stock liquidity and reducing the probability of informed trades via reduced information asymmetry. Results from comparing compliers with non-compliers (and non-compliers with non-users) suggest that SFAS 161 is only effective in reducing stock illiquidity and probability of informed trade for derivative users compliant with the standard. This paper provides an insight into SFAS 161 and contributes to the fairly limited empirical literature on the economic consequences of this particular disclosure regulation.

Second, this study contributes to the research on investor recognition (Grullon et al., 2004; Tetlock, 2010; Fang and Peress, 2009) by showing that the impact of SFAS 161 varies with the extent of investor attention. Given that the implementation of SFAS 161 has brought investors' attention to corporate derivative disclosures in notes to firms' financial statements, a higher firm visibility leads to a stronger regulatory effect on stock liquidity and probability of informed trading. Our findings provide practical implications that tabular disclosures of the designation of derivatives required by SFAS 161 are informative and assist investors in making their trading decision.

The paper is organized as follows. Sections 4.2 provides literature review and develops the main hypothesis. Section 4.3 describes data, variable measurements and sample selection procedure. Research design is provided in Section 4.4. Section 4.5 discusses the empirical results, followed by additional analyses in Section 4.6, and Section 4.7 concludes.

4.2 Literature review and hypothesis development

Companies use derivatives for various purposes. Many firms use derivative instruments to reduce risk as derivatives used for hedging lower cash flow volatility (Froot et al., 1993), alleviate financial distress costs (Smith and Stulz, 1985), and smooth earnings (DeMarzo and Duffie, 1995). Other evidence (e.g., Brown, 2001; Faulkender, 2005; Géczy et al., 2007; Chernenko and Faulkender, 2011; Manchiraju et al., 2018) shows that derivatives are also used for non-hedging purposes such as speculation and earnings management. The effect of corporate derivative use on firm valuation and risk remain ambiguous, which is largely due to firms' insufficient disclosures about the purposes of their derivative use that can be hard to disentangle by outsiders.

Disclosures by a firm essentially turn private information into public information. As the previous chairman of the Securities and Exchange Commission (SEC), Arthur Levitt, once said "*high quality accounting standards result in greater investor confidence, which improves liquidity, reduces capital costs, and makes market prices possible*" (Levitt, 1998, pp.81). Enhanced public disclosures can reduce information asymmetry by providing investors with better knowledge about firms (Healy and Palepu 2001; Eleswarapu et al., 2004; Fu et al., 2012). To guide firms' disclosures about their use of derivatives, FASB issued Statement of Financial Accounting Standards (SFAS) No. 161 in 2008. By disclosing the fair values of derivatives and their gains and losses in a tabular format, it is expected to provide a more complete picture of the effect of using derivatives during the reporting period (FASB, 2008). Campbell et al. (2018) argue that the mandatory derivative disclosures set forth in SFAS 161 facilitate investors to better understand the economic effects of firms' derivative use by showing that the mispricing of derivatives-using firms no longer persists after the implementation of SFAS 161. Previous work (e.g., Welker, 1995; Leuz and Verrecchia, 2000; Bushee and Leuz, 2005) documents a positive result of increased stock liquidity from higher levels of corporate disclosure and reduced information asymmetry. For example, Welker (1995) and Leuz and Verrecchia (2000) argue that more disclosure mitigates the adverse selection problem, attracting more uninformed investors to trade and thereby reducing bid-ask spreads.

SFAS 161 requires that the "*objectives for using derivative instruments be disclosed in terms of underlying risk and accounting designation*" (FASB, 2008). Firms need to put greater effort to distinguish between "derivatives designated as hedging instruments" and "derivatives not designated as hedging instruments" in a tabular disclosure. Manchiraju et al. (2018) argue that such accounting designation of derivatives provided under SFAS 161 is informative as to the manner and purposes for which firms use derivatives. They find that firms' use of derivatives that are designated as hedging instruments is negatively associated with firm risk and the use of derivatives not designated as hedges is positively related to firm risk. In other words, derivatives designated as hedging instruments are more likely to be used to manage firm risk whereas those not designated as hedging instruments are used for other purposes. Given that derivatives generally increase firm value if used for hedging purposes and reduce firm value if used for non-hedging purposes (Allayannis and Weston, 2001; Bartram et al., 2011; Gilje and Taillard, 2017), investors tend to be more satisfied with managers that use derivatives to address risks (Koonce et al., 2008). To the extent that the designation of derivatives captures the economic substance of these derivatives, we expect investors, especially previously uninformed investors, to be more confident to trade in the stock of derivatives-using firms after the implementation of SFAS 161, suggesting lower information asymmetry.

However, when more information is available following SFAS 161, it is possible that sophisticated investors use their professional advantage to better process the additional derivative disclosures while the relatively uninformed investors who are not able to digest such information will protect themselves by trading less. As prior studies (e.g., Kawaller, 2004; Chang et al., 2016) suggest, derivatives are one of the most complex types of financial contract. The value of a derivative contract is based on the price movements of the underlying asset over time, the fluctuation in which leaves investors a difficult task of interpreting the effects of derivatives on firm value. Furthermore, the complexity of derivative information may complicate the overall information environment of a firm. Since investors use derivative information in conjunction with other information in the financial statements to assess the risk profile and prospects of a firm, the complex nature of derivatives may require extra effort and expertise from investors to make an evaluation. Therefore, if SFAS 161 is more effective in improving the derivative information absorbed by informed investors relative to uninformed investors, the information asymmetry between the two will be greater.

Another possible outcome from enhanced derivative disclosures is that the information gap between informed and uninformed investors remains unchanged after SFAS 161. This might be because SFAS 161 has either the same or no impact on these two types of investor. First, it is possible that the enhanced derivative information provided, as prescribed by SFAS 161, is absorbed by informed and uninformed investors to the same extent. In other words, there is no difference in investors' abilities to decipher the derivative disclosures. Second, it is possible that even informed investors such as institutional investors or investors who take financial analyst advice, cannot absorb the improved derivative information, leading to no impact in improving informed and uninformed investor understanding. For example, Chang et al. (2016) find that even sell-side analysts, despite of their financial expertise, routinely misjudge the earnings implications of firms' derivative activities. Campbell et al. (2015) argue that sophisticated investors cannot fully incorporate information related to a firm's cash flow hedges (one type of derivatives designed as hedges under SFAS 161) into their earnings forecasts. Also, since derivative disclosures are provided in footnotes, investors may not pay sufficient attention to the improved information after SFAS 161, leading to minimal effect of the regulation. The second argument, however, is unlikely to hold according to recent studies. Campbell et al. (2018) find that mispricing of firms that use derivatives disappears after the implementation of SFAS 161, suggesting that investors' understanding of firms' hedging activities improves. Therefore, if no change in the information gap between informed and uninformed investors is observed after the passage of SFAS 161, this is probably due to the same impact on these two types of investor. Taken together, the three possible outcomes lead our hypothesis to be established in a null form as follows:

H1: *The enhanced derivative disclosures, as mandated by SFAS 161, lead to no change in information asymmetry between informed and uninformed investors for firms that provide tabular disclosures of the purposes of derivative use.*

Considering the SEC's aim of leveling the playing field and FASB's purpose to enhance the objectives of derivatives use in disclosures for them to be better understood by financial statement users, previously uninformed investors who are not able to possess such information are expected to benefit more. Accordingly, we expect the information asymmetry between informed and uninformed investors to be reduced after SFAS 161.

4.3 Data and sample

4.3.1 Data and sample selection

Following prior research on derivatives (e.g., Guay, 1999; Zhang, 2009; Bartram et al., 2011; Chang et al., 2016), our empirical analysis is based on a hand-collected sample of non-financial and non-utility firms in the U.S. Companies from the financial sectors (two-digit SIC codes 60-69) and utility industries (two-digit SIC code 49) use derivatives primarily for trading purposes and their financial statements are thus substantially different from those of other firms. Since SFAS 161 was issued in 2008, and is effective for annual reporting periods commencing after 15 November 2008, companies generally started applying this standard from the beginning of fiscal year 2009. To investigate the impact of SFAS 161, our sample period spans years 2006-2011, including the three-year pre-SFAS-161 (i.e., 2006-2008) and the three-year post-SFAS-161 (i.e., 2009-2011) periods. We use two measures of information asymmetry in this study: stock liquidity and probability of informed trades. We obtain data primarily from four sources including the Center for Research in Security Prices (CRSP), NYSE Trade and Quote (TAQ), Compustat, and I/B/E/S databases.[25] The information asymmetry measures are constructed using bid and ask price data from the CRSP and TAQ databases. Financial analyst data are from I/B/E/S database. Other stock and financial information is collected from the CRSP and Compustat databases. Firms are excluded from the sample if necessary data are not available from these data sources. For our regression analysis, we further require that firms must have at least three years of consecutive data prior to and after SFAS 161, including years 2008 and 2009.

We end up with 1,036 (1,177) unique firms in our stock liquidity (probability of

[25] We thank Brian Bushee for sharing the institutional investor classification data which we use to construct the variable for dedicated institutional ownership.

informed trade) sample, 670 (721) are derivative users and 366 (456) are non-users. Disclosures on firms' derivative usage and hedging activities are provided in notes to financial statements. We extract firms' 10-K reports manually from SEC's EDGAR database (see derivative disclosures in Dynegy Inc.'s 2007 and 2010 annual reports in Appendix 4.8.2 for example). Among the derivative users, we find that approximately 42% (44%) did not make a real change in response to the disclosure requirements of SFAS 161, in line with Drakopoulou (2014) who finds that most of the Dow 30 companies (Dow Jones Industrial Average) fail to disclose information required by SFAS 161. In order to pursue a rigorous test on the impact of SFAS 161, we identify three types of firm in our sample: (i) compliers – derivative users that follow SFAS 161 to provide tabular disclosures distinguishing between derivatives *designated* and *not designated* as hedging instruments; (ii) non-compliers – derivative users that do not comply with and make real changes in response to SFAS 161; and (iii) non-users – firms that do not use derivatives in any year over the sample period. In our main regression, we use compliers as the treatment group and non-users as the control group to test the effects of SFAS 161. We use compliers (non-compliers) and non-compliers (non-users) as alternative treatment and control groups correspondingly, as additional analyses.

4.3.2 Measuring information asymmetry

One common measure of information asymmetry used in previous work (e.g., Eleswarapu et al., 2004; Mohd, 2005; Silber, 2005; Fu et al., 2012; Chen et al., 2018) is stock liquidity. Since information asymmetry between informed and uninformed investors introduces adverse selection costs into transactions between buyers and sellers, bid-ask spreads, required by market-makers to cover the expected greater losses from trading with informed investors, increase with the level of information asymmetry. Welker (1995), Healy et al. (1999) and Heflin et al. (2005) document a negative relationship between disclosure quality and spreads-based measures of information asymmetry. Following Fang et al. (2009), we measure stock liquidity as the annual relative effective spread. For each stock, the annual relative effective spread is calculated as the arithmetic mean of daily relative effective spreads over a fiscal year.[26] By definition, the relative effective spread is the distance between the transaction price and midpoint of prevailing bid-ask quote divided by the midpoint of prevailing bid-ask quote. We employ the natural logarithm of annual relative effective spread (*LOG_SPREAD*) to deal with the non-normality of effective spreads and use *LOG_SPREAD* in our regression analyses. By construction, *LOG_SPREAD* is negatively related to stock market liquidity, i.e., it captures the level of stock illiquidity.

Second, we use the probability of informed trade (PIN) measure as another proxy for information asymmetry. Brown and Hillegeist (2007) argue that spreads-based measures of information asymmetry suffer from some problems. For example, market makers might protect themselves from information asymmetry by manipulating the quoted bid and ask prices. Therefore, studies solely rely on spread-based measures are incomplete. Following Brown and Hillegeist (2007), the extended measure of PIN based on Easley et al. (1997)'s model is calculated as follows:

$$PIN = \frac{\alpha\mu}{\alpha\mu + 2\varepsilon} = \frac{\alpha\nu\varepsilon}{\alpha\nu\varepsilon + 2\varepsilon} = \frac{\alpha\nu}{\alpha\nu + 2} \tag{4.1}$$

where $\mu = \nu\varepsilon$, meaning that the informed buy or sell orders arrive at a rate (ν) proportional to the arrival rates of uninformed orders ε. When an information event occurs with probability α, PIN increases with the absolute and relative trading intensity of informed investors (μ and ν) and decreases with trading intensity of uninformed investors (ε). This equation, based on Venter and De Jongh (2006)'s extension on Easley et al. (1997)'s model, assumes that the arrival rates of uninformed buy and sell orders are positively correlated on a particular day with events such as earnings announcements. In the context of releasing derivative disclosures to the

[26] Specifically, we use the daily CRSP data (intraday TAQ data) to calculate the bid-ask spreads (PINs) to avoid potential measurement errors arising from using the same database. Chung and Zhang (2014) find that the CRSP-based spread is highly correlated with the TAQ-based spread and argue that the simple CRSP-based spread can be used as a good approximation in research that focuses on cross-sectional analysis.

public, this measure is more appropriate for our study. By construction, probability of informed trades increases with information asymmetry.

4.4 Research design

4.4.1 Matching of treatment and control groups

In order to study the impact of SFAS 161, we define treatment firms as firms affected by SFAS 161 that make changes to their derivative disclosures in response to the requirements of the new standard, and require that these firms use derivatives in years both before and after the implementation of SFAS 161. Our control group consists of unaffected firms that do not use derivatives in any year over the sample period. Although a firm's decision to use derivatives and comply with SFAS 161 is believed to be uncorrelated with the outcome variable, stock liquidity or probability of informed trade, in this study, nonetheless we employ a propensity score matching approach to mitigate potential selection bias. Such a non-parametric matching technique facilitates causal inference by reducing bias due to potential imbalance in observed covariates (Rosenbaum and Rubin, 1985). Specifically, we match each treatment firm with a control firm with replacement by using the closest propensity score within a caliper of 10%. Because we have a limited hand-collected sample, allowing an untreated control firm to be used more than once in our matching procedure guarantees the power of our test. Furthermore, matching with replacement improves the quality of matching and reduces bias (Caliendo and Kopeinig, 2008; Shipman et al., 2017).

Following previous literature (Zhang, 2009; Donohoe, 2015; Chang et al., 2016), we use seven covariates as determinants for variation in derivative usage: market value of equity (*SIZE*), book-to-market ratio (*BTM*), leverage (*LEV*), analyst coverage (*LANACOV*), return-on-assets (*ROA*), dedicated investors' ownership (*DEDI*), and idiosyncratic return volatility (*IDIOSYN*). It is expected that larger and more profitable firms with more dedicated institutional investors are more likely to use derivatives (Donohoe, 2015; Chang et al., 2016). Also, high-growth firms with low book-to-market ratio are more inclined to hedge with derivatives than firms with less growth opportunities. We include analyst coverage to capture analysts' incentives as a relevant characteristic of derivative use (Chang et al., 2016). In addition, we use leverage ratio as a proxy for financial risk, which captures financial distress likelihood, together with firm-specific idiosyncratic risk to gauge firms' incentives for risk management.

In additional analyses, we also use compliers (non-compliers) and non-compliers (non-users) as treatment and control groups, respectively. First, the difference between non-compliers and non-users can also be explained by variation in derivative usage, hence the same set of covariates is used in our logistic regression for propensity-score-matching. Second, for comparison between compliers and non-compliers, the covariates used for matching should be determinants of firms' compliance decisions. Here, analyst coverage (*LANACOV*) and dedicated investor ownership (*DEDI*) are used as proxies for information asymmetry that capture the degree of information transparency of firms complying or not complying with SFAS 161. According to Ge and McVay (2005) and Krishnan et al. (2008), potential costs of compliance include documentation and auditing costs, which are determined by firm size (SIZE), the book-to-market ratio (*BTM*), financial condition such as return-on-assets (*ROA*), leverage (*LEV*), and auditing firm (*AUDIT*). Similar to the determinants of derivative usage, firm size, BTM ratio and idiosyncratic return volatility (*IDIOSYN*) capture the risk profile of derivative users.

4.4.2 Research specification

To examine the impact of SFAS 161 on information asymmetry, we use a difference-in-differences regression model. The research specifications can be expressed in the form as follows:

$$\begin{aligned} LOG\_SPREAD_{i,t} = {} & \alpha_0 + \alpha_1 TREAT_i + \alpha_2 POST_t + \alpha_3 TREAT_i \times POST_t \\ & + \sum_k \alpha_k CONTROLS_{i,t}^k + \sum_z \alpha_z IND_i^z + \sum_t \alpha_t YR_{i,t}^t + \varepsilon_{i,t} \end{aligned} \tag{4.2}$$

$$PIN_{i,t} = \beta_0 + \beta_1 TREAT_i + \beta_2 POST_t + \beta_3 TREAT_i \times POST_t + \sum_k \beta_k CONTROLS^k_{i,t} + \sum_z \beta_z IND^z_i + \sum_t \beta_t YR^t_{i,t} + u_{i,t} \tag{4.3}$$

As described in Section 4.3, the dependent variables *LOG_SPREAD* and *PIN* in models (4.2) and (4.3) are proxies for information asymmetry. *TREAT*×*POST* is an interaction term that captures the impact of SFAS 161 on information asymmetry between informed and uninformed investors, where *TREAT* is the group indicator variable that equals 1 (0) for treatment (control) firms, and *POST* is the time indicator variable that equals 1 (0) if the firm is in the post-(pre-) SFAS 161 period. If SFAS 161 is effective in reducing (increasing) the information gap between informed and uninformed investors, the coefficients on the interaction terms, α_3 and β_3, need to be statistically significant and negative (positive), rejecting the null hypothesis H1, i.e., SFAS 161 has a negative (positive) impact on information asymmetry in terms of decreasing (increasing) stock illiquidity and probability of informed trades. If the coefficients α_3 and β_3 are both statistically insignificant, H1 will not be rejected, suggesting no change in information asymmetry between informed and uninformed investors post SFAS 161.

Prior literature (Mohd, 2005; Fu et al., 2012; Dhaliwal et al., 2016) finds that information asymmetry is associated with firm size (*SIZE*), book-to-market ratio (*BTM*), financial leverage (*LEV*), analyst coverage (*LANACOV*), analyst forecast dispersion (*DISPERSION*), stock return volatility (*STDRET*), dedicated institutional ownership (*DEDI*), return-on-assets (*ROA*), and idiosyncratic risk (*IDIOSYN*). We include these as control variables in model (4.2). First, we measure firm size (*SIZE*) as the natural logarithm of the market value of equity. The amount of information available is greater for larger firms (Atiase, 1985). As information intermediaries, sell-side analysts help disseminate information in the capital market and hence reduce the information asymmetry among investors (Givoly and Lakonishok, 1979; Francis and Soffer, 1997). We expect firm size and analyst coverage are negatively associated with bid-ask spread. In parallel, we expect analyst forecast dispersion (*DISPERSION*), measured as the natural logarithm of the standard deviation of analyst forecasts, to be positively related to bid-ask spread. Since market makers require higher spreads to make up for uncertainty in stock returns, stock return volatility (*STDRET*) and idiosyncratic risk (*IDIOSYN*) are expected to be positively related to the spread (Stoll, 1978; Mohd, 2005). Firm characteristics including book-to-market ratio (*BTM*), leverage ratio (*LEV*), dedicated institutional ownership (*DEDI*), and return-on-assets (*ROA*) are controlled for their impact on the firm's information environment. Value firms with better operating performance and lower leverage tend to be more transparent. Also, if dedicated institutional investors can fulfil their fiduciary responsibilities and serve a monitoring role, they can prompt firms to disclose more information to investors (Bushee, 1998; Mitra and Cready 2005; Chen et al., 2007). Thus, we expect *DEDI* to be negatively associated with information asymmetry.

In model (4.3), we include three alternative control variables for PIN. According to previous literature, the probability of informed trading is lower for high volume stocks since higher arrival rates from informed traders are more than offset by higher arrival rates from uninformed traders (Easley et al., 1996). Trades in less active stocks are more likely by informed traders and hence we expect that trading volume (*TRADEVOL*) is negatively associated with PIN. Higher volatility of market return may result in a higher probability of noise trading (Lee et al., 2002) and imply a higher probability of informed trading. So, we expect a positive association between market excess return (*RETVOL*) and PIN. Also, higher firm risk implies a higher probability that insiders can gain from private information and hence we predict earnings volatility (*STDEARN*) is positively associated with PIN. Both models include year (*YR*) and industry (*IND*) fixed effects as controls for variation in derivative usage over time and across industries.[27] All variables are defined in detail in Appendix 4.8.1.

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[27] To mitigate the impact of managers' opportunistic behavior on information asymmetry between informed and uninformed investors, we add insider trade (*INSITRADE*) as a control variable in our models (4.2) and (4.3), and then re-test the treatment effects. The un-tabulated results show that the coefficients on the interaction term, *TREAT*×*POST*, remain negative and are statistically significant at 5% level for both stock liquidity sample and PIN sample.

4.5 Empirical results

4.5.1 Main results

Table 4.1 panel A and panel B report the descriptive statistics for the dependent variables *LOG_SPREAD* and *PIN*, as well as control variables used in models (4.2) and (4.3), respectively. In the *LOG_SPREAD* (*PIN*) sample, there are 3,627 (4,043) firm-year observations for a total of 1,036 (1,177) unique firms including 374 (404) compliers, 296 (317) non-compliers and 366 (456) non-users. As shown in Panel A, *LOG_SPREAD* has a mean of -6.43 and standard deviation of 1.06, which are consistent with and similar to the statistics in Fang et al. (2009) using the TAQ data. The average probability of informed trade for the firms in our sample is about 14.17%, which is somewhat lower than that (19%) of Brown and Hillegeist (2007)'s sample between 1986 and 1996. This is consistent with the overall information environment has improved over the recent years.

Before carrying out the difference-in-differences regression analysis, we conduct covariate balance checks for our matching procedure. We calculate standard t-statistics for the mean difference in all the covariates between treatment and control firms after matching. Table 4.2 report the results; it shows that majority of the covariates of the treatment group are statistically indifferent from those of the control firms at the conventional level after the propensity score matching. In addition, an alternative way to check the covariate balance is to examine the standardized bias (SB) for each covariate, which is defined by Rosenbaum and Rubin (1985) as:

$$SB = \frac{B}{\sqrt{\dfrac{V_1(X_k)+V_0(X_k)}{2}}} \times 100\% \tag{4.4}$$

where $V_1(X_k)$ ($V_0(X_k)$) is the variance of the covariate for all the observations in the treatment (control) group and B is the selection bias defined as the mean difference in the covariate between treatment and control group (Pan and Bai, 2015). The last columns in Table 4.2 Panels A and B show that the standardized bias for most covariates is below 10%, suggesting that the propensity score matching approach used has sufficiently reduced the imbalance between treatment and control firms in our stock liquidity and PIN samples.

Table 4.3 reports the main results for stock liquidity. By using *k*-to-*k* matching with replacement, the 2,800 firm-year observations in Table 4.3 consist of 1,400 observations from compliers and 1,400 from non-users. The coefficient on *TREAT*×*POST* is statistically significant and negative at the 1% level (p=0.002), leading to the rejection of H1. This result indicates that firms using derivatives and complying with SFAS 161 experience a greater increase in stock liquidity following the implementation of SFAS 161 relative to firms with no derivatives. Similarly, Table 4.4 reports the results from using PIN as the dependent variable. The coefficient on *TREAT*×*POST* is also significantly negative at the 1% level (p=0.001), consistent with the results from Table 4.3. These results imply that the required derivative disclosures by SFAS 161 effectively improve uninformed investors' understanding of the objectives and impact of firms' derivative use, thereby reducing the information asymmetry between informed and uninformed investors in terms of higher stock liquidity and lower probability of informed trades. Results also show that the majority of the control variables are significantly associated with relative effective spreads with expected signs, lending support to the related literature mentioned in Section 4.4.2.

Table 4.6 and Table 4.8 report the results from additional analyses examining the impact of SFAS 161 on information asymmetry, comparing compliers and non-compliers (and non-compliers and non-users), respectively, in the stock liquidity sample and the PIN sample. Before estimating the treatment effects using models (4.2) and (4.3), we also check the covariate balance for the propensity score matching approach applied (as specified in Section 4.4.1). Table 4.5 and Table 4.7 show that the standardized biases for all covariates between compliers and non-compliers (non-compliers and non-users) are reduced to below 10% after matching. Hence, our matching approaches can be considered effective in balancing the distributions of the covariates (Pan and Bai, 2015).

Table 4.6 Column (1) reports a significantly negative coefficient on the interaction term, *TREAT×POST*, when using compliers as the treatment group and non-compliers as the control group. Similarly, for the results from the PIN sample in Table 4.8 Column (1), the treatment effect remains negative when comparing compliers with non-compliers. As an additional analysis, results from Column (2) of Table 4.6 and Table 4.8 show that the effect of SFAS 161 on non-compliers is statistically indifferent from that on non-users, consistent with our expectation. Nevertheless, non-users ideally constitute a better control group than non-compliers because firms with no derivatives are not subject to SFAS 161 and hence are completely unaffected by the regulation and free from the compliance issue.

4.5.2 Robustness tests

In this section, we conduct several tests to verify the robustness of our main results. First, the parallel trend assumption behind the difference-in-differences estimation requires the difference in outcome variable between treatment and control groups to be constant over time. To test this assumption empirically, we first calculate the annual growth rates in stock liquidity (probability of informed trade) as the change in *LOG_SPREAD* (*PIN*) from previous year to current year, divided by the value of *LOG_SPREAD* (*PIN*) in the previous year, for the pre-SFAS 161 period. Un-tabulated t-tests results show that the difference in annual growth rates of *LOG_SPREAD* and *PIN* are statistically indifferent between treatment and control groups in both 2007 and 2008. In addition, we re-define 2007 and 2008 (as well as 2006 and 2007) as pre- and post-event periods, respectively, and then re-estimate the treatment effect in our difference-in-differences regression models (4.2) and (4.3). We find no evidence of significant changes in either *LOG_SPREAD* or *PIN* prior to SFAS 161. These results suggest that the parallel trend assumption is not violated in our analysis.

Second, it is realized that anticipation and early adoption of SFAS 161 may play a role in influencing the estimation of our results. Thus, when collecting data on firm classification, we ensure that all the firms included in our samples start applying SFAS 161 from fiscal year 2009 to eliminate any potential anticipation effect. Furthermore, we re-run the difference-in-differences regression models (4.2) and (4.3) using a new cutoff point of 2008 to test whether the regulation has already taken effect one year prior to the general adoption of SFAS 161. Accordingly, we define 2005-2007 and 2008-2010 as pre- and post-event periods, respectively. In our results not tabulated here, the difference-in-differences estimators are not statistically significant, suggesting that no anticipation effect is likely to be driving our results.

Third, another concern for our study on SFAS 161 is the potential countervailing effect from the recent financial crisis on information asymmetry between informed and uninformed investors. As the impact of the financial crisis lasts from 2007 to 2010 (Chang, 2011; Boyallian and Ruiz-Verdú, 2018), and the general adoption of SFAS 161 ((i.e., the end of 2008)) stands at the midpoint of this crisis period, our results from the difference-in-differences settings should not be confounded by the crisis. To further address the concern, we conduct a placebo test using a pre-SFAS 161 sample, specifically, we define 2005-2006 as the pre-crisis period and 2007-2008 as the crisis period and then re-run the DID regression models to test the treatment effect of the financial crisis. We use the same DID regression models (4.2) and (4.3), where *TREAT* equals 1 (0) for a complier (non-user) and *POST* is replaced by another time indicator variable, *CRISIS*, that equals 1 (0) if the firm is in the crisis (pre-crisis) period (i.e., 2007-2008 (2005-2006)). If financial crisis explains the greater information asymmetry prior to the implementation of SFAS 161, we should find positive and statistically significant results on the DID estimators. However, results in Column (1) and Column (2) in Table 4.9 show that the coefficients on the interaction term *TREAT* × *CRISIS*, are statistically insignificant, suggesting that the reduced information asymmetry after 2008 is not driven by the financial crisis.

4.6 Moderating effect of firm visibility

The implementation of SFAS 161 attracts greater attention from the public to firms' derivative disclosures, yet the scope of derivative usage and investors' attention vary from firm to firm. For less visible firms, retail investors might pay little, or even no attention to their derivative disclosures. In this case, as a result, the information asymmetry might not be reduced after SFAS 161. By contrast, investors may be more attentive to a firm that has good public visibility, in which case information asymmetry would be substantially reduced as a result. Previous research suggests that corporate voluntary disclosures increase analyst and investor following (e.g., Botosan, 1997), but this effect generally focuses on large, highly visible firms. By contrast, smaller and less-visible firms may not be able to attract the initial attention of investors even if they do provide enhanced disclosures (Bushee and Miller, 2012). Therefore, we expect that the impact of SFAS 161 on information asymmetry would be more pronounced for more visible firms.

To test the moderating effect of firm visibility, we first use firm size (*SIZE*) as a simple proxy. Larger firms are more visible and hence they attract more investors. We estimate the treatment effect using regression models (4.2) and (4.3) in two subsamples constructed based on the sample median of *SIZE*. Corresponding to our settings in the main regression analysis, we use compliers as treatment firms and non-users as control firms. Treatment and control firms are matched separately in subsamples and hence the numbers of observations are different. Results for this test are reported in Table 4.10, which shows that the coefficient on the interaction term, $TREAT \times POST$, is only statistically significant and negative in the large-size subsample (*LARGE*) as expected.

Following Da et al. (2011) and Drake et al. (2012), we employ another measure based on the Search Volume Index (SVI) for stock ticker symbols provided by Google Trends to capture investor attention and visibility of firms. Prior evidence shows that greater investor attention, which can be captured by greater advertising expenditure (Grullon et al., 2004), greater media coverage (Fang and Peress, 2009), or higher search frequencies of stock tickers in Google (Ding and Hou, 2015), improves stock liquidity. Thus, we predict the impact of SFAS 161 on information asymmetry between informed and uninformed investors to be more pronounced for firms with greater investor attention.

Data for the Search Volume Index (SVI) of search terms have been available from Google Trends (http://www.google.com/trends) since January 2004. We measure investor attention for a stock based on its daily SVI data. Specifically, the variable of abnormal search volume around the earnings announcement (*ASVI*) is calculated as follows:

$$ASVI_t = \ln\left[1 + \left(\text{Mean}(SVI_{t-1}, SVI_{t-2}) - \text{Mean}(SVI_{t-3}, \ldots, SVI_{t-10})\right)\right] \tag{4.5}$$

where $[\text{Mean}(SVI_{t-1}, SVI_{t-2})]$ is the average of the SVI over the prior two weeks and $[\text{Mean}(SVI_{t-3}, \ldots, SVI_{t-10})]$ is the average of the SVI over the prior eight weeks ending at the beginning of the prior two weeks. Following previous studies (e.g., Da et al., 2011; Drake et al., 2012; Ding and Hou, 2015), we exclude SVIs with value of zero and use the natural logarithm to normalize the distribution. By construction, a high value of *ASVI* indicates a surge in investor attention prior to the earnings announcement. Since investors may start to pay attention to a stock by searching in Google well ahead of the earnings announcement date, we measure the abnormal search volume (*ASVI*) as in equation (4.5) to proxy for investor attention.

We conduct similar subsample tests to examine the moderating effect of investor attention. Table 4.11 reports the results, Column (1) in Panel A (Panel B) shows that the coefficient on the interaction term is not significant in the low-attention subsample whereas the coefficient for *TREAT*×*POST* is statistically significant at the 1% (10%) level with the correct sign in stock liquidity (PIN) sample, consistent with our prediction. Compared to firm size, Google search volume is a more direct measure of investor attention (Da et al., 2011). However, our results should be interpreted with caution because the sample size is substantially reduced after combining the SVI data.

4.7 Conclusion

The objective of this study is to examine whether SFAS 161 reduces the information asymmetry between informed and uninformed investors. The implementation of SFAS 161 could suppress informed trading by sophisticated investors and improve overall stock liquidity, or widen the information gap due to the more complex information provided that less informed investors are not able to digest, or even leads to no change in the information asymmetry between informed and uninformed investors. The impact depends crucially on the various abilities of investors to incorporate the enhanced derivative information into firm valuation. Our results show that derivative users compliant with SFAS 161 which provide enhanced disclosures distinguishing the purposes of their derivative use experience an increase in stock liquidity and a reduction in probability of informed trades following SFAS 161. This implies that SFAS 161 is effective in reducing information asymmetry, shrinking the information gap between informed and uninformed investors. We also find that such impact is stronger for larger, more visible firms with greater investor attention.

Our findings have both practical and regulatory implications. The significantly different impacts of SFAS 161 on compliant and non-compliant derivatives users suggest that less informed investors gain additional knowledge from the enhanced derivative disclosures. In this context, this study demonstrates the importance of enhancing the enforcement of SFAS 161, which succeeds in promoting SEC's aim of leveling the playing field.

4.8 Appendices

4.8.1 Summary of variable definitions

Variables	Definitions
LOG_SPREAD	As per Fang et al. (2009), the stock illiquidity measure is calculated as the natural logarithm of annual relative effective spread, which is the arithmetic mean of daily relative effective spreads for a stock. The relative effective spread is calculated as the absolute value of difference between the transaction price (either buy or sell) and the midpoint of the prevailing bid-ask quote, divided by the midpoint of the prevailing bid-ask quote.
PIN	Probability of informed trade as per Brown and Hillegeist (2007), based on Venter and De Jongh (2006)'s extension on Easley et al. (1997)'s model.
POST	1 if a firm is in the three fiscal years (i.e., 2009-2011) after the enforcement of SFAS 161 in 2008, and 0 if a firm is in the three fiscal years of the pre-SFAS 161 period (i.e., 2006-2008).
TREAT	1 for a treated firm that provides tabular disclosures of derivatives designated and not designated as hedging instruments in the 10-K report in any year after the SFAS 161, and 0 for a control firm that reports no derivatives in any year over the sample period.
CRISIS	1 if a firm is in the two-year crisis period (i.e., 2007-2008), and 0 if a firm is in the two-year pre-crisis period (i.e., 2005-2006).
NONCOMPLIER	1 for a treated firm that does not complied with SFAS 161, i.e., it does not provide the tabular disclosures distinguishing between derivatives designated as hedges and those not designated as hedges, and 0 for a non-user of derivatives.
SIZE	The natural logarithm of the market value of a firm's equity at the end of a fiscal year.
BTM	The book value of firm equity divided by the market value of firm equity at the end of a fiscal year, winsorized at the 1% and 99% levels, respectively.
DEDI	Dedicated institutional investors' stock ownership as a percentage of a firm's outstanding shares at the end of the fiscal year.
LANACOV	The natural logarithm of 1 plus the number of analysts that make at least one annual EPS forecast for a firm over a fiscal year.
DISPERSION	The dispersion in analyst forecasts is measured as the standard deviation of analysts' annual EPS forecasts made four months prior to the end of a fiscal year, divided by stock price at the beginning of the fiscal year.
STDRET	The standard deviation of firm-specific weekly returns for a fiscal year.
ROA	Return on assets calculated as income before extraordinary items divided by total assets at the beginning of fiscal year.
LEV	The sum of short-term and long-term debt divided by total assets for a firm over a fiscal year. We set missing values of short-term debt equal to zero and drop the observation for which long-term debt is missing.
FIRMAGE	The number of years a firm has been listed.
TRADEVOL	The average of monthly trading volume for a firm over a fiscal year, scaled by shares outstanding at the end of the year.

IDIOSYN	Idiosyncratic return volatility calculated as the standard deviation of the residuals from the following market model regression over the 52-week window before the end of a fiscal year: $r_{i,t} = \alpha_i + \beta_{1i} r_{m,t-1} + \beta_{2i} r_{m,t-2} + \beta_{3i} r_{m,t} + \beta_{4i} r_{m,t+1} + \beta_{5i} r_{m,t+2} + \varepsilon_{i,t}$, where $r_{i,t}$ is the weekly return on firm i and $r_{m,t}$ is the value-weighted CRSP index return (Kim et al., 2011).
ASVI	$ASVI_t = \ln\left[1 + \left(\text{Mean}(SVI_{t-1}, SVI_{t-2}) - \text{Mean}(SVI_{t-3}, \dots, SVI_{t-10})\right)\right]$, where $[\text{Mean}(SVI_{t-1}, SVI_{t-2})]$ is the average of the SVI during the prior two weeks and $[\text{Mean}(SVI_{t-3}, \dots, SVI_{t-10})]$ is the average of the SVI during the prior eight weeks ending at the beginning of the prior two weeks. Following Drake et al. (2012), SVI data is on a daily basis.
RETVOL	The standard deviation of daily market excess return over a year ending at the end of the fiscal year.
STDEARN	The standard deviation of income before extraordinary items in the current and previous four fiscal years.
AUDIT	The auditor code indicates the auditing firms which audited the financial statements of a company for a fiscal year. It ranges from 0 to 27, where 0 indicates an unaudited firm.

4.8.2 Examples of Derivative Disclosures Before and After the SFAS 161

1. An excerpt from notes to Consolidated Financial Statements of Dynegy Inc. for the fiscal year ended December 31, 2007

"The absolute notional contract amounts associated with our commodity risk-management and interest rate contracts are discussed in Item 7A. Quantitative and Qualitative Disclosures About Market Risk below.

Item 7A. Quantitative and Qualitative Disclosures About Market Risk

We are exposed to commodity price variability related to our power generation business and legacy trading portfolio. In addition, fuel requirements at our power generation facilities represent additional commodity price risks to us. In order to manage these commodity price risks, we routinely utilize various fixed-price forward purchase and sales contracts, futures and option contracts traded on the New York Mercantile Exchange and swaps and options traded in the over-the-counter financial markets to:

- *manage and hedge our fixed-price purchase and sales commitments;*
- *reduce our exposure to the volatility of cash market prices; and*
- *hedge our fuel requirements for our generating facilities.*

The potential for changes in the market value of our commodity, interest rate and currency portfolios is referred to as "market risk". A description of each market risk category is set forth below:

- *commodity price risks result from exposures to changes in spot prices, forward prices and volatilities in commodities, such as electricity, natural gas, coal, fuel oil, emissions and other similar products; and*
- *interest rate risks primarily result from exposures to changes in the level, slope and curvature of the yield curve and the volatility of interest rates.*

In the past, we have attempted to manage these market risks through diversification, controlling position sizes and executing hedging strategies. The ability to manage an exposure may, however, be limited by adverse changes in market liquidity, our credit capacity or other factors.

Credit Risk. *Credit risk represents the loss that we would incur if a counterparty fails to perform pursuant to the terms of its contractual obligations. To reduce our credit exposure, we execute agreements that permit us to offset receivables, payables and mark-to-market exposure. We attempt to further reduce credit risk with certain counterparties by obtaining third party guarantees or collateral as well as the right of termination in the event of default.*

Our Credit Department, based on guidelines approved by the Board of Directors, establishes our counterparty credit limits. Our industry typically operates under negotiated credit lines for physical delivery and financial contracts. Our credit risk system provides current credit exposure to counterparties on a daily basis.

The following table represents our credit exposure at December 31, 2007 associated with the mark-to-market portion of our risk-management portfolio, on a net basis.

Credit Exposure Summary

		Investment Grade Quality (in millions)
Type of Business:		
Financial institutions	$	263
Utility and power generators		35
Total	$	298

Interest Rate Risk. *Interest rate risk primarily results from variable rate debt obligations. Although changing interest rates impact the discounted value of future cash flows, and therefore the value of our risk management portfolios, the relative near-term nature and size of our risk management portfolios minimizes the impact. Management continues to monitor our exposure to fluctuations in interest rates and may execute swaps or other financial instruments to change our risk profile for this exposure.*

We are exposed to fluctuating interest rates related to variable rate financial obligations. As of December 31, 2007, our fixed rate debt instruments as a percentage of total debt instruments was 78 percent. Adjusted for interest rate swaps, net notional fixed rate debt as a percentage of total debt was approximately 82 percent. Based on sensitivity analysis of the variable rate financial obligations in our debt portfolio as of December 31, 2007, it is estimated that a one percentage point interest rate movement in the average market interest rates (either higher or lower) over the twelve months ended December 31, 2008 would either decrease or increase interest expense by approximately $11 million. However, interest rate risk associated with our $850 million variable rate term letter of credit facility is mitigated by restricted cash backing this facility. Variable rate interest income earned on the investment of the restricted cash effectively offsets the risk associated with the variable rate interest expense. Over time, we may seek to adjust the variable rate exposure in our debt portfolio through the use of swaps or other financial instruments.

Derivative Contracts. *The absolute notional financial contract amounts associated with our interest rate contracts were as follows at December 31, 2007 and 2006, respectively:"*

Absolute Notional Contract Amounts

		December 31, 2007		December 31, 2006
Cash flow hedge interest rate swaps (in millions of U.S. dollars)	$	310	$	—
Fixed interest rate paid on swaps (percent)		5.32		—
Fair value hedge interest rate swaps (in millions of U.S. dollars)	$	25	$	525
Fixed interest rate received on swaps (percent)		5.70		4.33
Interest rate risk-management contracts (in millions of U.S. dollars)	$	231	$	306
Fixed interest rate paid (percent)		5.35		5.29
Interest rate risk-management contracts (in millions of U.S. dollars)	$	206	$	281
Fixed interest rate received (percent)		5.28		5.23

2. **An excerpt from notes to Consolidated Financial Statements of Dynegy Inc. for the fiscal year ended on December 31, 2010**

"On January 1, 2009, we adopted authoritative guidance which requires disclosure of the fair values of derivative instruments and their gains and losses in a tabular format. It also provides more information about an entity's liquidity by requiring disclosure of derivative features that are credit risk-related and it requires cross-referencing within footnotes to enable financial statement users to locate important information about derivative instruments.

The following disclosures and tables present information concerning the impact of derivative instruments on our consolidated balance sheets and statements of operations. In the table below, commodity contracts primarily consist of derivative contracts related to our power generation business that we have not designated as accounting hedges, that are entered into for purposes of economically hedging future fuel requirements and sales commitments and securing commodity prices. Interest rate contracts primarily consist of derivative contracts related to managing our interest rate risk. As of December 31, 2010, our commodity derivatives were comprised of both long and short positions; a long position is a contract to purchase a commodity, while a short position is a contract to sell a commodity. As of December 31, 2010, we had net long/(short) commodity derivative contracts outstanding and notional interest rate swaps outstanding in the following quantities:

Contract Type	**Hedge Designation**	**Quantity**	**Unit of Measure**	**Net Fair Value**
		(in millions)		**(in millions)**
Commodity derivative contracts:				
Electric energy (1)	Not designated	(63)	MW	$ 264
Natural gas (1)	Not designated	134	MMBtu	$ (207)
Electricity/natural gas spread options	Not designated	(7)/60	MW/MMBtu	$ (31)
Other (2)	Not designated	—	Misc.	$ 8
Interest rate contracts:				
Interest rate swaps	Fair value hedge	(25)	Dollars	$ 1
Interest rate swaps	Not designated	206	Dollars	$ (5)
Interest rate swaps	Not designated	25	Dollars	$ (1)
Interest rate swaps	Not designated	(206)	Dollars	$ 5

(1) Mainly comprised of swaps, options and physical forwards.
(2) Comprised of coal, crude oil, fuel oil options, swaps and physical forwards.

Derivatives on the Balance Sheet. *The following table presents the fair value and balance sheet classification of derivatives in the consolidated balance sheet as of December 31, 2010 and 2009, segregated between designated, qualifying hedging instruments and those that are not, and by type of contract segregated by assets and liabilities. We do not offset fair value amounts* *recognized for derivative instruments executed with the same counterparty under a master netting agreement and we did not elect to adopt the netting provisions that allow an entity to offset the fair value amounts recognized for the Daily Cash Settlements paid or received against the fair value amounts recognized for derivative instruments executed with the same counterparty under a master netting agreement. As a result, our consolidated balance sheets present derivative assets and liabilities, as well as related Daily Cash Settlements, on a gross basis.*

Contract Type	**Balance Sheet Location**	**December 31, 2010**	**December 31, 2009**
		(in millions)	
Derivatives designated as hedging instruments:			
Derivative Assets:			
Interest rate contracts	Assets from risk management activities	$ 1	$ 2
Derivative Liabilities:			
Interest rate contracts	Liabilities from risk management activities	—	—
Total derivatives designated as hedging instruments, net		1	2
Derivatives not designated as hedging instruments:			
Derivative Assets:			
Commodity contracts	Assets from risk management activities	1,265	861
Interest rate contracts	Assets from risk management activities	5	13
Derivative Liabilities:			
Commodity contracts	Liabilities from risk management activities	(1,231)	(844)
Interest rate contracts	Liabilities from risk management activities	(6)	(65)
Total derivatives not designated as hedging instruments, net		33	(35)
Total derivatives, net		$ 34	$ (33)

Impact of Derivatives on the Consolidated Statements of Operations

The following discussion and tables present the disclosure of the location and amount of gains and losses on derivative instruments in our consolidated statements of operations for the twelve months ended December 31, 2010, 2009 and 2008 segregated between designated, qualifying hedging instruments and those that are not, by type of contract.

Cash Flow Hedges. *We may enter into financial derivative instruments that qualify, and* *that we may elect to designate, as cash flow hedges. Interest rate swaps have been used to convert floating interest rate obligations to fixed interest rate obligations.*

Our former investee, PPEA, which we consolidated through December 31, 2009, had certain interest rate swap agreements which were designated as cash flow hedges. Therefore, the effective portion of the changes in value prior to July 28, 2009 was reflected in other comprehensive income (loss). On July 28, 2009, we determined the interest rate swap agreements no longer qualified for cash flow hedge accounting because the hedged forecasted transaction (that is, the future interest payments arising from the PPEA Credit Agreement Facility) was no longer probable of occurring. We performed a final effectiveness test as of July 28, 2009 and no ineffectiveness was recorded. The associated risk management liability was classified as current at December 31, 2009, as the interest rate swap agreements could have been terminated at the discretion of a third party guarantor of PPEA's obligations under the agreements. Effective January 1, 2010, we deconsolidated our investment in PPEA Holding, and we sold our interest in this entity in the fourth quarter of 2010. Please read Note 15—Variable Interest Entities—PPEA Holding Company LLC for further discussion of our association with PPEA. The amounts previously deferred in Accumulated other comprehensive income (loss) were recognized in earnings upon our sale of our investment in PPEA Holding in the fourth quarter of 2010, resulting in a loss of $28 million, included in Losses from unconsolidated investments on our consolidated statement of operations.

During the twelve month periods ended December 31, 2010, 2009 and 2008, we recorded zero, zero and $2 million, respectively, related to ineffectiveness from changes in fair value of derivative positions and no amounts were excluded from the assessment of hedge effectiveness related to the hedge of future cash flows in any of the periods. During the twelve month periods ended December 31, 2010, 2009 and 2008, no amounts were reclassified to earnings in connection with forecasted transactions that were considered probable of not occurring.

The amount of gain (loss) recognized in Other comprehensive loss on the effective portion of interest rate swap contracts designated as cash flow hedges was a gain of $166 million and a loss of $142 million for the years ended December 31 2009 and 2008, respectively. As of July 28, 2009, these derivatives no longer qualified for cash flow hedge accounting, and therefore, no additional gains or losses have been recognized in Other comprehensive income since that date.

Fair Value Hedges. *We also enter into derivative instruments that qualify, and that we may elect to designate, as fair value hedges. We use interest rate swaps to convert a portion of our non-prepayable fixed-rate debt into floating-rate debt. The maximum length of time for which we have hedged our exposure for fair value hedges is through 2011. During the twelve month periods ended December 31, 2010, 2009 and 2008, there was no ineffectiveness from changes in the fair value of hedge positions and no amounts were excluded from the assessment of hedge effectiveness. During the twelve month periods ended December 31, 2010, 2009 and 2008, there were no gains or losses related to the recognition of firm commitments that no longer qualified as fair value hedges.*

The impact of interest rate swap contracts designated as fair value hedges and the related hedged item on our consolidated statements of operations for the twelve months ended December 31, 2010, 2009 and 2008 was immaterial.

Financial Instruments Not Designated as Hedges. *We elect not to designate derivatives related to our power generation business and certain interest rate instruments as cash flow or fair value hedges. Thus, we account for changes in the fair value of these derivatives within the consolidated statements of operations (herein referred to as "mark-to-market accounting treatment"). As a result, these mark-to-market gains and losses are not reflected in the consolidated statements of operations in the same period as the underlying activity for which the derivative instruments serve as economic hedges.*

For the twelve months ended December 31, 2010, our revenues included approximately $21 million of mark-to-market gains related to this activity compared to $180 million of mark-to-market losses and $252 million of mark-to-market gains in the periods ended December 31, 2009 and 2008, respectively.

The impact of derivative financial instruments that have not been designated as hedges on our consolidated statements of operations for the twelve month periods ended December 31, 2010 and 2009 is presented below. Note that this presentation does not reflect the expected gains or losses arising from the underlying physical transactions associated with these financial instruments. Therefore, this presentation is not indicative of the economic gross profit we expect to realize when the underlying physical transactions settle.

Derivatives Not Designated as Hedging Instruments	**Location of Gain (Loss) Recognized in Income on Derivatives**	**Amount of All Gain (Loss) Recognized in Income on Derivatives for the Twelve Months Ended December 31,**		
		2010	**2009**	**2008**
		(in millions)		
Commodity contracts	Revenues	$ 185	$ 337	$ 264
Interest rate contracts	Interest expense	—	(12)	(2)

Fair Value of Financial Instruments. *On June 30, 2009, we adopted authoritative guidance which requires the disclosure of the estimated fair value of financial instruments. We have determined the estimated fair-value amounts using available market information and selected valuation methodologies. Considerable judgment is required in interpreting market data to develop the estimates of fair value. The use of different market assumptions or valuation methodologies could have a material effect on the estimated fair value amounts.*

The carrying values of financial assets and liabilities (cash, accounts receivable, short-term investments and accounts payable), not presented in the table below, approximate fair values due to the short-term maturities of these instruments. The carrying amounts and fair values of debt are reflected in Note 18—Debt.

	December 31, 2010		December 31, 2009	
	Carrying Amount	Fair Value	Carrying Amount	Fair Value
	(in millions)			
Interest rate derivatives designated as fair value accounting hedges (1)	$ 1	$ 1	$ 2	$ 2
Interest rate derivatives not designated as accounting hedges (1)	(1)	(1)	(52)	(52)
Commodity-based derivative contracts not designated as accounting hedges (1)	34	34	17	17
Other—DHI (2)	175	175	8	8
Other—Dynegy (3)	16	16	1	1

(1) Included in both current and non-current assets and liabilities on the consolidated balance sheets.

(2) Other represents short-term investments, including $85 million of short-term investments included in the

(3) Other represents short-term investments at December 31, 2010."

Table 4.1: Descriptive statistics

Panel A. Stock liquidity (*LOG_SPREAD*) sample

Variables	No. of firm-years	No. of firms	Mean	Std. dev.	25th	Median	75th
LOG_SPREAD	3,627	1,036	-6.4336	1.0577	-7.1000	-6.6051	-6.0141
SIZE	3,627	1,036	6.9610	1.6971	5.9211	6.9373	7.9746
BTM	3,627	1,036	0.5942	0.7272	0.2743	0.4460	0.7145
LEV	3,627	1,036	0.1685	0.1724	0.0016	0.1315	0.2715
LANACOV	3,627	1,036	3.3976	1.2658	2.8332	3.6109	4.2485
DISPERSION	3,627	1,036	0.3256	2.8728	0.0527	0.1086	0.2156
STDRET	3,627	1,036	0.0649	0.0308	0.0453	0.0589	0.0773
DEDI	3,627	1,036	0.0775	0.0943	0.0084	0.0528	0.1185
ROA	3,627	1,036	0.1124	1.3138	0.0356	0.0657	0.1040
IDIOSYN	3,627	1,036	0.0550	0.0247	0.0395	0.0510	0.0656
ASVI	1,424	457	0.0240	0.4619	-0.0912	-0.0038	0.0913
AUDIT	2,453	674	6.4851	3.2272	4	6	7

Panel B. Probability of informed trade (*PIN*) sample

Variables	No. of firm-years	No. of firms	Mean	Std. dev.	25th	Median	75th
PIN	4,043	1,177	0.1417	0.0883	0.0875	0.1177	0.1696
SIZE	4,043	1,177	6.6253	1.7579	5.4747	6.6128	7.7246
BTM	4,043	1,177	0.6852	1.4501	0.2675	0.4632	0.7658
LEV	4,043	1,177	0.1689	0.1802	0.0006	0.1225	0.2764
LANACOV	4,043	1,177	3.2814	1.2829	2.7081	3.4965	4.1589
DISPERSION	4,043	1,177	8.5229	430.1907	0.0566	0.1216	0.2589
DEDI	4,043	1,177	0.0775	0.0963	0.0041	0.0499	0.1197
TRADEVOL	4,043	1,177	0.4612	0.9426	-0.0038	0.5720	1.0634
RETVOL	4,043	1,177	0.1393	0.0870	0.0873	0.1195	0.1655
STDEARN	4,043	1,177	108.4451	563.9920	5.6351	16.6536	56.0563
ROA	4,043	1,177	0.0370	1.2588	-0.0053	0.0440	0.0894
IDIOSYN	4,043	1,177	0.0607	0.0291	0.0421	0.0553	0.0724
ASVI	1,262	489	0.0760	0.5449	-0.0513	0.0356	0.1244
AUDIT	2,388	673	6.5188	3.2859	4	6	7

Notes: These tables present the descriptive statistics for the variables used in the multivariate tests before matching, in stock liquidity (*LOG_SPREAD*) and probability of informed trade (*PIN*) sample, respectively. The full samples contain firm-year observations for the period of 2006-2011. All the variables are defined in Appendix 4.8.1.

Table 4.2: Covariate balance check for Table 4.3

Panel A. Stock liquidity (*LOG_SPREAD*) sample

Variables	Mean *TREAT*=1 (N=1,400)	Mean *TREAT*=0 (N=1,400)	Mean Difference (t-stat)	Standardized Bias (%)
SIZE	7.6623	7.6943	-0.0320	-2.0
			(-0.50)	
BTM	0.5834	0.5984	-0.0150	-2.1
			(-0.38)	
LEV	0.2120	0.2094	0.0026	1.7
			(0.37)	
LANACOV	3.7183	3.7112	0.0071	0.6
			(0.18)	
ROA	0.1557	0.0932	0.0626	4.2
			(1.11)	
DEDI	0.0866	0.0818	0.0048	5.2
			(1.38)	
IDIOSYN	0.0494	0.0513	-0.0018**	-7.3
			(-2.25)	

Panel B. Probability of informed trade (*PIN*) sample

Variables	Mean *TREAT*=1 (N=1,521)	Mean *TREAT*=0 (N=1,521)	Mean Difference (t-stat)	Standardized Bias (%)
SIZE	7.4568	7.4292	0.0276	1.7
			(0.44)	
BTM	0.7036	0.8252	-0.1216**	-9.3
			(-2.07)	
LEV	0.3825	0.1531	0.2295	4.0
			(1.1)	
LANACOV	3.6655	3.5399	0.1256***	10.3
			(2.97)	
ROA	0.1164	0.0560	0.0604	4.2
			(1.16)	
DEDI	0.0872	0.0863	0.0010	1.0
			(0.24)	
IDIOSYN	0.0525	0.0529	-0.0003	-1.1
			(-0.37)	

Notes: These tables report the descriptive statistics of complier (*TREAT*=1) group and non-user (*TREAT*=0) group after propensity score matching, in stock liquidity (*LOG_SPREAD*) and probability of informed trade (*PIN*) sample, respectively. The t-statistic from two-sample test of mean and standardized bias are calculated as a check for balance of measured covariates. The sample period covers the years of 2006-2011. The indicator variable *TREAT* equals to 1 if a firm provides tabular disclosures of designated and non-designated hedges, complying with SFAS 161, and 0 if a firm reports no derivatives in any year over the sample period. All the variables are defined in Appendix 4.8.1. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 4.3: The impact of SFAS 161 on information asymmetry between informed and uninformed investors: evidence from stock liquidity

Variables	Predicted Sign	Dependent Variable= LOG_SPREAD_t
Intercept		-4.5769***
		(-10.323)
$TREAT_i$		-0.0570**
		(-2.047)
$POST_t$		0.0072
		(0.205)
$TREAT_i \times POST_t$	**?**	**-0.1048*** **
		(-3.059)
$SIZE_t$	-	-0.1921***
		(-22.199)
BTM_t	-	0.2316***
		(21.605)
LEV_t	+	0.1568***
		(3.047)
$LANACOV_t$	-	-0.3045***
		(-26.543)
$DISPERSION_t$	+	-0.0048**
		(-2.312)
$STDRET_t$	+	2.0022***
		(3.601)
$DEDI_t$	-	-0.3198***
		(-3.290)
ROA_t	-	-0.0057
		(-1.011)
$IDIOSYN_t$	+	3.2875***
		(4.872)
Year-fixed effects		included
Industry-fixed effects		included
No. of observations		2,800
Adjusted R-squared		0.8000

Notes: This table reports the results of difference-in-differences tests for the impact of SFAS 161 on information asymmetry between informed and uninformed investors. The sample period covers the years of 2006-2011. The dependent variable is stock illiquidity (LOG_SPREAD_t). The group indicator variable, $TREAT_i$, equals to 1 if a firm provides tabular disclosures of designated and non-designated hedges, complying with SFAS 161, and 0 if a firm reports no derivatives. The time indicator variable, $POST_t$, equals 1 (0) if a firm is in the post-SFAS 161 (pre-SFAS 161) period (2009-2011 (2006-2008)). The interaction term, $TREAT_i \times POST_t$, is the variable of interest, coefficient for which measures the effect of SFAS 161 on relative effective spreads for complier (*TREAT*=1) relative to non-users (*TREAT*=0). All the variables are defined in Appendix 4.8.1. Industry dummies and year dummies are included in all the regression but are not reported for simplicity. The t-statistics are reported in parentheses and the robust standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 4.4: The impact of SFAS 161 on information asymmetry between informed and uninformed investors: evidence from probability of informed trade

Variables	Predicted Sign	Dependent Variable= PIN_t
Intercept		0.2338***
		(4.267)
$TREAT_i$		0.0035
		(1.046)
$POST_t$		-0.0039
		(-0.840)
$TREAT_i \times POST_t$	**?**	**-0.0139*** **
		(-3.373)
$SIZE_t$	-	-0.0187***
		(-20.035)
BTM_t	-	-0.0012
		(-1.553)
LEV_t	+	9.79E-06
		(0.057)
$LANACOV_t$	-	-0.0039**
		(-2.523)
$DISPERSION_t$	+	0.0008***
		(4.069)
$DEDI_t$	-	-0.0235**
		(-2.382)
$TRADEVOL_t$	-	-0.0383***
		(-23.861)
$RETVOL_t$	+	0.0799***
		(4.471)
$STDEARN_t$	+	4.08E-06**
		(2.204)
Year-fixed effects		included
Industry-fixed effects		included
No. of observations		3,042
Adjusted R-squared		0.5474

Notes: This table reports the results of difference-in-differences tests for the impact of SFAS 161 on information asymmetry between informed and uninformed investors. The sample period covers the years of 2006-2011. The dependent variable is probability of informed trade (PIN_t). The group indicator variable, $TREAT_i$, equals to 1 if a firm provides tabular disclosures of designated and non-designated hedges, complying with SFAS 161, and 0 if a firm reports no derivatives. The time indicator variable, $POST_t$, equals 1 (0) if a firm is in the post-SFAS 161 (pre-SFAS 161) period (2009-2011 (2006-2008)). The interaction term, $TREAT_i \times POST_t$, is the variable of interest, coefficient for which measures the effect of SFAS 161 on relative effective spreads for complier (*TREAT*=1) relative to non-users (*TREAT*=0). All the variables are defined in Appendix 4.8.1. Industry dummies and year dummies are included in all the regression but are not reported for simplicity. The t-statistics are reported in parentheses and the robust standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 4.5: Covariate balance check for Table 4.6

Panel A. Compliers and non-compliers

Variables	Mean *TREAT*=1 (N=1,451)	Mean *TREAT*=0 (N=1,451)	Mean Difference (t-stat)	Standardized Bias (%)
SIZE	7.6748	7.7154	-0.0406 (-0.66)	-2.5
BTM	0.5963	0.5688	0.0275 (1.08)	4.1
LEV	0.3901	0.1735	0.2166 (0.99)	3.7
LANACOV	3.7270	3.7696	-0.0426 (-1.02)	-3.5
ROA	0.1537	0.0742	0.0794 (1.46)	5.4
DEDI	0.0858	0.0852	0.0006 (0.16)	0.6
IDIOSYN	0.0497	0.0496	0.0001 (0.19)	0.7
AUDIT	6.1902	6.1268	0.0634 (0.62)	1.9

Panel B Non-compliers and non-users

Variables	Mean *TREAT*=1 (N=1,023)	Mean *TREAT*=0 (N=1,023)	Mean Difference (t-stat)	Standardized Bias (%)
SIZE	6.8148	6.8435	-0.0287 (-0.38)	-1.8
BTM	0.6270	0.6798	-0.0527 (-1.29)	-7.1
LEV	0.2052	0.2076	-0.0024 (-0.30)	-1.5
LANACOV	3.3350	3.3304	0.0046 (0.08)	0.4
ROA	0.0724	0.0773	-0.0049* (-1.90)	-6.5
DEDI	0.0762	0.0768	-0.0006 (-0.16)	-0.7
IDIOSYN	0.0549	0.0556	-0.0007 (-0.67)	-2.7

Notes: These tables report the descriptive statistics of different treatment groups and control groups after propensity score matching, in stock liquidity (*LOG_SPREAD*) sample. Panel A shows the comparison between compliers (*TREAT*=1) and non-compliers (*TREAT*=0). Panel B shows the comparison between non-compliers (*TREAT*=1) and non-users (*TREAT*=0). The t-statistic from two-sample test of mean and standardized bias are calculated as a check for balance of measured covariates. The sample period covers the years of 2006-2011. All the variables are defined in Appendix 4.8.1. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 4.6: The impact of SFAS 161 on stock liquidity: comparison between compliers and non-compliers (vs. non-compliers and non-users)

Variables		Dependent Variable = *LOG_SPREAD_t*	
	Predicted Sign	(1) Compliers vs. Non-Compliers	(2) Non-Compliers vs. Non-Users
Intercept		0.3025***	-3.9889***
		(13.698)	(-17.291)
$TREAT_i$		-0.0037	-0.1315***
		(-0.143)	(-3.963)
$POST_t$		-0.2029***	-0.7307***
		(-5.096)	(-16.011)
$TREAT_i \times POST_t$	**?**	**-0.0635****	**0.0073**
		(-1.961)	**(0.168)**
$SIZE_t$	-	-0.1972***	-0.2390***
		(-22.875)	(-21.402)
BTM_t	-	0.2031***	0.1815***
		(16.098)	(12.555)
LEV_t	+	0.0004	0.1941***
		(0.198)	(2.822)
$LANACOV_t$	-	-0.2535***	-0.3772***
		(-22.181)	(-28.245)
$DISPERSION_t$	+	-0.0128	-0.0031
		(-1.423)	(-1.635)
$STDRET_t$	+	1.8040***	2.0996***
		(3.508)	(3.430)
$DEDI_t$	-	-0.5065***	-0.0450
		(-6.317)	(-0.348)
ROA_t	-	-0.0089	0.0634
		(-1.011)	(0.318)
$IDIOSYN_t$	+	5.1722***	2.5399***
		(8.219)	(3.060)
Year-fixed effects		included	included
Industry-fixed effects		included	included
No. of observations		2,902	2,046
Adjusted R-squared		0.7764	0.8057

Notes: This table reports the results from difference-in-differences analyses for the impact of SFAS 161 on stock liquidity, using different treatment and control groups. In column (1), the group indicator variable, *$TREAT_i$*, equals to 1 (0) for a complier (non-complier). In column (2), the group indicator variable, *$TREAT_i$*, equals to 1 (0) for a non-complier (non-user). The sample period covers the years of 2006-2011. The dependent variable is stock illiquidity (*LOG_SPREAD_t*). The time indicator variable, *$POST_t$*, equals 1 (0) if a firm is in the post-SFAS 161 (pre-SFAS 161) period (2009-2011 (2006-2008)). The coefficient on interaction term, *$TREAT_i \times POST_t$*, captures the treatment effects. All the variables are defined in Appendix 4.8.1. Industry dummies and year dummies are included in all the regression but are not reported for simplicity. The t-statistics are reported in parentheses and the robust standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 4.7: Covariate balance check for Table 4.8

Panel A. Compliers and non-compliers

Variables	Mean *TREAT*=1 (N=1,259)	Mean *TREAT*=0 (N=1,259)	Mean Difference (t-stat)	Standardized Bias (%)
SIZE	7.6723	7.6525	0.0198 (0.30)	1.2
BTM	0.5933	0.5937	-0.0003 (-0.01)	0.0
LEV	0.2096	0.2112	-0.0016 (-0.24)	-0.9
LANACOV	3.7087	3.7812	-0.0725 (-1.61)	-5.9
ROA	0.1633	0.0731	0.0902 (1.45)	5.7
DEDI	0.0882	0.0919	-0.0037 (-0.94)	-3.9
IDIOSYN	0.0497	0.0513	-0.0017* (-1.88)	-7.6
AUDIT	6.2433	6.1969	0.0464 (0.41)	1.4

Panel B. Non-compliers and non-users

Variables	Mean *TREAT*=1 (N=966)	Mean *TREAT*=0 (N=966)	Mean Difference (t-stat)	Standardized Bias (%)
SIZE	6.4581	6.5675	-0.1094 (-1.36)	-6.9
BTM	0.7581	0.8013	-0.0431 (-0.54)	-3.0
LEV	0.1837	0.1751	0.0086 (1.01)	5.4
LANACOV	3.1935	3.1815	0.0120 (0.20)	0.9
ROA	0.0016	0.0154	-0.0139* (-1.74)	-6.4
DEDI	0.0810	0.0783	0.0027 (0.61)	2.8
IDIOSYN	0.0594	0.0593	0.0001 (0.11)	0.4

Notes: This table reports the descriptive statistics of different treatment groups and control groups after propensity score matching, in probability of informed trade (*PIN*) sample. Panel A shows the comparison between compliers (*TREAT*=1) and non-compliers (*TREAT*=0). Panel B shows the comparison between non-compliers (*TREAT*=1) and non-users (*TREAT*=0). The t-statistic from two-sample test of mean and standardized bias are calculated as a check for balance of measured covariates. The sample period covers the years of 2006-2011. All the variables are defined in Appendix 4.8.1. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 4.8: The impact of SFAS 161 on probability of informed trade: comparison between compliers and non-compliers (vs. non-compliers and non-users)

Variables		Dependent Variable = PIN_t	
	Predicted Sign	(1) Compliers vs. Non-Compliers	(2) Non-Compliers vs. Non-Users
Intercept		0.3025***	0.3197***
		(13.698)	(7.702)
$TREAT_i$		0.0059**	-0.0035
		(2.141)	(-0.854)
$POST_t$		0.0329***	-0.0153**
		(7.073)	(-2.505)
$TREAT_i \times POST_t$	**?**	**-0.0065***	**-0.0020**
		(-1.780)	**(-0.363)**
$SIZE_t$	-	-0.0164***	-0.0199***
		(-16.697)	(-16.076)
BTM_t	-	-0.0012	-0.0008
		(-0.899)	(-0.993)
LEV_t	+	0.0014	-0.0045
		(0.235)	(-0.557)
$LANACOV_t$	-	-0.0062***	-0.0090***
		(-4.181)	(-4.786)
$DISPERSION_t$	+	0.0012	-6.64E-07
		(1.405)	(-0.307)
$DEDI_t$	-	-0.0223**	-0.0034
		(-2.416)	(-0.235)
$TRADEVOL_t$	-	-0.0315***	-0.0391***
		(-20.501)	(-19.463)
$RETVOL_t$	+	0.0884***	0.1252***
		(5.042)	(6.562)
$STDEARN_t$	+	1.91E-06**	7.00E-06**
		(2.487)	(2.212)
Year-fixed effects		included	included
Industry-fixed effects		included	included
No. of observations		2,518	1,932
Adjusted R-squared		0.5788	0.6167

Notes: This table reports the results from difference-in-differences analyses for the impact of SFAS 161 on probability of informed trade, using different treatment and control groups. In column (1), the group indicator variable, $TREAT_i$, equals to 1 (0) for a complier (non-complier). In column (2), the group indicator variable, $TREAT_i$, equals to 1 (0) for a non-complier (non-user). The sample period covers the years of 2006-2011. The dependent variable is probability of informed trade (PIN_t). The time indicator variable, $POST_t$, equals 1 (0) if a firm is in the post-SFAS 161 (pre-SFAS 161) period (2009-2011 (2006-2008)). The coefficient on interaction term, $TREAT_i \times POST_t$, captures the treatment effects. All the variables are defined in Appendix 4.8.1. Industry dummies and year dummies are included in all the regression but are not reported for simplicity. The t-statistics are reported in parentheses and the robust standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 4.9: Placebo test – the potential confounding effect of financial crisis

Variables	(1) Dependent Variable = LOG_SPREAD_t	(2) Dependent Variable = PIN_t
Intercept	-4.7115***	0.3394***
	(-29.116)	(25.821)
$TREAT_i$	0.0273	0.0026
	(0.323)	(0.451)
$CRISIS_t$	0.0997	-0.0272***
	(1.121)	(-5.294)
$\boldsymbol{TREAT_i \times CRISIS_t}$	**-0.0851**	**0.0036**
	(-1.212)	**(0.568)**
$SIZE_t$	-0.1710***	-0.0254***
	(-8.506)	(-21.138)
BTM_t	0.2216***	-0.0002
	(9.584)	(-0.210)
LEV_t	0.0331	0.0226***
	(0.193)	(3.048)
$LANACOV_t$	-0.3296***	-0.0017
	(-12.906)	(-0.965)
$DISPERSION_t$	-0.0031	0.0007
	(-0.704)	(0.567)
$DEDI_t$	-0.0283	-0.0929***
	(-0.121)	(-8.440)
ROA_t	-0.0012	
	(-0.472)	
$IDIOSYN_t$	2.9414	
	(1.595)	
$STDRET_t$	1.6236	
	(1.174)	
$TRADEVOL_t$		-0.0410***
		(-21.482)
$RETVOL_t$		0.0949***
		(3.065)
$STDEARN_t$		1.43E-05**
		(2.234)
Year-fixed effects	included	included
Industry-fixed effects	included	included
No. of observations	1,334	1,196
Adjusted/Pseudo R-squared	0.7953	0.7391

Notes: This table reports the results from the placebo tests examining the potential confounding effect of financial crisis on information asymmetry between informed and uninformed investors. The dependent variable is stock illiquidity (LOG_SPREAD_t) in Column (1) and probability of informed trade (PIN_t) in Column (2). The group indicator variable, $TREAT_i$, equals to 1 if a firm provides tabular disclosures of designated and non-designated hedges, complying with SFAS 161, and 0 if a firm reports no derivatives. The time indicator variable, $CRISIS_t$, equals 1 (0) if a firm is in the crisis (pre-crisis) period (i.e., 2007-2008 (2005-2006)). The interaction term, $TREAT_i \times CRISIS_t$, is the variable of interest. All the variables are defined in Appendix 4.8.1. Industry dummies and year dummies are included in all the regression but are not reported for simplicity. The t-statistics are reported in parentheses and the robust standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 4.10: The moderating effect of firm visibility: evidence from firm size

Panel A. Stock liquidity (*LOG_SPREAD*) sample

Variables	Dependent Variable = *LOG_SPREAD*$_t$	
Firm Size (*SIZE*)	**(1) SMALL**	**(2) LARGE**
Intercept	-2.7254***	-5.0033***
	(-6.627)	(-14.009)
TREAT$_i$	-0.1243***	0.0738**
	(-3.583)	(1.972)
POST$_t$	-0.6027***	-0.6945***
	(-13.863)	(-14.951)
***TREAT*$_i$×*POST*$_t$**	**0.0019**	**-0.1833*****
	(0.042)	**(-4.393)**
SIZE$_t$	-0.4873***	-0.0498***
	(-29.672)	(-4.489)
BTM$_t$	0.1536***	0.1555***
	(13.575)	(4.169)
LEV$_t$	0.0126	0.2302***
	(0.200)	(3.141)
LANACOV$_t$	-0.2397***	-0.2910***
	(-17.979)	(-18.678)
DISPERSION$_t$	-0.0013	-0.1141***
	(-0.497)	(-8.885)
STDRET$_t$	1.3299**	0.7806
	(2.042)	(0.942)
DEDI$_t$	-0.1486	0.1714*
	(-1.104)	(1.735)
ROA$_t$	0.8178***	-0.0033
	(4.768)	(-0.760)
IDIOSYN$_t$	2.5521***	10.9287***
	(3.472)	(11.000)
Year-fixed effects	included	included
Industry-fixed effects	included	included
No. of observations	1,388	1,412
Adjusted R-squared	0.8340	0.7490

Table 4.10 (Continued)

Panel B. Probability of informed trade (*PIN*) sample

Variables	Dependent Variable = PIN_t	
Firm Size (*SIZE*)	**(1) SMALL**	**(2) LARGE**
Intercept	0.3476***	0.2609***
	(5.751)	(4.994)
$TREAT_i$	-0.0003	0.0166***
	(-0.063)	(3.469)
$POST_t$	-0.0157**	0.0770***
	(-2.237)	(13.758)
$\boldsymbol{TREAT_i \times POST_t}$	**-0.0078**	**-0.0285*** **
	(-1.212)	**(-5.134)**
$SIZE_t$	-0.0303***	-0.0125***
	(-13.382)	(-6.394)
BTM_t	-0.0030***	0.0040
	(-3.385)	(0.764)
LEV_t	0.0033	0.0001
	(0.332)	(0.383)
$LANACOV_t$	-0.0046**	-0.0084***
	(-2.154)	(-3.130)
$DISPERSION_t$	0.0007**	-0.0004
	(2.263)	(-0.648)
$DEDI_t$	-0.0134	-0.0149
	(-0.744)	(-1.311)
$TRADEVOL_t$	-0.0429***	-0.0197***
	(-16.537)	(-7.222)
$RETVOL_t$	0.0189	0.0766**
	(0.712)	(2.533)
$STDEARN_t$	1.53E-05	3.02E-07
	(1.348)	(0.168)
Year-fixed effects	included	included
Industry-fixed effects	included	included
No. of observations	1,458	1,584
Adjusted R-squared	0.5602	0.5240

Notes: This table reports the results for subsample test examining the moderating effect of firm visibility. The sample period covers the years of 2006-2011. The dependent variable is stock illiquidity (LOG_SPREAD_t) and probability of informed trade (PIN_t), respectively, in Panel A and Panel B. The moderator variable is firm size (*SIZE*). Difference-in-differences test is run separately in the small-size subsample and large-size subsample, which is split based on the sample median of *SIZE*. The group indicator variable, $TREAT_i$, equals to 1 if a firm provides tabular disclosures of designated and non-designated hedges, complying with SFAS 161, and 0 if a firm reports no derivatives. The time indicator variable, $POST_t$, equals 1 (0) if a firm is in the post-SFAS 161 (pre-SFAS 161) period (2009-2011 (2006-2008)). The interaction term, $TREAT_i \times POST_t$, is the variable of interest. All the variables are defined in Appendix 4.8.1. Industry dummies and year dummies are included in all the regression but are not reported for simplicity. The t-statistics are reported in parentheses and the robust standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 4.11: The moderating effect of investor attention: evidence from Google Trends' Search Volume Index (SVI)

Panel A. Stock liquidity (*LOG_SPREAD*) sample

Variables	Dependent Variable = LOG_SPREAD_t	
Investor Attention (*ASVI*)	**(1) LOW**	**(2) HIGH**
Intercept	-4.1013***	-5.4634***
	(-9.518)	(-14.602)
$TREAT_i$	-0.2398***	-0.0586
	(-3.274)	(-1.175)
$POST_t$	-0.6179***	-0.6849***
	(-7.353)	(-13.365)
$\boldsymbol{TREAT_i \times POST_t}$	**0.0743**	**-0.2347*** **
	(0.960)	**(-4.283)**
$SIZE_t$	-0.2293***	-0.1109***
	(-12.952)	(-7.433)
BTM_t	-0.0051	0.1714***
	(-0.183)	(4.669)
LEV_t	-0.0238	-0.0862
	(-0.190)	(-0.848)
$LANACOV_t$	-0.2658***	-0.2520***
	(-9.962)	(-12.426)
$DISPERSION_t$	0.0313**	0.0037
	(2.386)	(0.096)
$STDRET_t$	-1.4107	3.2796***
	(-1.118)	(3.408)
$DEDI_t$	0.8923***	0.8445***
	(4.887)	(6.166)
ROA_t	1.2648***	-0.3527
	(3.261)	(-1.115)
$IDIOSYN_t$	6.6156***	3.9811***
	(4.043)	(3.688)
Year-fixed effects	included	included
Industry-fixed effects	included	included
No. of observations	610	806
Adjusted R-squared	0.8613	0.8032

Table 4.11 (Continued)

Panel B. Probability of informed trade (*PIN*) sample

Variables	Dependent Variable = PIN_t	
Investor Attention (*ASVI*)	**(1) LOW**	**(2) HIGH**
Intercept	0.2976***	0.3337***
	(7.796)	(7.296)
$TREAT_i$	0.0082	-0.0083
	(1.329)	(-1.292)
$POST_t$	-0.0156**	0.0177***
	(-2.311)	(2.849)
$\boldsymbol{TREAT_i \times POST_t}$	**-0.0049**	**-0.0141***
	(-0.609)	**(-1.847)**
$SIZE_t$	-0.0249***	-0.0227***
	(-12.454)	(-10.363)
BTM_t	-0.0061***	0.0075**
	(-3.326)	(2.287)
LEV_t	0.0148	0.0263**
	(1.275)	(2.104)
$LANACOV_t$	0.0024	-0.0041
	(0.707)	(-1.289)
$DISPERSION_t$	0.0034***	-0.0003
	(3.661)	(-0.050)
$DEDI_t$	-0.0370*	0.0641***
	(-1.917)	(3.375)
$TRADEVOL_t$	-0.0385***	-0.0421***
	(-11.587)	(-11.774)
$RETVOL_t$	0.0984***	0.0165
	(2.719)	(0.465)
$STDEARN_t$	1.42E-05**	4.70E-06
	(2.534)	(0.846)
Year-fixed effects	included	included
Industry-fixed effects	included	included
No. of observations	550	588
Adjusted R-squared	0.7391	0.7101

Notes: This table reports the results for subsample test examining the moderating effect of investor attention. The sample period covers the years of 2006-2011. The dependent variable is stock illiquidity (LOG_SPREAD_t) and probability of informed trade (PIN_t), respectively, in Panel A and Panel B. The moderator variable is the investor attention (*ASVI*), constructed based on daily SVI data. Difference-in-differences test is run separately in the low-attention subsample and high-attention subsample, which is split based on the sample median of *SVI*. The group indicator variable, $TREAT_i$, equals to 1 if a firm provides tabular disclosures of designated and non-designated hedges, complying with SFAS 161, and 0 if a firm reports no derivatives. The time indicator variable, $POST_t$, equals 1 (0) if a firm is in the post-SFAS 161 (pre-SFAS 161) period (2009-2011 (2006-2008)). The interaction term, $TREAT_i \times POST_t$, is the variable of interest. All the variables are defined in Appendix 4.8.1. Industry dummies and year dummies are included in all the regression but are not reported for simplicity. The t-statistics are reported in parentheses and the robust standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Chapter 5

Concluding Remarks

In summary, the three main chapters provide insights and empirical evidence on the implications and market consequences of corporate disclosures in the context of empirical corporate finance.

In Chapter 2, we find that financial constraints are positively associated with future stock price crash risk. This finding contributes directly to the literature on the determinants of stock price crash risk (e.g., Kim et al., 2011a, b; Andreou et al., 2017; Chang et al., 2017; Hong et al., 2017). While most studies in the literature focus on the bad news hoarding mechanism, we also establish the default risk mechanism through which financial constraints will increase stock price crash risk. In addition, this chapter adds to the literature by providing insights into the tension between benefits and costs associated with managerial bad news hoarding. Moreover, there is a wide literature on financial constraint examines its association with corporate investment (Fazzari et al., 1988; Kaplan and Zingales, 1997; Denis and Sibilkov, 2010), real business activities (Campello et al., 2010), and provides a mixed evidence on the relationship between financial constraints and stock returns (Lamont et al., 2001; Whited and Wu, 2006; Livdan et al., 2009; Li, 2011). We contribute to this literature by focusing on the extreme future returns of financially constrained firms and showing that financial constraints can also play a role in firms' information management.

In addition, cross-sectional analyses in Chapter 2 further reveal that firms can pursue less earnings management, improve their corporate governance system, commit tax avoidance and generate higher credit ratings to mitigate the impact of financial constraints on future crash risk. In summary, Chapter 2 provides the implications of financial constraints on future extreme negative stock returns and is of interest to investors as well as other stakeholders concerned about firms' creditworthiness and viability.

One limitation of Chapter 2 is that we have not tested the two mechanisms of crash risk directly in our empirical analysis. Managers' bad news hoarding behavior is unlikely to be observed by outsiders without the access to private information and hence is hard to be empirically measured and tested. Default risk is the probability of firms going bankrupt that is unanticipated by investors, which is also difficult to estimate. These issues may be left to future experimental research.

Despite that examples of the ways that managers can use accruals to withhold bad news are provided in Appendix 2.7.2, we do not study in detail the practices of managerial bad news hoarding behavior in the real world. The existing stock price crash risk literature builds on the bad news hoarding theory (Jin and Myers, 2006; Bleck and Liu, 2007) and expands on the determinants of crashes through this mechanism, but none has provided direct evidence on this impact. Future research can therefore address the nature of firm-specific crashes and provides direct evidence on the bad news hoarding behavior causing future crashes.

Chapter 3 finds that enhanced derivative disclosures curb managerial opportunism via reduced information asymmetry between insiders and outsiders and via more efficient use of derivatives for hedging. Both insider trades and stock price crash risk of the firm are alleviated after the implementation of SFAS 161. We also find that such impact is stronger for firms with high information opacity, financial risk, and business risk.

Chapter 4 examining another level of information asymmetry finds that improved derivative disclosures reduce the information gap between informed and uninformed investors, leading to a reduction in stock illiquidity and probability of informed trades. Chapter 4 also shows that in the case of firms with greater investor attention, the impact of enhanced derivative disclosures will be more pronounced.

Looking into firms' derivative disclosures, in Chapter 3 and 4, we find that many firms fail to comply with SFAS 161; their disclosures are qualitatively the same as those before SFAS 161. To test the impact of SFAS 161, a simple distinction between users and non-users of derivatives may lead to biased results. Our additional analyses show that SFAS 161 is only effective for firms that comply with the regulation. These results call for stronger enforcement actions to be taken by authorities to ensure the compliance with disclosure requirements. Future studies can evaluate the potential reasons for not complying with SFAS 161. Since we employ a difference-in-differences approach using six-year data around the implementation of SFAS 161 to test the effect of the regulation, future research can use more recent data to investigate whether firms' compliance with derivative disclosure requirements has improved over the past few years. Because our research is limited to the information in financial statements provided by firms, a case study or survey can be conducted in future research to examine firms' practices in using derivatives for non-hedging purposes such as speculation and earnings management.

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