Derivative disclosures and managerial opportunism

ABSTRACT

Derivatives are increasingly used by managers not only to hedge risks but also to pursue non-hedging activities for fulfilling opportunistic incentives. The Statement of Financial Accounting Standards No. 161 (hereafter, SFAS 161) requires firms to disclose their objectives and strategies of using derivatives. Using the adoption of this standard, 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 research 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. We further provide evidence to suggest that SFAS 161 curbs managerial opportunism via reducing information asymmetry between corporate insiders and outside investors and enhancing the effectiveness of derivative hedging. 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.

Keywords: derivative disclosures; insider trading; crash risk; hedging; information asymmetry; business risk

JEL Classification: G14; G32; M48

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 pursued at the expense of outside investors induce the use of derivatives for non-hedging 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, as the Statement of Financial Accounting Standards No. 161 (henceforth, SFAS 161) mandates. The aim of our study is to examine whether such derivative disclosures mandated by SFAS 161 reduce managerial opportunism.[1] We define managerial opportunism as managers' opportunistic behavior 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

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[1] 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 the 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 the year 1998. SFAS 133 and SFAS 161 were codified under the Accounting Standards Codification Topic 815 (ASC 815) *Derivatives and Hedging* in the year 2014.

(Brown, 2001; Faulkender, 2005; Géczy et al., 2007; Chernenko and Faulkender, 2011; Manchiraju et al., 2016, 2018), giving rise to information uncertainty and/or asymmetry faced by investors. Unfortunately, different managerial incentives for using derivatives cannot be easily distinguished by investors, 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 varied 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 value. 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.[2]

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 that SFAS 161 induces 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).[3] 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 had better knowledge about how derivative usage affects stock performance and traded on such information, might 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 hedging purpose

[2] Throughout this paper, we refer to information asymmetry as that between managers and outsider investors, rather than that between informed and uninformed investors as examined by some prior research (Fu et al., 2012).

[3] 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.

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 1,164 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 further test the two mechanisms through which the derivative disclosures stipulated by SFAS 161 mitigate managerial opportunism. To the extent that SFAS 161 restrains managerial opportunism via reducing information asymmetry between corporate insiders and outside investors (and via encouraging prudent risk management and prompting more derivative usage for hedging purposes), we expect that the information asymmetry (and business risk) of derivative-using compliers would be reduced after the passage of SFAS 161. Consistent with our conjecture on the information-asymmetry (and real-effect) mechanism, we find that SFAS 161 leads to lower information opacity and smaller bid-ask spreads (and lower idiosyncratic risk as well as lower earnings volatility) of the compliers.

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 (Bhattacharya et al., 2022). 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.

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 among the existing literature to provide evidence that disclosures of firms' objectives and strategies of using derivatives curb managerial opportunism.

Our study also makes several contributions to the extant literature on regulations. First, while this literature focuses 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.

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, "*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, we account for issues about firms' compliance with a disclosure regulation when investigating its impacts on firms. 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 induce 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.

Last but not least, prior studies (e.g., Chang et al., 2016; Khan et al., 2018; Campbell et al., 2021; Chen et al., 2021) provide mixed findings on the information usefulness of SFAS 161. While these studies focus on the informational perspective in their analyses, our study provides insights into the effect of SFAS 161 on managerial opportunism from both the informational and real-effect perspectives, and suggests that the derivative disclosures stipulated by SFAS 161 not only reduce information asymmetry but also restrain managers from using derivatives for non-hedging purposes.

The remainder of the paper is organized as follows. In Section 2, we develop our main hypotheses. Section 3 provides details of the data sources, sample selection, and variables. Section 4 explains research design. Section 5 discusses our empirical results. Section 6 conducts further analyses, and Section 7 concludes.

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).[4] 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. Companies which possess private information about prospective development trends in their industry could engage in trading activities with use of derivatives for the purpose of generating profits from market price changes of commodities (Manchiraju et al., 2018), and such

[4] Both SFAS 133 and SFAS 161 require that an ineffective (effective) hedge be recognized in earnings when a firm enters into the hedge contract (only after the hedge expires). However, the concept and reporting of hedge effectiveness are difficult for investors to understand (Tysiac, 2017). Thus, in August 2017, FASB issued an update on hedge accounting, which allows a firm to have an ineffective hedge recognized in earnings after the hedge expiry date (FASB, 2017). This update is effective for the fiscal years beginning after 15 December 2019.

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. To the extent that SFAS 161 achieves its objectives of reducing information asymmetry between managers and outside investors (Campbell et al., 2021; Chen et al., 2021), it should help 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 curb 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 frequency of 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., personal 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.*

Secondly, 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., 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 corporate 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 outside investors. 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, 2021) 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 reducing 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. Sample construction

3.1. Data and sample selection

Our empirical analysis is based on a sample of U.S. listed 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 (the first two-digit standard industrial classification (SIC) codes 60-69) and utility industry (the first 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 the years 2006-2011, covering the three-year pre-SFAS-161 period (i.e., 2006-2008) and the three-year 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), respectively. To constitute our sample for the hypothesis tests, we begin with all non-financial and non-utility firms with available data on Compustat and CRSP for the fiscal years 2006-2011. As with Donohoe (2015) and Chang et al. (2016), a company is included in our sample if it has data for at least three consecutive years including the years

2008 and 2009 that surround the regulatory event. 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 observations of which the 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 were hand-collected from 10-K filings in the Securities and Exchange Commission's EDGAR files (see, for example, the tabular disclosures in the Kadant Inc.'s 2010 annual report in Appendix B).[5] 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.[6]

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, which are segregated by types of risk exposures as required by SFAS 161, although this standard 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

5 The example disclosures made by Kadant Inc. show that the company uses both interest swap agreements and forward currency exchange contracts for hedging purposes in the pre-SFAS 161 period, but some forward currency exchange contracts are not designated as hedging instruments in the post-SFAS 161 disclosures. Thus, the tabular disclosures distinguishing between "*derivatives designated as hedging instruments*" and "*derivatives not designated as hedging instruments*" in the post-SFAS 161 period might provide intuitive information to investors about the purposes of the firm's derivative use, thereby facilitating their investment decision-making.

6 The titles can also be "designated hedges" and "non-designated hedges".

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 (388 firms), non-compliers (321 firms), and non-users (455 firms).

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 regression 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 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 non-compliers 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 concerning managerial opportunism. To avoid this problem, we define compliers as our treatment sample and non-users as our control sample. After removing the observations that have missing values in our difference-in-differences regressors, the compliers (as well as noncompliers) retained in our sample are those 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 (i.e., 2009-2011). As such, we obtain 2,757 firm-year observations for insider trades and 2,849 firm-year observations for crash risk, corresponding with 711 (372 compliers and 339 non-users) and 735 (388 compliers and 347 non-users) unique firms, respectively.[7] The summary statistics for variables are presented in Table 1. To deal with potential outliers, we winsorize all continuous variables at the 1% and 99% levels, respectively.

3.3. Measures of managerial opportunism

We employ two proxies for managerial opportunism for 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.[8] 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

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[7] In our initial sample of 1,208 firms, there are only 17 firms which use derivatives prior to SFAS 161 (i.e., 2005-2008) but not after SFAS 161 (i.e., 2009-2011). Any firm, which stops or starts using derivatives during our sample period as a result of SFAS 161 implemented in 2008, is excluded from our sample used for the DID regression estimations. As such, the effect of SFAS 161 on a firm's choice of whether to use derivatives is unlikely to confound our results.

[8] 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. Noticeably, data are not available for distinguishing whether stocks sold by insiders are attributed to those granted by their company or to those purchased from the open stock market. As a result, the variables for insider trading profits are likely to involve nontrivial measurement error. Hence, we do not use insider trading profits to proxy for managerial opportunism in our main tests.

Kim et al. (2011a), we use an indicator variable (*CRASH*) to capture the likelihood of extremely low firm-specific 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:

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

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.

4. Research design

4.1. Matching of samples 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 coincide with a treatment effect on 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 changes observed in our control firms which are unaffected by the standard. The treatment and control samples are defined as in Section 3.

A firm's decision to use or not use derivatives might be influenced by the enforcement of SFAS 161, thus engendering a selectivity issue with our DID analysis. To mitigate potential selection bias and mimic the condition of a random assignment of observations into the treatment and control groups for our DID analysis, we use a propensity-score-matching approach (e.g., Irani and Oesch, 2013; Chang et al., 2016; Ke et al., 2019; Chen et al., 2021) 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), better-performing firms (Chang et al., 2016), and firms with high dedicated institutional stock holdings (e.g., Bodnar et al., 2003; Chang et al., 2016). Furthermore, highly leveraged firms (Tufano, 1996; Haushalter, 2000; Graham and Rogers, 2002), financially constrained firms (Acharya et al., 2007), and firms that have high cash flow volatility, high earnings volatility, high stock return volatility, or high idiosyncratic risk (Froot et al., 1993; DeMarzo and Duffie, 1995; Minton and Schrand, 1999; Bartram et al., 2011) are more likely to hedge. In addition, firms might use derivatives for tax planning and thereby lower cash effective tax rate (Donohoe, 2015). Therefore, based on the related literature, we use the market value of equity (*SIZE*), the book-to-market ratio (*BTM*), financial leverage (*LEV*), financial constraints (*SA*), return on assets (*ROA*), dedicated institutional stock holdings (*DEDI*), cash flow volatility (*STDCFO*), earnings volatility (*STDEARN*), idiosyncratic risk (*IDIOSYN*), cash effective tax rate (*CETR*), and stock return volatility (*RETVOL*) as our matching covariates in the logistic regression. We also control for year dummies and industry dummies, since corporate use of derivatives are likely to vary substantially across industries and years. All the covariates are measured for the years before the implementation of SFAS 161 (i.e., 2006-2008) to avoid the matching being affected by the event. The results from the logistic regression on the eleven matching covariates are presented in Panel A of Table 2, and are generally consistent with those reported in Chang et al. (2016).

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 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 matching covariates has standardized bias greater than 10%, suggesting that the matching procedure effectively reduces the covariate imbalance between the treatment and control groups for our sample. After the matching, we end up with 2,834 and 2,790 firm-year observations for the insider trading sample and the crash risk sample, corresponding to 702 and 714 firms, respectively.

4.2. Difference-in-differences regression specification

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

$$\begin{aligned} INSITRADE_{i,t} &= \alpha_0 + \alpha_1 TREAT_i + \alpha_2 TREAT_i \times POST_t \\ &+ \sum_k \alpha_k CONTROLS_{i,t}^k + \sum_t \alpha_t YR_{i,t}^t + \sum_z \alpha_z IND_i^z + \varepsilon_{i,t} \end{aligned} \tag{2}$$

$$\begin{aligned} CRASH_{i,t+1} &= \beta_0 + \beta_1 TREAT_i + \beta_2 TREAT_i \times POST_t \\ &+ \sum_k \beta_k CONTROLS_{i,t}^k + \sum_t \beta_t YR_{i,t}^t + \sum_z \beta_z IND_i^z + u_{i,t} \end{aligned} \tag{3}$$

Models (2) and (3) specify insider trades and one-year-ahead stock price crash risk,[9]

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

respectively, as the dependent variable. The treatment indicator variable, $TREAT_i$, equals 1 for a treatment firm and 0 for a control firm. Because SFAS 161 was effective for annual reporting periods commencing after 15 November 2008, all our treatment firms start applying this standard from the fiscal year 2009. Accordingly, the time indicator variable, $POST_t$, 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_2 in Model (2) and β_2 in Model (3)) indicate greater impacts of SFAS 161 on insider trades and crash risk. Hence, to support the hypotheses H1 and H2, the coefficients for the interaction terms should be negative and statistically significant at conventional levels. We control for year-fixed effects (*YR*) and industry-fixed effects (*IND*) in the regressions since insider trades and crash risk are likely to vary systematically across years and industries. Meanwhile, we do not include $POST_t$ in our difference-in-differences regression models as this variable is multicollinear with the year-fixed effects.[10]

We include a range of control variables in Models (2) and (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 on assets (*ROA*) and the 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

stock price crash risk often results from managers' hoarding of bad news.

[10] We get qualitatively identical results and the same inferences, should we include industry-year interacted dummies in our DID regressions. We also do not include *POST* either in such a case because *POST* is multicollinear with the industry-year interacted dummies too.

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 stock ownership (*DEDI*) as controls for the 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 stock 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), we include firm size (*SIZE*), the book-to-market ratio (*BTM*), analyst coverage (*LANACOV*), dedicated institutional stock ownership (*DEDI*), return on assets (*ROA*), stock trading volume (*TRADEVOL*), cash flow volatility (*STDCFO*), corporate tax avoidance (*CETR*), and negative skewness of firm-specific weekly stock 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), and 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* are more likely to have high crash risk in year *t*+1. Thus, we also control for negative skewness of weekly returns (*NCSKEW*). Definitions of all the foregoing control variables are detailed in Appendix A.

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 the 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 two-sample t-tests in Panel A of Table 3 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 (2) and (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 shown in Panel B of Table 3, we do not find any significant change in insider trading or crash risk for the treatment firms relative to the control firms. The foregoing results are all supportive of the parallel trends assumption for our DID regression analysis.

5. Empirical results

5.1. Baseline regression results for the hypotheses H1 and H2

Table 4 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 = 0.01). The coefficient for $TREAT_i \times POST_t$ amounts to 1.2221, which accounts for 24.5% 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, the hypothesis H1 is supported.

Column (2) shows a similar result of a statistically significant coefficient on *TREAT*× *POST* with a negative sign (p-value=0.088), 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 the hypothesis 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 lowering stock price crash risk. The marginal effect of $TREAT_i \times POST_t$ for crash risk amounts to 4.79 percentage points, which is equivalent to 24.6% of the mean of *CRASH* for the treatment sample and hence is economically significant.

The coefficients for *TREAT* are statistically insignificant in both Columns (1) and (2), suggesting that there is no difference in our outcome variable, *INSITRADE* or *CRASH*, between the treatment and control firms for our pre-SFAS-161 sample period. Such insignificant coefficients provide further support for the parallel trend assumption for our DID regression analyses. In addition, we conduct variance-inflation-factors (VIF) tests 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.

5.2. Checks of robustness of baseline regression results

5.2.1. 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 (2) and (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 reported in Table 5, we find no statistically significant result for the DID estimators, suggesting that the anticipation effect is unlikely to be at play to drive our baseline results.

5.2.2. Financial crisis

A potential countervailing force that might weaken the inference from our baseline regression results is the impact of the global financial crisis, which, as documented in Chang (2011) and Boyallian and Ruiz-Verdú (2018), lasts from 2007 to 2010. Nevertheless, unlike SFAS 161, the financial crisis affects both our treatment and control firms. Furthermore, the

SFAS 161 event stands at the midpoint of the crisis period of 2007-2010 (i.e., the end of 2008). Therefore, the effect of the crisis should not confound our DID regression results. To further allay the concern about the confounding effect of financial crisis, we conduct three robustness checks using placebo tests and alternative samples.

First, 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 could play a role in explaining the reduction in managerial opportunism post SFAS 161. However, our results in Columns (1) and (2) of Panel A in Table 6 show that the coefficients on the interaction terms, $TREAT_i \times POSTCRISIS_t$, of our re-run DID regressions are not statistically significant.

Second, we use 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 for the DID estimators. Nonetheless, we do not find such evidence: the coefficients on the interaction term, $TREAT_i \times CRISIS_t$, are not statistically significant in Columns (3) and (4) of Panel A in Table 6.

Third, we exclude the years 2008 and 2009 from our sample period (i.e., 2006-2011) and re-estimate Models (2) and (3). Results are reported in Panel B of Table 6. The coefficients for $TREAT_i \times POST''_t$ are negative and statistically significant at the 10% and 1% levels for the insider trades sample and the crash risk sample, respectively. Collectively, the results in Table 6 suggest that our earlier finding of the reduced managerial opportunism is attributed to SFAS 161 rather than financial crisis.

5.2.3. Firm-fixed effects

Although our baseline regression models (2) and (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.[11] The treatment indicator variable, $TREAT_i$, is excluded on account of its multicollinearity with the firm-fixed effects. Table 7 presents the results. In Columns (1) and (2), the coefficients for the interaction terms are significant at the 1% and 10% levels 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 factor(s). To avoid the potential problem of overcontrolling variables, we also run a firm-fixed effects regression model that includes only $TREAT_i \times POST_t$, 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).

6. Further tests

6.1. Tests of mechanisms through which SFAS 161 curbs managerial opportunism

As discussed previously, there are two mechanisms through which the derivative disclosures prescribed by SFAS 161 curb managerial opportunism. The first mechanism is that the derivative disclosures reduce the extent of asymmetry of derivative-related information between managers and investors. The second mechanism is that the derivative disclosures induce managers to shift derivative usage away from speculative activities and towards hedging

[11] 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), where the dependent variable is an indicator variable of crash risk (*CRASH*), drops to 1,487 observations.

activities to reduce business risk. To test these two mechanisms, we analyze whether SFAS 161 mitigates the information asymmetry and business risk of derivative-using firms that comply with the disclosure requirements.

6.1.1. Test of the information asymmetry mechanism

The first mechanism for our main hypotheses concerns whether SFAS 161 is effective in reducing the information asymmetry and thereby deterring managerial opportunism. A lack of information transparency enables managers to conceal bad news or malpractices from outside investors for an extended period (Jin and Myers, 2006), and hence the probability of stock price crashes for these firms will be higher. The likelihood and extent of insider trading are also higher when information opacity is high (Huddart and Ke, 2007). Following Hutton et al. (2009), we measure information opacity by the three-year moving sum of absolute abnormal accruals (*ACCRUALS*), which capture the multi-year effects of potential earnings management used to withhold corporate bad news. We also measure the information asymmetry by a market-based measure of bid-ask spread, which is calculated as the natural logarithm of the average daily relative effective spreads over a fiscal year (*LOG_SPREAD*) as per Fang et al. (2009). A higher level of information asymmetry is associated with greater abnormal accruals (*ACCRUALS*) and larger bid-ask spread (*LOG_SPREAD*). Provided that SFAS 161 curbs managerial opportunism by means of lowering the information asymmetry, the reduction in abnormal accruals (*ACCRUALS*) or bid-ask spread (*LOG_SPREAD*) subsequent to the implementation of SFAS 161 should be greater for the derivative-using firms compliant with the standard, compared to the non-derivative-users. To test this supposition, we employ the following DID regression model:

$$ACCRUALS_{i,t}(LOG\_SPREAD_{i,t}) = \gamma_0 + \gamma_1 TREAT_i \times POST_t + \sum_k \gamma_k CONTROLS_{i,t}^k + \sum_t \gamma_t YR_{i,t}^t + \sum_z \gamma_z FIRM_i^z + \nu_{i,t} \quad (4)$$

Based on previous literature (e.g., Hutton et al., 2009; Chang et al., 2016; Steffen, 2020; Chen et al., 2021), the control variables included in Model (4) are firm size (*SIZE*), the book-to-market ratio (*BTM*), analyst coverage (*LANACOV*), dedicated institutional ownership (*DEDI*), return on assets (*ROA*), audit fees (*AUDITFEE*), stock return volatility (*RETVOL*), sales growth (*SALESGROWTH*), and intangible assets (*INTANGIBLE*). All the variables are defined in detail in Appendix A. The interaction term, $TREAT_i \times POST_t$, captures the degree to which the information asymmetry of compliers changes in response to the implementation of SFAS 161, relative to that of the control firms as to non-derivative-users. Since we control for year-fixed effects (*YR*) and firm-fixed effects (*FIRM*) in Model (4), the indicator variables, $TREAT_i$ and $POST_t$, are excluded from the model to avoid multicollinearity issues. The regression results are reported in Table 8. The coefficients of $TREAT_i \times POST_t$ are significantly negative for both the *ACCRUALS* and *LOG_SPREAD* regressions in Columns (1) and (2). The point estimate on the DID estimator amounts to -1.1453 (-0.0517), which accounts for about 14% (5%) of one standard deviation of *ACCRUALS* (*LOG_SPREAD*) in our sample. These findings, in combination with our baseline results, suggest that SFAS 161 decreases the information asymmetry of compliers and thereby reduces their insider trades and stock price crash risk post SFAS 161.

6.1.2. Test of the real-effect mechanism

Risk management theory suggests that firms may use derivatives to reduce business 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). The higher the business risk 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 business risk, lessens associated bad news, and reduces investor uncertainty about stock performance. Thus, if SFAS 161 is effective in decreasing managerial opportunism through directing firms to use derivatives to hedge against business risk, we expect the risk to become lower after the passage of SFAS 161. To test this supposition, we use the following DID regression model:

$$\begin{aligned} IDIOSYN_{i,t}(STDEARN_{i,t}) = \lambda_0 + \lambda_1 TREAT_i \times POST_t + \sum_k \lambda_k CONTROLS_{i,t}^k \\ + \sum_t \lambda_t YR_{i,t}^t + \sum_z \lambda_z FIRM_i^z + \mu_{i,t} \end{aligned} \quad (5)$$

The dependent variables are our two proxies for business risk: idiosyncratic risk (*IDIOSYN*) and earnings volatility (*STDEARN*). The interaction term, $TREAT_i \times POST_t$, reflects the extent to which the business risk of compliers changes relative to that of non-derivative-users after the passage of SFAS 161. Following prior literature (e.g., Bartram et al., 2011; Chang et al., 2016), we control for firm size (*SIZE*), the book-to-market ratio (*BTM*), financial leverage (*LEV*), return on assets (*ROA*), financial constraints (*SA*), trading volume (*TRADEVOL*), stock return volatility (*RETVOL*), cash flow volatility (*STDCFO*), intangible assets (*INTANGIBLE*), as well as year- and firm-fixed effects in the regression. The detailed definitions of all the regressors are provided in Appendix A. Table 9 reports the regression results. As seen in Columns (1) and (2), the coefficients of $TREAT_i \times POST_t$ for the *IDIOSYN* and *STDEARN* regressions are negative and statistically significant, indicating that SFAS 161 lowers the business risk of compliers to a larger degree than that of non-derivative users. The point estimate amounts to -0.0035 (-80.2832), which is equivalent to about 16% (32%) of one standard deviation of *IDIOSYN* (*STDEARN*) for our sample. These findings, in conjunction with our baseline results, suggest that an increase in hedging against business risk is yet another mechanism through which SFAF 161 curbs managerial opportunism.

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 321 derivative-using firms, which are not in compliance with SFAS 161 to provide tabular derivative disclosures, as opposed to 388 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). The 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 the SEC and firm stakeholders (Pecht et al., 2014). Nonetheless, the 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 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 (2) and (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 nonsignificant, if our baseline DID results are attributed to the treatment effects of the enhanced derivative disclosures pursuant to SFAS 161, rather than to other omitted factor(s). 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 similar to Models (2) and (3), where 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 described in Section 4. Table 10 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 baseline DID results being free from potential omitted-variable(s) bias.

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 evidence to suggest that the decreased information asymmetry and increased hedging against business risk are the mechanisms that explain the mitigating impact of SFAS 161 on managerial opportunism. Nevertheless, we do not find evidence that derivative users which 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.

Appendix A. 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.
POST'	1 if a firm is in the three-year period of 2008-2010, and 0 if a firm is in the three-year period of 2005-2007.
POST''	1 if a firm is in the two-year period of 2010-2011, and 0 if a firm is in the two-year period of 2006-2007.
POSTCRISIS	1 if a firm is in the post-crisis period of 2011-2012, and 0 if a firm is in the crisis period of 2009-2010.
CRISIS	1 if a firm is in the crisis period of 2007-2008, and 0 if a firm is in the pre-crisis period of 2005-2006.
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 flows of a firm for the current and previous four fiscal years.
IDIOSYN	Idiosyncratic stock return volatility, calculated as the standard deviation of the residuals from the following market model 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 (see Kim et al., 2011a).

RETVOL	The standard deviation of daily market excess returns over a 12-month period ending at the end of the fiscal year.
ACCRUALS	The three-year moving sum of the absolute value of annual abnormal accruals, 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).
LOG_SPREAD	The natural logarithm of annual relative effective spread, which is the arithmetic mean of daily relative effective spreads for a stock. The daily relative effective spread is calculated as the absolute value of the difference between the closing transaction price and the midpoint of the prevailing bid-ask quote, divided by the midpoint of the prevailing bid-ask quote, at a trading date.
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.
AUDITFEE	The natural logarithm of audit fees incurred by a firm for a fiscal year.
SALESGROWTH	The difference between sales revenue for the current fiscal year and sales revenue for the previous fiscal year, divided by that for the previous fiscal year.
INTANGIBLE	The ratio of intangible assets to total assets of a firm at the end of a fiscal year.

Appendix B. 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 ending 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 ending 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 Position:					
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 Position:					
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."

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Table 1: Descriptive statistics

Panel A: Insider trades (*INSITRADE*) sample

Variables	No. of obs.	No. of firms	Mean	Std. dev.	5th	25th	Median	75th	95th
INSITRADE	2,757	711	4.6563	6.3384	0	0	0	11.986	14.9567
SIZE	2,757	711	7.1496	1.7085	4.2896	6.0894	7.1207	8.1470	10.2038
BTM	2,757	711	0.5392	0.4433	0.1158	0.2674	0.4237	0.6730	1.2937
LANACOV	2,757	711	3.4749	1.2190	0.6931	2.8904	3.6889	4.3041	5.0876
DEDI	2,757	711	0.0766	0.0769	0	0.0114	0.0576	0.1202	0.2253
ROA	2,757	711	0.0845	0.0634	0.0113	0.0400	0.0709	0.1122	0.2082
TRADEVOL	2,757	711	2.3176	1.7977	0.3448	1.1428	1.8798	2.9895	5.6901
STDCFO	2,757	711	99.4460	234.735	1.9171	8.3953	23.990	75.265	469.856
FIRMAGE	2,757	711	20.9427	18.9066	3	9	15	26	69
SA	2,757	711	-1.2521	1.2238	-3.3347	-2.2656	-0.6928	-0.2168	-0.0526
LEV	2,757	711	0.1170	0.1479	0	0	0.0500	0.2039	0.4183
STDEARN	2,757	711	102.6876	252.692	1.3902	6.0320	17.741	69.154	513.334
IDIOSYN	2,757	711	0.0526	0.0242	0.0240	0.0372	0.0489	0.0629	0.0922
CETR	2,757	711	0.2324	0.6838	0.0013	0.0940	0.2216	0.3252	0.5034
RETVOL	2,757	711	0.1171	0.0551	0.0490	0.0785	0.1063	0.1431	0.2211

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

Variables	No. of obs.	No. of firms	Mean	Std. dev.	5th	25th	Median	75th	95th
CRASH	2,849	735	0.1979	0.3985	0	0	0	0	1
SIZE	2,849	735	7.0367	1.6800	4.1132	5.9816	7.0512	8.0608	9.9964
BTM	2,849	735	0.5779	0.8099	0.1146	0.2647	0.4215	0.6744	1.3499
LANACOV	2,849	735	3.4365	1.2437	0	2.8904	3.6636	4.2905	5.0499
DEDI	2,849	735	0.0750	0.0761	0	0.0099	0.0557	0.1177	0.2157
ROA	2,849	735	0.0832	0.0601	0.0101	0.0389	0.0703	0.1122	0.2103
TRADEVOL	2,849	735	2.3123	1.8363	0.3217	1.0948	1.8619	3.0100	5.7763
STDCFO	2,849	735	91.0231	218.276	1.8957	7.9368	22.290	69.846	421.500
NCSKEW	2,849	735	-2.3930	13.5814	-23.6954	-8.6664	-2.2276	4.2824	17.9250
SA	2,849	735	-1.1955	1.2045	-3.3319	-2.0945	-0.6327	-0.2009	-0.0493
LEV	2,849	735	0.1188	0.1523	0	0	0.0426	0.2072	0.4360
STDEARN	2,849	735	94.9529	238.527	1.3572	5.8093	16.883	63.069	445.931
IDIOSYN	2,849	735	0.0539	0.0248	0.0244	0.0381	0.0502	0.0641	0.0950
CETR	2,849	735	0.2962	0.1354	0.0312	0.2242	0.3232	0.3764	0.4581
RETVOL	2,849	735	0.1189	0.0567	0.0493	0.0791	0.1080	0.1455	0.2233

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. All continuous variables are winsorized at 1% and 99% levels. The periods for both samples cover six years from 2006 to 2011. All the variables are defined in Appendix A.

Table 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$_t$	0.4396***	0.4022***
	(3.115)	(2.894)
BTM$_t$	0.2103*	0.1509
	(1.788)	(1.154)
LEV$_t$	3.8482***	2.7159***
	(4.868)	(3.325)
SA$_t$	-0.0004**	-0.0005**
	(-2.058)	(-2.218)
ROA$_t$	0.0354	-2.1158
	(1.441)	(-0.906)
DEDI$_t$	1.3333	0.1748
	(0.982)	(0.144)
STDCFO$_t$	-0.0001	-0.0000
	(-0.256)	(-0.005)
STDEARN$_t$	-0.0005	-0.0003
	(-0.768)	(-0.403)
IDIOSYN$_t$	-8.5902*	-8.9821*
	(-1.765)	(-1.840)
CETR$_t$	0.1035*	-2.2731*
	(1.900)	(-1.807)
RETVOL$_t$	0.6551	0.0835
	(0.500)	(0.069)
Intercept	-4.7424***	-3.9131***
	(-3.692)	(-3.114)
Year-fixed effects	included	included
Industry-fixed effects	included	included
No. of observations	2,644	2,670
Pseudo R-squared	0.3211	0.2958

Table 2 (Continued)

Panel B: Covariate balance between treatment and control groups

Insider trades (*INSITRADE*) sample

Variables	Un(matched)	Mean *TREAT*=1 (N=1,417)	Mean *TREAT*=0 (N=1,417)	t-stat.	Standardized Bias (%)
$SIZE_t$	Unmatched	7.802	6.338	23.69***	93.1
	Matched	7.754	7.849	-1.43	-6.1
BTM_t	Unmatched	0.531	0.531	0.00	0.0
	Matched	0.536	0.548	-0.80	-2.8
LEV_t	Unmatched	0.158	0.064	17.15***	68.1
	Matched	0.155	0.163	-1.29	-5.4
SA_t	Unmatched	-1.780	-0.587	-28.34***	-113.3
	Matched	-1.751	-1.798	1.00	4.5
ROA_t	Unmatched	0.152	0.096	0.92	3.8
	Matched	0.109	0.084	0.77	1.8
$DEDI_t$	Unmatched	0.085	0.066	6.69***	26.3
	Matched	0.085	0.091	-1.97**	-7.5
$STDCFO_t$	Unmatched	174.580	36.480	9.98***	41.0
	Matched	169.340	158.530	0.72	3.2
$STDEARN_t$	Unmatched	157.610	37.631	12.32***	50.0
	Matched	155.170	168.890	-1.14	-5.7
$IDIOSYN_t$	Unmatched	0.047	0.059	-14.41***	-55.8
	Matched	0.048	0.048	-0.02	-0.1
$CETR_t$	Unmatched	0.225	0.238	-0.47	-1.8
	Matched	0.225	0.223	0.09	0.3
$RETVOL_t$	Unmatched	0.111	0.127	-7.17***	-27.8
	Matched	0.111	0.108	1.20	4.7

Table 2 (Continued)

Stock price crash risk (*CRASH*) sample

Variables	Un(matched)	Mean *TREAT*=1 (N=1,395)	Mean *TREAT*=0 (N=1,395)	t-stat.	Standardized Bias (%)
$SIZE_t$	Unmatched	7.678	6.256	23.91***	93.0
	Matched	7.624	7.554	1.16	4.6
BTM_t	Unmatched	0.572	0.562	0.33	1.3
	Matched	0.580	0.634	-1.50	-6.6
LEV_t	Unmatched	0.383	0.066	1.32	5.3
	Matched	0.155	0.169	-2.15**	-0.2
SA_t	Unmatched	-1710.800	-555.140	-28.11***	-110.9
	Matched	-1666.600	-1683.600	0.36	1.6
ROA_t	Unmatched	0.076	0.093	-7.38***	-28.5
	Matched	0.076	0.078	-0.61	-2.1
$DEDI_t$	Unmatched	0.084	0.068	4.92***	19.1
	Matched	0.084	0.092	-2.12**	-8.8
$STDCFO_t$	Unmatched	138.890	33.429	12.85***	51.4
	Matched	134.850	141.220	-0.67	-3.1
$STDEARN_t$	Unmatched	146.420	35.290	12.25***	48.8
	Matched	143.410	158.410	-1.26	-6.6
$IDIOSYN_t$	Unmatched	0.048	0.061	-13.44***	-51.6
	Matched	0.049	0.049	-0.87	-2.6
$CETR_t$	Unmatched	0.021	0.012	2.99***	11.7
	Matched	0.021	0.025	-1.37	-5.7
$RETVOL_t$	Unmatched	0.112	0.131	-7.61***	-29.2
	Matched	7.678	6.256	23.91	93.0

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. t-statistics 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. Industry dummies (constructed from the first two digits of SIC codes) and year dummies are included but are not reported for simplicity. 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 A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Table 3: Tests of parallel trends assumption

Panel A: Univariate tests

Year	Annual Growth Rates in *INSITRADE*			Annual Growth Rates in *CRASH*		
	Mean (*TREAT*=0)	Mean (*TREAT*=1)	Mean Differences (t-stat)	Mean (*TREAT*=0)	Mean (*TREAT*=1)	Mean Differences (t-stat)
2006	-0.4141	-0.4033	-0.0108	-0.7500	-0.7273	-0.0227
			(-0.124)			(-0.153)
2007	-0.4773	-0.3731	0.1042	-0.8333	-0.7368	-0.0965
			(-1.341)			(-0.945)
2008	-0.4913	-0.5429	-0.0516	-0.8125	-0.7755	-0.0370
			(0.688)			(-0.395)

Panel B: Multivariate tests

Insider trades (*INSITRADE*) sample

Variables	Dependent Variable = $INSITRADE_t$		
	(1) 2005 vs. 2006	(2) 2006 vs. 2007	(3) 2007 vs. 2008
$\boldsymbol{TREAT_i \times POST_t}$	**-2.0552**	**0.3117**	**0.2754**
	(-1.564)	**(0.312)**	**(0.333)**
Year-fixed effects	included	included	included
Industry-fixed effects	included	included	included
No. of observations	432	768	1,016
Adjusted R-squared	0.1257	0.0821	0.2582

Stock price crash risk (*CRASH*) sample

Variables	Dependent Variable = $CRASH_{t+1}$		
	(1) 2005 vs. 2006	(2) 2006 vs. 2007	(3) 2007 vs. 2008
$\boldsymbol{TREAT_i \times POST_t}$	**-0.7775**	**0.2939**	**0.1330**
	(-1.220)	**(0.711)**	**(0.356)**
Year-fixed effects	included	included	included
Industry-fixed effects	included	included	included
No. of observations	390	807	841
Pseudo R-squared	0.2174	0.1673	0.1139

Notes: This table presents the results from testing the parallel trends assumption. Panel A reports the univariate results comparing the average annual growth rates in insider trades (*INSITRADE*) and crash risk (*CRASH*) of the treatment firms with those of the control firms for the pre-SFAS-161 sample period (i.e., 2006-2008). 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. Two-sample t-tests are performed to compare the mean differences. Columns (1), (2), and (3) of Panel B report the results of the multivariate tests, which use 2005 and 2006, 2006 and 2007, and 2007 and 2008 as the pre- and post-treatment periods, respectively, for the estimation of DID regression models (2) and (3). Only the coefficients for the interaction terms, $TREAT_i \times POST_t$, are reported. t-statistics in parentheses are based on robust standard errors clustered by firm. Other variables, inclusive of industry dummies (constructed from the first two digits of SIC codes) and year dummies, are included but are not reported for simplicity. All the variables in the tables are defined in Appendix A. *, **, and *** indicate the two-tailed statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Difference-in-differences regression analysis of the impact of SFAS 161 on managerial opportunism

Variables	(1) Dependent Variable = $INSITRADE_t$	(2) Dependent Variable = $CRASH_{t+1}$
Intercept	-1.5275	-0.3096
	(-0.523)	(-0.473)
$TREAT_i$	0.0621	0.0853
	(0.171)	(0.540)
$\boldsymbol{TREAT_i \times POST_t}$	**-1.2221*** **	**-0.3506***
	(-2.567)	**(-1.705)**
$SIZE_t$	0.8488***	-0.1759***
	(5.955)	(-2.727)
BTM_t	-0.1121	-0.3510**
	(-0.626)	(-2.328)
$LANACOV_t$	0.3936**	0.3153***
	(2.081)	(3.737)
$DEDI_t$	-3.6972***	0.8677
	(-2.938)	(1.249)
ROA_t	4.7679**	2.7815***
	(2.039)	(2.722)
$TRADEVOL_t$	-0.0195	-0.0007
	(-0.229)	(-0.019)
$STDCFO_t$	-0.0003	-0.0008**
	(-0.724)	(-2.150)
$FIRMAGE_t$	-0.0032	
	(-0.445)	
$CETR_t$		0.8236
		(1.224)
$NCSKEW_t$		0.0076*
		(1.941)
Year-fixed effects	included	included
Industry-fixed effects	included	included
No. of observations	2,834	2,790
Adjusted/Pseudo R-squared	0.1506	0.0832

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 effects 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 A. 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. t-statistics 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 5: Tests of anticipation effects

Variables	(1) Dependent Variable = $INSITRADE_t$	(2) Dependent Variable = $CRASH_{t+1}$
Intercept	3.2454	-0.0567
	(1.114)	(-0.084)
$TREAT_i$	0.0884	-0.0991
	(0.340)	(-0.559)
$TREAT_i \times POST'_t$	**0.1692**	**0.0977**
	(0.511)	**(0.438)**
$SIZE_t$	0.1585	-0.1832***
	(1.076)	(-2.765)
BTM_t	-0.2401	-0.4049**
	(-1.168)	(-2.253)
$LANACOV_t$	0.5764***	0.3269***
	(2.989)	(3.602)
$DEDI_t$	-2.4344*	1.9409***
	(-1.952)	(2.807)
ROA_t	3.1750	-0.0870
	(1.283)	(-0.080)
$TRADEVOL_t$	0.0521	0.0602
	(0.572)	(1.575)
$STDCFO_t$	0.0010**	-0.0008**
	(2.337)	(-2.064)
$FIRMAGE_t$	-0.0337***	
	(-4.480)	
$CETR_t$		0.0002
		(0.004)
$NCSKEW_t$		-0.0059
		(-1.587)
Year-fixed effects	included	included
Industry-fixed effects	included	included
No. of observations	2,526	2,507
Adjusted/Pseudo R-squared	0.0802	0.0995

Notes: This table reports the results from testing the anticipation effects explicated in Section 5.2.1. 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 years of 2008-2010 (2005-2007). The interaction term, $TREAT_i \times POST'_t$, is the variable of interest which captures the effects of the anticipated placebo event 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 A. 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. t-statistics 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 6: Robustness tests of the potential confounding effect of financial crisis

Panel A: Comparisons among pre-crisis period, crisis period, and post-crisis period

Variables	2009-2010 vs. 2011-2012		2005-2006 vs. 2007-2008	
	(1)	(2)	(3)	(4)
	$INSITRADE_t$	$CRASH_{t+1}$	$INSITRADE_t$	$CRASH_{t+1}$
Intercept	-5.2300	-2.6880***	-2.9539	0.6974
	(-1.220)	(-3.030)	(-0.467)	(0.781)
$TREAT_i$	-0.5979	-0.0099	0.7361	-0.4776*
	(-1.562)	(-0.059)	(1.293)	(-1.768)
$\boldsymbol{TREAT_i \times POSTCRISIS_t}$	**0.5979**	**0.2381**		
	(1.101)	**(1.024)**		
$\boldsymbol{TREAT_i \times CRISIS_t}$			**-1.0474**	**0.2613**
			(-1.534)	**(0.818)**
$SIZE_t$	0.8026***	-0.0909	0.5566***	-0.1917**
	(4.859)	(-1.247)	(2.761)	(-2.281)
BTM_t	0.1961	-0.1215	-0.2072	-0.1062
	(1.095)	(-1.106)	(-0.757)	(-0.571)
$LANACOV_t$	0.0907	0.3435***	0.3883	0.1773*
	(0.367)	(2.925)	(1.576)	(1.674)
$DEDI_t$	-0.4141	1.0912	-4.8917***	1.7611*
	(-0.268)	(1.349)	(-2.986)	(1.911)
ROA_t	7.6603***	2.2880*	2.9544	0.3150
	(2.597)	(1.951)	(0.847)	(0.222)
$TRADEVOL_t$	0.1702*	0.0296	-0.0054	0.0313
	(1.667)	(0.624)	(-0.043)	(0.631)
$STDCFO_t$	0.0002	-0.0007**	0.0006	0.0002
	(0.516)	(-2.119)	(0.934)	(0.465)
$FIRMAGE_t$	-0.0135*		0.0108	
	(-1.674)		(1.103)	
$CETR_t$		0.0489		-0.3440
		(0.825)		(-1.453)
$NCSKEW_t$		0.0040		-0.0161***
		(0.890)		(-3.472)
Year-fixed effects	included	included	included	included
Industry-fixed effects	included	included	included	included
No. of observations	1,972	1,979	1,592	1,442
Adjusted/Pseudo R-squared	0.1203	0.0908	0.1649	0.1451

Notes: This table reports the results from the placebo tests, which analyze 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, $POSTCRISIS_t$, for Columns (1) and (2) equals 1 (0) if a firm is in the post-crisis (crisis) period (i.e., 2011-2012 (2009-2010)); The other time indicator variable, $CRISIS_t$, for Columns (3) and (4) equals 1 (0) if a firm is in the crisis (pre-crisis) period (i.e., 2007-2008 (2005-2006)). The interaction terms, $TREAT_i \times POSTCRISIS_t$ and $TREAT_i \times CRISIS_t$, are the variables of interest which capture the effects of the crisis event 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 A. 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. t-statistics 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 6 (Continued)

Panel B: Excluding 2008-2009

Variables	(1) Dependent Variable = $INSITRADE_t$	(2) Dependent Variable = $CRASH_{t+1}$
Intercept	-0.3607	-2.3721**
	(-0.098)	(-2.049)
$TREAT_i$	0.7162	0.2052
	(1.601)	(1.009)
$TREAT_i \times POST''_t$	**-1.1316***	**-0.7496*****
	(-1.927)	**(-2.982)**
$SIZE_t$	0.9640***	0.0693
	(5.346)	(0.886)
BTM_t	0.3690*	-0.7182***
	(1.908)	(-3.170)
$LANACOV_t$	0.4877**	0.0049
	(2.033)	(0.048)
$DEDI_t$	-2.2726	0.2521
	(-1.508)	(0.304)
ROA_t	6.6862**	-0.9297
	(2.245)	(-0.728)
$TRADEVOL_t$	0.2493**	0.1598***
	(2.215)	(3.575)
$STDCFO_t$	-0.0017***	-0.0012**
	(-3.291)	(-2.450)
$FIRMAGE_t$	0.0130	
	(1.467)	
$CETR_t$		-0.6317
		(-0.728)
$NCSKEW_t$		0.0040
		(1.019)
Year-fixed effects	included	included
Industry-fixed effects	included	included
No. of observations	1,852	1,889
Adjusted/Pseudo R-squared	0.1800	0.1034

Notes: This table reports the results from the tests, which analyze the potential confounding effect of financial crisis on managerial opportunism by excluding the years 2008-2009 from our sample period of 2006-2011. 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 that spans the years 2010-2011 (2006-2007)). 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 compliers (*TREAT*=1) relative to the non-users of derivatives (*TREAT*=0). All the variables are defined in Appendix A. 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. t-statistics 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 7: Firm-fixed-effects difference-in-differences regression analysis of the impact of SFAS 161 on managerial opportunism

Variables	Dependent Variable =			
	(1) $INSITRADE_t$	(2) $CRASH_{t+1}$	(3) $INSITRADE_t$	(4) $CRASH_{t+1}$
Intercept	32.3533***	3.9761*	-15.9102	-14.4536
	(4.467)	(1.831)	(-0.019)	(-0.025)
$TREAT_i \times POST_t$	**-1.4835*** **	**-0.5238***	**-1.4679*** **	**-0.4229***
	(-3.298)	**(-1.761)**	**(-3.287)**	**(-1.902)**
$SIZE_t$	-0.1017	-0.0076		
	(-0.225)	(-0.030)		
BTM_t	-2.2058***	-0.0081		
	(-4.772)	(-0.026)		
$LANACOV_t$	0.2019	0.0330		
	(0.586)	(0.135)		
$DEDI_t$	-8.1650***	2.2206		
	(-3.294)	(1.419)		
ROA_t	13.1154***	3.5477*		
	(3.844)	(1.875)		
$TRADEVOL_t$	-0.0553	-0.0360		
	(-0.385)	(-0.339)		
$STDCFO_t$	-0.0014***	-0.0029***		
	(-2.622)	(-4.996)		
$FIRMAGE_t$	-2.2482***			
	(-4.624)			
$CETR_t$		-6.3898***		
		(-2.940)		
$NCSKEW_t$		-0.0234***		
		(-4.509)		
Year-fixed effects	included	included	included	included
Firm-fixed effects	included	included	included	included
No. of observations	2,834	1,487	2,834	1,487
Adjusted/Pseudo R-squared	0.4053	0.1743	0.3860	0.1354

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 the 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 A. Firm dummies and year dummies are included in all the regression but are not reported for simplicity. t-statistics 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 8: Tests of the information-asymmetry mechanism through which SFAS 161 curbs managerial opportunism

Variables	(1) Dependent Variable = $ACCRUALS_t$	(2) Dependent Variable = LOG_SPREAD_t
Intercept	-65.5932***	-5.5715***
	(-4.937)	(-9.853)
$\boldsymbol{TREAT_i \times POST_t}$	**-1.1453****	**-0.0517***
	(-2.067)	**(-1.905)**
$SIZE_t$	0.5695	-0.2573***
	(0.892)	(-8.185)
BTM_t	1.7672**	0.0272
	(2.489)	(0.783)
$LANACOV_t$	-1.2893***	-0.1501***
	(-2.788)	(-6.641)
$DEDI_t$	8.8388***	0.4992***
	(2.771)	(3.148)
ROA_t	0.9406	0.0310
	(0.211)	(0.141)
SA_t	3.3751***	0.0847***
	(5.078)	(2.584)
$AUDITFEE_t$	4.7733***	0.0879***
	(5.890)	(2.637)
$RETVOL_t$	-2.6690	1.0918***
	(-0.639)	(5.314)
$SALESGROWTH_t$	-0.1419	0.0733***
	(-0.254)	(2.637)
$INTANGIBLE_t$	-2.9089	0.5241***
	(-1.282)	(4.644)
Year-fixed effects	included	included
Firm-fixed effects	included	included
No. of observations	1,966	1,980
Adjusted R-squared	0.7271	0.9253

Notes: This table reports the results of the firm-fixed-effects difference-in-differences regression analysis of the impact of SFAS 161 on information asymmetry. The sample period spans the years 2006-2011. The dependent variable is abnormal accruals ($ACCRUALS_t$) in Column (1) and bid-ask spread (LOG_SPREAD_t) in Column (2). The interaction term, $TREAT_i \times POST_t$, is the variable of interest which captures the effects of SFAS 161 on information asymmetry for the compliers relative to the non-derivative-users. All the variables are defined in Appendix A. Firm dummies and year dummies are included in all the regression but are not reported for simplicity. t-statistics 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 9: Tests of the real-effect mechanism through which SFAS 161 curbs managerial opportunism

Variables	(1) Dependent Variable = $IDIOSYN_t$	(2) Dependent Variable = $STDEARN_t$
Intercept	0.0784***	428.2279***
	(15.651)	(4.047)
$TREAT_i \times POST_t$	**-0.0035*** **	**-80.2832*** **
	(-6.152)	**(-6.712)**
$SIZE_t$	-0.0071***	-47.7703***
	(-12.616)	(-4.041)
BTM_t	-0.0052***	-59.0263***
	(-10.159)	(-5.451)
LEV_t	0.0103***	166.4458***
	(4.704)	(3.609)
ROA_t	0.0230***	71.2020
	(5.063)	(0.741)
SA_t	0.0023***	110.3295***
	(3.428)	(7.795)
$TRADEVOL_t$	0.0016***	14.3071***
	(8.692)	(3.589)
$RETVOL_t$	0.0705***	156.5317*
	(16.883)	(1.774)
$STDCFO_t$	-0.0000***	0.2425***
	(-6.157)	(16.343)
$INTANGIBLE_t$	0.0080***	270.5169***
	(3.043)	(4.888)
Year-fixed effects	included	included
Firm-fixed effects	included	included
No. of observations	2,464	2,464
Adjusted R-squared	0.9143	0.8574

Notes: This table reports the results of the firm-fixed-effects difference-in-differences regression analysis of the impact of SFAS 161 on business risk. The sample period spans the years 2006-2011. The dependent variable is idiosyncratic risk ($IDIOSYN_t$) in Column (1) and earnings volatility ($STDEARN_t$) in Column (2). The interaction term, $TREAT_i \times POST_t$, is the variable of interest which captures the effects of SFAS 161 on business risk for the compliers relative to the non-derivative-users. All the variables are defined in Appendix A. Firm dummies and year dummies are included in all the regression but are not reported for simplicity. t-statistics 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 10: Tests of whether managerial opportunism is reduced for the non-compliers post SFAS 161

Variables	(1) Dependent Variable = $INSITRADE_t$	(2) Dependent Variable = $CRASH_{t+1}$
Intercept	15.4582	-2.6788
	(0.682)	(-1.177)
$NONCOMPLIER_i \times POST_t$	**-0.3733**	**-0.2468**
	(-0.705)	**(-0.677)**
$SIZE_t$	0.2016	0.0276
	(0.511)	(0.108)
BTM_t	0.3406	-0.5356*
	(1.117)	(-1.752)
$LANACOV_t$	-0.2143	0.1954
	(-0.712)	(0.994)
$DEDI_t$	-6.2950***	-0.3548
	(-2.911)	(-0.224)
ROA_t	-0.6486	1.5759*
	(-0.539)	(1.878)
$TRADEVOL_t$	0.3171**	0.0173
	(2.287)	(0.175)
$STDCFO_t$	-0.0003	-0.0010
	(-0.838)	(-0.790)
$FIRMAGE_t$	-0.0878	
	(-0.099)	
$CETR_t$		-1.2022
		(-0.645)
$NCSKEW_t$		-0.0265***
		(-3.574)
Year-fixed effects	included	included
Firm-fixed effects	included	included
No. of observations	2,324	1,082
Adjusted/Pseudo R-squared	0.3266	0.1294

Notes: This table reports the firm-fixed-effects regression results of the placebo tests of whether managerial opportunism is reduced for the non-compliers post SFAS 161. The sample period covers the 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 does not comply 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 A. 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. t-statistics 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.