Received: 24 June 2023 | Accepted: 20 October 2023

DOI: 10.1002/fut.22472

RESEARCH ARTICLE

WILEY

Derivative disclosures and managerial opportunism

Guanming He[1] | **Helen Mengbing Ren**[2]

[1]Durham University Business School, Durham University, Durham, UK

[2]University of Liverpool Management School, University of Liverpool, Liverpool, UK

Correspondence
Guanming He, Durham University Business School, Durham University, Durham, UK.
Email: guanming.he@durham.ac.uk

Abstract

Derivatives are increasingly used by managers not only to hedge risks but also to pursue nonhedging activities for fulfilling opportunistic incentives. The Statement of Financial Accounting Standards No. 161 (SFAS 161) requires firms to disclose their objectives and strategies for 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 nonderivative-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.

KEYWORDS

business risk, crash risk, derivative disclosures, hedging, information asymmetry, insider trading

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, the notional amount of outstanding over-the-counter 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 nonhedging activities, such as speculation and earnings manipulation (Brown, 2001; Chernenko & Faulkender, 2011; Géczy et al., 2007). 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 nonhedging purposes. One possible way to restrain the use of derivatives for nonhedging 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 (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 & Duffie, 1995), alleviate financial distress, and lower expected tax liabilities (Smith & Stulz, 1985). However, derivatives also serve nonhedging purposes, such as earnings management and speculation (Brown, 2001; Chernenko & Faulkender, 2011; Faulkender, 2005; Géczy et al., 2007; 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 nonhedging 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 nonhedging purposes (Bartram et al., 2011; Guay, 1999), 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). 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. A vast literature (e.g., Hutton et al., 2009; Jin & Myers, 2006; 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.[3] Since derivatives can serve nonhedging purposes such as earnings management which can be used as a means for managers to withhold bad news, more derivatives used for hedging purpose following SFAS 161 would also lessen the bad-news-hoarding behavior and associated stock price crash risk.

A plausible countervailing argument is that managers may falsify their purposes and strategies of derivative usage in the tabular disclosures that are made in compliance with SFAS 161. As such, SFAS 161 would not restrain managerial

[1]The SFAS 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 (SFAS 133), *Accounting for Derivative Instruments and Hedging Activities*, was issued by FASB (1998). SFAS 133 and SFAS 161 were codified under the Accounting Standards Codification Topic 815 (ASC 815) *Derivatives and Hedging* in the year 2014.

[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; Steffen, 2022).

[3]As with the literature, macro and market-wide factors triggering stock price crashes are not within the scope of this study.

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 1164 US listed firms in the nonfinancial and nonutility industries from 2006 to 2011. We employ a difference-in-differences (DID) 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 nonderivative-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 nonderivative-users. This finding supports our conjecture that the derivative disclosures prescribed by SFAS 161 reduce managerial opportunism. We also find that this reduction is more prominent for firms with more derivative usage.

We further conduct a path analysis on 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. The decreased levels of information asymmetry and business risk would in turn mitigate managerial opportunism. Consistent with our conjecture on the information-asymmetry and real-effect mechanisms, we find that SFAS 161 lowers both information opacity and idiosyncratic risk of the compliers and thereby curbs their managerial opportunism.

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 noncompliers experience a greater reduction in either insider trades or stock price crash risk post-SFAS 161, compared with a matched control sample of nonderivative-users. This suggests that SFAS 161 does not reduce managerial opportunism in the noncompliant 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 for 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 & 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 (Jayaraman & Wu, 2019; Kanodia & Sapra, 2016) 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.

Lastly, prior studies (e.g., Campbell et al., 2021; Chang et al., 2016; Chen et al., 2021; He et al., 2022; Khan et al., 2018; Steffen, 2022) provide mixed findings on the information usefulness of SFAS 161. Unlike these studies that focus on the information perspective in their analyses,[4] our study provides insights into the effect of SFAS 161 on

[4]For instance, He et al. (2022) and Steffen (2022) examine the impact of SFAS 161 on the information asymmetry among investors, which is measured by bid-ask spreads. In contrast, we focus our analysis on the information asymmetry between corporate insiders and outside investors. A reduction in information asymmetry between informed and uninformed investors, as found by He et al. (2022) and Steffen (2022), does not necessarily suggest a decrease in the information asymmetry between insiders and outsiders.

managerial opportunism, and suggests that the derivative disclosures stipulated by SFAS 161 not only reduce the information asymmetry between insiders and outsiders but also restrain managers from using derivatives for opportunistic 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, foreign exchange rates, and so forth. 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 is 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).[5] If derivatives are not designated as hedges, the changes in fair values of these nondesignated 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 the use of derivatives for the purpose of generating profits from market price changes of commodities (Manchiraju et al., 2018), and such derivatives are often not designated as hedging instruments.

SFAS 161 aims to enhance disclosures about (i) how and why a firm uses derivative instruments, (ii) how derivative instruments are accounted for, and (iii) how derivative instruments affect a firm's financial position, financial performance, and cash flow (FASB, 2008). This standard should increase the attention paid by investors to corporate derivative disclosures. SFAS 161 requires firms to distinguish derivatives designated as hedges and derivatives not designated as hedges in the tabular format. Manchiraju et al. (2018) argue that the accounting designation of derivatives is informative about the purposes and strategies of derivative use. They find that, while derivatives designated as hedges are negatively associated with firm risk, firms tend to use derivatives not designated as hedges to achieve or beat performance benchmarks, leading to higher firm risk. 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 (Adam & Fernando, 2006; Bartram et al., 2011; Gilje & Taillard, 2017; Guay, 1999). 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 nonhedging 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 nonhedging activities and associated opportunistic behavior.

[5]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 December 15, 2019.

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. The discussion above leads to a general hypothesis (H1) for this study:

H1: SFAS 161 curbs managerial opportunism.

We further develop two separate hypotheses for the impact of SFAS 161 on insider trades and stock price crash risk, which pertain to two different forms of managerial opportunism. First, as previous literature (e.g., He, 2023; Huddart & Ke, 2007; Huddart et al., 2007; Ke et al., 2003; 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 & 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 nonhedging purposes, leading to more effective risk management. If so, firms' risk exposures will decrease and firm value will increase (Bartram et al., 2011; Gilje & 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 the following hypothesis:

H1a: Firms that follow SFAS 161 to provide tabular disclosures of derivative usage experience a decrease in insider trades.

Second, more transparent and informative disclosures of derivative usage are likely to reduce information asymmetry and help investors better correct mispricing, thereby lowering the probability of a stock price crash. Also, the reduced information asymmetry increases the difficulty managers have in withholding bad news about a firm. As documented in the crash risk literature (e.g., He, 2015; Hutton et al., 2009; Zhu, 2016; Kim et al., 2011a, 2011b), 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 the degree of stock overvaluation, and the higher the 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 who 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., Chernenko & Faulkender, 2011; He & Ren, 2023; Manchiraju et al., 2018; Pincus & Rajgopal, 2002) 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 & 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 (nonhedging), stock price crash risk should decrease following the passage of SFAS 161. Accordingly, we put forth the following hypothesis:

H1b: 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 US listed firms in nonfinancial and nonutility industries. As with some previous studies (e.g., Chang et al., 2016; Donohoe, 2015), 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 November 15, 2008, companies generally started applying this standard from the fiscal year 2009. Accordingly, our sample period spans the years 2006–2011, covering the 3-year pre-SFAS 161 period (i.e., 2006–2008) and the 3-year post-SFAS 161 period (i.e., 2009–2011).

Insider trading data are obtained from Thomson Financial Insider Research Services (2005–2012) Historical Files and include stock transactions by directors and officers only. Financial statement data and stock information come from Compustat (2005–2012) and Center for Research in Security Prices (CRSP, 2005–2012), respectively. To constitute our sample for the hypothesis tests, we begin with all nonfinancial and nonutility 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 in 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 (SEC's) EDGAR files (see, e.g., the tabular disclosures in the Kadant Inc.'s 2010 annual report in Appendix B).[6] Keywords such as "designated," "derivative," "hedge," "risk," "SFAS No. 133," and "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.[7]

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 (2014) finding that "most companies failed with the requirements of SFAS No. 161 to disclose required information," approximately 45% of the derivative-using companies in our hand-collected sample do not provide tabular disclosures distinguishing between designated and nondesignated hedges in the 3-year post-SFAS 161 period (2009–2011).[8] Thus, we categorize our sample firms into three groups: compliers (388 firms), noncompliers (321 firms), and nonusers (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 3-year post-SFAS 161 period. Through manual inspection, it is identified that the derivative disclosures of noncompliers in our sample are structured without tabular disclosures and qualitatively similar to those before the implementation of SFAS 161. Firms that do not use derivatives in any year during our sample period, either before or after SFAS 161, are

[6]The example disclosures made by Kadant Inc. (2008, 2011) 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.

[7]The titles can also be "designated hedges" and "nondesignated hedges."

[8]We seek to examine how changes in firms' derivative disclosures, as required essentially by SFAS 161 in terms of the derivative designation and tabular display, impact managerial opportunism, rather than focus on the *amount of narrative information changed* in firms' derivative disclosures as a result of SFAS 161.

named nonusers. They are not affected by the standard, thus satisfying the condition of being classified into a control group for a DID regression analysis. Following previous literature (e.g., Chang et al., 2016; Donohoe, 2015), we define our control sample as consisting of nonusers, 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, that is, the nonusers who do not use derivatives in any year during our sample period. The noncompliers identified in our sample cannot be used as control firms, because the comparison between the compliers and noncompliers relates to managers' decision to comply or not comply with SFAS 161, which would induce self-selection bias. Or rather, if the noncompliers 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 nonusers as our control sample. After removing the observations that have missing values in our DID regressors, the compliers (as well as noncompliers) retained in our sample are those that use derivatives in at least 1 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 2757 firm-year observations for insider trades and 2849 firm-year observations for crash risk, corresponding with 711 (372 compliers and 339 nonusers) and 735 (388 compliers and 347 nonusers) unique firms, respectively.[9] 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.[10] As with prior literature (Ataullah et al., 2014; Dechow et al., 2016; Skaife et al., 2013), missing values of insider trades are set as zero.

Our second measure of managerial opportunism is stock price crash risk. The crash risk literature (e.g., Hutton et al., 2009; Jin & Myers, 2006) 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 prior literature (e.g., Callen & Fang, 2013; Chen et al., 2001; Kim et al., 2011a), we use the negative skewness of firm-specific weekly returns (*NCSKEW*) to capture the likelihood of extremely low firm-specific weekly returns in a year. Firm-specific weekly return ($R_{i,t}$,) is defined as the natural logarithm of one plus the residual return, $\varepsilon_{i,t}$, from the following regression model, adjusted for market-wide factors:

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

where $r_{i,t}$ is the return on stock i, and $r_{m,t}$ is the return on the CRSP value-weighted market index, in week t. Accordingly, *NCSKEW* is calculated as the negative of the third moment of firm-specific weekly returns, divided by the standard deviation of the firm-specific weekly returns raised to the third power, and can be expressed in the equation below:

$$NCSKEW_{i,t} = -\left(n(n-1)^{\frac{3}{2}} \sum R_{i,t}^3\right) / \left((n-1)(n-2)\left(\sum R_{i,t}^2\right)^{\frac{3}{2}}\right). \tag{2}$$

By construction, a higher value of *NCSKEW* indicates a more left-skewed return distribution and hence a higher probability of stock price crashes.

[9]In our initial sample of 1208 firms, there are only 17 firms which use derivatives before 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.

[10]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.

TABLE 1 Descriptive statistics.

Variables	No. of observations	No. of firms	Mean	SD	5th	25th	Median	75th	95th
Panel A: Insider trades (INSITRADE) sample									
INSITRADE	2757	711	4.6563	6.3384	0	0	0	11.9866	14.9567
SIZE	2757	711	7.1496	1.7085	4.2896	6.0894	7.1207	8.1470	10.2038
BTM	2757	711	0.5392	0.4433	0.1158	0.2674	0.4237	0.6730	1.2937
LANACOV	2757	711	3.4749	1.2190	0.6931	2.8904	3.6889	4.3041	5.0876
DEDI	2757	711	0.0766	0.0769	0	0.0114	0.0576	0.1202	0.2253
ROA	2757	711	0.0845	0.0634	0.0113	0.0400	0.0709	0.1122	0.2082
TRADEVOL	2757	711	2.3176	1.7977	0.3448	1.1428	1.8798	2.9895	5.6901
STDCFO	2757	711	0.0355	0.0309	0.0056	0.0152	0.0271	0.0453	0.0958
FIRMAGE	2757	711	20.9427	18.9066	3	9	15	26	69
SA	2757	711	−1.2521	1.2238	−3.3347	−2.2656	−0.6928	−0.2168	−0.0526
LEV	2757	711	0.1170	0.1479	0	0	0.0500	0.2039	0.4183
STDEARN	2757	711	0.0363	0.0494	0.0039	0.0106	0.0202	0.0389	0.1264
IDIOSYN	2757	711	0.0522	0.0214	0.0240	0.0372	0.0489	0.0629	0.0922
CETR	2757	711	0.2286	0.1937	0.0013	0.0940	0.2216	0.3252	0.5034
RETVOL	2757	711	0.1171	0.0551	0.0490	0.0785	0.1063	0.1431	0.2211
Panel B: Stock price crash risk (NCSKEW) sample									
NCSKEW	2849	735	−2.2344	14.5287	−27.5963	−8.4265	−1.8409	4.9730	20.0738
SIZE	2849	735	7.0367	1.6800	4.1132	5.9816	7.0512	8.0608	9.9964
BTM	2849	735	0.5779	0.8099	0.1146	0.2647	0.4215	0.6744	1.3499
LANACOV	2849	735	3.4365	1.2437	0	2.8904	3.6636	4.2905	5.0499
DEDI	2849	735	0.0750	0.0761	0	0.0099	0.0557	0.1177	0.2157
ROA	2849	735	0.0832	0.0601	0.0101	0.0389	0.0703	0.1122	0.2103
TRADEVOL	2849	735	2.3123	1.8363	0.3217	1.0948	1.8619	3.0100	5.7763
STDCFO	2849	735	0.0374	0.0347	0.0055	0.0154	0.0277	0.0469	0.1007
SA	2849	735	−1.1955	1.2045	−3.3319	−2.0945	−0.6327	−0.2009	−0.0493
LEV	2849	735	0.1188	0.1523	0	0	0.0426	0.2072	0.4360
STDEARN	2849	735	0.0402	0.0594	0.0039	0.0107	0.0206	0.0413	0.1534
IDIOSYN	2849	735	0.0534	0.0221	0.0244	0.0381	0.0502	0.0641	0.0950
CETR	2849	735	0.2962	0.1354	0.0312	0.2242	0.3232	0.3764	0.4581
RETVOL	2849	735	0.1189	0.0567	0.0493	0.0791	0.1080	0.1455	0.2233

Note: 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 (*NCSKEW*) sample. All continuous variables are winsorized at 1% and 99% levels. The periods for both samples cover 6 years from 2006 to 2011. All the variables are defined in Appendix A.

4 | RESEARCH DESIGN

4.1 | Matching of samples between treatment and control groups

Our main research specification is a 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 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 (PSM) approach (e.g., Chang et al., 2016; Chen et al., 2021; Irani & Oesch, 2013; Ke et al., 2019) to match a complier with a nonuser. We estimate propensity scores from a logistic regression of derivative usage on its determinant variables measured before SFAS 161. The prevailing literature shows that derivatives are more likely to be used by large firms (e.g., Géczy et al., 1997; Graham & Rogers, 2002; Haushalter, 2000; Mian, 1996; Nance et al., 1993), high-growth firms (e.g., Géczy et al., 1997), well-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 (Graham & Rogers, 2002; Haushalter, 2000; Tufano, 1996), 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 (Bartram et al., 2011; DeMarzo & Duffie, 1995; Froot et al., 1993; Minton & Schrand, 1999) 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 is 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 & Kopeinig, 2008; Shipman et al., 2017). After applying our PSM, 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 2836 and 2782 firm-year observations for the insider trading sample and the crash risk sample, corresponding to 744 and 776 firms, respectively.

4.2 | DID regression specification

To test the hypotheses H1a and H1b, we use the following DID regression models:

$$\begin{aligned} INSITRADE_{i,t} = \alpha_0 + \alpha_1 TREAT_i + \alpha_2 POST_t + \alpha_3 TREAT_i \times POST_t \\ + \textstyle\sum_k \alpha_k CONTROLS_{i,t}^k + \sum_z \alpha_z IND \times YR_i^z + \varepsilon_{i,t}, \end{aligned} \tag{3}$$

$$\begin{aligned} NCSKEW_{i,t+1} = \beta_0 + \beta_1 TREAT_i + \beta_2 POST_t + \beta_3 TREAT_i \times POST_t \\ + \textstyle\sum_k \beta_k CONTROLS_{i,t}^k + \sum_z \beta_z IND \times YR_i^z + u_{i,t}. \end{aligned} \tag{4}$$

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) *NCSKEW* sample dependent variable = $TREAT_i$
$SIZE_t$	0.4117***	0.4714***
	(2.869)	(3.263)
BTM_t	0.1868	0.1362
	(1.570)	(1.035)
LEV_t	3.7065***	2.3916***
	(4.664)	(2.969)
SA_t	−0.0004*	−0.0004*
	(−1.951)	(−1.807)
ROA_t	0.0345	−5.7138***
	(1.449)	(−3.845)
$DEDI_t$	1.3040	−0.0844
	(0.972)	(−0.070)
$STDCFO_t$	−3.8866*	−2.9062
	(−1.677)	(−1.191)
$STDEARN_t$	−0.2052	0.9714
	(−0.127)	(1.021)
$IDIOSYN_t$	−8.0310*	−10.6601**
	(−1.721)	(−2.118)
$CETR_t$	0.1120**	−2.3813*
	(1.992)	(−1.861)
$RETVOL_t$	0.7476	1.4106
	(0.580)	(0.948)
Intercept	−4.4235***	−3.9457***
	(−3.421)	(−2.999)
Year-fixed effects	Yes	Yes
Industry-fixed effects	Yes	Yes
No. of observations	2644	2670
Pseudo-R^2	0.3221	0.3054

Panel B: Covariate balance between treatment and control groups

Insider trades (*INSITRADE*) sample

Variables	Un (matched)	Mean *TREAT* = 1 (*N* = 1418)	Mean *TREAT* = 0 (*N* = 1418)	*t* Statistics	Standardized bias (%)
$SIZE_t$	Unmatched	7.802	6.338	23.69***	93.1
	Matched	7.750	7.740	0.16	0.6
BTM_t	Unmatched	0.573	0.570	0.08	0.3
	Matched	0.581	0.555	0.80	3.2
LEV_t	Unmatched	0.200	0.085	19.42***	77.0
	Matched	0.193	0.201	−1.32	−5.5

TABLE 2 (Continued)

Panel B: Covariate balance between treatment and control groups

Insider trades (*INSITRADE*) sample

Variables	Un (matched)	Mean *TREAT* = 1 (*N* = 1418)	Mean *TREAT* = 0 (*N* = 1418)	*t* Statistics	Standardized bias (%)
SA_t	Unmatched	−1.780	−0.587	−28.34***	−113.3
	Matched	−1.724	−1.789	1.38	6.2
ROA_t	Unmatched	0.076	0.096	−7.76***	−30.1
	Matched	0.077	0.077	−0.07	−0.2
$DEDI_t$	Unmatched	0.086	0.067	7.29***	24.3
	Matched	0.085	0.092	−0.98	−5.5
$STDCFO_t$	Unmatched	0.030	0.043	−9.77***	−37.3
	Matched	0.031	0.030	1.05	3.1
$STDEARN_t$	Unmatched	0.033	0.041	−3.92***	−15.2
	Matched	0.034	0.038	−1.86*	−7.7
$IDIOSYN_t$	Unmatched	0.047	0.059	−12.94***	−50.1
	Matched	0.048	0.049	−1.13	−3.9
$CETR_t$	Unmatched	0.225	0.238	−0.47	−1.8
	Matched	0.216	0.159	1.30	7.9
$RETVOL_t$	Unmatched	0.111	0.127	−7.17***	−27.8
	Matched	0.112	0.112	−0.06	−0.2

Stock price crash risk (*NCSKEW*) sample

Variables	Un(matched)	Mean *TREAT* = 1 (*N* = 1391)	Mean *TREAT* = 0 (*N* = 1391)	*t* Statistics	Standardized bias (%)
$SIZE_t$	Unmatched	7.678	6.256	23.91***	93.0
	Matched	7.617	7.489	2.21**	8.3
BTM_t	Unmatched	0.572	0.562	0.32	1.3
	Matched	0.583	0.648	−1.74*	−8.0
LEV_t	Unmatched	0.383	0.066	1.32	5.3
	Matched	0.155	0.165	−1.68*	−0.2
SA_t	Unmatched	−1710.80	−555.25	−28.11***	−110.9
	Matched	−1641.80	−1644.00	0.05	0.2
ROA_t	Unmatched	0.076	0.093	−7.38***	−28.5
	Matched	0.076	0.074	1.27	4.6
$DEDI_t$	Unmatched	0.082	0.066	5.73***	22.3
	Matched	0.083	0.090	−2.17**	−9.0
$STDCFO_t$	Unmatched	0.031	0.045	−9.85***	−37.8
	Matched	0.032	0.034	−1.46	−4.5
$STDEARN_t$	Unmatched	0.038	0.067	−7.04***	−31.1
	Matched	0.039	0.048	−2.45**	−9.5
$IDIOSYN_t$	Unmatched	0.048	0.061	−13.44***	−51.6
	Matched	0.049	0.051	−2.04**	−7.3

(Continues)

TABLE 2 (Continued)

Stock price crash risk (*NCSKEW*) sample					
Variables	**Un(matched)**	**Mean *TREAT* = 1 (*N* = 1391)**	**Mean *TREAT* = 0 (*N* = 1391)**	***t* Statistics**	**Standardized bias (%)**
$CETR_t$	Unmatched	0.021	0.012	2.99***	11.7
	Matched	0.021	0.025	−1.15	−5.1
$RETVOL_t$	Unmatched	0.111	0.129	−7.99***	−30.9
	Matched	0.112	0.115	−1.43	−5.4

Note: 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 nonderivative-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 (*NCSKEW*) sample, respectively. Industry dummies (constructed from the first two digits of standard industrial classification 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 nonuser (*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 postmatching 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.

Models (3) and (4) specify insider trades and 1-year-ahead stock price crash risk,[11] respectively, as the dependent variable. The treatment indicator variable, $TREAT_i$, equals 1 for a treatment firm and 0 for a control firm. The coefficient on $TREAT_i$ measures $INSITRADE_t$ or $NCSKEW_{t+1}$ of the treatment firms, relative to $INSITRADE_t$ or $NCSKEW_{t+1}$ of the control firms, in the pre-SFAS 161 period. Because SFAS 161 was effective for annual reporting periods commencing after November 15, 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 coefficient on $POST_t$ measures $INSITRADE_t$ or $NCSKEW_{t+1}$ of the control firms in the post-SFAS 161 period relative to that in the pre-SFAS 161 period. 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 nonusers. A larger magnitude of the DID estimators (α_3 in Model 3 and β_3 in Model 4) indicates greater impact of SFAS 161 on insider trades and crash risk. Hence, to support the hypotheses H1a and H1b, the coefficients for the interaction terms should be negative and statistically significant at conventional levels. We include the interacted industry–year ($IND \times YR$) fixed effects in the regressions since insider trades and crash risk are likely to vary systematically across industries and years.[12]

We include a range of control variables in Models (3) and (4) 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 & Lee, 2001). Piotroski and Roulstone (2005) find that insider trading is positively associated with future firm performance and growth prospect. Thus, we include *ROA* and the *BTM* ratio as controls. Because it is easier for insiders to trade on stocks with low transaction costs, insider trades should increase with a decrease in transaction costs, which are measured by trading volume (*TRADEVOL*) (e.g., Mendenhall, 2004). *TRADEVOL* is also a proxy for stock liquidity, which is expected to be positively related to insider trades. We also include analyst coverage (*LANACOV*) and dedicated institutional 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 & 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 (He et al., 2021; Huddart & Ke, 2007); *STDCFO* (*FIRMAGE*) is expected to be positively (negatively) related to insider trades.

As regards the control variables for crash risk in Model (4), we include firm size (*SIZE*), the *BTM* ratio, analyst coverage (*LANACOV*), dedicated institutional stock ownership (*DEDI*), *ROA*, stock trading volume (*TRADEVOL*), cash flow volatility (*STDCFO*), corporate tax avoidance (*CETR*), and the previous year's negative skewness of firm-specific weekly stock returns (*NCSKEW*). Large firms and high-growth firms are more likely to experience stock price crashes (Chen et al., 2001; Harvey

[11]Consistent with the crash risk literature (e.g., Callen & Fang, 2013; Kim et al., 2011a, 2011b; Zhu, 2016), we measure the likelihood of stock price crashes in a 1-year-ahead forecast window, given that high stock price crash risk often results from managers' hoarding of bad news.

[12]We include the interacted industry–year dummies, instead of industry dummies and year dummies, in the regressions because year dummies are more likely to multicollinear with the time indicator variable, $POST_t$.

& Siddique, 2000; Hutton et al., 2009), and hence *SIZE* (*BTM*) should be positively (negatively) correlated with crash risk. Previous studies (e.g., He & Tian, 2013; Kim et al., 2011a; Kothari et al., 2009) show that financial analysts may pressure firm management into concealing bad news 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 *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 include the previous year's negative skewness of weekly returns (*NCSKEW*) as a control. 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 the treatment and control groups before the treatment event (Roberts & 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 rerun our DID regression models (3) and (4) by using 2005 and 2006 (as well as 2006 and 2007, or 2007 and 2008) as the pre- and posttreatment 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 hypothesis H1

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 ($p = 0.005$). The coefficient for $TREAT_i \times POST_t$ amounts to 1.3650, which accounts for 27.2% of the mean of $INSITRADE_t$ 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 nonusers that are not affected by the standard. Thus, the hypothesis H1a is supported. Column (2) shows a similar result of a statistically significant coefficient on $TREAT_i \times POST_t$ ($p = 0.039$) with a negative sign, indicating that, compared with the nonusers, the compliers experience a greater reduction in the 1-year-ahead stock price crash risk post-SFAS 161. This result supports the hypothesis H1b. The coefficient for $TREAT_i \times POST_t$ amounts to 1.0615, which is equivalent to 44.0% of the mean of $NCSKEW_{t+1}$ for the treatment sample and hence is economically significant.

The coefficients for $TREAT_i$ are statistically insignificant in both Columns (1) and (2), suggesting that there is no difference in our outcome variable, $INSITRADE_t$ or $NCSKEW_{t+1}$, 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. On the other hand, the insignificant coefficients for $POST_t$ indicate that $INSITRADE_t$ or $NCSKEW_{t+1}$ of the control firms in the post-SFAS 161 period does not differ from that in the pre-SFAS 161 period, thus ensuring that our results for the DID estimators would not be explained alternatively by a change in managerial opportunism of the control firms. Furthermore, we run the regression that excludes year-fixed effects, as they might be multicollinear with $POST_t$. Results are reported in Columns (3) and (4) of Table 4 and remain qualitatively unchanged. We also conduct variance-inflation-factors (VIF) tests. The untabulated 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.

TABLE 3 Tests of parallel trends assumption.

Panel A: Univariate tests						
	Annual growth rates in *INSITRADE*			**Annual growth rates in *NCSKEW***		
Year	**Mean ($TREAT = 0$)**	**Mean ($TREAT = 1$)**	**Mean differences (t statistics)**	**Mean ($TREAT = 0$)**	**Mean ($TREAT = 1$)**	**Mean differences (t statistics)**
2006	−0.4141	−0.4033	−0.0108 (−0.124)	−2.3042	−2.4358	0.1316 (0.041)
2007	−0.4773	−0.3731	−0.1042 (−1.341)	0.7630	−1.4132	2.1762 (1.430)
2008	−0.4913	−0.5429	0.0516 (0.688)	−0.1217	0.0550	−0.1767 (−0.113)
Panel B: Multivariate tests						
Insider trades (*INSITRADE*) sample						
	Dependent variable = $INSITRADE_t$					
Variables	**(1) 2005 versus 2006**	**(2) 2006 versus 2007**	**(3) 2007 versus 2008**			
$TREAT_i \times POST_t$	**0.1768**	**−1.4108**	**−0.3076**			
	(0.197)	**(−1.470)**	**(−0.342)**			
	(0.023)	(0.036)	(0.190)			
Controls and industry × year dummies	Included	Included	Included			
No. of observations	402	752	876			
Adjusted R^2	0.1453	0.1781	0.1909			
Stock price crash risk (NCSKEW) sample						
	Dependent variable = $NCSKEW_{t+1}$					
Variables	**(1) 2005 versus 2006**	**(2) 2006 versus 2007**	**(3) 2007 versus 2008**			
$TREAT_i \times POST_t$	**−2.9135**	**2.2389**	**0.4428**			
	(−1.458)	**(1.215)**	**(0.359)**			
	(0.023)	(0.036)	(0.190)			
Controls and industry × year dummies	Included	Included	Included			
No. of observations	365	881	966			
Adjusted R^2	0.2197	0.2588	0.2557			

Note: 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 (*NCSKEW*) 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 nonderivative-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 posttreatment periods, respectively, for the estimation of DID regression models (3) and (4). 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 the interacted industry dummies (constructed from the first two digits of standard industrial classification codes) and year dummies, are included in the regressions, but their results are not reported for simplicity. All the variables in the tables are defined in Appendix A. The results highlighted in bold are those for the key independent variables of our regressions. *, **, and *** indicate the two-tailed statistical significance at the 10%, 5%, and 1% levels, respectively.

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 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. Thus, the anticipation effect is unlikely to be at play to drive our baseline results.

TABLE 4 Difference-in-differences regression analysis of the impact of SFAS 161 on managerial opportunism.

Variables	(1) $INSITRADE_t$	(2) $NCSKEW_{t+1}$	(3) $INSITRADE_t$	(4) $NCSKEW_{t+1}$
Intercept	6.9085	−16.8952	−1.9169	−12.2320**
	(1.196)	(−1.235)	(−0.602)	(−2.294)
$TREAT_i$	0.1882	0.3661	0.1637	−0.2421
	(0.524)	(0.426)	(0.468)	(−0.297)
$POST_t$	−13.3245	18.7824	0.3301	1.0696
	(−1.642)	(0.975)	(0.975)	(1.310)
$TREAT_i \times POST_t$	**−1.3650*** **	**−2.3908** **	**−1.0615** **	**−2.0554* **
	(−2.822)	**(−2.069)**	**(−2.277)**	**(−1.873)**
$SIZE_t$	0.7745***	0.6190**	0.8268***	1.2862***
	(5.571)	(2.031)	(6.290)	(4.416)
BTM_t	−0.0554	−0.5454	−0.1684	−0.6642*
	(−0.305)	(−1.496)	(−1.005)	(−1.840)
$LANACOV_t$	0.3147	0.5981	0.3222*	−0.0735
	(1.605)	(1.426)	(1.691)	(−0.178)
$DEDI_t$	−3.9071***	0.5744	−3.6556***	−1.1505
	(−3.451)	(0.151)	(−2.869)	(−0.307)
ROA_t	0.1958	10.9207*	6.5528***	2.6886
	(0.086)	(1.875)	(2.783)	(0.479)
$TRADEVOL_t$	0.1684*	−0.3606*	−0.0121	−0.3658*
	(1.936)	(−1.794)	(−0.145)	(−1.883)
$STDCFO_t$	−10.6360**	−27.4818**	−12.7701***	−26.1229**
	(−2.247)	(−2.498)	(−2.709)	(−2.445)
$FIRMAGE_t$	−0.0075		−0.0029	
	(−1.066)		(−0.419)	
$CETR_t$		7.9116**		3.9670
		(2.140)		(1.149)
$NCSKEW_t$		0.0802***		0.0002
		(3.652)		(0.012)
Industry × year dummies	Included	Included	Excluded	Excluded
Industry dummies	Excluded	Excluded	Included	Included
No. of observations	2836	2782	2836	2782
Adjusted R^2	0.2436	0.1173	0.1608	0.0423

Note: This table reports the results of the difference-in-differences regressions for the impact of Statement of Financial Accounting Standards No. 161 (SFAS 161) on managerial opportunism. The sample period covers 6 years from 2006 to 2011. In Columns (1) and (3), the dependent variable is insider trades ($INSITRADE_t$). In Columns (2) and (4), the dependent variable is stock price crash risk ($NCSKEW_{t+1}$). The treatment indicator variable, $TREAT_i$, equals 1 for a derivative-using firm that complies with SFAS 161, and 0 for a nonderivative-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 nonusers of derivatives ($TREAT = 0$). All the variables are defined in Appendix A. The industry dummies (constructed from the first two digits of standard industrial classification codes) or the interacted industry–year dummies are included in the regressions, but their results are not reported for simplicity. t Statistics in parentheses are based on robust standard errors clustered by firm. The results highlighted in bold are those for the key independent variables of our regressions. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

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 the 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 postcrisis period to rerun our DID regression models and then analyze the treatment effects of the financial crisis on our managerial opportunism variables. Provided that the effect of the 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, the 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 5 show that the coefficients on the interaction terms, $TREAT_i \times POSTCRISIS_t$, of our rerun DID regressions are not statistically significant.

Second, we use 2005–2006 as the precrisis period and 2007–2008 as the crisis period to rerun our DID regressions. If the financial crisis explains higher managerial opportunism before 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 5.

Third, we exclude the years 2008 and 2009 from our sample period (i.e., 2006–2011) and re-estimate Models (3) and (4). Results are reported in Panel B of Table 5. The coefficients for $TREAT_i \times POST'_t$ are negative and statistically significant at the 5% level for both the insider trades sample and the crash risk sample. Collectively, the results in Table 5 suggest that our earlier finding of the reduced managerial opportunism is attributed to SFAS 161 rather than the financial crisis.

5.2.3 | Exogeneity of SFAS 161

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

To further confute the possibility that the promulgation of SFAS 161 is a response to a significant increase in managerial opportunism, we re-do both the univariate and multivariate tests of parallel trends assumptions, as in Section 4, by extending our pre-SFAS 161 period to the years 2003–2008. The untabulated results show that, in this pre-SFAS 161 period, there is no evidence of a statistically significant increase in either insider trades (*INSITRADE*) or stock price crash risk (*NCSKEW*); insider trades and crash risk do not increase substantially or peak before SFAS 161. This thus implies that the implementation of SFAS 161 is not endogenous to managers' opportunistic behavior in our study.

5.2.4 | Firm-fixed effects

Although our baseline regression models (3) and (4) control for an extensive list of the determinants of insider trades and stock price crash risk, alongside the interacted industry–year-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 in lieu of $TREAT_i$. In Columns (1) and (2) of Table 6, the coefficients for the interaction terms are statistically significant at the 1% and 5% levels, respectively, 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 $POST_t$, $TREAT_i \times POST_t$, and the interacted industry–year dummies. Results are shown in Columns (3) and (4), and elicit the same inferences as do the results in Columns (1) and (2).

TABLE 5 Robustness tests of the potential confounding effect of financial crisis.

Panel A: Comparisons among precrisis period, crisis period, and postcrisis period				
	2009–2010 versus 2011–2012		**2005–2006 versus 2007–2008**	
Variables	**(1) $INSITRADE_t$**	**(2) $NCSKEW_{t+1}$**	**(3) $INSITRADE_t$**	**(4) $NCSKEW_{t+1}$**
Intercept	−1.9214	−2.4943	6.4683	13.2783
	(−0.328)	(−0.127)	(1.059)	(1.260)
$TREAT_i$	−0.5309	−1.6011	1.0750	−1.0238
	(−1.330)	(−1.161)	(1.440)	(−0.756)
$POSTCRISIS_t$	1.3341	2.9040		
	(0.161)	(0.107)		
$TREAT_i \times POSTCRISIS_t$	**−0.6050**	**−3.4548**		
	(−0.862)	**(−1.504)**		
$CRISIS_t$			7.3337	−17.2288
			(0.870)	(−1.181)
$TREAT_i \times CRISIS_t$			**−1.5453**	**1.7306**
			(−0.815)	**(1.131)**
$SIZE_t$	0.5783***	1.1090*	0.9357***	−0.1432
	(3.077)	(1.744)	(4.334)	(−0.441)
BTM_t	−1.8404***	−0.4678	−1.1174**	−0.4978
	(−3.517)	(−0.708)	(−2.078)	(−1.120)
$LANACOV_t$	0.3095	0.7803	−0.0116	0.4727
	(1.024)	(0.822)	(−0.043)	(1.141)
$DEDI_t$	−0.4439	3.6165	−4.9365***	−2.4319
	(−0.230)	(0.480)	(−2.765)	(−0.585)
ROA_t	−4.5345	21.7197*	−4.3182	3.1720
	(−1.295)	(1.850)	(−1.308)	(0.495)
$TRADEVOL_t$	−0.2398*	−0.7880**	0.2783**	−0.2218
	(−1.909)	(−2.001)	(2.174)	(−0.986)
$STDCFO_t$	−5.9752	−32.8311	0.0018*	−4.0655
	(−0.895)	(−1.626)	(1.726)	(−0.307)
$FIRMAGE_t$	0.0057		−0.0350***	
	(0.631)		(−3.245)	
$CETR_t$		19.8866*		8.3087**
		(1.835)		(2.539)
$NCSKEW_t$		0.1747***		0.0389*
		(3.617)		(1.773)

(Continues)

TABLE 5 (Continued)

Panel A: Comparisons among precrisis period, crisis period, and postcrisis period				
	2009–2010 versus 2011–2012		2005–2006 versus 2007–2008	
Variables	(1) $INSITRADE_t$	(2) $NCSKEW_{t+1}$	(3) $INSITRADE_t$	(4) $NCSKEW_{t+1}$
Industry × year dummies	Included	Included	Included	Included
No. of observations	1500	1421	1372	1331
Adjusted R^2	0.2014	0.0435	0.2544	0.2350

Note: 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 ($NCSKEW_{t+1}$) 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 nonderivative-user. The time indicator variable, $POSTCRISIS_t$, for Columns (1) and (2) equals 1 (0) if a firm is in the postcrisis (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 (precrisis) 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 captures the effects of the crisis event on insider trading and crash risk for the compliers ($TREAT = 1$) relative to the nonderivative-users ($TREAT = 0$). All the variables are defined in Appendix A. The interacted industry dummies (constructed from the first two digits of standard industrial classification codes) and year dummies are included in all the regressions, but their results are not reported for simplicity. *t* Statistics in parentheses are based on robust standard errors clustered by firm. The results highlighted in bold are those for the key independent variables of our regressions. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Panel B: Excluding the years 2008–2009		
Variables	(1) Dependent variable = $INSITRADE_t$	(2) Dependent variable = $NCSKEW_{t+1}$
Intercept	5.0202	−8.1839
	(0.829)	(−1.208)
$TREAT_i$	0.4884	0.1348
	(1.058)	(0.117)
$POST''_t$	−10.3589	1.8689
	(−1.228)	(0.233)
$TREAT_i \times POST'_t$	**−1.2088****	**−3.5608****
	(−1.977)	**(−2.383)**
$SIZE_t$	0.7688***	1.5507***
	(4.447)	(3.820)
BTM_t	0.1001	−0.0104
	(0.508)	(−0.024)
$LANACOV_t$	0.2136	0.0470
	(0.875)	(0.082)
$DEDI_t$	−1.9952	6.1000
	(−1.215)	(1.217)
ROA_t	3.4512	0.8655
	(1.098)	(0.108)
$TRADEVOL_t$	0.0647	−0.7475***
	(0.561)	(−2.662)
$STDCFO_t$	−12.4380**	−26.5166*
	(−2.035)	(−1.920)
$FIRMAGE_t$	−0.0003	

TABLE 5 (Continued)

Panel B: Excluding the years 2008–2009		
Variables	(1) Dependent variable = $INSITRADE_t$	(2) Dependent variable = $NCSKEW_{t+1}$
	(−0.029)	
$CETR_t$		6.8570
		(1.394)
$NCSKEW_t$		0.0500**
		(2.048)
Industry × year dummies	Included	Included
No. of observations	1824	1787
Adjusted R^2	0.2006	0.1597

Note: This table reports the results from the tests, which analyze the potential confounding effect of the 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 ($NCSKEW_{t+1}$) 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 nonderivative-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 nonusers of derivatives ($TREAT = 0$). All the variables are defined in Appendix A. The interacted industry dummies (constructed from the first two digits of standard industrial classification codes) and year dummies are included in all the regressions, but their results are not reported for simplicity. t Statistics in parentheses are based on robust standard errors clustered by firm. The results highlighted in bold are those for the key independent variables of our regressions. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

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 activities to reduce business risk. Direct tests on the two mechanisms are difficult, because the degree of asymmetry of the information about corporate derivative uses is hard to measure empirically, and the quantity of derivatives used for nonhedging purposes in the pre-SFAS 161 period is hard to obtain as firms generally do not disclose the purposes of derivative usage in detail before SFAS 161. Hence, we test these mechanisms indirectly by using empirical measures of information asymmetry and business risk as documented in the related literature (e.g., Hutton et al., 2009; Michelacci & Schivardi, 2013). With the measures, we do a two-step path analysis (e.g., Lennox & Payne-Mann, 2023; Lleras, 2005). The first is to analyze whether SFAS 161 mitigates the information asymmetry and business risk of derivative-using firms that comply with the disclosure requirements. In the second step, we test whether the resulted lower levels of information asymmetry and business risk would lead to reduced managerial opportunism. Given that the differences in information asymmetry and business risk between the treatment and control groups can be driven not only by derivative disclosures but also derivative usage per se, we also employ the PSM approach as in Section 4.1 to isolate the effect of derivative usage in our analysis.

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 & 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 & Ke, 2007). We measure information opacity by abnormal accruals (*ACCRUALS*), which capture the degree of potential earnings management used to withhold corporate bad news (e.g.,

TABLE 6 Firm-fixed-effects difference-in-differences regression analysis of the impact of SFAS 161 on managerial opportunism.

Variables	(1) $INSITRADE_t$	(2) $NCSKEW_{t+1}$	(3) $INSITRADE_t$	(4) $NCSKEW_{t+1}$
Intercept	63.6712	12.8265	−30.5577***	15.9434
	(1.175)	(0.579)	(−2.854)	(0.766)
$POST_t$	−4.1295	12.6925	16.8274	7.8964
	(−0.521)	(0.735)	(1.277)	(0.455)
$TREAT_i \times POST_t$	**−1.2814*** **	**−3.1275** **	**−1.2370*** **	**−2.7022** **
	(−2.737)	**(−2.415)**	**(−2.639)**	**(−2.150)**
$SIZE_t$	1.3825***	2.2496**		
	(2.804)	(1.963)		
BTM_t	−0.0357	−3.4605***		
	(−0.051)	(−2.711)		
$LANACOV_t$	0.4494	−2.5364***		
	(1.277)	(−2.708)		
$DEDI_t$	−2.5963	−2.7826		
	(−1.055)	(−0.386)		
ROA_t	0.0210	−12.9222		
	(0.144)	(−1.388)		
$TRADEVOL_t$	0.1899	−0.1666		
	(1.293)	(−0.411)		
$STDCFO_t$	−16.3872**	−36.5273**		
	(−2.543)	(−2.103)		
$FIRMAGE_t$	−1.5156			
	(−1.487)			
$CETR_t$		−7.0297		
		(−1.174)		
$NCSKEW_t$		−0.0515**		
		(−2.143)		
Firm-fixed effects	Included	Included	Included	Included
Industry × year dummies	Included	Included	Included	Included
No. of observations	2840	2752	2840	2782
Adjusted R^2	0.4691	0.2853	0.4637	0.2670

Note: This table reports the results of the difference-in-differences tests for the impact of Statement of Financial Accounting Standards No. 161 (SFAS 161) on managerial opportunism after including firm-fixed effects in the regressions. The sample period spans the years 2006–2011. In Columns (1) and (3), the dependent variable is insider trades ($INSITRADE_t$). In Columns (2) and (4), the dependent variable is stock price crash risk ($NCSKEW_{t+1}$). 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 nonderivative-users ($TREAT = 0$). All the variables are defined in Appendix A. The interacted industry dummies (constructed from the first two digits of standard industrial classification codes) and year dummies are included in all the regressions, but their results are not reported for simplicity. *t* Statistics in parentheses are based on robust standard errors clustered by firm. The results highlighted in bold are those for the key independent variables of our regressions. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Hutton et al., 2009). A higher level of information asymmetry is associated with greater abnormal accruals (*ACCRUALS*). Provided that SFAS 161 curbs managerial opportunism by means of lowering the information asymmetry, the reduction in abnormal accruals (*ACCRUALS*) subsequent to the implementation of SFAS 161 should be greater for the derivative-using firms compliant with the standard, compared to the nonderivative-users. To test this supposition, we employ the following DID regression model in the first step of our path analysis:

$$ACCRUALS_{i,t} = \gamma_0 + \gamma_1 TREAT_i + \gamma_2 POST_t + \gamma_3 TREAT_i \times POST_t + \sum_k \gamma_k CONTROLS^k_{i,t} + \sum_z \gamma_z IND \times YR^z_i + \nu_{i,t}. \quad (5)$$

On the basis of previous literature (e.g., Chang et al., 2016; Chen et al., 2021; Hutton et al., 2009; Steffen, 2022), the control variables included in Model (5) are firm size (*SIZE*), the *BTM* ratio, analyst coverage (*LANACOV*), dedicated institutional ownership (*DEDI*), *ROA*, audit fees (*AUDITFEE*), stock *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 nonderivative-users. We also control for the interacted industry–year-fixed effects in Model (5). As for the second-step path analysis, we test the impact of information asymmetry on managerial opportunism via the following regression model:

$$INSITRADE_{i,t}(NCSKEW_{i,t+1}) = n_0 + n_1 PREDICTED\_ACCRUALS_{i,t} + \sum_k n_k CONTROLS^k_{i,t} + \sum_z n_z IND \times YR^z_i + e_{i,t}. \quad (6)$$

The dependent variable is insider trades ($INSITRADE_t$) or stock price crash risk ($NCSKEW_{t+1}$). The predicted values of abnormal accruals (*PREDICTED_ACCRUALS*), which are estimated from Model (5), are used as the key independent variable in Model (6). The control variables for insider trades (stock price crash risk) are the same as those included in Model (3) (Model 4). Panel A (Panel B) of Table 7 shows the regression results for the first-step (second-step) path analysis. In Panel A, the coefficient of $TREAT_i \times POST_t$ for the regression of abnormal accruals (*ACCRUALS*) is significantly negative at the 1% level. In Panel B, abnormal accruals are positively associated with $INSITRADE_t$ and $NCSKEW_{t+1}$ with the significant levels of 5% and 1%, respectively. Together, these findings 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 (e.g., Zhang, 2009). Business risk is the overall risk inherent in a firm and is independent of the way the firm is financed (Gabriel & 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 nonhedging 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 by 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 for the first step of our path analysis, we use the following DID regression model:

$$IDIOSYN_{i,t} = \lambda_0 + \lambda_1 TREAT_i + \lambda_2 POST_t + \lambda_3 TREAT_i \times POST_t + \sum_k \lambda_k CONTROLS^k_{i,t} + \sum_z \lambda_z IND \times YR^z_i + \mu_{i,t}. \quad (7)$$

The dependent variable is idiosyncratic risk (*IDIOSYN*), a proxy for business risk (e.g., Michelacci & Schivardi, 2013). The interaction term, $TREAT_i \times POST_t$, reflects the extent to which the business risk of compliers changes relative to that of nonderivative-users after the passage of SFAS 161. If the compliers continue to use derivatives for hedging as well as they did before the regulation, we would not expect to find lower business risk (*IDIOSYN*) as a result of the implementation of SFAS 161. Following prior literature (e.g., Bartram et al., 2011; Chang

TABLE 7 Tests of the information-asymmetry mechanism through which SFAS 161 curbs managerial opportunism.

Panel A: The impact of SFAS 161 on information asymmetry

Variables	Dependent variable = $ACCRUALS_t$
Intercept	−2.9880
	(−1.511)
$TREAT_i$	0.4024*
	(1.736)
$POST_t$	0.3880
	(0.167)
$TREAT_i \times POST_t$	**−0.9045*** **
	(−3.584)
$SIZE_t$	0.0819
	(1.181)
BTM_t	0.0558
	(0.823)
$LANACOV_t$	−0.1471**
	(−2.360)
$DEDI_t$	2.7156***
	(6.633)
ROA_t	1.8969**
	(2.024)
SA_t	0.0001
	(1.462)
$AUDITFEE_t$	0.1685**
	(2.164)
$RETVOL_t$	0.2814
	(0.287)
$SALESGROWTH_t$	−0.1131
	(−0.729)
$INTANGIBLE_t$	−0.3724
	(−0.535)
Industry × year dummies	Included
No. of observations	1724
Adjusted R^2	0.2074

Panel B: The impact of information asymmetry on managerial opportunism

Variables	(1) Dependent variable = $INSITRADE_t$	(2) Dependent variable = $NCSKEW_{t+1}$
Intercept	−4.7225	−10.5547
	(−0.789)	(−0.765)
$PREDICTED\_ACCRUALS_t$	**1.3582** **	**3.5255*** **
	(2.333)	**(2.687)**
$SIZE_t$	0.8641***	−0.3547

TABLE 7 (Continued)

Panel B: The impact of information asymmetry on managerial opportunism		
Variables	**(1) Dependent variable = *INSITRADE*$_t$**	**(2) Dependent variable = *NCSKEW*$_{t+1}$**
	(5.266)	(−1.100)
BTM_t	−0.0660	−0.4884
	(−0.324)	(−0.958)
$LANACOV_t$	0.6127**	1.8671***
	(2.368)	(3.292)
$DEDI_t$	−5.6510**	−11.6783**
	(−2.575)	(−2.450)
ROA_t	−2.5441**	−6.3021***
	(−2.385)	(−2.585)
$TRADEVOL_t$	−0.0322	−0.3131
	(−0.312)	(−1.274)
$STDCFO_t$	−17.0228***	−8.8384
	(−2.873)	(−0.627)
$FIRMAGE_t$	−0.0107	
	(−1.282)	
$CETR_t$		0.0210
		(0.034)
$NCSKEW_t$		0.0559**
		(2.224)
Industry × year dummies	Included	Included
No. of observations	1980	2031
Adjusted R^2	0.2264	0.1726

Note: This table reports the results of testing the information-asymmetry mechanism through which Statement of Financial Accounting Standards No. 161 (SFAS 161) curbs managerial opportunism. The sample period spans the years 2006–2011. Panel A reports the results of the first-step path analysis. The dependent variable is abnormal accruals ($ACCRUALS_t$). 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 nonderivative-users. Panel B reports the results of the second-step path analysis as to the impact of information asymmetry on managerial opportunism. In Column (1), the dependent variable is insider trades ($INSITRADE_t$). In Column (2), the dependent variable is stock price crash risk ($NCSKEW_{t+1}$). The key independent variable is the predicted values of abnormal accruals ($PREDICTED\_ACCRUALS_t$), which are estimated from the first-step regression. All the variables are defined in Appendix A. The interacted industry dummies (constructed from the first two digits of standard industrial classification codes) and year dummies are included in all the regressions, but their results are not reported for simplicity. *t* Statistics in parentheses are based on robust standard errors clustered by firm. The results highlighted in bold are those for the key independent variables of our regressions. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

et al., 2016), we control for firm size (*SIZE*), the *BTM* ratio, financial *LEV*, *ROA*, financial constraints (*SA*), trading volume (*TRADEVOL*), stock *RETVOL*, cash flow volatility (*STDCFO*), intangible assets (*INTANGIBLE*), as well as the interacted industry–year-fixed effects in the regression. The detailed definitions of all the regressors are provided in Appendix A. In the second-step path analysis, we test the relationship between business risk and managerial opportunism via the following regression model:

$$INSITRADE_{i,t}\,(NCSKEW_{i,t+1}) = m_0 + m_1 PREDICTED\_IDIOSYN_{i,t} + \sum_k m_k CONTROLS^k_{i,t} + \sum_z m_z IND \times YR^z_i + \theta_{i,t}. \quad (8)$$

The predicted values of idiosyncratic risk (*PREDICTED_IDIOSYN*), estimated from Model (7), are used as the key independent variable in Model (8). Table 8 reports the regression results of the two-step path analysis for the real-effect

TABLE 8 Tests of the real-effect mechanism through which SFAS 161 curbs managerial opportunism.

Panel A: The impact of SFAS 161 on business risk

Variables	Dependent variable = $IDIOSYN_t$
Intercept	0.0336***
	(3.215)
$TREAT_i$	−0.0004
	(−0.552)
$POST_t$	−0.0128
	(−0.881)
$TREAT_i \times POST_t$	**−0.0019****
	(−2.146)
$SIZE_t$	−0.0032***
	(−10.764)
BTM_t	−0.0010
	(−1.467)
LEV_t	0.0069***
	(4.315)
ROA_t	0.0147***
	(3.427)
SA_t	0.0000
	(0.225)
$TRADEVOL_t$	0.0010***
	(7.125)
$RETVOL_t$	0.2175***
	(39.891)
$STDCFO_t$	0.0797***
	(9.638)
$INTANGIBLE_t$	−0.0021
	(−0.928)
Industry × year dummies	Included
No. of observations	2852
Adjusted R^2	0.7498

Panel B: The impact of business risk on managerial opportunism

Variables	(1) Dependent variable = $INSITRADE_t$	(2) Dependent variable = $NCSKEW_{t+1}$
Intercept	9.3288	−5.8586
	(1.551)	(−0.403)
$PREDICTED\_IDIOSYN_t$	**52.1267*****	**77.9887****
	(3.853)	**(2.257)**
$SIZE_t$	1.0933***	1.4675***
	(7.170)	(4.479)
BTM_t	−1.0994***	−0.7875
	(−3.111)	(−1.015)

TABLE 8 (Continued)

Panel B: The impact of business risk on managerial opportunism		
Variables	(1) Dependent variable = $INSITRADE_t$	(2) Dependent variable = $NCSKEW_{t+1}$
$LANACOV_t$	0.3336*	0.6001
	(1.872)	(1.318)
$DEDI_t$	−2.1150	−12.3470***
	(−1.396)	(−3.241)
ROA_t	−0.0973	0.2362
	(−0.785)	(0.783)
$TRADEVOL_t$	−0.1050	−0.4896**
	(−1.085)	(−1.972)
$STDCFO_t$	−15.0097***	−26.7344**
	(−2.865)	(−2.176)
$FIRMAGE_t$	−0.0127*	
	(−1.859)	
$CETR_t$		−1.5485
		(−1.607)
$NCSKEW_t$		0.0079
		(0.373)
Industry × year dummies	Included	Included
No. of observations	2834	2684
Adjusted R^2	0.2247	0.1870

Note: This table reports the results of testing the real-effect mechanism through which Statement of Financial Accounting Standards No. 161 (SFAS 161) curbs managerial opportunism. The sample period spans the years 2006–2011. Panel A reports the results of the first-step path analysis. The dependent variable is an idiosyncratic risk ($IDIOSYN_t$). 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 nonderivative-users. Panel B reports the results of the second-step path analysis as to the impact of business risk on managerial opportunism. In Column (1), the dependent variable is insider trades ($INSITRADE_t$). In Column (2), the dependent variable is stock price crash risk ($NCSKEW_{t+1}$). The key independent variable is the predicted values of idiosyncratic risk ($PREDICTED\_IDIOSYN_t$), which are estimated from the first-step regression. All the variables are defined in Appendix A. The interacted industry dummies (constructed from the first two digits of standard industrial classification codes) and year dummies are included in all the regressions, but their results are not reported for simplicity. *t* Statistics in parentheses are based on robust standard errors clustered by firm. The results highlighted in bold are those for the key independent variables of our regressions. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

mechanism. In Panel A which shows the first-step results, the coefficient of $TREAT_i \times POST_t$ is negative and statistically significant, indicating that SFAS 161 lowers the business risk of compliers to a larger degree than that of nonderivative-users. In Column (1) (Column 2) of Panel B, *IDIOSYN* is positively associated with $INSITRADE_t$ ($NCSKEW_{t+1}$) at the 1% (5%) level. Collectively, these findings suggest that an increase in hedging against business risk is yet another mechanism through which SFAF 161 curbs managerial opportunism.

6.2 | The moderating effect of derivative usage

The information asymmetry and business risk of firms could be higher in cases in which a larger number of derivatives are being used especially for nonhedging purposes. Therefore, we expect that SAFS 161 deters managerial opportunism more prominently for firms with more derivative usage. To test the prediction, we follow Friberg and Seiler (2017) to construct the proxy (*USAGE*) for the extent of derivative usage by a firm. *USAGE* equals the absolute values of unrealized gains and losses that result from a firm using derivatives over a fiscal year. We split our prematched sample of compliers into two subsamples based on the median of *USAGE*. We then match each complier with a control firm in

TABLE 9 Tests of the moderating effect of derivative usage.

Variables	Dependent variable = *INSITRADE*$_t$		Dependent variable = *NCSKEW*$_{t+1}$	
Derivative usage (*USAGE*)	**(1) Low**	**(2) High**	**(3) Low**	**(4) High**
Intercept	−8.2695	−11.6857**	−3.3459	−11.0379
	(−1.350)	(−2.191)	(−0.220)	(−0.914)
$TREAT_i$	−0.1527	4.6204	−0.5322	3.1533
	(−0.249)	(1.363)	(−0.357)	(1.530)
$POST_t$	15.1221	11.9824	1.8598	31.6585
	(1.619)	(1.422)	(0.093)	(1.574)
$\boldsymbol{TREAT_i \times POST_t}$	**0.0484**	**−9.9791*** **	**1.1359**	**−6.1290** **
	(0.053)	**(−9.583)**	**(0.541)**	**(−2.555)**
$SIZE_t$	0.4729	0.7081**	−0.9375	0.3560
	(1.595)	(2.199)	(−1.622)	(0.640)
BTM_t	0.0284	−0.3757	−0.5186	−1.2115
	(0.122)	(−0.831)	(−0.873)	(−1.151)
$LANACOV_t$	0.7597**	−0.0559	1.4365*	0.5020
	(2.079)	(−0.123)	(1.876)	(0.483)
$DEDI_t$	1.5690	−0.4587	−20.7023***	−2.3605
	(0.666)	(−0.155)	(−3.217)	(−0.342)
ROA_t	3.8273	3.0806	0.0242	5.5386
	(0.980)	(0.651)	(0.078)	(0.545)
$TRADEVOL_t$	0.1957	0.3990**	0.5631	−0.5699
	(1.184)	(2.081)	(1.389)	(−1.308)
$STDCFO_t$	−0.0612***	0.0173**	−0.3264	−2.3243
	(−3.121)	(2.110)	(−0.012)	(−0.128)
$FIRMAGE_t$	−0.0289*	−0.0248		
	(−1.803)	(−1.553)		
$CETR_t$			−0.1654	−1.1034
			(−0.046)	(−1.150)
$NCSKEW_t$			0.0168	0.1006***
			(0.393)	(2.608)
Industry × year dummies	Included	Included	Included	Included
No. of observations	816	744	814	829
Adjusted R^2	0.1687	0.4780	0.0956	0.0951

Note: This table reports the results from testing the moderating effect of derivative usage on our baseline regression results. The sample period spans the years 2006–2011. The moderator variable is derivative usage (*USAGE*), which equals the absolute values of unrealized gains and losses that result from a firm using derivatives over a fiscal year. A higher value of *USAGE* indicates a larger extent of derivative usage. The difference-in-differences regressions are run separately in the low-derivative-usage subsample and the high-derivative-usage subsample, which are split based on the full-sample median of *USAGE*. In Columns (1) and (2), the dependent variables are insider trades ($INSITRADE_t$). In Columns (3) and (4), the dependent variables are stock price crash risk ($NCSKEW_{t+1}$). The interaction term, $TREAT_i \times POST_t$, is the variable of interest to the analysis. All the variables are defined in Appendix A. The interacted industry dummies (constructed from the first two digits of standard industrial classification codes) and year dummies are included in all the regressions, but their results are not reported for simplicity. *t* Statistics in parentheses are based on robust standard errors clustered by firm. The results highlighted in bold are those for the key independent variables of our regressions. *, **, 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 noncompliers post-SFAS 161.

Variables	(1) Dependent variable = $INSITRADE_t$	(2) Dependent variable = $NCSKEW_{t+1}$
Intercept	0.9738	6.0611
	(0.168)	(0.523)
$NONCOMPLIER_i$	0.7165*	−0.8933
	(1.787)	(−1.119)
$POST_t$	−2.0078	0.2616
	(−0.246)	(0.016)
$NONCOMPLIER_i \times POST_t$	**0.1696**	**0.4312**
	(0.325)	**(0.416)**
$SIZE_t$	−0.1551	0.9469***
	(−1.141)	(3.403)
BTM_t	−0.1735*	0.0363
	(−1.781)	(0.187)
$LANACOV_t$	0.1265	0.3498
	(0.740)	(0.987)
$DEDI_t$	0.7930	−2.7998
	(0.641)	(−0.884)
ROA_t	−1.0298	2.5305
	(−1.198)	(1.371)
$TRADEVOL_t$	0.1828**	0.3192*
	(2.079)	(1.939)
$STDCFO_t$	1.3544	6.2338
	(0.389)	(0.862)
$FIRMAGE_t$	0.0045	
	(0.510)	
$CETR_t$		−3.1551
		(−0.845)
$NCSKEW_t$		0.0209
		(0.970)
Industry × year dummies	Included	Included
No. of observations	2354	2318
Adjusted R^2	0.0719	0.0897

Note: This table reports the firm-fixed-effects regression results of the placebo tests of whether managerial opportunism is reduced for the noncompliers post-Statement of Financial Accounting Standards No. 161 (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 ($NCSKEW_{t+1}$) 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 nonderivative-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 noncompliers ($NONCOMPLIER_i = 1$) relative to the nonderivative-users ($NONCOMPLIER_i = 0$). All the variables are defined in Appendix A. The interacted industry dummies (constructed from the first two digits of standard industrial classification codes) and year dummies are included in all the regressions, but their results are not reported for simplicity. t Statistics in parentheses are based on robust standard errors clustered by firm. The results highlighted in bold are those for the key independent variables of our regressions. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

each subsample using the same matching approach as in Section 4.1, and estimate Models (3) and (4) for each postmatched subsample. Table 9 reports the results for the moderating effect of derivative usage. The coefficients on $TREAT_i \times POST_t$ in the regressions of $INSITRADE_t$ and $NCSKEW_{t+1}$ are negative and statistically significant for the high-derivative-usage subsamples, but are not statistically significant for the low-usage subsamples. These results indicate that the mitigating effects of SFAS 161 on insider trades and stock price crash risk are more pronounced for firms with high derivative usage, and are thus consistent with our expectation.

6.3 | Is managerial opportunism reduced in the noncompliers 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 noncompliance 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 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 noncompliance 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 redefine our treatment firms to be the noncompliers, and re-estimate Models (3) and (4). 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 DID regression models are similar to Models (3) and (4), 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 nonderivative-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 noncomplying 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 further 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 for 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 DID 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 deter managerial opportunism. We also find that such a deterrence effect is stronger for firms with more derivative usage. Furthermore, we provide 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.

ACKNOWLEDGMENTS

We thank an anonymous reviewer, Jun Chen, Shane Dikolli, Marc Goergen, John Campbell, Edward Lee, Philippe Mueller, Thaddeus Neururer, Richard Taffler, Onur Tosun, Francesco Vallascas, Jiang Wei, Chendi Zhang, and conference/seminar participants at the 2023 International Conference on Empirical Economics, 2022 International Conference on Derivatives and Capital Markets, 2022 Derivative Markets Conference, 2022 Hawai'i Accounting Research Conference, 72nd European Meeting of the Econometric Society, American Accounting Association 2021 Annual Meeting, 35th Annual Conference of the Academy of Finance, Canadian Academic Accounting Association 2021 Annual Conference, 2021 European Accounting Association annual meeting, China International Risk Forum & China Finance Review International Joint Conference, 1st Boca Corporate Finance and Governance Conference, 2019 Financial Management Association annual meeting, 37th International Conference of the French Finance Association, 11th Financial Markets and Corporate Governance conference, 1st China Financial Market Conference, 5th Annual International Corporate Governance Society Conference, 27th Annual Conference on Pacific Basin Finance, Economics, Accounting and Management, 5th Young Finance Scholars Conference, 12th Japanese Accounting Review (TJAR) conference, 7th Warwick Economics PhD Conference, Midlands Regional Doctoral Conference, 28th Eurasia Business and Economics Society Conference, 9th International Conference on Futures and Other Derivatives, University of Warwick, University of Leeds, and Jiangxi University of Finance and Economics for their helpful comments and suggestions. All errors remain our own.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from the corresponding author upon reasonable request. The data that support the findings of this study are available from the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system of the U.S. Securities and Exchange Commission (SEC), the Center for Research in Security Prices (CRSP), Compustat, Thomson Financial Insider Research Services, and Institutional Brokers' Estimate System (I/B/E/S, 2005–2012). Third-party restrictions apply to the data from CRSP, Compustat, Thomson (Refinitiv), and I/B/E/S, which were used under license for this study and hence are not available to share.

ORCID

Guanming He http://orcid.org/0000-0002-4879-6795
Helen Mengbing Ren http://orcid.org/0000-0001-6721-6269

REFERENCES

Acharya, V. V., Almeida, H., & Campello, M. (2007). Is cash negative debt? A hedging perspective on corporate financial policies. *Journal of Financial Intermediation*, *16*(4), 515–554.

Adam, T., & Fernando, C. (2006). Hedging, speculation, and shareholder value. *Journal of Financial Economics*, *81*(2), 283–309.

Ataullah, A., Goergen, M., & Le, H. (2014). Insider trading and financing constraints. *Financial Review*, *49*(4), 685–712.

Bartram, S. M., Brown, G. W., & Conrad, J. (2011). The effects of derivatives on firm risk and value. *Journal of Financial and Quantitative Analysis*, *46*(4), 967–999.

Bhattacharya, N., Chang, H. S., & Chiorean, R. (2022). Regulatory interventions in response to noncompliance with mandatory derivatives disclosures rules. *Review of Accounting Studies*, forthcoming.

Bodnar, G. M., Jong, A., & Macrae, V. (2003). The impact of institutional differences on derivatives usage: A comparative study of US and Dutch firms. *European Financial Management*, *9*(3), 271–297.

Boyallian, P., & Ruiz-Verdú, P. (2018). Leverage, CEO risk-taking incentives, and bank failure during the 2007–10 financial crisis. *Review of Finance*, *22*(5), 1763–1805.

Bratton, W. W. (2002). Enron and the dark side of shareholder value. *Tulane Law Review*, *76*, 1275–1361.

Brown, G. W. (2001). Managing foreign exchange risk with derivatives. *Journal of Financial Economics*, *60*(2–3), 401–448.

Caliendo, M., & Kopeinig, S. (2008). Some practical guidance for the implementation of propensity score matching. *Journal of Economic Surveys*, *22*(1), 31–72.

Callen, J. L., & Fang, X. (2013). Institutional investor stability and crash risk: Monitoring versus short-termism? *Journal of Banking & Finance*, *37*(8), 3047–3063.

Campbell, J. L., Khan, U., & Pierce, S. (2021). The effect of mandatory disclosure on market inefficiencies: Evidence from FASB Statement No. 161. *The Accounting Review*, *96*(2), 153–176.

Chang, H. S., Donohoe, M., & Sougiannis, T. (2016). Do analysts understand the economic and reporting complexities of derivatives? *Journal of Accounting and Economics*, *61*(2–3), 584–604.
Chang, W. W. (2011). Financial crisis of 2007–2010. *Keio Economic Studies*, *47*, 25–56.
Chang, X., Chen, Y., & Zolotoy, L. (2017). Stock liquidity and stock price crash risk. *Journal of Financial and Quantitative Analysis*, *52*(4), 1605–1637.
Chen, J., Dou, Y., & Zou, Y. (2021). Information externalities of SFAS 161: Evidence from supply chains. *The Accounting Review*, *96*(4), 179–202.
Chen, J., Hong, H., & Stein, J. C. (2001). Forecasting crashes: Trading volume, past returns, and conditional skewness in stock prices. *Journal of Financial Economics*, *61*(3), 345–381.
Chen, X., Harford, J., & Li, K. (2007). Monitoring: Which institutions matter? *Journal of Financial Economics*, *86*(2), 279–305.
Chernenko, S., & Faulkender, M. (2011). The two sides of derivatives usage: Hedging and speculating with interest rate swaps. *Journal of Financial and Quantitative Analysis*, *46*(6), 1727–1754.
dataset Center for Research in Security Prices (CRSP). (2005–2012). *Stocks*. Wharton Research Data Service.
dataset Compustat. (2005–2012). *Fundamentals annual*. Wharton Research Data Service.
dataset Institutional Brokers' Estimate System (I/B/E/S). (2005–2012). *Detail history*. Wharton Research Data Service.
dataset Thomson Financial Insider Research Services. (2005–2012). *Historical files*. Wharton Research Data Service.
Dechow, P., Sloan, R., & Sweeney, A. (1995). Detecting earnings management. *Accounting Review*, *70*(2), 193–225.
Dechow, P. M., Lawrence, A., & Ryans, J. P. (2016). SEC comment letters and insider sales. *The Accounting Review*, *91*(2), 401–439.
DeMarzo, P. M., & Duffie, D. (1995). Corporate incentives for hedging and hedge accounting. *Review of Financial Studies*, *8*(3), 743–771.
Donohoe, M. P. (2015). The economic effects of financial derivatives on corporate tax avoidance. *Journal of Accounting and Economics*, *59*(1), 1–24.
Drakopoulou, V. (2014). Accounting for derivative instruments and hedging activities. *Journal of Financial Risk Management*, *3*(4), 151–165.
Dyreng, S. D., Hanlon, M., & Maydew, E. L. (2010). The effects of executives on corporate tax avoidance. *The Accounting Review*, *85*(4), 1163–1189.
Faulkender, M. (2005). Hedging or market timing? Selecting the interest rate exposure of corporate debt. *The Journal of Finance*, *60*(2), 931–962.
Financial Accounting Standards Board (FASB). (1998). *Statement of financial accounting standards No. 133: Accounting for derivative instruments and hedging activities*.
Financial Accounting Standards Board (FASB). (2008). *Statement of financial accounting standards No. 161: Disclosures about derivative instruments and hedging activities*.
Financial Accounting Standards Board (FASB). (2017). *Accounting standards update No. 2017–12, derivatives and hedging (Topic 815): Targeted improvements to accounting for hedging activities*.
Frankel, R., & Li, X. (2004). Characteristics of a firm's information environment and the information asymmetry between insiders and outsiders. *Journal of Accounting and Economics*, *37*(2), 229–259.
Friberg, R., & Seiler, T. (2017). Risk and ambiguity in 10-Ks: An examination of cash holding and derivatives use. *Journal of Corporate Finance*, *45*, 608–631.
Froot, K. A., Scharfstein, D. S., & Stein, J. C. (1993). Risk management: Coordinating corporate investment and financing policies. *The Journal of Finance*, *48*(5), 1629–1658.
Fu, R., Kraft, A., & Zhang, H. (2012). Financial reporting frequency, information asymmetry, and the cost of equity. *Journal of Accounting and Economics*, *54*(2–3), 132–149.
Gabriel, S. C., & Baker, C. B. (1980). Concepts of business and financial risk. *American Journal of Agricultural Economics*, *62*(3), 560–564.
Géczy, C., Minton, B. A., & Schrand, C. (1997). Why firms use currency derivatives? *The Journal of Finance*, *52*(4), 1323–1354.
Géczy, C. C., Minton, B. A., & Schrand, C. M. (2007). Taking a view: Corporate speculation, governance, and compensation. *The Journal of Finance*, *62*(5), 2405–2443.
Gilje, E. P., & Taillard, J. P. (2017). Does hedging affect firm value? Evidence from a natural experiment. *The Review of Financial Studies*, *30*(12), 4083–4132.
Graham, J. R., & Rogers, D. A. (2002). Do firms hedge in response to tax incentives? *The Journal of Finance*, *57*(2), 815–839.
Guay, W. R. (1999). The impact of derivatives on firm risk: An empirical examination of new derivative users. *Journal of Accounting and Economics*, *26*(1–3), 319–351.
Hadlock, C. J., & Pierce, J. R. (2010). New evidence on measuring financial constraints: Moving beyond the KZ index. *Review of Financial Studies*, *23*(5), 1909–1940.
Hanlon, M., & Heitzman, S. (2010). A review of tax research. *Journal of Accounting and Economics*, *50*(2–3), 127–178.
Harvey, C. R., & Siddique, A. (2000). Conditional skewness in asset pricing tests. *The Journal of Finance*, *55*(3), 1263–1295.
Haushalter, G. D. (2000). Financing policy, basis risk, and corporate hedging: Evidence from oil and gas producers. *The Journal of Finance*, *55*(1), 107–152.
He, G. (2015). The effect of CEO inside debt holdings on financial reporting quality. *Review of Accounting Studies*, *20*(1), 501–536.
He, G. (2023). How do insider trading incentives shape nonfinancial disclosures? Evidence from product and business expansion disclosures. *Review of Quantitative Finance and Accounting*, *60*(1), 147–194.

He, G., & Ren, H. M. (2023). Are financially constrained firms susceptible to a stock price crash? *The European Journal of Finance*, *29*(6), 612–637.

He, G., Ren, H. M., & Taffler, R. (2021). Do corporate insiders trade on future stock price crash risk? *Review of Quantitative Finance and Accounting*, *56*(4), 1561–1591.

He, G., Ren, H. M., & Taffler, R. (2022). Do enhanced derivative disclosures work? An informational perspective. *Journal of Futures Markets*, *42*(1), 24–60.

He, J., & Tian, X. (2013). The dark side of analyst coverage: The case of innovation. *Journal of Financial Economics*, *109*(3), 856–878.

Huddart, S., Ke, B., & Shi, C. (2007). Jeopardy, non-public information, and insider trading around SEC 10-K and 10-Q filings. *Journal of Accounting and Economics*, *43*(1), 3–36.

Huddart, S. J., & Ke, B. (2007). Information asymmetry and cross-sectional variation in insider trading. *Contemporary Accounting Research*, *24*(1), 195–232.

Hutton, A. P., Marcus, A. J., & Tehranian, H. (2009). Opaque financial reports, R^2, and crash risk. *Journal of Financial Economics*, *94*(1), 67–86.

Irani, R. M., & Oesch, D. (2013). Monitoring and corporate disclosure: Evidence from a natural experiment. *Journal of Financial Economics*, *109*(2), 398–418.

Jayaraman, S., & Wu, J. S. (2019). Is silence golden? Real effects of mandatory disclosure. *The Review of Financial Studies*, *32*(6), 2225–2259.

Jin, L., & Myers, S. (2006). R2 around the world: New theory and new tests. *Journal of Financial Economics*, *79*(2), 257–292.

Kadant Inc. (2008). *2007 10-K form* (pp. F16–F17). https://www.sec.gov/Archives/edgar/data/886346/000119312508053030/d10k.htm

Kadant Inc. (2011). *2010 10-K form* (pp. F36–F38). https://www.sec.gov/Archives/edgar/data/886346/000119312511069984/d10k.htm

Kanodia, C., & Sapra, H. (2016). A real effects perspective to accounting measurement and disclosure: Implications and insights for future research. *Journal of Accounting Research*, *54*(2), 623–676.

Ke, B., Huddart, S., & Petroni, K. (2003). What insiders know about future earnings and how they use it: Evidence from insider trades. *Journal of Accounting and Economics*, *35*(3), 315–346.

Ke, R., Li, M., Ling, Z., & Zhang, Y. (2019). Social connections within executive teams and management forecasts. *Management Science*, *65*(1), 439–457.

Khan, U., Li, B., Rajgopal, S., & Venkatachalam, M. (2018). Do the FASB's standards add shareholder value? *The Accounting Review*, *93*(2), 209–247.

Kim, J.-B., Li, Y., & Zhang, L. (2011a). Corporate tax avoidance and stock price crash risk: Firm-level analysis. *Journal of Financial Economics*, *100*(3), 639–662.

Kim, J.-B., Li, Y., & Zhang, L. (2011b). CFOs versus CEOs: Equity incentives and crashes. *Journal of Financial Economics*, *101*(3), 713–730.

Koonce, L., Lipe, M. G., & McAnally, M. L. (2008). Investor reactions to derivative use and outcomes. *Review of Accounting Studies*, *13*(4), 571–597.

Kothari, S. P., Shu, S., & Wysocki, P. D. (2009). Do managers withhold bad news? *Journal of Accounting Research*, *47*(1), 241–276.

Lakonishok, J., & Lee, I. (2001). Are insider trades informative? *Review of Financial Studies*, *14*(1), 79–111.

Lennox, C., & Payne-Mann, C. (2023). *An explanation of path analysis and recommendations for best practice* [Working Paper]. https://ssrn.com/abstract=4573444

Leuz, C., & Wysocki, P. D. (2016). The economics of disclosure and financial reporting regulation: Evidence and suggestions for future research. *Journal of Accounting Research*, *54*(2), 525–622.

Lleras, C. (2005). Path analysis. *Encyclopedia of Social Measurement*, *3*, 25–30.

Manchiraju, H., Hamlen, S., Kross, W., & Suk, I. (2016). Fair value gains and losses in derivatives and CEO compensation. *Journal of Accounting, Auditing & Finance*, *31*(3), 311–338.

Manchiraju, H., Pierce, S., & Sridharan, S. S. (2018). *Is hedge accounting designation informative about how firms use derivatives*? [Working Paper]. Indian School of Business, Florida State University, and Northwestern University.

Mendenhall, R. R. (2004). Arbitrage risk and post-earnings-announcement drift. *The Journal of Business*, *77*(4), 875–894.

Mian, S. L. (1996). Evidence on corporate hedging policy. *The Journal of Financial and Quantitative Analysis*, *31*(3), 419–439.

Michelacci, C., & Schivardi, F. (2013). Does idiosyncratic business risk matter for growth? *Journal of the European Economic Association*, *11*(2), 343–368.

Minton, B. A., & Schrand, C. (1999). The impact of cash flow volatility on discretionary investment and the costs of debt and equity financing. *Journal of Financial Economics*, *54*(3), 423–460.

Nance, D. R., Smith, Jr., C. W., & Smithson, C. W. (1993). On the determinants of corporate hedging. *The Journal of Finance*, *48*(1), 267–284.

Pecht, G. G., Oakes, M. T., & Stokes, P. A. (2014). *SEC steps up scrutiny of financial reporting and internal controls*. Norton Rose Fulbright Publications. http://www.nortonrosefulbright.com/knowledge/publications/123389/sec-steps-up-scrutiny-of-financial-reporting-and-internal-controls

Pincus, M., & Rajgopal, S. (2002). The interaction between accrual management and hedging: Evidence from oil and gas firms. *The Accounting Review*, *77*(1), 127–160.

Piotroski, J. D., & Roulstone, D. T. (2005). Do insider trades reflect both contrarian beliefs and superior knowledge about future cash flow realizations? *Journal of Accounting and Economics*, *39*(1), 55–81.

Roberts, M. R., & Whited, T. M. (2013). Endogeneity in empirical corporate finance. *Handbook of the Economics of Finance*, *2*, 493–572.

Rosenbaum, P. R., & Rubin, D. B. (1985). Constructing a control group using multivariate matched sampling methods that incorporate the propensity score. *The American Statistician*, *39*(1), 33–38.

Shipman, J. E., Swanquist, Q. T., & Whited, R. L. (2017). Propensity score matching in accounting research. *The Accounting Review*, *92*(1), 213–244.

Skaife, H. A., Veenman, D., & Wangerin, D. (2013). Internal control over financial reporting and managerial rent extraction: Evidence from the profitability of insider trading. *Journal of Accounting and Economics*, *55*(1), 91–110.

Smith, C. W., & Stulz, R. M. (1985). The determinants of firms' hedging policies. *The Journal of Financial and Quantitative Analysis*, *20*(4), 391–405.

Steffen, T. D. (2022). The information asymmetry effects of expanded disclosures about derivative and hedging activities. *Management Science*, *68*(8), 6298–6325.

Tufano, P. (1996). Who manages risk? An empirical examination of risk management practices in the gold mining industry. *The Journal of Finance*, *51*(4), 1097–1137.

Tysiac, K. (2017). New FASB standard aims to simplify hedge accounting. *Journal of Accountancy*. https://www.journalofaccountancy.com/news/2017/aug/new-fasb-standard-hedge-accounting-201717342.html

Zhang, H. (2009). Effect of derivative accounting rules on corporate risk-management behavior. *Journal of Accounting and Economics*, *47*(3), 244–264.

Zhu, W. (2016). Accruals and price crashes. *Review of Accounting Studies*, *21*(2), 349–399.

How to cite this article: He, G., & Ren, H. M. (2023). Derivative disclosures and managerial opportunism. *The Journal of Futures Markets*, 1–36. https://doi.org/10.1002/fut.22472

APPENDIX A: SUMMARY OF VARIABLE DEFINITIONS

Variables	Definitions
NCSKEW	The negative of the third moment of firm-specific weekly returns, divided by the standard deviation of the firm-specific weekly returns raised to the third power. 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 Statement of Financial Accounting Standards No. 161 (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 3-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 nonuser of derivatives.
POST′	1 if a firm is in the 2-year period of 2010–2011, and 0 if a firm is in the 2-year period of 2006–2007.
POSTCRISIS	1 if a firm is in the postcrisis 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 precrisis 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 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.

Variables	Definitions
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, scaled by the total assets of the firm at the end of the current fiscal year.
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	Abnormal accruals for a firm over a fiscal year, which is estimated using the industry-specific modified Jones model (Dechow et al., 1995).
STDEARN	The standard deviation of income before extraordinary items for the current and previous four fiscal years, scaled by total assets of the firm at the end of the current fiscal year.
CETR	The cash effective tax rate, calculated as cash taxes paid (TXPD) divided by pretax income net of special items. We set missing values of TXPD to be zero, and exclude observations for which the denominator of *CETR* is zero or negative.
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 rescaled by dividing 1000.
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.
USAGE	The absolute values of the Compustat variable AOCIDERGL, which stands for the unrealized gains and losses that result from a firm using derivatives over a fiscal year.

APPENDIX B: EXAMPLES OF DERIVATIVE DISCLOSURES BEFORE AND AFTER SFAS 161

B.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 (OCIs). 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 OCIs (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.

B.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 OCIs. 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 London Interbank Offered Rate (LIBOR) rate on future outstanding debt. The 2008 Swap Agreement has a 5-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 3-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 OCIs (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 on 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 OCIs (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:

		2010		2009	
(In thousands)	**Balance sheet location**	**Asset (liability) (a)**	**Notional amount (b)**	**Asset (liability) (a)**	**Notional amount (b)**
Derivatives designated as hedging instruments					
Derivatives in an asset position:					
Forward currency-exchange contracts	Other current assets	$ 131	$ 1794	$ 207	$ 7856
Derivatives in a liability position:					
Forward currency-exchange contracts	Other current liabilities	$ (59)	$ 1056	$ –	$ –
Interest rate swap agreements	Other long-term liabilities	$ (1595)	$ 17,750	$ (1517)	$ 23,250
Derivatives not designated as hedging instruments					
Derivatives in a liability position:					
Forward currency-exchange contracts	Other current liabilities	$ (48)	$ 1816	$ (98)	$ 1728

(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 OCIs 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	$	1212	$	(138)	$	1074
(Loss) gain reclassified to earnings (a)		(710)		138		(572)
Loss (gain) recognized in other comprehensive item		788		(50)		738
Unrealized loss (gain), net of tax, at January 1, 2011	$	1290	$	(50)	$	1240

(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 12 months.