Three Studies on Hedging Activities and Managerial Opportunism

by Kenuo Li

Research School of Accounting

College of Business and Economics

The Australian National University

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Declaration of Originality

I, Kenuo Li, certify that this thesis does not incorporate without acknowledgement any material previously submitted for a degree or diploma in any university and that to the best of my knowledge and belief it does not contain any material previously published or written by another person except where due reference is made in the text.

Kenuo Li

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Abstract

This thesis presents three studies on managerial opportunism in hedging-related activities. Managers have incentives to engage in firm-level and individual-level hedging activities. This thesis investigates managers' opportunistic behaviours when conducting hedging activities.

The first study examines whether the implementation of mandatory derivative disclosures under Financial Accounting Standard No. 161: Disclosures about Derivative Instruments and Hedging Activities (FAS 161) altered the relative merit of discretionary accruals and financial derivatives for income smoothing. Discretionary accruals and financial derivatives have been identified as complements in terms of income smoothing during the Financial Accounting Standard (FAS) 133 period (Choi et al., 2015). However, the subsequent adoption of FAS 161 may affect the relationship between derivative hedging and accruals management. Using a sample of S&P 500 non-financial firms during 2002–2016, I find that the positive relationship between discretionary (current and working capital) accruals and financial derivatives documented by Choi et al. (2015) is attenuated following the implementation of FAS 161. The results are robust to the use of alternative measures of discretional accruals and a sample of continuing firms, excluding firms using derivatives for trading purposes and those making derivative-related misstatements. I also document that the weakening in the relationship between derivatives and discretionary accruals after the implementation of FAS 161 is more pronounced for firms that bear high disclosure costs and those in highly competitive industries. Overall, my results suggest that a material change in disclosure rules can influence the method of income smoothing.

The second study investigates the impact of share pledging by corporate insiders (i.e. directors, executives) on non-GAAP reporting. Using a hand-collected dataset covering publicly listed US firms for the period 2006-2019, I find that share pledging is positively

associated with the likelihood of non-GAAP reporting and negatively related to the quality of non-GAAP reporting. The results are robust to employing alternative measures of non-GAAP reporting quality, to using a propensity score matched sample, and to conducting a difference-in-differences analysis based on pledging initiation. Further analyses show that the effect of share pledging on the likelihood and quality of non-GAAP reporting is more pronounced for firms with less analyst coverage and fewer independent boards. Overall, the results suggest that insiders who have shares under pledge report non-GAAP earnings opportunistically.

The third study explores the impact of share pledging on workplace safety. Using establishment-level data on workplace safety from the Occupational Safety and Health Administration, I find that firms with insider share pledging exhibit higher workplace injury/illness. The channel analyses suggest that the higher injury/illness in pledging firms are associated with declines in safety investments. Further analysis shows that the documented association between share pledging and workplace safety is weaker for firms that have more analyst coverage, face higher levels of pressure from labour unions, are located in states that have higher workers' compensation premiums and operate in counties that have higher levels of social capital. The results are robust to using injury rate as an alternative measure of workplace safety, to controlling for accrual-based earnings management, and to firm-level analyses. Overall, the findings suggest that managers with share pledging compromise workplace safety to support share prices.

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List of Abbreviations

DCBS	Department of Consumer and Business Services
DID	difference-in-differences
FASB	Financial Accounting Standards Board
FX	foreign currency
GAAP	generally accepted accounting principles
HHI	Herfindahl–Hirschman index
IMR	inverse Mills ratios
IR	interest rate
M&A	merger and acquisition
NRCRD	Northeast Regional Center for Rural Development
ODI	OSHA Data Initiative
OLS	ordinary least squares
OSHA	Occupational Safety Health Administration
PP&E	property, plant and equipment
PSM	propensity score matching
SEC	Securities and Exchange Commission
TCR	total case rate
WCA	working capital accruals

Chapter 1: Introduction

1.1 Introduction

This thesis aims to explore whether managers behave opportunistically when conducting hedging-related activities. Hedging is the practice of taking a position in one market to reduce exposure to undesirable risks (Petersen & Thiagarajan, 2000). Managers may involve in firm-level and individual-level hedging activities. My first study focuses on firm-level hedging activities. At the firm level, managers have incentives to engage in hedging activities because undesirable risks can expose firms to financial distress, force managers to delay or to forego promising investment opportunities and increase the cost of external financing (DeMarzo & Duffie, 1995; Froot et al., 1993; Smith & Stulz, 1985). Financial derivatives (e.g. futures, forwards and options) are typically used by managers in corporate-level risk management. Although financial derivatives can be efficient for reducing the volatility of earnings and cash flow (Zhang, 2009), managers may also employ derivatives for speculative purposes and for the management of earnings (Bodnar et al., 1998; Chernenko & Faulkender, 2011; Faulkender, 2005).

The anecdotal and the empirical evidence suggests that managers can behave opportunistically when conducting firm-level hedging activities (Chernenko & Faulkender, 2011; Faulkender, 2005). Employees in corporate treasury departments admit that they use financial derivatives to time the market and thus inflate quarterly earnings (Faulkender, 2005). Prior studies document that managers opportunistically time interest-rate swaps to meet analyst forecasts (Chernenko & Faulkender, 2011).

A growing body of literature shows that managers use a combination of financial derivatives and discretionary accruals to smooth earnings (Barton, 2001; Choi et al., 2015; Huang et al., 2009; Pincus & Rajgopal, 2002). Managers' decisions to choose a particular

strategy to smooth earnings reflect the costs and the effectiveness of that strategy compared with those of alternative methods. Changes to accounting standards can alter the relative costs and the effectiveness of alternative strategies. Specifically, as documented by Choi et al. (2015), adopting a hedge accounting standard, FAS 133, alters the relative costs and the effectiveness of using derivatives for income smoothing. However, FAS 133 was criticised for not requiring sufficient information to comprehend the effects of derivative use (Financial Accounting Standards Board [FASB], 2008). Therefore, the Financial Accounting Standards Board (FASB) issued FAS 161 to impose derivative disclosure requirements on firms.

Motivated by concerns about whether there was adequate disclosure of derivatives (Manchiraju et al., 2014), potentially making derivatives more attractive for smoothing earnings, my first study investigates the impact of FAS 161 implementation on the relative use of derivatives and discretionary accruals for income smoothing. Given that FAS 161 requires more detailed disclosures about derivative hedging than FAS 133, firms risk disclosing proprietary information that benefits their competitors and other counterparties at their own cost (Zou, 2018). Moreover, the increased disclosure frequency from annually to quarterly under FAS 161 may make it harder for firms to 'hide' derivative gains or losses at the end of the reporting period (Bean & Irvine, 2015) thus reducing the effectiveness of using derivatives as income-smoothing strategies. Therefore, I expect managers take these into account and reduce their use of derivative hedging relative to discretionary accruals for income smoothing, consequently attenuating the complementary relationship that is documented by Choi et al. (2015).

Consistent with my prediction, the empirical findings from Study 1 suggest that the positive relationship between derivatives and discretionary accruals is weakened after the adoption of FAS 161. Study 1 provides insights into the effects of mandatory derivative disclosures on managerial decisions and extends Choi et al.'s (2015) work by documenting that

derivatives and discretionary accruals are no longer complements for income smoothing post-FAS 161.

Having documented the incentives behind firm-level hedging activities and managers' opportunistic use of corporate derivatives, this thesis then discusses individual-level hedging activities and managerial opportunism. Managers usually hold a large proportion of company shares and thus are exposed to firm-specific risks (Larcker & Tayan, 2010). Their wealth shrinks if the financial performance of their companies is poor. Managers can pledge their companies' shares as collateral for loans to 'diversify or consume a portion of their holdings' (Larcker & Tayan, 2010). The proceeds from pledging shares can be used to purchase other assets or businesses (Dou et al., 2019). Thus, managers can limit their exposure to firm-specific risks. The practice of corporate insiders using their company shares as collateral to secure their loans is known as share pledging.

My second study examines whether share pledging affects managers' non-GAAP reporting practices. This study was motivated by the Securities and Exchange Commission's (SEC) concern that share pledging by insiders potentially affects managerial behaviour and the evidence that managers disclose non-GAAP earnings opportunistically to bias investor perceptions about firms. Theoretically, when the stock price falls below a certain level, it automatically triggers margin calls, which force managers to sell shares if they cannot repay the loan or deposit more funds (Dou et al., 2019; Pang & Wang, 2020). Managers who pledge significant amounts of their wealth have strong incentives to avoid margin calls and forced sales if they wish to maintain control rights (Chan et al., 2018; Singh, 2018).

Consistent with this margin-call hypothesis, prior studies document that managers that have pledged shares repurchase their firms' shares (Chan et al., 2018), positively bias the tone of earnings communication in conference calls (Zhao et al., 2019) and initiate mergers and acquisitions (Zhu et al., 2021) to avoid potential stock-price drops. Following this line of

research, given that non-GAAP disclosures are effective for managing investors' perceptions about firms (Abdel-Meguid et al., 2021; Black et al., 2017; Black & Christensen, 2009), pledging managers could resort to optimistic non-GAAP disclosures to counteract the price pressure that is imposed on share pledging. Consequently, insider share pledging would have an impact on non-GAAP reporting practices.

Study 2 documents a positive relationship between insider share pledging and the propensity and the frequency of non-GAAP reporting. Managers are more likely to report non-GAAP earnings when there is a significant amount of margin-call pressure. Share pledging is negatively associated with the quality of non-GAAP reporting. The deterioration in non-GAAP reporting quality, combined with the more frequent non-GAAP earnings disclosures, is consistent with the notion that managers who have shares under pledge report non-GAAP earnings opportunistically. This study contributes to the discussion on the motives (informative v. opportunistic) behind non-GAAP reporting and provides evidence that share pledging affects managerial behaviour (more opportunistic non-GAAP reporting).

My third study investigates whether managers that have pledged shares compromise workplace safety to avoid potential margin calls. Similar to the second study, Study 3 was motivated by the concern that share pledging can encourage managers to conduct opportunistic activities to support share prices and thus avoid margin calls. A recent study documents that managers compromise workplace safety to inflate earnings (Caskey & Ozel, 2017). It is well established that managers can support share prices through earnings management (Cheng & Warfield, 2005; Cohen et al., 2008). Managers that have pledged shares may also conduct real activity management (compromises in workplace safety) to secure share prices.

Multitask principal–agent theory indicates that managers tend to allocate more (less) time and attention to activities that are easy (hard) to assess and to reward. Given that safety issues are more challenging to measure than operating performance (Holmstrom & Milgrom, 1991;

Wachter & Wright, 1990) and it is more difficult for managers to be rewarded for safety-related performance (Lin et al., 2021), managers may reduce safety investments and focus more on operating performance because existing studies provide evidence that earnings directly affect managers' compensation (Sloan, 1993; Curtis et al., 2021). Empirical studies document that managers invest less than the optimal amount in the safety of their employees when faced with pressures from the capital market (Bai et al., 2020; Bradley et al., 2022; Caskey & Ozel, 2017). Given that managers can increase earnings through reductions in safety investments and increases in employee workloads, pledging managers have the potential to compromise workplace safety to inflate earnings and thus secure share prices.

Study 3 concludes that insider share pledging is positively related to workplace injury and illness cases. The higher injuries in pledging firms are associated with reduced safety investments. Overall, the findings are consistent with the view that pledging managers conduct opportunistic behaviours (compromises on workplace safety) to relieve margin-call pressure and to maintain control rights. However, these activities have unwanted consequences because they are harmful to employee welfare.

1.2 Research Design and Method

1.2.1 Research Design and Method: Study 1

In line with Barton (2001) and Choi et al. (2015), I employ simultaneous equations to investigate the impact of FAS 161 implementation on the relationship between derivatives and discretionary accruals because the magnitude of derivative use is endogenously determined at the same time as discretionary accruals. Consistent with the existing literature, I use the notional amount of derivatives (scaled by total assets) as a proxy for derivative use where the data are hand collected from corporate 10-K filings or the annual reports of non-financial S&P 500 firms during the 2002 to 2016 period. Further, I construct three measures for discretionary accruals:

discretionary total accruals, discretionary working capital accruals and discretional current accruals.

I expand Choi et al.'s (2015) simultaneous equations by including the FAS 161 indicator variable, the interactions between the derivatives and the FAS 161 dummy and the interactions between the discretionary accruals and the FAS 161 dummy. The two interaction terms capture the changes in the relationship between financial derivatives and accruals management after the implementation of FAS 161. I cluster the standard errors by firm and by year to adjust for heteroskedasticity (Petersen, 2009).

1.2.2 Research Design and Method: Study 2

I estimate a logit model to investigate the relationship between share pledging and the propensity to report non-GAAP earnings. Following the existing literature (Pang & Wang, 2020), I construct two measures of share pledging; one is the pledge indicator variable and the other is the percentage of shares that is pledged by insiders. The data on share pledging are hand collected from all the US publicly listed firms' proxy statements (DEF 14A) between 2007 and 2019. I control for the variables that affect the likelihood of non-GAAP disclosures, as is documented in the existing literature (Kolev et al., 2008; Kyung et al., 2019; Lougee & Marquardt, 2004).

Next, I examine the impact of share pledging on the frequency of non-GAAP disclosures by estimating an ordinary least squares (OLS) regression model. The percentage of quarters in a year in which managers disclose non-GAAP earnings is the proxy for the frequency of non-GAAP disclosures (Hsu et al., 2021).

Then, I analyse whether share pledging affects the quality of non-GAAP reporting, which has the quality of non-GAAP exclusions as the proxy. Prior studies (Black et al., 2021b; Christensen et al., 2021; Doyle et al., 2003; Kyung et al., 2019) recognise non-GAAP

exclusions as high-quality (low-quality) exclusions if they are transitory (recurring). For this purpose, I follow prior studies (Kolev et al., 2008; Kyung et al., 2019) and estimate an OLS regression model.

To improve the covariate balance between the pledging and control firms, I employ the propensity score matching (PSM) approach (Pang & Wang, 2020). Further, this study conducts a difference-in-differences (DID) analysis to compare the non-GAAP reporting changes around the initiation of insider pledging. A DID analysis can mitigate concerns that unobservable time-invariant variables drive the observed changes in the dependent variable.

1.2.3 Research Design and Method: Study 3

This study investigates whether share pledging can affect workplace safety, where the proxy is the number of injury and illness cases, which is a count variable. Prior studies use Poisson and negative binomial regression as models for the count data (Caskey & Ozel, 2017; Rock et al., 2000). Thus, I estimate the Poisson and negative binomial models. The two measures of share pledging are the pledge indicator variable and the percentage of shares that is pledged by insiders.

The data about workplace injuries is at the establishment level and is available from the Occupational Safety Health Administration (OSHA). Following Caskey and Ozel (2017), I control for establishment and year fixed effects in the Poisson model but only for year fixed effects in the negative binomial model.[1] I incorporate firm characteristics and establishment-level variables that may influence workplace safety, as is documented in the existing literature (Bradley et al., 2022; Caskey & Ozel, 2017; Cohn & Wardlaw, 2016), into the model.

[1] Establishment fixed effects cannot be included in a negative binomial model (Caskey & Ozel, 2017).

1.3 Summary of Key Findings

1.3.1 Summary of Key Findings: Study 1

Study 1 investigates whether the implementation of FAS 161 affects the relationship between corporate derivative use and accruals management. First, I find that derivatives are positively and significantly associated with discretionary accruals pre-FAS 161, which confirms Choi et al.'s (2015) finding that derivatives and discretionary accruals are complements in the pre-FAS 161 period. Next, the regression results from estimating the simultaneous equations suggest that the positive relationship between derivatives and discretionary (current and working capital) accruals is weaker after the implementation of FAS 161. Therefore, this study provides evidence that FAS 161 alters the relative merit of using derivatives versus discretionary accruals for income smoothing. The findings are similar when using alternative measures of discretionary accruals, controlling for cash-flow volatility, excluding speculators and restatement firms, using a sample of continuing firms and taking into consideration macroeconomic shocks in foreign exchange rates and interest rates. Further, this study shows that the impact of FAS 161 on the relationship between derivatives and discretionary accruals is more pronounced for firms that are more affected by the implementation of FAS 161. Specifically, the weakening of the relationship between derivatives and discretionary accruals is more pronounced for firms that have higher disclosure costs and for firms that are in highly competitive industries.

1.3.2 Summary of Key Findings: Study 2

Study 2 examines whether insiders' share pledges affect non-GAAP reporting practices. First, the results of the tests that examine the impact of share pledging on the likelihood and the frequency of non-GAAP reporting show that share pledging is positively related to the propensity of non-GAAP reporting and is associated with more frequent non-GAAP earnings

disclosures. Second, the regression results suggest that share pledging can diminish the quality of non-GAAP reporting in which the quality of the excluded items is the proxy for non-GAAP reporting quality. Specifically, the non-GAAP exclusions are of a lower quality (less transitory) for firms that have insider share pledging. This is consistent with the view that managers that have pledged shares tend to report non-GAAP earnings opportunistically. In addition, I find that share pledging is positively associated with the prominence of non-GAAP earnings disclosures, which suggests that managers that have pledged shares prioritise non-GAAP earnings over GAAP earnings. This finding also provides evidence of managerial opportunism in non-GAAP reporting.

Cross-sectionally, the effect of share pledging on the likelihood and the quality of non-GAAP reporting is more pronounced for firms that have worse corporate governance, which has analyst coverage and board independence as proxies. These findings indicate that good corporate governance can constrain managerial opportunism in non-GAAP reporting. The main tests use the quality of the excluded items as the proxy for the quality of non-GAAP reporting. In the robustness tests, I use two alternative measures of non-GAAP reporting quality that have been widely documented in the existing literature and are considered to capture the aggressiveness of non-GAAP reporting (Black & Christensen, 2009; Christensen et al., 2019, 2021). The findings are consistent with the primary finding that share pledging is negatively associated with the quality of non-GAAP reporting. Last, I find that managers are more likely to report non-GAAP earnings when there is a significant amount of margin-call pressure.

Further, I employ the PSM approach to improve the covariate balance between the pledging and control firms and apply a DID analysis to compare the changes in the non-GAAP earnings disclosures of pledging firms versus control firms around the pledging initiation. I obtain similar results as in the main tests.

1.3.3 Summary of Key Findings: Study 3

Study 3 explores whether insiders' share pledges have an impact on workplace safety. The regression results from estimating the negative binomial and Poisson models suggest that firms that have insider share pledging exhibit higher numbers of workplace injuries and illnesses. Economically, the injury case rates for pledging firms are 15 to 26% higher than those for non-pledging firms. Next, I examine the plausible channels through which pledging may affect workplace safety. The existing literature documents that workplace injuries can be attributed to increased employee workloads and to decreased safety-related investments (Amin et al., 2021; Caskey & Ozel, 2017). The channel test results suggest that reduced safety investments can explain the higher number of workplace injuries in pledging firms but increased employee workloads cannot. One possible explanation is that increasing employee workloads is not efficient for improving earnings and thus supporting share prices for pledging firms.

I provide evidence of the cross-sectional differences in the association between share pledging and workplace safety. Managers are less likely to compromise workplace safety when their abilities are constrained (Caskey & Ozel, 2017). Therefore, factors that limit managers' abilities to sacrifice workplace safety can potentially mitigate the positive impact of share pledging on workplace injuries. The findings show that the impact of share pledging on workplace injuries is weaker for firms that have more analysts following, face higher levels of pressure from labour unions, are located in states that have higher workers' compensation premiums and operate in counties that have higher social capital.

The findings are similar when using an alternative measure of workplace safety, controlling for accrual-based earnings management and conducting firm-level analyses. In addition, the results of the tests that examine the relationship between pledging, meeting earnings benchmarks and workplace safety show that pledging insiders manage earnings to

support share prices and the attempts to manage earnings are related to an increase in workplace injuries.

1.4 Contributions

This thesis has several contributions. The contributions of each study are discussed below.

1.4.1 Contributions: Study 1

First, Study 1 adds to the literature on the relationship between derivative use and accruals management for income smoothing. The existing literature documents that derivatives and discretionary accruals were partial substitutes for income smoothing before FAS 133 (Barton, 2001; Pincus & Rajgopal, 2002). Choi et al. (2015) conclude that derivatives and discretionary accruals have become complements in the post-FAS 133 periods. I provide evidence that the complementary relationship between derivatives and discretionary accruals is weaker after the implementation of FAS 161. Second, Study 1 extends the current research on the consequences of adopting FAS 161. Existing studies document that information asymmetry (Steffen, 2022) and investor mispricing (Campbell et al., 2021) decrease after the adoption of FAS 161. I find that FAS 161 affects managerial decisions about the choice of income-smoothing method. Further, this study echoes Kanodia and Sapra's (2016) call for more research into managerial decisions and corporate behaviour change after mandatory disclosures. This study has practical implications. Study 1 could be of interest to regulators and to the standard setters who issued ASU 2017–12 to simplify hedge accounting requirements. By showing that enhanced financial statement disclosures on derivatives alter the costs of using derivatives for income smoothing, standard setters can better evaluate the benefits and costs associated with mandatory derivative disclosures.

1.4.2 Contributions: Study 2

The contributions of Study 2 are as follows. First, Study 2 contributes to the literature on the factors that influence non-GAAP reporting and the debate over the motivations (informative v. opportunistic) behind non-GAAP reporting. Recent studies find that non-GAAP disclosures are generally informative (Bentley et al., 2018; Black et al., 2021b). However, this study documents that managers opportunistically report non-GAAP earnings when they have pledged shares. Second, Study 2 adds to the growing literature on the consequences of insider share pledging. Prior studies document that share pledging is detrimental to firm value (Dou et al., 2019), deters firms' future innovation outputs and quality (Pang & Wang, 2020) and motivates opportunistic activities such as income smoothing (Huang & Xue, 2016) and share repurchases (Chan et al., 2018). This study provides evidence that share pledging could encourage opportunistic non-GAAP reporting. Third, Study 2 should be of interest to proxy advisors and to regulators. The largest proxy advisory firm, Institutional Shareholder Services, recognises insider share pledging as a corporate governance concern and claims that a negative vote recommendation may be given if insiders' aggregated pledged shares increase over time (Singh, 2018). The SEC is concerned that share-pledging activities 'have the potential to influence management's performance and decisions'. This study provides evidence that share pledging affects management's performance by documenting that managers who have pledged shares tend to opportunistically report non-GAAP earning.

1.4.3 Contributions: Study 3

First, Study 3 extends the literature on workplace safety. Existing studies show that firms that are financially constrained (Cohn & Wardlaw, 2016), under pressure to meet earnings benchmarks (Caskey & Ozel, 2017) and under threat of short selling (Bai et al., 2020) make fewer investments in workplace safety and have higher incidences of workplace accidents. This

study documents that firms that have insider share pledging also experience higher numbers of workplace injuries. Second, similar to Study 2, Study 3 extends the literature on the consequences of share pledging. Existing research generally investigates the economic consequences of share pledging (Dou et al., 2019; Pang & Wang, 2020), this study documents that share pledging is also harmful to employee welfare. Furthermore, this study has practical implications. By documenting that managers with pledged shares have incentives to reduce safety-related investments, which ultimately leads to higher numbers of workplace injuries, Study 3 might provide insights into the need to establish anti-pledging policies.

1.5 Structure of Thesis

The remainder of this thesis is organised as follows. Chapter 2 presents the first study, which examines whether the implementation of mandatory derivative disclosures under Financial Accounting Standard No. 161: Disclosures about Derivative Instruments and Hedging Activities (FAS 161) alters the relative merit of discretionary accruals and financial derivatives for income smoothing. The second study, presented in Chapter 3, examines the impact of share pledging by corporate insiders (i.e. directors and executives) on non-GAAP reporting. The third study in Chapter 4 examines the impact of share pledging on workplace safety. Chapter 5 concludes and discusses the limitations of the thesis and suggestions for future research.

Chapter 2: Study 1: Derivative Hedging and Earnings Management—Evidence from FAS 161

2.1 Introduction

Prior studies suggest that managers use a combination of discretionary accruals and derivative hedging to smooth income. Barton (2001) and Pincus and Rajgopal (2002) show that discretionary accruals and derivative hedging are partial substitutes in smoothing earnings under FAS 52 whereas Choi et al. (2015) show that the derivative hedging–discretionary accrual relationship became complementary after the implementation of Accounting for Derivative Instruments and Hedging Activities (FAS 133).[2] In this study, I examine whether and in what ways the subsequent adoption of Financial Accounting Standard No. 161: Disclosures about Derivative Instruments and Hedging Activities (FAS 161) altered the relationship between derivative hedging and accruals management for non-financial firms, assuming no cross-sectional or time-series variation in the incentives for income smoothing.

FAS 161, effective from 15 November 2008, comprises mandatory disclosure rules that aim to enhance investors' understanding of complex derivative instruments. Despite FAS 161 not altering the accounting treatment of derivatives, some practitioners and policymakers are concerned that it may have affected the cost and the effectiveness of using derivatives to smooth earnings thereby altering the relationship between the accrual and derivative income-smoothing methods (Campbell et al., 2021). Specifically, given that FAS 161 requires more detailed disclosures about derivative hedging, including separately reporting the fair value of derivatives

[2] In 1998, the FASB issued Statement 133, Accounting for Derivative Instruments and Hedging Activities (FAS 133). FAS 133, which took effect on 15 June 2000, required firms to record all derivatives at fair values on the balance sheet and to recognise portions of unrealised gains or losses because of valuation changes in the income statement (e.g. fair value accounting rules, stipulating rules for reporting gain and losses).

by accounting designation (i.e. hedging v. non-hedging) and various measures of risk exposures (e.g. interest rate risk, foreign currency risk), firms risk disclosing proprietary information that benefits their competitors and other counterparties at their own cost. For example, in its comment letter to the FASB, the directors of ForestCity claimed that the collateral information that is revealed under FAS 161 could encourage unsecured counterparties to request collateral, leading to higher negotiation costs. Moreover, given that managers in non-financial firms typically 'close' some of their derivative positions at the end of the reporting period (Bean & Irvine, 2015), the increased disclosure frequency from annually to quarterly under FAS 161 may make it harder for firms to 'hide' derivative assets or liabilities (gains or losses) during the reporting period thus reducing the effectiveness of using derivatives as income-smoothing strategies. Prior studies suggest that managers form disclosure strategies after assessing all the costs and benefits that are associated with each strategy (Ajinkya et al., 2005; Kasznik & Lev, 1995; Lang & Lundholm, 1993). Given that the implementation of FAS 161 increased the costs and reduced the effectiveness of using derivatives as an income-smoothing strategy, I expect managers take these into account and thus reduce their use of derivative hedging for income smoothing, consequently attenuating the complementary relationship that is documented by Choi et al. (2015).

However, my prediction is not without tension because FAS 161 enhances a firm's overall information transparency (Cassell et al., 2015; Fischer & Stocken, 2004) and hence managers' abilities and opportunities to engage in accruals management may also be constrained. This could result in a reduction in use of the two derivatives and accruals management, leaving the complementary relationship between derivatives and discretionary accruals that is documented by Choi et al. (2015) unchanged. As a result, a weakening in such a relationship might not be observed after the implementation of FAS 161.

To test my prediction, I hand collect information on derivatives for a sample of non-financial S&P 500 firms during the 2002 to 2016 period from corporate 10-K filings or annual reports. The sample starts from 2002 to avoid the effect of the implementation of FAS 133 on the association between derivatives and discretionary accruals and ends in 2016 to maintain approximately even pre- and post-FAS 161 sample periods. Given that derivative use and discretionary accruals are endogenously determined, I follow Barton (2001) and Choi et al. (2015) and employ simultaneous equations to test the prediction.

Consistently with Choi et al. (2015), I show that derivative use is positively associated with discretionary accruals in the pre-FAS 161 period, which suggests a complementary relationship between these two income-smoothing methods. However, the positive relationship between financial derivatives and discretionary (*current* and *working capital*) accruals is weaker after the implementation of FAS 161, which supports my contention. These findings suggest that FAS 161 affects managerial decisions in regard to the use of derivatives and discretionary (*current* and *working capital*) accruals for income smoothing. Specifically, managers use more discretionary (*current* and *working capital*) accruals relative to derivatives to smooth earnings after the implementation of FAS 161. However, I find no evidence that the positive relationship between derivatives and discretionary *total* accruals in the pre-FAS 161 period is altered by FAS 161. I argue that the reason for this difference likely reflects the fact that total accruals suffer from greater measurement errors than current and working capital accruals. Therefore, my robustness tests and further analyses use discretionary current and working capital accruals only.

The primary results from using discretionary current and working capital accruals are robust to alternative measures of current and working capital accruals, controls for cash-flow volatility, the exclusion of firms that use derivatives for trading purposes, the exclusion of restatement firms, the use of a sample of continuing firms and changes in derivative use around

FAS 161. I also show that the weakening in the relationship between derivatives and discretionary (*current* and *working capital*) accruals is not driven by macroeconomic shocks in foreign exchange rates and interest rates. However, additional analysis shows that such weakening is more pronounced for firms that bear higher disclosures costs and those in more competitive industries (which are thus more affected by FAS 161).

This study makes two contributions to the literature. First, it adds to the literature on the relationship between derivative use and accruals management for income smoothing. The existing literature documents that derivatives and discretionary accruals are partial substitutes for income smoothing (Barton, 2001; Huang et al., 2009; Pincus & Rajgopal, 2002). However, Choi et al. (2015) find that this relationship attenuated after the implementation of FAS 133 and that derivatives and discretionary accruals have become complements in the post-FAS 133 period. I provide evidence that the complementary relationship between derivatives and discretionary accruals is weaker after the introduction of FAS 161.

Second, this study contributes to the growing literature that investigates the consequences of adopting FAS 161. Existing studies document that firm risk (Manchiraju et al., 2014), information asymmetry (Steffen, 2022) and investor mispricing (Campbell et al., 2021) decreased after the implementation of FAS 161. Further, I find that FAS 161 affects managerial decisions about the choice of income-smoothing method. Kanodia and Sapra (2016) call for more research into managerial decisions and corporate behaviour change after mandatory disclosures. This study provides evidence of the real effects of mandatory derivative disclosures.

This study is of interest to regulators and the standard setters who released ASU 2017–12 to simplify hedge accounting requirements.[3] By showing that enhanced financial statement

[3] The FASB (2017) stated that 'ASU 2017–12 should ease the operational burden of applying hedge accounting'.

disclosures on derivatives alter managers' earnings management choices, standard setters can better evaluate the benefits and the costs that are associated with mandatory derivative disclosures.

The remainder of this study is organised as follows. Section 2.2 introduces the institutional background of FAS 161. Section 2.3 reviews the existing literature and develops the hypothesis. Section 2.4 describes the data and variable construction. Section 2.5 presents the research design. Section 2.6 reports the descriptive statistics and the primary results. Section 2.7 discusses the robustness tests and the additional analyses. Section 2.8 concludes.

2.2 Institutional Background

2.2.1 Hedge Accounting and Disclosure Before FAS 161

FAS Statement 133, Accounting for Derivative Instruments and Hedging Activities, applies to fiscal years that begin after 15 June 2000. Initially, the FASB believed that the enhanced derivative *accounting* under FAS 133 'would obviate the need for additional disclosure requirements' (FASB, 2008; see also Chen et al., 2020). To ease the burden of implementation, FAS 133 superseded several previous derivative *disclosure* standards (e.g. FAS 107 and FAS 119). However, FAS 133 was later criticised for not requiring sufficient information to comprehend the effects of derivative use (FASB, 2008). The derivative disclosure requirements under FAS 133 were insufficient and inconsistent. For example, some firms aggregated the gains or the losses arising from derivatives with other financial statement items (e.g. fuel expenses), which made the analysis of whether derivatives affect firms' financial position and financial performance more difficult (Manchiraju et al., 2014). To address the concern about the lack of disclosures under FAS 133, the FASB issued FAS 161 to impose further *disclosure* requirements on firms without changing the hedge *accounting* standard. Section 2.2.2 explains the details of FAS 161.

2.2.2 Mandatory Derivative Disclosures Under FAS 161

FAS 161, which is effective for periods after 15 November 2008, was issued by the FASB to enhance financial statement users' understanding of how and why firms use derivatives and how these derivative instruments affect firms' financial position, performance and cash flow (FASB, 2008, para. 1).[4] FAS 161 mandates the disaggregation of previously required quantitative information by risk exposure category and accounting designation and the disclosure of new items including balance sheet and income statement line items that would be affected by derivative use (FASB, 2008; Steffen, 2022).

First, FAS 161 requires more detailed disclosures of firms' derivative use. Specifically, firms are required to disclose the fair value of derivative assets and liabilities, segregated by risk exposure category (e.g. interest rate [IR] and foreign currency [FX]) and accounting designation (i.e. hedge accounting v. non-hedge accounting). Under FAS 161, firms also need to disclose fair value gains and losses on derivatives in the income statement, separated by risk exposure category (e.g. IR or FX) and accounting designation (i.e. hedge accounting v. non-hedge accounting) in a tabular format in the footnotes. These segregated gains and losses may assist financial statement users to better understand derivatives that are used for different objectives or types of risk (Manchiraju et al., 2014).

FAS 161 mandates new disclosures on the balance sheet and income statement line items that would be affected by derivative use and other aspects of derivatives (e.g. the volume of derivative activities). Firms are also required to disclose events that could trigger credit-risk-related contingencies and the fair value of assets that are (or would be) posted as collateral in such an event (Steffen, 2022).

[4] Linsmeier, a FASB member, stated that this standard was not a reaction to the global financial crisis because it was conceived earlier. Retrieved from https://www.complianceweek.com/fas-161-debuts-to-clarify-derivatives/5598.article

Second, FAS 161 increases the frequency of derivative disclosures. The FASB argued that reporting 'frequent and often significant changes in derivative fair values' (FASB, 2008, para. A50) would help users to better assess derivative information. Thus, firms that implement FAS 161 should prepare quarterly and annual reports to disclose their derivatives. Finally, FAS 161 requires firms to disclose this quantitative derivative information in a tabular format and to apply cross-referencing if it appears in more than one footnote (FASB, 2008).

Enhanced derivative disclosures under FAS 161 may help investors to better evaluate firms' derivative use; however, critics argue that FAS 161 might also impose high proprietary costs on firms. For example, Betsy Link, the director of ForestCity, stated in a comment letter to FASB that 'providing [contingent features] information would be very detrimental from a business perspective as we feel it would give both our counterparties and competitors proprietary information'.[5]

2.3 Literature and Hypothesis Development

The existing literature suggests that because hedging with derivatives and accruals management are costly and imperfect methods for income smoothing, managers use a combination of derivatives and discretionary accruals to smooth earnings (Barton, 2001; Choi et al., 2015; Petersen & Thiagarajan, 2000; Zhang, 2009). Although early studies suggest that derivatives and accruals are substitutes, Choi et al. (2015) show that derivative hedging and discretionary accruals became complementary after the implementation of FAS 133.

Specifically, Barton (2001) finds that managers use financial derivatives and discretionary accruals as substitutes for income smoothing for their sample period of 1994 to 1996. Pincus and Rajgopal (2002) provide similar evidence that, for a sample of oil and gas

[5] Available at https://www.fasb.org/document/blob?fileName=51219.pdf.

firms, the greater the extent of derivative use, the less managers smooth earnings by using discretionary accruals. However, Choi et al. (2015) argue that accounting standard FAS 133 has made derivatives less effective for smoothing earnings and thus altered the relative use of derivatives and accruals management for income smoothing. They document a positive relationship between derivatives and discretionary accruals in the post-FAS 133 period of 2000 to 2006, which is consistent with a complementary relationship between derivatives and discretionary accruals.

I argue that managers would be expected to use more discretionary accruals relative to derivatives for income smoothing after the implementation of FAS 161 for two reasons. First, FAS 161 increases firms' proprietary cost of disclosure so that firms may engage in less derivative hedging to reduce such costs. Firms are required to provide more detailed information about their derivative hedging under FAS 161. Specifically, firms must separately report the fair value of derivatives by accounting designation (i.e. hedging v. non-hedging) and by various measures of risk exposure (i.e. IR risk, FX risk), any event that could trigger credit-risk-related contingencies and the fair value of assets that are posted as collateral. Such proprietary information could be beneficial to competitors and other counterparties, at a cost to the firm. For example, detailed derivative disclosures that are required by FAS 161 may help competitors to infer firms' production strategies, increasing their production costs (Zou, 2018). The director of ForestCity has also noted her concern that collateral information that is revealed under FAS 161 could incentivise unsecured counterparties to ask for collateral, which results in higher negotiation costs.

Second, the implementation of FAS 161 is likely to reduce the effectiveness of using derivatives for income smoothing. FAS 161 increases the frequency of derivative disclosures from annual to quarterly and requires the tabulation of the fair value of derivatives along with their gains and losses and cross-referencing within footnotes, all of which may reduce

information-processing costs for financial statement users and, in turn, constrain managers' opportunities to time the recognition of derivatives.[6] A recent study suggests that investors do pay attention to information about derivatives in firms' quarterly disclosures (Steffen, 2022). Arguably, investors can better monitor firms' derivative positions on a quarterly basis. Another prior study documents that managers often manage their year-end derivative positions to disguise intra-period activities (Bean & Irvine, 2015). The quarterly disclosures that are required by FAS 161 make it more difficult for managers to conceal these derivative activities. In other words, managers have less discretion in terms of recognising derivative-related assets, liabilities, gains or losses after the implementation of FAS 161 and, as a result, are less likely to use derivatives for income smoothing.

In summary, after the implementation of FAS 161, more proprietary information is assumed to be revealed to competitors and other counterparties and hence managers potentially have less discretion in terms of timing the recognition of derivatives. This leads to an increase in the cost and a reduction in the effectiveness of using derivatives as a tool for smoothing earnings. If managers are assumed to use both derivatives and discretionary accruals as a means of income smoothing, then FAS 161 would lead to less hedging. Taken together, I predict that:

Hypothesis: *The positive relationship between derivatives and discretionary accruals in the pre-FAS 161 period is attenuated after the implementation of FAS 161.*

I might not observe a relative decrease in the relationship between derivatives and discretionary accruals for income smoothing after the implementation of FAS 161 for two reasons. First, enhanced transparency (Cassell et al., 2015) and better-informed investors (Fischer & Stocken, 2004) can constrain managers' abilities and opportunities to engage in

[6] In interviews with analysts from Australia's four major banks, Bean and Irvine (2015) provide evidence that managers in non-financial firms reduce their derivative positions dramatically for year-end reporting.

derivative- and accrual-based earnings management. Second, managers may use other risk-management tools to reduce earnings volatility. For example, an integrated risk-management approach can be used by firms to reduce earnings and stock-price volatility (Liebenberg & Hoyt, 2003).

2.4 Data and Variable Construction

2.4.1 Data and Sample Selection

My initial sample consists of all the non-financial firms that were members of the S&P 500 index for any time between 2002 and 2016 and that are covered by the ExecuComp database. The sample period starts from 2002 to avoid the effect of the changes to hedge accounting because of the implementation of FAS 133 on the relationship between derivatives and discretionary accruals and ends in 2016 to have approximately even pre- and post-FAS 161 sample periods. Given that the accounting treatment of derivatives remains the same for the whole sample period, any change in the relative use of the two income-smoothing methods can be attributed to the implementation of FAS 161 rather than to FAS 133. Financial firms (two-digit SIC codes between 60 and 69) are excluded because the FASB and the SEC require additional disclosures about derivative instruments from financial firms (FASB, 2008).

I hand collect derivative information (e.g. notional amount of derivatives) from corporate 10-K filings or annual reports. To locate information about firms' derivative use, I use the following keywords: 'derivative', 'hedge', 'forward', 'futures', 'option', 'swaps', 'notional' and 'risk' (Huang et al., 2019). I then read the surrounding text to obtain the fiscal year-end notional amount and the fair value of the IR, FX and commodity derivatives. I obtain 6,041 firm-year observations for the sample period (2002–2016).

I use the CRSP/Compustat merged annual data set to obtain the annual accounting data; some 703 firm-year observations are removed because of a lack of required accounting data.

Further, the firms are required to have available segment data from Compustat Segment files, stock returns from CRSP and analyst coverage data from the I/B/E/S database. As a result, the final sample consists of 4,519 firm-year observations that pertain to 531 unique firms. Table 2.1 describes the sample-selection process.

Table 2.1: Description of Sample Selection

	Number of firm-year observations	Number of firms
Initial sample (Non-financial firms listed on S&P 500 for any time during 2002 to 2016 and covered by the ExecuComp database. Firm-years for the period 2002 to 2016 or until exit from index)	6,041	627
Less: Observations without CRSP/Compustat merged data	(703)	(50)
Less: Observations without Compustat Segment data	(14)	(0)
Less: Observations without I/B/E/S information (detail file)	(805)	(46)
Final sample	4,519	531

2.4.2 Measurement of Derivative Use

I use the derivative ratio (*DERIVATIVES*) as a proxy for derivative use. Following Choi et al. (2015), I calculate the derivative ratio as the notional amount of derivatives scaled by the total assets. The notional amount of derivatives is a reasonable measurement of hedging activities because it represents the total value of an underlying asset at its spot price and provides the basis for calculating the amounts that are needed to be exchanged among the parties of the derivatives (Barton, 2001).

2.4.3 Measurement of Discretionary Accruals

Following Choi et al. (2015), I construct three measures for discretionary accruals ($|DTC_t|$): discretionary total accruals, discretionary working capital accruals and discretional current accruals.

2.4.3.1 Total Accruals Model

Consistent with Choi et al. (2015), the discretionary accruals are estimated by using the Jones (1991) model (see Equation (1)). The model is estimated cross-sectionally for each industry-year and each industry has a two-digit SIC industry code. The absolute value of the regression residuals from Equation (1) is my first measure of discretionary accruals: discretionary total accruals (|*DTACC*|).

$$TAC_t / A_{t-1} = \alpha 1[1 / A_{t-1}] + \alpha_2[\Delta\, Sales_t / A_{t-1}] + \alpha_3[PPE_t / A_{t-1}] + \varepsilon_t \qquad (1)$$

where TAC_t represents the total accruals in year t estimated as income before extraordinary items minus cash flow from operating activities minus special items;[7] A_{t-1} measures firms' total assets at the end of year $t-1$; $\Delta Sales_t$ is calculated as the difference in sales revenues between years $t-1$ and t; and PPE_t is gross property, plant and equipment at the end of year t.

2.4.3.2 Working Capital Accruals Model

As is suggested by Teoh et al. (1998), working capital accruals are estimated as:

$$WCA_t = \Delta\, CA_t - \Delta\, CL_t - \Delta\, Cash_t + \Delta\, Debt_t \qquad (2)$$

where WCA_t represents working capital accruals in year t; ΔCA_t measures the change in current assets for firms between years $t-1$ and t; ΔCL_t measures the change in current liabilities; $\Delta Cash_t$ is the change in cash holdings between year $t-1$ and t; and $\Delta Debt_t$ captures the change in short-term debt.

[7] Given that total accruals includes deferred taxes, restructuring charges and special items (Hribar & Collins, 2002), they may suffer from greater measurement error than working capital accruals. To make total accruals more comparable to working capital accruals, I exclude special items from the calculation of total accruals. Notably, the results for discretionary total accruals and derivatives are sensitive to the exclusion of special items.

Then, I replace TAC_t (*total accruals*) in Equation (1) with WCA_t (*working capital accruals*). The absolute value of the residuals from this regression is discretionary working capital accruals (|*DWCACC*|).

2.4.3.3 Current Accruals Model

Following Bergstresser and Philippon (2006), current accruals are calculated as:

$$CAC_t = \triangle CA_t - \triangle CL_t - \triangle Cash_t + \triangle Debt_t - DEP_t \quad (3)$$

where CAC_t is current accruals in year *t*; DEP_t is the depreciation and amortisation expenses in year *t*; and all other variables are as defined above.

I replace TAC_t (*total accruals*) in Equation (1) with CAC_t (*current accruals*). The absolute value of the residuals from this regression is discretionary current accruals (|*DCACC*|).

2.5 Research Methodology

As the magnitude of derivative use is endogenously determined at the same time as discretionary accruals, following Barton (2001) and Choi et al. (2015), I employ the following simultaneous equations to test my hypothesis that the positive relationship between derivatives and discretionary accruals is attenuated after the implementation of FAS 161.

$$\begin{aligned} DERIVATIVES_{it} = {} & \alpha + \beta_1 |DTC_{it}| + \beta_2 |DTC_{it}| \times FAS161_{it} + \beta_3 FAS161_{it} + \beta_4 Cash\_Camp_{it} \\ & + \beta_5 CEO\_Option_{it} + \beta_6 CEO\_Stock_{it} + \beta_7 Tax\ Convexity_{it} \\ & + \beta_8 Leverage_{it} + \beta_9 Distress_{it} + \beta_{10} R\&D_{it} + \beta_{11} R\&D \times Leverage_{it} \\ & + \beta_{12} Analyst_{it} + \beta_{13} Segment_{it} + \beta_{14} Foreign\ Sale_{it} + \beta_{15} Firm\ Size_{it} \\ & + \beta_{16} Div\_Yield_{it} + \beta_{17} Short\_Debt_{it} + \beta_{18} Cash\_Cycle_{it} + \beta_{19} IMR_{it} \\ & + \sum_{y=1}^{48} \beta_x Industry + \varepsilon_{it} \end{aligned} \quad (4)$$

$$
\begin{aligned}
|DTC_{it}| \quad = \quad & \alpha + \lambda_1 DERIVATIVES_{it} + \lambda_2 DERIVATIVES_{it} \times FAS161_{it} + \lambda_3 FAS161_{it} \\
& + \lambda_4 Cash\_Camp_{it} + \lambda_5 CEO\_Option_{it} + \lambda_6 CEO\_Stock_{it} \\
& + \lambda_7 Tax\ Convexity_{it} + \lambda_8 Leverage_{it} + \lambda_9 Distress_{it} + \lambda_{10} R\&D_{it} \\
& + \lambda_{11} R\&D \times Leverage_{it} + \lambda_{12} Analyst_{it} + \lambda_{13} Segment_{it} \\
& + \lambda_{14} Foreign\ Sale_{it} + \lambda_{15} |DTC|_{it-1} + \lambda_{16} LIFO\_Reserve_{it} \\
& + \lambda_{17} Div\_Pay_{it} + \lambda_{18} Flexibility_{it} + \lambda_{19} |CFO|_{it} + \lambda_{20} IMR_{it} \\
& + \sum_{y=1}^{48} \lambda_x Industry + \varepsilon_{it} \qquad (5)
\end{aligned}
$$

where $DERIVATIVES_{it}$ represents the derivative ratio for firm *i* in year *t* and $|DTC_{it}|$ is one of my three measures of discretionary accruals for firm *i* in year *t*. To investigate the ways that the association between derivatives and accruals management changed after FAS 161, I include the FAS 161 indicator variables and an interaction term $|DTC_{it}| \times FAS\ 161_{it}$ in Equation (4). FAS 161 is an indicator variable that takes a value of 1 if the fiscal year is between 2009 and 2016 and 0 for fiscal years between 2002 and 2008.[8] I include the FAS 161 dummy and the interaction term $DERIVATIVES_{it} \times FAS\ 161_{it}$ in Equation (5) to examine the potential changes in the relationship between financial derivatives and accruals management after the implementation of FAS 161.

I predict that the positive relationship between derivatives and discretionary accruals is likely to be weaker after the implementation of FAS 161; thus, the primary variables of interest are the regression coefficients of the two interaction terms $DERIVATIVES \times FAS\ 161(\beta_2)$ and $|DTC| \times FAS\ 161(\lambda_2)$, which I predict to be negative. The coefficients of $|DTC|$ (β_1) and

[8] FAS 161 became effective on 15 November 2008 but firms typically adopted it starting from fiscal year 2009. Firms could choose to adopt it in fiscal year 2008 because FASB encouraged early adoption. To address this potential mis-specification concern, I manually check the adoption date of FAS 161 for each firm in the sample from the 10-K reports. For the early-adoption firms, the variable FAS 161 is coded 1 if the firm-year is 2008. Only two firms in our sample adopted FAS 161 in fiscal year 2008.

DERIVATIVES (λ_1) capture the relationship between derivatives and discretionary accruals in the pre-FAS 161 period. As is suggested by Choi et al. (2015), I predict β_1 and λ_1 to be positive. All the control variables use Barton (2001) and Choi et al. (2015) and are defined in Appendix A. I control for industry fixed effects using two-digit SIC industry codes in the aforementioned equations. Finally, I cluster the standard errors by firm and by year to adjust for heteroskedasticity (Petersen, 2009).

Approximately 72.7% of my sample firms before FAS 161 and 81.4% after FAS 161 disclose derivative use. This gives rise to a potential self-selection bias that might weaken my research design because the reasons behind using derivatives can affect the magnitude of derivative use. To correct for this self-selection bias, I follow prior studies (Barton, 2001; Choi et al., 2015) and estimate the following equation to explain a firm's decision to become a derivative user:

$$\begin{aligned} User_i = {} & \alpha + \gamma_1 FAS161_{it} + \gamma_2 Cash\_Camp_{it} + \gamma_3 CEO\_Option_{it} + \gamma_4 CEO\_Stock_{it} \\ & + \gamma_5 Tax\ Convexity_{it} + \gamma_6 Leverage_{it} + \gamma_7 Distress_{it} + \gamma_8 R\&D_{it} \\ & + \gamma_9 R\&D \times Leverage_{it} + \gamma_{10} Analyst_{it} + \gamma_{11} Segment_{it} + \gamma_{12} Foreign\ Sale_{it} \\ & + \gamma_{13} Firm\ Size_{it} + \gamma_{14} Div\_Yield_{it} + \gamma_{15} Short\_Debt_{it} + \gamma_{16} Cash\_Cycle_{it} \\ & + \sum_{y=1}^{48} \gamma_x Industry + \varepsilon_{it} \end{aligned} \quad (6)$$

where $User_i$ is an indicator variable that takes a value of 1 if the firm uses derivatives and 0 otherwise. The other variables are as defined in Appendix A. I use a probit regression for the full sample of 4,519 firm-year observations to estimate Equation (6). Then, I obtain the fitted value of the probit regression index function and calculate the inverse Mills ratios (IMR). I include the IMR as an additional control variable in the simultaneous Equations (4) and (5) in which I only use derivative users (3,082 firm-year observations) to perform the analyses.

2.6 Results

2.6.1 Descriptive Statistics

Panel A of Table 2.2 presents the descriptive statistics for the variables that are used in my main analyses and the tests of the difference between the pre-FAS 161 and post-FAS 161 periods. Firms have a mean value of US$17.048 billion in assets in the pre-FAS 161 period, which is significantly lower than the US$24.308 billion in average assets in the post-FAS 161 period. The absolute value of discretionary total accruals (|*DTACC*|) increases significantly, from 0.169 in the pre-FAS 161 period to 0.229 in the post-FAS 161 period. The mean absolute value of discretionary current accruals (|*DCACC*|) and discretionary working capital accruals (|*DWCACC*|) are 0.067 and 0.063, respectively, in the pre-FAS 161 period. These ratios increase to 0.098 and 0.095, respectively, after the implementation of FAS 161. Thus, there is evidence that firms use a higher level of accruals post-FAS 161.

Table 2.2: **Descriptive Statistics of Sample in Pre- and Post-FAS 161 Periods**

Panel A: Summary Statistics of All Sample Firms

	Pre-FAS 161 (n = 1,965)			Post-FAS 161 (n = 2,554)			Difference in mean	Difference in median
Variable	Mean	p50	SD	Mean	p50	SD	(*p*-value)	(*p*-value)
|DTACC|	0.169	0.088	0.330	0.229	0.093	0.857	0.003	0.151
|DCACC|	0.067	0.032	0.162	0.098	0.035	0.314	0.000	0.016
|DWCACC|	0.063	0.029	0.163	0.095	0.030	0.316	0.000	0.042
Derivative notional amount ($ million)	837.93	164.90	2,478.2	2,614.9	750.0	6,978.0	0.000	0.000
Firm Size ($ billion)	17.048	8.479	26.408	24.308	11.736	34.498	0.000	0.000
Foreign Sale	0.333	0.304	0.298	0.365	0.345	0.323	0.001	0.015
Segment	6.409	6.000	3.163	6.513	6.000	3.677	0.318	0.567
R&D	0.029	0.005	0.052	0.025	0.001	0.051	0.004	0.000
Cap Intensity	0.051	0.042	0.039	0.049	0.036	0.045	0.144	0.000
Div_Pay	0.185	0.173	1.419	0.338	0.263	1.547	0.001	0.000
Div_Yield	0.016	0.012	0.018	0.020	0.017	0.024	0.000	0.000
Leverage	0.249	0.237	0.152	0.289	0.268	0.163	0.000	0.000
ROA	0.053	0.059	0.100	0.060	0.060	0.080	0.008	0.405
BTM	0.417	0.351	0.405	0.381	0.340	0.384	0.003	0.007
Earning Volatility	0.044	0.024	0.080	0.035	0.022	0.050	0.000	0.000
Tax Convexity	0.156	0.110	0.518	0.143	0.117	0.449	0.374	0.256
Distress	0.200	0.119	0.201	0.194	0.123	0.185	0.295	0.737
LIFO_Reserve	0.007	0.000	0.023	0.005	0.000	0.017	0.000	0.000
Cash Cycle	68.700	51.450	77.970	69.900	51.190	83.910	0.626	0.381
Short Debt	0.170	0.095	0.218	0.116	0.064	0.165	0.000	0.000
|CFO|	0.117	0.110	0.064	0.117	0.108	0.058	0.911	0.832
CEO_Incen	0.700	0.766	0.222	0.838	0.872	0.129	0.000	0.000
Cash_Comp	0.300	0.234	0.222	0.162	0.128	0.129	0.000	0.000
CEO_Option	0.329	0.308	0.270	0.180	0.139	0.206	0.000	0.000
CEO_Stock	0.170	0.000	0.229	0.400	0.417	0.253	0.000	0.000
Flexibility	0.229	0.148	0.303	0.316	0.157	0.501	0.000	0.561
Analyst	17.360	14.000	14.760	22.180	18.000	18.080	0.000	0.000
User	0.727	1	0.445	0.814	1	0.389	0.000	0.000

Panel B: Summary Statistics of Derivative Ratio for Derivative Users Only

	Pre-FAS 161			Post-FAS 161				
Variable	Mean	Median	*n*	Mean	Median	*n*	Diff-in-mean	Diff-in-median
DERIVATIVES (notional ratio)	0.091	0.043	1,134 (79%)	0.124	0.079	1,876 (90%)	0.033 (0.000)	0.036 (0.000)
DERIVATIVES (fair value ratio)	0.007	0.002	1,210 (85%)	0.011	0.003	1,988 (96%)	0.004 (0.000)	0.001 (0.000)
Users (total)			1,429			2,079		
Derivative user reports neither notional nor fair value			272 (19%)			101 (5%)		

Panel C: Summary Statistics of Non-Hedging Derivative Gains and Amount of Cash Collateral (for Derivative Users Only)

	Pre-FAS 161			Post-FAS 161				
Variable	Mean	Median	*n*	Mean	Median	*n*	Diff-in-mean	Diff-in-median
Gain (loss) on non-hedging derivative (US$ m)	6.408	1.000 31.500	111 (7%)	7.847	–1.000 37.000	1,108 (53%)	1.439 (0.000)	–2.000 (0.000)
Cash collateral posted (US$ m)	90.226		54 (4%)	99.723		310 (15%)	9.497 (0.000)	5.500 (0.000)

Note. This table presents descriptive statistics. Panel A reports the summary statistics for the entire sample of firms during the pre-FAS 161 (2002–2008) and post-FAS 161 (2009–2016) periods. Panel B presents the summary statistics of the derivative ratios for the derivative users during the two subsample periods. The derivative ratio is computed as the notional amount or the fair value of the derivatives divided by the total assets. Panel C reports the gains and losses from non-hedging derivatives and the amount of cash collateral that is posted by derivative users during the two subsample periods. All the continuous variables are winsorised at 1% and 99%.

Approximately 72.7% of my sample firms used financial derivatives during the pre-FAS 161 period. The proportion of derivative users increases to 81.4% after the implementation of FAS 161. On average, the total notional amount of derivatives increases from US$837.93 million before FAS 161 to US$2,612.9 million after FAS 161. The median notional amount increases from US$164.9 million before FAS 161 to US$750 million after FAS 161.

Panel B of Table 2.2 presents the summary statistics of the derivative ratios for the derivative users only. There is an increase in notional amount disclosure in the post-FAS 161 period (from 79% to 90%). The percentage of firms that disclose the fair value of derivatives is 85% before FAS 161 and this increases to 96% after the implementation. The mean notional derivative ratios (the monetary notional amount of derivatives scaled by the total assets) in Panel B are 9.1% of the total assets before FAS 161 and 12.4% of the total assets after FAS 161. The average fair derivative ratios (the absolute fair value of the derivatives divided by the total assets) increase significantly from 0.7% in the pre-FAS 161 period to 1.1% in the post-FAS 161 period. The average fair derivative ratios are much smaller than the average notional derivative ratios in the two periods. Overall, there is a significant increase in the use of derivatives between the pre- and post-FAS 161 periods.

FAS 161 mandates that derivatives be presented separately for hedging derivatives and for non-hedging derivatives. Hedging (non-hedging) derivatives are derivatives that (do not) receive hedge accounting treatment under FAS 133. My untabulated results show that approximately 67% of derivative users disclose the notional amount of hedging and non-hedging derivatives in the post-FAS 161 period. Further, I find that about 7% of derivative users voluntarily reported their notional amount of derivatives segregated by accounting designation before the FAS 161 implementation. Panel C of Table 2.2 reports the gains and the losses from the non-hedging derivatives and the amount of cash collateral that was posted by derivative users during the sample period.[9]

[9] Before FAS 161, firms typically did not disclose gains or losses on non-hedging derivatives. Panel C of Table 2.2 suggests that only 7% of derivative users reported non-hedging derivative gains and losses in the pre-FAS 161 period, for an average gain of US$6.408 million. After the adoption of FAS 161, 53% of derivative users reported this figure to help financial statement users to better understand the impact of non-hedging derivative gains and losses on the income statement. Similarly, the percentage of derivative users that reported credit-risk-related contingent features increased from 4% to 15% after the FAS 161 implementation. On average, firms were required to post US$90.226 million cash as collateral before FAS 161, as opposed to US$99.723 million after FAS 161.

2.6.2 Main Results

This study examines whether the implementation of FAS 161 affects the relationship between corporate derivative use and accruals management. I predicted that the positive relationship between derivatives and discretionary accruals would be attenuated after the implementation of FAS 161. Column 1 of Table 2.3 presents the results for the *User* self-selection model (Equation (6)). I find that firms that are highly leveraged, more financially distressed, larger and have a longer cash cycle are more likely to use derivatives. I do not find evidence that R&D-intensive firms and firms that have greater foreign sales ratios tend to use derivatives.

Table 2.3: Simultaneous Equation Estimation of Notional Derivative Ratio and Absolute Value of Accruals Model for Period 2002 to 2016

Panel A: Discretionary Total Accruals (|DTACC|)

$User_i = \gamma_0 + \gamma_1 FAS161 + \gamma_2 CONTROLS_{it} + \varepsilon_t$ (1)

$DERIVATIVES_{it} = \beta_0 + \beta_1 \times |DTC_{it}| + \beta_2 \times (|DTC_{it}| \times FAS161) + \beta_3 \times FAS161 + \sum \beta \times CONTROLS_{it} + \varepsilon_t$ (2)

$|DTC_{it}| = \lambda_0 + \lambda_1 \times DERIVATIVES_{it} + \lambda_2 \times (DERIVATIVES_{it} \times FAS161) + \lambda_3 \times FAS161 + \sum \lambda \times CONTROLS_{it} + \varepsilon_t$ (3)

	Exp. sign	(1) User	(2) DERIVATIVES	(3) |DTACC|
|DTACC| (β_1)	+		0.002	
			(0.13)	
|DTACC| × FAS 161 (β_2)	–		**–0.082***	
			(–1.84)	
DERIVATIVES (notional) (λ_1)	+			**1.494***
				(1.65)
DERIVATIVES × FAS 161 (λ_2)	–			**–0.507**
				(–0.60)
FAS 161	+/–	0.201***	0.042***	0.076
		(3.77)	(2.99)	(1.03)
Cash_Comp	+	–0.147	–0.044	0.097
		(–0.95)	(–0.87)	(0.91)
CEO_Option	+/–	–0.260**	–0.074	0.199

	Exp. sign	(1) User	(2) DERIVATIVES	(3) |DTACC|
		(–2.00)	(–1.17)	(1.34)
CEO_Stock	+	–0.004	–0.047	0.010
		(–0.03)	(–0.90)	(0.15)
Tax Convexity	+	–0.026	0.007	0.067
		(–0.58)	(0.87)	(1.46)
Leverage	+	0.631***	0.018	–0.203*
		(2.96)	(0.37)	(–1.72)
Distress	+	–0.375**	–0.012	0.040
		(–2.39)	(–0.22)	(0.60)
R&D	+	–1.357*	–0.227	0.191
		(–1.78)	(–1.03)	(0.29)
R&D × Leverage	+	0.121	0.0619	0.416
		(0.08)	(0.19)	(0.55)
Analyst	+	–0.000	0.000	0.000
		(–0.25)	(0.72)	(0.38)
Segment	–	0.030***	–0.001	0.001
		(2.70)	(–0.30)	(0.30)
Foreign Sale	+	0.148	0.037	–0.041
		(1.31)	(1.22)	(–0.61)
Firm Size	+	0.173***	–0.018	
		(6.06)	(–0.98)	
Div_Yield	+	–1.641	0.172	
		(–1.44)	(0.49)	
Short_Debt	+	–0.022	0.006	
		(–0.18)	(0.18)	
Cash_Cycle	+	0.002***	0.000	
		(5.63)	(1.26)	
$DTACC_{t-1}$	+			–0.011
				(–0.19)
LIFO_Reserve	+			3.949**
				(2.02)
Div_Pay	–			–0.001
				(–0.39)
Flexibility	+			0.261*
				(1.89)
|CFO|	+			0.031
				(0.13)
IMR	+/–		–0.046	–0.291
			(–0.32)	(–1.61)
Intercept	+/–	–2.626***	0.272	–0.034
		(–5.17)	(1.02)	(–0.20)
N		4,519	3,082	3,082
Pseudo/Adj. R^2		0.170	0.017	0.034
Industry Fixed Effects		Yes	Yes	Yes
$\beta1 + \beta2$ or $\lambda1 + \lambda2$			–0.080	0.987
p-value for *F*-test			0.165	0.162

Panel B: Discretionary Current Accruals (|DCACC|) and Discretionary Working Capital Accruals (|DWCACC|)

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
|DTC| (β1)	+	0.075		0.089	
		(1.21)		(1.38)	
|DTC| × FAS 161 (β2)	–	**–0.155****		**–0.160****	
		(–2.20)		**(–2.29)**	
DERIVATIVES (notional) (λ1)	+		**0.487***		**0.439***
			(1.89)		**(1.78)**
DERIVATIVES × FAS 161 (λ2)	–		**–0.741*****		**–0.736*****
			(–2.62)		**(–2.64)**
FAS 161	+/–	0.037**	0.100***	0.036**	0.099***
		(2.45)	(3.01)	(2.39)	(2.95)
Cash_Comp	+	–0.044	0.048	–0.044	0.048
		(–0.87)	(1.64)	(–0.88)	(1.56)
CEO_Option	+/–	–0.082	0.001	–0.082	–0.001
		(–1.23)	(0.02)	(–1.22)	(–0.02)
CEO_Stock	+	–0.044	0.010	–0.044	0.010
		(–0.88)	(0.56)	(–0.87)	(0.63)
Tax Convexity	+	0.002	–0.009**	0.002	–0.009**
		(0.44)	(–2.46)	(0.44)	(–2.11)
Leverage	+	0.032	–0.040	0.033	–0.036
		(0.73)	(–1.11)	(0.76)	(–0.99)
Distress	+	–0.014	–0.058**	–0.013	–0.062**
		(–0.26)	(–1.99)	(–0.25)	(–1.98)

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
R&D	+	–0.181	0.049	–0.178	0.054
		(–0.87)	(0.50)	(–0.85)	(0.59)
R&D × Leverage	+	–0.024	–0.228	–0.027	–0.238
		(–0.09)	(–1.14)	(–0.09)	(–1.28)
Analyst	+	0.000	0.001*	0.000	0.001*
		(0.68)	(1.81)	(0.67)	(1.82)
Segment	–	–0.001	0.001**	–0.001	0.001*
		(–0.31)	(2.12)	(–0.31)	(1.95)
Foreign Sale	+	0.038	–0.008	0.038	–0.009
		(1.27)	(–0.44)	(1.27)	(–0.42)
Firm Size	+	–0.016		–0.016	
		(–0.94)		(–0.95)	
Div_Yield	+	0.134		0.129	
		(0.36)		(0.35)	
Short_Debt	+	0.004		0.005	
		(0.17)		(0.18)	
Cash_Cycle	+	0.000		0.000	
		(1.11)		(1.10)	
$\lvert DTC_{t-1} \rvert$	+		–0.006		–0.011
			(–0.11)		(–0.19)
LIFO_Reserve	+		0.275		0.208
			(0.72)		(0.57)
Div_Pay	–		0.004		0.004
			(1.04)		(1.05)
Flexibility	+		0.224***		0.240***
			(4.51)		(5.36)
|CFO|	+		–0.143*		–0.128
			(–1.75)		(–1.40)
IMR	+/–	–0.020	0.012	–0.020	0.008

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
		(–0.16)	(0.29)	(–0.15)	(0.20)
Intercept	+/–	0.245	–0.076**	0.244	–0.078***
		(0.96)	(–2.27)	(0.96)	(–2.64)
N		3,082	3,082	3,082	3,082
Adj. R^2		0.108	0.096	0.111	0.113
$\beta 1 + \beta 2$ or $\lambda 1 + \lambda 2$		–0.080	–0.254	–0.071	–0.297
p-value for *F*-test		0.103	0.209	0.105	0.193

Note. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level, respectively, by using two-tailed tests. This table presents the results of the simultaneous equations regressions by using the two-stage least-squares (2SLS) approach. The results for the absolute value of the discretionary total accruals (|*DTACC*|) are reported in Panel A and the results for the absolute value of the discretionary current accruals (|*DCACC*|) and the discretionary working capital accruals (|*DWCACC*|) are reported in Panel B. Column 1 of Panel A reports the results of the probit model that explains the choice of being a derivative user and the inverse Mills ratio that is computed from the probit model is included in the system of simultaneous equations to correct for potential self-selection bias. The simultaneous equations results are reported in Columns 2 and 3 in Panel A and Columns 1 to 4 in Panel B. *DERIVATIVES* is the ratio of notional amount of derivatives to total assets. The FAS 161 takes a value of 1 if the sample year is between 2009 and 2016 and 0 otherwise. The two interaction terms (|*DTC*| × *FAS 161* and *DERIVATIVES* × *FAS 161*) capture the effect of FAS 161 on the relationship between derivative use and accruals management. The continuous variables are winsorised at 1% and 99%. The *t*-statistics that are reported in parentheses use the robust standard errors that are clustered by firm and year. The sample period runs from 2002 to 2016. The variables are defined in Appendix A.

I generate the IMR from Equation (6) and include this as an additional control in Equations (4) and (5). I estimate Equations (4) and (5) for derivative users only and present the simultaneous regression results in Table 2.3. I control for industry fixed effects according to the two-digit SIC industry codes. Robust standard errors are clustered by firm and by year.

Panel A of Table 2.3 reports the results for the relationship between derivatives and the absolute value of discretionary total accruals (|*DTACC*|). Column 2 of Table 2.3 uses *DERIVATIVES* as the dependent variable. I find that the regression coefficient of |*DTACC*| is positive but not significantly different from zero. However, when the absolute value of discretionary total accruals (|*DTACC*|) is used as the dependent variable, I find that the coefficient estimate of *DERIVATIVES* ($\lambda 1$) is positive and significant at the 10% level (Column 3), which confirms Choi et al.'s (2015) finding that derivatives and discretionary total accruals are complements in the pre-FAS 161 period.[10] The regression coefficient of the interaction term |*DTACC*| × *FAS 161* ($\beta 2$) is negative and statistically significant at the 10% level (Column 2). However, the regression coefficient of the interaction term *DERIVATIVES* × *FAS 161* ($\lambda 2$) is negative and statistically insignificant. Given that $\beta 2$ and $\lambda 2$ capture the change in the relationship between the two income-smoothing methods, I cannot conclude that the relationship between derivatives and discretionary total accruals is weaker after the implementation of FAS 161.

Panel B presents the results for the association between derivatives and discretionary *current* accruals (|*DCACC*|) or discretionary *working capital* accruals (|*DWCACC*|). Columns 2 and 4 show that the regression coefficients of *DERIVATIVES* ($\lambda 1$) are positive and statistically significant at the 10% level, regardless of whether |*DCACC*| or |*DWCACC*| is used as the

[10] Choi et al. (2015) find a complementary relationship between financial derivatives and discretionary total accruals in the post-FAS 133 period. They define the years 2002, 2004 and 2006 as their post-FAS 133 period, which overlaps with the pre-FAS 161 period (2002–2008) in our study. Choi et al. (2015) provide evidence that the coefficient of the derivative ratio in the post-FAS 133 period is positive and statistically significant at the 10% level.

dependent variable. These results confirm the finding of a complementary relationship between derivatives and discretionary accruals in Choi et al. (2015) for the FAS 133 period that corresponds to my pre-FAS 161 period. In addition, the FAS 161 indicator variable is positive and statistically significant in all the columns of Panel B of Table 2.3, which indicates that the use of derivatives and discretionary (current and working capital) accruals increases after the implementation of FAS 161.

Notwithstanding the positive association between derivatives and discretionary accruals in the pre-FAS 161 period, my key interest is whether this positive relationship is attenuated after the implementation of FAS 161. Columns 2 and 4 indicate that the regression coefficients of the interaction term *DERIVATIVES* × *FAS 161* (*λ2*) are negative and statistically significant at the 5% level, which supports my contention. A joint *F*-test suggests that the sum of λ1 and λ2 is statistically insignificant, which implies that derivatives and discretionary accruals are not necessarily complements for income smoothing in the post-FAS 161 period.

In summary, similar to Choi et al. (2015), I find a significant and positive relationship between financial derivatives and discretionary accruals during the pre-FAS 161 period. The positive relationship between derivatives and discretionary (*current* and *working capital*) accruals is weaker after the implementation of FAS 161; however, there is no evidence that the implementation of FAS 161 attenuates the relationship between derivatives and discretionary total accruals. One possible explanation is that discretionary total accruals suffer from more severe measurement errors than discretionary current and working capital accruals.

2.7 Further Analyses

2.7.1 Robustness Tests

2.7.1.1 Alternative Measurement of Discretionary Accruals

The estimation of |*DTC*| (discretionary accruals) may suffer from measurement errors. To investigate whether the primary results are robust to alternative measurement, I estimate the discretionary accruals by using the modified Jones model (Kothari et al., 2005). The untabulated results suggest that financial derivatives and discretionary accruals are complements before FAS 161. The implementation of FAS 161 attenuates the positive relationship between derivatives and discretionary (current and working capital) accruals. These results are similar to the main results.

2.7.1.2 Controlling for Cash Flow Volatility

Given that derivative use and earnings management can reduce cash flow volatility (Barton, 2001; Choi et al., 2015), I incorporate cash flow volatility (*CF_Volatility*) into Equations (4) and (5). Following Jayaraman (2008), the volatility of cash flows is calculated as the variance in a firm's operating cash flows during the past five years scaled by the total assets. Columns 2 and 4 of Table 2.4 show that the coefficient estimates of *DERIVATIVES* ($\lambda 1$) are positive and significant, which suggests that derivatives and discretionary (current and working capital) accruals are complements during the pre-FAS 161 period. The coefficients of the interaction term *DERIVATIVES* × *FAS 161* ($\lambda 2$) are negative and statistically significant, which suggests that derivatives are less positively associated with discretionary (current and working capital) accruals after the implementation of FAS 161. Thus, my inference about the significant and negative association between derivatives and discretionary accruals remains the same.

Table 2.4: Robustness Test: Controlling for Cash Flow Volatility

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
|DTC| (β1)	+	0.071		0.073	
		(1.16)		(1.22)	
|DTC| × FAS 161 (β2)	**–**	**–0.148****		**–0.141****	
		(–2.18)		**(–2.37)**	
DERIVATIVES (notional) (λ1)	**+**		**0.487***		**0.442***
			(1.85)		**(1.77)**
DERIVATIVES × FAS 161 (λ2)	–		**–0.739****		**–0.736****
			(–2.55)		**(–2.56)**
FAS 161	+/–	0.034**	0.099***	0.033**	0.099***
		(2.18)	(2.96)	(2.07)	(2.91)
Cash_Comp	+	–0.038	0.048	–0.038	0.048
		(–0.79)	(1.64)	(–0.80)	(1.56)
CEO_Option	+/–	–0.078	0.001	–0.078	–0.001
		(–1.20)	(0.02)	(–1.20)	(–0.01)
CEO_Stock	+	–0.046	0.010	–0.046	0.010
		(–0.91)	(0.57)	(–0.90)	(0.65)
Tax Convexity	+	0.003	–0.009**	0.003	–0.009**
		(0.60)	(–2.47)	(0.60)	(–2.12)
Leverage	+	0.033	–0.040	0.033	–0.036
		(0.76)	(–1.11)	(0.77)	(–0.99)
Distress	+	–0.009	–0.058**	–0.008	–0.061**
		(–0.17)	(–2.01)	(–0.16)	(–2.00)

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
R&D	+	–0.114	0.048	–0.111	0.054
		(–0.51)	(0.51)	(–0.49)	(0.61)
R&D ×	+	0.109	–0.232	0.107	–0.239
Leverage		(0.38)	(–1.16)	(0.37)	(–1.29)
Analyst	+	0.000	0.001*	0.000	0.001*
		(0.93)	(1.78)	(0.92)	(1.79)
Segment	–	–0.000	0.001**	–0.000	0.001**
		(–0.26)	(2.17)	(–0.26)	(2.02)
Foreign Sale	+	0.035	–0.008	0.035	–0.009
		(1.24)	(–0.43)	(1.24)	(–0.42)
Firm Size	+	–0.021		–0.021	
		(–1.10)		(–1.10)	
Div_Yield	+	0.138		0.132	
		(0.38)		(0.36)	
Short_Debt	+	0.005		0.005	
		(0.21)		(0.22)	
Cash_Cycle	+	0.000		0.000	
		(1.11)		(1.11)	
$\lvert DTC_{t-1} \rvert$	+		–0.006		–0.011
			(–0.11)		(–0.19)
LIFO_Reserve	+		0.274		0.210
			(0.72)		(0.57)
Div_Pay	–		0.004		0.004
			(1.04)		(1.05)
Flexibility	+		0.224***		0.240***
			(4.53)		(5.39)
|CFO|	+		–0.144*		–0.128
			(–1.79)		(–1.43)
CF_Volatility	+	–0.640**	0.017	–0.640**	0.004

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
		(–2.01)	(0.21)	(–2.01)	(0.05)
IMR	+/–	–0.032	0.012	–0.032	0.008
		(–0.25)	(0.28)	(–0.25)	(0.20)
Intercept	+/–	0.314	–0.076**	0.314	–0.079***
		(1.12)	(–2.34)	(1.12)	(–2.72)
N		3,082	3,082	3,082	3,082
Adj. R^2		0.115	0.096	0.118	0.112
Industry Fixed Effects		Yes	Yes	Yes	Yes
$\beta 1 + \beta 2$ or $\lambda 1 + \lambda 2$		–0.077	–0.252	–0.068	–0.294
p-value for *F*-test		0.104	0.189	0.106	0.175

Note. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level, respectively, by using two-tailed tests. This table presents the robustness test for controlling for cash-flow volatility. The results for the absolute value of the discretionary current accruals (|*DCACC*|) are reported in Columns 1 and 2 and the results for the absolute value of the discretionary working capital accruals (|*DWCACC*|) are reported in Columns 3 and 4. *DERIVATIVES* is a proxy for the extent of derivative use. FAS 161 equals 1 if the fiscal year is between 2009 and 2016 and 0 otherwise. The continuous variables are winsorised at 1% and 99%. The *t*-statistics that are reported in parentheses use robust standard errors that are clustered by firm and year. The sample period runs from 2002 to 2016. The variables are defined in Appendix A.

2.7.1.3 Firms' Exposure to Foreign Exchange Risks, Interest Rate Risks and the 2007 Financial Crisis

Macroeconomic shocks in FX (e.g. currency crises in Russia and Brazil) and IRs may affect firms' use of derivatives, depending on their exposure to these risks. To exclude the alternative explanation that firms' exposure to FX and IR risks alters the relationship between derivative use and discretionary accruals, I incorporate two additional control variables into Equations (4) and (5). The first variable is *Term Spread*, which is calculated as the difference between the US 10-year Treasury constant maturity rate and the US two-year Treasury constant maturity rate. The second variable is *Dollar Strength* for which I use the broad US dollar index value as a proxy.[11] The results are reported in Table 2.5.

[11] Obtained from the Board of Governors of the Federal Reserve System.

Table 2.5

Robustness Test: Macroeconomic Shocks in Foreign Exchange and Interest Rates

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
|DTC| (β1)	+	0.111*		0.122**	
		(1.95)		(1.97)	
|DTC| × FAS 161 (β2)	**–**	**–0.188****		**–0.191****	
		(–2.25)		**(–2.31)**	
DERIVATIVES (notional) (λ1)	**+**		**0.497***		**0.446***
			(1.92)		**(1.80)**
DERIVATIVES × FAS 161 (λ2)	–		**–0.779*****		**–0.773*****
			(–2.71)		**(–2.73)**
FAS 161	+/–	0.039***	0.091***	0.038***	0.090***
		(2.86)	(2.64)	(2.74)	(2.58)
Cash_Comp	+	–0.045	0.033	–0.046	0.032
		(–0.87)	(1.06)	(–0.88)	(1.01)
CEO_Option	+/–	–0.082	–0.012	–0.082	–0.014
		(–1.23)	(–0.30)	(–1.22)	(–0.33)
CEO_Stock	+	–0.044	0.001	–0.044	0.001
		(–0.88)	(0.09)	(–0.87)	(0.09)
Tax Convexity	+	0.002	–0.010**	0.002	–0.009**
		(0.46)	(–2.42)	(0.45)	(–2.08)
Leverage	+	0.033	–0.035	0.034	–0.030
		(0.76)	(–1.11)	(0.78)	(–0.95)
Distress	+	–0.013	–0.056**	–0.012	–0.061**

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
		(–0.25)	(–2.04)	(–0.23)	(–2.04)
R&D	+	–0.182	0.049	–0.179	0.054
		(–0.88)	(0.50)	(–0.86)	(0.58)
R&D × Leverage	+	–0.023	–0.221	–0.026	–0.231
		(–0.08)	(–1.10)	(–0.09)	(–1.25)
Analyst	+	0.000	0.001*	0.000	0.001*
		(0.65)	(1.90)	(0.65)	(1.92)
Segment	–	–0.001	0.001**	–0.001	0.001*
		(–0.30)	(2.04)	(–0.30)	(1.86)
Foreign Sale	+	0.037	–0.008	0.037	–0.009
		(1.27)	(–0.45)	(1.27)	(–0.43)
Firm Size	+	–0.016		–0.016	
		(–0.94)		(–0.94)	
Div_Yield	+	0.135		0.130	
		(0.37)		(0.35)	
Short_Debt	+	0.004		0.005	
		(0.17)		(0.19)	
Cash_Cycle	+	0.000		0.000	
		(1.11)		(1.11)	
$\|DTC_{t-1}\|$	+		–0.006		–0.010
			(–0.09)		(–0.17)
LIFO_Reserve	+		0.257		0.187
			(0.65)		(0.49)
Div_Pay	–		0.004		0.004
			(1.09)		(1.10)
Flexibility	+		0.224***		0.240***
			(4.52)		(5.38)
|CFO|	+		–0.143*		–0.127

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
			(–1.72)		(–1.38)
Term Spread	+/–		0.014**		0.014*
			(2.00)		(1.93)
Dollar Strength	+/–		–0.000		–0.000
			(–0.10)		(–0.14)
IMR	+/–	–0.019	0.007	–0.019	0.002
		(–0.15)	(0.15)	(–0.14)	(0.06)
Intercept	+/–	0.243	–0.071	0.243	–0.070
		(0.96)	(–0.68)	(0.96)	(–0.66)
N		3,082	3,082	3,082	3,082
Adj. R^2		0.108	0.091	0.110	0.108
Industry Fixed Effects		Yes	Yes	Yes	Yes
$\beta 1 + \beta 2$ or $\lambda 1 + \lambda 2$		–0.077	–0.282	–0.069	–0.327
p-value for *F*-test		0.074	0.129	0.077	0.126

Note. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level, respectively, by using two-tailed tests. This table presents the robustness test of controlling for macroeconomic shocks in foreign exchange rates and interest rates. The results for the absolute value of the discretionary current accruals (|*DCACC*|) are reported in Columns 1 and 2 and the results for the absolute value of the discretionary working capital accruals (|*DWCACC*|) are reported in Columns 3 and 4. *DERIVATIVES* is a proxy for the extent of derivative use. FAS 161 equals 1 if the fiscal year is between 2009 and 2016 and 0 otherwise. The continuous variables are winsorised at 1% and 99%. The *t*-statistics that are reported in parentheses use robust standard errors that are clustered by firm and year. The sample period runs from 2002 to 2016. The variables are defined in Appendix A.

Columns 2 and 4 of Table 2.5 show that the coefficient estimates of *DERIVATIVES* ($\lambda 1$) are positive and significant, which suggests that derivatives and discretionary (current and working capital) accruals are complements during the pre-FAS 161 period. Importantly, the coefficients of the interaction term *DERIVATIVES* × *FAS 161* ($\lambda 2$) are negative and statistically significant in Columns 2 and 4, which suggests that derivatives are less positively associated with discretionary (current and working capital) accruals after the implementation of FAS 161, which confirms my main findings.

Finally, to investigate whether my results are affected by the uncertain macro environment after the financial crisis of 2007, I conduct tests that use five years before and five years after the financial crisis (2002–2006 v. 2011–2015). The negative coefficients of the interaction terms in Table 2.6 suggest that my main inferences are robust to the exclusion of the post-financial crisis period.

Table 2.6: Robustness Test: Exclusion of Post-Financial Crisis Period

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
|DTC| (β1)	+	0.094		0.108	
		(1.07)		(1.24)	
|DTC| × FAS 161 (β2)	**–**	**–0.274*** **		**–0.260*** **	
		(–2.72)		**(–2.60)**	
DERIVATIVES (notional) (λ1)	**+**		–0.104		–0.131
			(–0.34)		(–0.42)
DERIVATIVES × FAS 161 (λ2)	–		**–0.406** **		**–0.446*** **
			(–2.38)		**(–2.74)**
FAS 161	+/–	0.045**	0.067	0.042**	0.073
		(2.14)	(1.49)	(2.01)	(1.64)
CONTROLS included					
N		1,739	1,739	1,739	1,739
Adj. R^2		0.066	0.103	0.082	0.104
Industry Fixed Effects		Yes	Yes	Yes	Yes
β1 + β2 or λ1 + λ2		–0.180	–0.510	–0.152	–0.577
p-value for *F*-test		0.067	0.089	0.087	0.076

Note. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level, respectively, by using two-tailed tests. This table reports the robustness tests for excluding the post-financial crisis period. I use five years before (2002–2006) and five years after (2011–2015) the financial crisis. The results for the absolute value of the discretionary current accruals (|*DCACC*|) are reported in Columns 1 and 2 and the results for the absolute value of the discretionary working capital accruals (|*DWCACC*|) are reported in Columns 3 and 4. *DERIVATIVES* is a proxy for the extent of derivative use. FAS 161 equals 1 if the fiscal year is between 2011 and 2015 and 0 if the year is between 2002 and 2006. The

two interaction terms (|*DTC*| × *FAS 161* and *DERIVATIVES* × *FAS 161*) capture the potential change in the relationship between derivative use and accruals management after FAS 161 adoption. The continuous variables are winsorised at 1% and 99%. The *t*-statistics that are reported in parentheses use robust standard errors that are clustered by firm and year. The variables are defined in Appendix A.

2.7.1.4 Excluding Firms That Use Derivatives for Trading Purposes

Firms that use derivatives for speculative purposes generally have incentives and risk exposures that differ from those of firms that use derivatives for hedging purposes. Speculators take a view on the market, betting that the price of an underlying asset will either go up or down. They can make large gains if the market behaves as expected but large losses if they are wrong. In other words, speculators are unable to reduce earnings volatility via derivative use. To examine whether my primary findings are robust to the exclusion of speculators, I remove the firms that use derivatives for trading purposes and re-estimate Equations (4) and (5). Notably, I classify firms as speculators if they disclose that they use derivatives for trading purposes in their 10-K filings or annual reports. The limitation of this method is that I cannot identify the firms that are speculators but do not disclose this.

Table 2.7 shows that derivative use is positively associated with the level of discretionary current accruals (Column 2) and discretionary working capital accruals (Column 4) before FAS 161. The two interaction terms in Columns 2 and 4 are negative and statistically significant, which suggests that the positive relationship between derivatives and discretionary accruals is weaker after the introduction of FAS 161. Thus, my main findings are robust to the exclusion of firms that use derivatives for trading purposes.

Table 2.7: Robustness Test: Exclusion of Firms Using Derivatives for Trading Purposes

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
|DTC| (β1)	+	0.073		0.086	
		(1.16)		(1.32)	
|DTC| × FAS 161 (β2)	–	**–0.157****		**–0.160****	
		(–2.00)		**(–2.08)**	
DERIVATIVES (notional) (λ1)	+		**0.544***		**0.492***
			(1.92)		**(1.84)**
DERIVATIVES × FAS 161 (λ2)	–		**–0.799****		**–0.792****
			(–2.51)		**(–2.52)**
FAS 161	+/–	0.036**	0.107***	0.036**	0.106***
		(2.33)	(2.86)	(2.26)	(2.80)
CONTROLS included					
N		2,927	2,927	2,927	2,927
Adj. R^2		0.097	0.083	0.100	0.103
Industry Fixed Effects		Yes	Yes	Yes	Yes
β1 + β2 or λ1 + λ2		–0.084	–0.255	–0.074	–0.300
p-value for *F*-test		0.089	0.236	0.091	0.212

Note. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level, respectively, by using two-tailed tests. This table reports the robustness tests of excluding firms that use derivatives for trading purposes. The results for the absolute value of discretionary current accruals (|*DCACC*|) are reported in Columns 1 and 2 and the results for the absolute value of the discretionary working capital accruals (|*DWCACC*|) are reported in Columns 3 and 4. *DERIVATIVES* is a proxy for the extent of derivative use. FAS 161 equals 1 if the fiscal year is between 2009 and 2016 and 0 otherwise. The two interaction terms (|*DTC*| × *FAS 161* and *DERIVATIVES* × *FAS 161*) capture the potential change in

the relationship between derivative use and accruals management after FAS 161 adoption. The continuous variables are winsorised at 1% and 99%. The *t*-statistics that are reported in parentheses use robust standard errors that are clustered by firm and year. The sample period runs from 2002 to 2016. The variables are defined in Appendix A.

2.7.1.5 Excluding Firms That Are Charged with Derivative-Related Misstatements

Several firms have been found to apply derivative accounting rules incorrectly or inconsistently and must restate their financial statements in a later period (Chang et al., 2016). Given that the misstatement of derivative information can potentially affect the relationship between derivative use and accruals management, I investigate whether the primary findings are robust to the exclusion of restatement firms. Restatement data are obtained from the Audit Analytics database. Specifically, I classify firms as restatement firms if they restate their financial reports because of derivative- or hedging-related issues at least once during my sample period and re-estimate Equations (4) and (5) after excluding these firms. The untabulated results suggest that financial derivatives are positively associated with discretionary (current and working capital) accruals in the pre-FAS 161 period. The introduction of FAS 161 weakened the positive relationship between derivatives and discretionary (current and working capital) accruals.

2.7.1.6 Sample of Continuing Firms

I use a sample of firms that are present in the pre- and post-FAS 161 periods to exclude the alternative explanation that firms' changes in derivative use and in accruals management is driven by their exclusion from the sample. Table 2.8 shows that, similar to the primary findings, derivatives are positively associated with the level of discretionary current accruals (Column 2) and discretionary working capital accruals (Column 4) before FAS 161. Consistent with the main findings, the positive relationship between derivatives and discretionary accruals is weaker after the introduction of FAS 161 because the interaction terms in Columns 2 and 4 are negative and statistically significant.

Table 2.8: Robustness Test: Sample of Continuing Firms

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
|DTC| (β1)	+	0.029		0.044	
		(0.57)		(0.85)	
|DTC| × FAS 161 (β2)	–	**–0.106****		**–0.113****	
		(–2.06)		**(–2.31)**	
DERIVATIVES (notional) (λ1)	+		**0.617***		**0.589***
			(1.93)		**(1.89)**
DERIVATIVES × FAS 161 (λ2)	–		**–0.770****		**–0.761****
			(–2.30)		**(–2.23)**
FAS 161	+/–	0.033**	0.092**	0.033**	0.090**
		(2.24)	(2.42)	(2.21)	(2.33)
Cash_Comp	+	–0.045	0.034	–0.046	0.035
		(–0.87)	(0.91)	(–0.88)	(0.90)
CEO_Option	+/–	–0.096	–0.005	–0.096	–0.004
		(–1.39)	(–0.10)	(–1.39)	(–0.08)
CEO_Stock	+	–0.060	0.006	–0.059	0.008
		(–1.11)	(0.33)	(–1.11)	(0.48)
Tax Convexity	+	0.001	–0.012***	0.002	–0.011***
		(0.37)	(–3.52)	(0.39)	(–3.03)
Leverage	+	0.022	–0.026	0.023	–0.020
		(0.44)	(–0.72)	(0.47)	(–0.58)
Distress	+	–0.017	–0.054**	–0.016	–0.055**
		(–0.29)	(–1.99)	(–0.28)	(–2.02)
R&D	+	–0.177	0.065	–0.176	0.063

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
		(–0.73)	(0.48)	(–0.73)	(0.44)
R&D ×	+	–0.016	–0.300	–0.019	–0.320
Leverage		(–0.05)	(–1.18)	(–0.06)	(–1.31)
Analyst	+	0.000	0.001*	0.000	0.001*
		(0.88)	(1.88)	(0.86)	(1.83)
Segment	–	0.000	0.002***	0.000	0.002**
		(0.19)	(2.85)	(0.19)	(2.43)
Foreign Sale	+	0.043	–0.003	0.043	–0.005
		(1.19)	(–0.16)	(1.18)	(–0.21)
Firm Size	+	–0.017		–0.017	
		(–0.93)		(–0.93)	
Div_Yield	+	0.250		0.244	
		(0.72)		(0.71)	
Short_Debt	+	0.005		0.005	
		(0.17)		(0.18)	
Cash_Cycle	+	0.000		0.000	
		(1.24)		(1.24)	
$\|DTC_{t-1}\|$	+		–0.004		–0.008
			(–0.07)		(–0.15)
LIFO_Reserve	+		0.475		0.430
			(1.00)		(0.94)
Div_Pay	–		0.004		0.004
			(1.25)		(1.25)
Flexibility	+		0.225***		0.240***
			(4.44)		(5.20)
|CFO|	+		–0.185*		–0.176*
			(–1.93)		(–1.73)
Term Spread	+/–		0.013*		0.012
			(1.73)		(1.63)

	Exp. sign	(1) DERIVATIVES	(2) |DCACC|	(3) DERIVATIVES	(4) |DWCACC|
Dollar Strength	+/–		–0.000		–0.000
			(–0.17)		(–0.21)
IMR	+/–	0.001	0.033	0.001	0.026
		(0.01)	(0.55)	(0.01)	(0.45)
Intercept	+/–	0.234	–0.098	0.232	–0.098
		(0.85)	(–0.88)	(0.85)	(–0.88)
N		2,785	2,785	2,785	2,785
Adj. R^2		0.116	0.064	0.119	0.081
Industry Fixed Effects		Yes	Yes	Yes	Yes
$\beta 1 + \beta 2$ or $\lambda 1 + \lambda 2$		–0.077	–0.153	–0.069	–0.172
p-value for *F*-test		0.045	0.405	0.043	0.407

Note. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level, respectively, by using two-tailed tests. This table presents the robustness test for using a sample of continuing firms. I define continuing firms as firms that are present in the sample in the pre- and post-FAS 161 periods. The results for the absolute value of the discretionary current accruals (|*DCACC*|) are reported in Columns 1 and 2 and the results for the absolute value of the discretionary working capital accruals (|*DWCACC*|) are reported in Columns 3 and 4. *DERIVATIVES* is a proxy for the extent of derivative use. FAS 161 equals 1 if the fiscal year is between 2009 and 2016 and 0 otherwise. The continuous variables are winsorised at 1% and 99%. The *t*-statistics that are reported in parentheses use robust standard errors that are clustered by firm and year. The sample period runs from 2002 to 2016. The variables are defined in Appendix A.

2.7.1.7 Changes in Derivative Use Around FAS 161

Prior studies find relatively little change in derivative use around FAS 161 (Campbell et al., 2021; Steffen, 2022). Following existing studies, I calculate the change in derivative use by comparing the average derivative ratios before FAS 161 with those after FAS 161. My findings show that 237 firms increase their derivative use whereas 155 firms reduce their derivative use after the implementation of FAS 161. To examine whether the documented association between derivatives and discretionary accruals differs between the increase and decrease subsamples, I conduct tests for each subsample.

As is shown in Table 2.9, the coefficients of the interaction term *DERIVATIVES* × *FAS 161* (λ2) are significant and negative in all the columns, which suggests that the positive relationship between derivatives and discretionary accruals is weaker after the introduction of FAS 161. Overall, these findings suggest that FAS 161 has real effects on derivative use and my main findings are robust to the changes in derivative use around FAS 161.

Table 2.9: Robustness Test: Change in Derivative Use Around FAS 161

		|DCACC|		|DWCACC|	
	Exp. sign	(1) Derivatives increase	(2) Derivatives decrease	(3) Derivatives increase	(4) Derivatives decrease
DERIVATIVES (notional) (λ1)	+	1.080	0.261	0.989	0.261
		(1.56)	(1.44)	(1.60)	(1.41)
DERIVATIVES × FAS 161 (λ2)	–	**–1.214***	**–0.299****	**–1.151***	**–0.310****
		(–1.77)	**(–2.02)**	**(–1.77)**	**(–2.06)**
FAS 161	+/–	0.114*	0.052**	0.112*	0.052***
		(1.85)	(2.53)	(1.89)	(2.79)
Cash_Comp	+	–0.006	0.079	–0.012	0.092**
		(–0.38)	(1.62)	(–0.43)	(2.33)
CEO_Option	+/–	–0.007	0.017	–0.011	0.024
		(–0.15)	(0.32)	(–0.21)	(0.62)
CEO_Stock	+	–0.014	0.027	–0.016	0.035
		(–1.48)	(0.57)	(–0.68)	(0.98)
Tax Convexity	+	–0.009	–0.009	–0.009	–0.006
		(–1.61)	(–1.23)	(–0.62)	(–0.46)
Leverage	+	–0.108**	0.061**	–0.105**	0.057
		(–2.30)	(2.05)	(–2.24)	(0.75)
Distress	+	–0.072	–0.048	–0.077*	–0.048
		(–1.43)	(–1.20)	(–1.74)	(–1.07)
R&D	+	–0.072	0.097	–0.058	0.071
		(–0.85)	(0.60)	(–0.35)	(0.50)
R&D × Leverage	+	–0.036	–0.211	0.026	–0.207
		(–0.17)	(–1.00)	(0.07)	(–0.99)
Analyst	+	0.001	0.001***	0.001	0.001***

	Exp. sign	|DCACC|		|DWCACC|	
		(1) Derivatives increase	(2) Derivatives decrease	(3) Derivatives increase	(4) Derivatives decrease
		(0.79)	(3.15)	(0.72)	(2.69)
Segment	–	0.001	0.001	0.001	0.001
		(0.54)	(0.62)	(0.93)	(0.55)
Foreign Sale	+	–0.024	0.032	–0.023	0.025
		(–1.20)	(1.58)	(–0.80)	(0.90)
$\|DTC_{t-1}\|$	+	–0.051	0.075	–0.056	0.070
		(–0.80)	(0.90)	(–0.92)	(1.36)
LIFO_Reserve	+	0.496	0.037	0.351	0.071
		(0.78)	(0.15)	(0.61)	(0.18)
Div_Pay	–	0.006	–0.001	0.006	–0.001
		(1.18)	(–0.38)	(1.11)	(–0.36)
Flexibility	+	0.244***	0.185***	0.259***	0.205***
		(3.97)	(4.13)	(5.44)	(4.96)
|CFO|	+	–0.111	–0.166	–0.099	–0.153
		(–0.89)	(–1.54)	(–0.59)	(–1.42)
IMR	+/–	0.065	–0.035	0.072	–0.058
		(1.09)	(–0.76)	(0.84)	(–0.86)
Intercept	+/–	–0.054	–0.108*	–0.056	–0.115*
		(–0.84)	(–1.90)	(–0.76)	(–1.76)
N		1,849	1,233	1,849	1,233
Adj. R^2		0.135	0.133	0.145	0.152
Industry Fixed Effects		Yes	Yes	Yes	Yes
$\beta1 + \beta2$ or $\lambda1 + \lambda2$		–0.134	–0.038	–0.162	–0.049
p-value for *F*-test		0.699	0.624	0.766	0.706

Note. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level, respectively, by using two-tailed tests. This table presents the robustness test for examining the changes of derivative use around FAS 161. The results for firms that use more derivatives after FAS 161 are reported in Columns 1 and 3 and the results for firms that use fewer derivatives after FAS 161 are reported in Columns 2 and 4. *DERIVATIVES* is a proxy for the extent of derivative use. FAS 161 equals 1 if the fiscal year is between 2009 and 2016 and 0 otherwise. The continuous variables are winsorised at 1% and 99%. The *t*-statistics that are reported in parentheses use robust standard errors that are clustered by firm and year. The sample period runs from 2002 to 2016. The variables are defined in Appendix A.

2.7.2 Additional Analyses

2.7.2.1 Effect of Disclosure Costs

The main finding of this study suggests that FAS 161 weakens the positive relationship between derivatives and discretionary accruals. I expect this effect to be more pronounced for firms that are more affected by the implementation of FAS 161. Firms are more affected by FAS 161 if the magnitude of their change in derivative disclosures is larger. Furthermore, these firms bear relatively high disclosure costs.

Following Chiorean (2016), I construct a derivative disclosure score according to firms' disclosure of gains and losses in regard to derivatives.[12] The data are hand collected from corporate 10-K filings. The derivative disclosure score ranges from 0 to 6, where 0 represents no disclosure of any selected item and 6 represents disclosure of all the selected items. The score is computed each year.

Then, I calculate the change in the derivative disclosure score after the implementation of FAS 161. The mean change in the derivative disclosure score between the pre- and post-FAS 161 periods is 1.78. Thus, I classify firms as high disclosure cost firms if their change in derivative disclosure score between the two subsample periods is above 1.78; these firms are argued to be more affected by FAS 161.

Using derivatives and discretionary working capital accruals,[13] Columns 1 and 3 of Table 2.10 show that the coefficients of the interaction terms $|DWCACC| \times FAS\ 161$ ($\beta 2$) and $DERIVATIVES \times FAS\ 161$ ($\lambda 2$) are negative and statistically significant, which indicates that derivatives are less positively associated with discretionary working capital accruals after FAS 161 implementation for firms that bear high disclosure costs. However, Columns 2 and 4

[12] Refer to Chiorean (2016) for more details.

[13] The findings are similar to those for the use of discretionary current accruals.

show that the coefficients of $\beta 2$ and $\lambda 2$ are statistically insignificant for firms that have low disclosure costs.

Table 2.10

Additional Test: Effect of Disclosure Costs

		DERIVATIVES		|DWCACC|	
		(1)	(2)	(3)	(4)
	Exp. sign	High disclosure cost	Low disclosure cost	High disclosure cost	Low disclosure cost
|DWCACC| (β1)	+	0.042	–0.040		
		(0.69)	(–0.52)		
|DWCACC| × FAS 161 (β2)	–	**–0.103****	0.006		
		(–2.00)	(0.09)		
DERIVATIVES (notional) (λ1)	+			**0.510***	–0.012
				(1.75)	(–0.04)
DERIVATIVES × FAS 161 (λ2)	–			**–0.968****	0.133
				(–2.15)	(0.31)
FAS 161	+/–	0.016	0.031*	0.130*	0.022***
		(0.85)	(1.94)	(1.90)	(2.77)
CONTROLS included					
N		1,771	1,014	1,771	1,014
Adj. R^2		0.169	0.148	0.134	0.221
Industry Fixed Effects		Yes	Yes	Yes	Yes

Note. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level, respectively, by using two-tailed tests. This table reports the additional test for the effect of disclosure costs. The results for the high disclosure cost firms are reported in Columns 1 and 3 and the results for the low disclosure cost firms are reported in Columns 2 and 4. *DERIVATIVES* is a proxy for the extent of derivative use. FAS 161 equals 1 if the fiscal year is between 2009 and 2016 and 0 otherwise. The two interaction terms

(|*DWCACC*| × *FAS 161* and *DERIVATIVES* × *FAS 161*) capture the potential change in the relationship between the derivatives and the discretionary working capital accruals after FAS 161 adoption. The continuous variables are winsorised at 1% and 99%. The *t*-statistics that are reported in parentheses use robust standard errors that are clustered by firm and year. The sample period runs from 2002 to 2016. The variables are defined in Appendix A.

2.7.2.2 Effect of Competition

Competitors can draw more inferences from incumbents' increased derivative disclosures under FAS 161 and act accordingly (Zou, 2018), which indicates that firms in highly competitive industries are more likely to be affected by the implementation of FAS 161 than firms in less competitive industries.

I measure industry competition by using the Herfindahl–Hirschman index (HHI), which is the sum of the squared market shares of firms within an industry (Wilson et al., 2012). Industries are identified by their four-digit SIC code. Firms are classified as high competition firms if their HHI is lower than the mean value because a low HHI value corresponds to greater competition.

Table 2.11 shows that the coefficients of the interaction terms $|DWCACC| \times FAS\ 161$ ($\beta 2$) and $DERIVATIVES \times FAS\ 161$ ($\lambda 2$) are negative and statistically significant for high competition firms (Columns 1 and 3) and not significant for low competition firms (Columns 2 and 4). These findings are consistent with my prediction that derivatives are less positively associated with discretionary accruals after FAS 161 implementation for firms in highly competitive industries.

Table 2.11: Additional Test: Effect of Competition

		DERIVATIVES		|DWCACC|	
		(1)	(2)	(3)	(4)
	Exp. sign	High competition	Low competition	High competition	Low competition
|DWCACC| (β1)	+	0.065	–0.063		
		(0.89)	(–0.76)		
|DWCACC| × FAS 161 (β2)	–	**–0.174*** **	–0.039		
		(–2.73)	(–0.44)		
DERIVATIVES (notional) (λ1)	+			0.251	–0.792
				(1.29)	(–1.50)
DERIVATIVES × FAS 161 (λ2)	–			**–0.442***	–0.187
				(–1.84)	(–0.32)
FAS 161	+/–	0.015	0.041*	0.048	0.083**
		(0.87)	(1.73)	(0.90)	(2.28)
CONTROLS included					
N		1,769	1,006	1,769	1,006
Adj. R^2		0.134	0.188	0.098	0.111
Industry Fixed Effects		Yes	Yes	Yes	Yes

Note. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level, respectively, by using two-tailed tests. This table reports the additional test for the effect of industry competition. The results for firms that are in more competitive industries are reported in Columns 1 and 3 and the results for firms that are in less competitive industries are reported in Columns 2 and 4. *DERIVATIVES* is a proxy for the extent of derivative use. FAS 161 equals 1 if the fiscal year is between 2009 and 2016 and 0 otherwise. The two interaction terms (*|DWCACC| × FAS 161* and *DERIVATIVES × FAS 161*) capture the potential change in the relationship between the derivatives and the discretionary

working capital accruals after FAS 161 adoption. The continuous variables are winsorised at 1% and 99%. The *t*-statistics that are reported in parentheses use robust standard errors that are clustered by firm and year. The sample period runs from 2009 to 2016. The variables are defined in Appendix A.

These combined findings suggest that the weakening in the relationship between derivatives and discretionary accruals is more pronounced for firms that are more affected by the implementation of FAS 161.

2.8 Conclusion

Barton (2001) and Pincus and Rajgopal (2002) find that financial derivatives and accruals management were partial substitutes for income smoothing before 1996. Given the adoption of a hedge accounting standard (FAS 133) in 2000, Choi et al. (2015) document a complementary relationship between derivatives and discretionary accruals in the post-FAS 133 period (2002–2006) and argue that FAS 133 makes derivative hedging less effective in smoothing earnings. Recent studies document that the implementation of FAS 161 helps investors and analysts to better understand corporate derivative use (Campbell et al., 2021) and reduces information asymmetry (Steffen, 2022). In this study, I examined whether the implementation of the derivative disclosure standard FAS 161 in November 2008 altered managerial decisions on the relative use of the two income-smoothing methods of financial derivatives and accruals management.

Using a sample of non-financial firms that were listed on the S&P 500 index for the sample period of 2002 to 2016, I found that, consistent with Choi et al. (2015), financial derivatives have a complementary relationship with discretionary accruals in the pre-FAS 161 period. However, the positive relationship between derivatives and discretionary (current and working capital) accruals is weaker after the implementation of FAS 161. This suggests that FAS 161 altered the relative merit of using derivatives vis-à-vis discretionary accruals for income smoothing.

My main findings are robust to the use of alternative measures of discretionary accruals, controlling for cash-flow volatility, the exclusion of speculators and restatement firms and the use of a sample of continuing firms. In addition, the main findings are not driven by

macroeconomic shocks in FX rates and IRs. Further, I provide evidence that the weakening in the relationship between derivatives and accruals management is more significant for firms that are more affected by the implementation of FAS 161, that is, firms that bear high disclosure costs and those in highly competitive industries.

This study has several limitations. FAS 161 was implemented at a single point in time and the usual concerns emerge about the possibility of the results arising from other events that occurred during the post-FAS 161 implementation period. One caveat of this study is that neither the notional amount nor the fair value of derivatives is a perfect measure of the extent of firms' derivative use (Campbell et al., 2019). Another limitation of this type of study is the inability to separate the derivatives that are primarily used for risk management from the derivatives that are used for smoothing earnings. Future research is needed, where data allow, to better understand manager preferences for smooth earnings streams and, specifically, how managers use derivatives to hedge risk and to smooth earnings.

Chapter 3: Study 2: Impact of Share Pledging on Non-GAAP Reporting

3.1 Introduction

The frequency of non-GAAP earnings disclosure has increased during the past two decades (Bentley et al., 2018; Bhattacharya et al., 2003; Brown et al., 2012). Managers voluntarily disclose these earnings metrics to signal the relative importance of the earnings components that they include in, versus exclude from, their non-GAAP earnings calculations (Bhattacharya et al., 2003). These manager-disclosed earnings matrices are controversial. On the one hand, managers claim that non-GAAP earnings disclosures are informative (e.g. Bhattacharya et al., 2003; Bradshaw et al., 2018; Lougee & Marquardt, 2004). However, investors and regulators (and standard setters) are concerned that managers opportunistically use non-GAAP earnings metrics to bias investors' perceptions (Rapoport, 2013).[14] Given the widespread interest in non-GAAP disclosures, numerous prior studies show that external forces (e.g. analysts, debtholders, short sellers and regulations) play an important role in influencing non-GAAP reporting practices (Bhattacharya et al., 2022; Christensen et al., 2019; Frankel et al., 2011; Kolev et al., 2008; Kyung et al., 2019). However, far less research attention has been given to internal forces such as corporate insiders (i.e. directors and executives). I address this void. In particular, I empirically explore the ways that insider share pledging influences the likelihood and the quality of non-GAAP disclosure.

Share pledging by corporate insiders is common across the world. Larcker and Tayan (2010) find that 982 directors or officers reported share pledging in proxy statements during the

[14] For example, non-GAAP reporting is regarded as a 'fraud risk factor' by the SEC (Leone, 2010). Further, standard setters such as the Public Company Accounting Oversight Board are concerned about the role of auditors in ensuring firm's non-GAAP reporting (Black & Christensen, 2018).

2006 to 2009 period in the US. Approximately 35 to 50% of publicly listed firms in China and Taiwan have share pledging by insiders (Dou et al., 2019; Zhu et al., 2021). Prior studies focus primarily on the impact of pledging on firms' financial and mandatory disclosure decisions (e.g. share repurchases, real or accruals earnings management). In this study, I focus on firms' non-GAAP reporting, which is unaudited and less legally binding yet gives managers large discretion over when and how to report (Black et al., 2017).

Ex ante it is unclear how insiders' share pledges affect non-GAAP reporting practices. On the one hand, share pledges could motivate managers to disclose non-GAAP earnings. Practitioners and scholars suggest that share pledges make firms vulnerable to negative price shocks (Dou et al., 2019). Specifically, when stock prices fall below a certain level, it automatically triggers margin calls, which force managers to sell shares if they cannot repay the loan or deposit more funds.[15] This forced sale of shares could lead to a cascading negative effect on share prices that triggers further margin calls and forced sales. Prior studies suggest that managers have strong incentives to avoid margin calls and forced sales (Chan et al., 2018; Singh, 2018). Given that non-GAAP disclosures are effective in managing investors' perceptions about firms (Abdel-Meguid et al., 2021; Black & Christensen, 2009; Black et al., 2017), I expect that pledging managers may resort to optimistic non-GAAP disclosures to counteract the price pressure that is imposed on share pledging and to support the stock price. Consequently, I expect that managers' share pledging increases non-GAAP disclosures.

On the other hand, share pledging could invite investor scrutiny that deters managers from disclosing non-GAAP earnings. Specifically, given that share pledging could enable managers to diversify a portion of their shareholdings, it reduces the interest alignment between managers and shareholders (Larcker & Tayan, 2010). For example, prior studies find that

[15] It is difficult for managers to repay loans or to deposit more funds because the reason behind share pledging is typically inadequate liquidity.

pledging managers extract personal benefits at the expense of shareholders because shareholders bear higher levels of equity risk but cannot obtain additional benefits (Anderson & Puleo, 2020). Investors respond negatively when managers increase their share pledging (Dou et al., 2019). Consequently, investors are likely to pay closer attention to share pledging and the potentially heightened investor monitoring would increase the costs of opportunistic non-GAAP reporting. Thus, I expect that managers' share pledging decreases non-GAAP disclosures.

Furthermore, the effect of share pledging on the quality of non-GAAP reporting remains an empirical question. Pledging managers have incentives to positively bias investor perceptions to avoid a margin call by reporting non-GAAP earnings more opportunistically (Abdel-Meguid et al., 2021; Black & Christensen, 2009). Consequently, share pledging would deteriorate the quality of non-GAAP reporting. Alternatively, share pledging can signal agency problems (Anderson & Puleo, 2020), potentially increasing investor scrutiny. This could constrain managerial opportunism in non-GAAP reporting. Consequently, the quality of non-GAAP reporting is likely to be increased.

To examine the effect of share pledging on non-GAAP reporting, I hand collected share-pledging data that covers publicly listed US firms for the period of 2006 to 2019.[16] I find that insider share pledging is positively associated with the propensity and the frequency of non-GAAP reporting. The result is economically significant: a 1% increase in insider pledging is associated with a 0.156% increase in the frequency of non-GAAP earnings disclosures. Further, I find that managers are more likely to report non-GAAP earnings when there is significant margin-call pressure.

[16] The sample period starts in 2006 because the fiscal year 2006 is the first year in which all firms were required to disclose the pledging activities of their named executives and directors.

The results are robust to two alternative measures for the quality of non-GAAP earnings, the exclusion of financial and utility firms, and the control for earnings management. To improve the covariate balance between the pledging and non-pledging firms, I use a PSM approach by comparing observations that are otherwise similar but are distinguished by share pledging. The results are robust to these tests. Overall, these findings suggest that firms that have insider share pledging are more likely to disclose non-GAAP earnings and to report non-GAAP earnings more frequently than firms that do not have insider pledging.

Next, I examine the effect of share pledging on the quality of non-GAAP reporting. Following prior studies (Abdel-Meguid et al., 2021; Doyle et al., 2003; Kyung et al., 2019), I use the quality of non-GAAP exclusions as a proxy for non-GAAP reporting quality. Non-GAAP exclusions are high-quality (low-quality) exclusions if they are transitory (recurring). The findings show that insider share pledging is negatively associated with the quality of non-GAAP reporting. The deterioration in non-GAAP reporting quality, combined with the more frequent non-GAAP earnings disclosures, is consistent with the notion that managers who have shares under pledge report non-GAAP earnings opportunistically. Additional analysis suggests that pledging managers prefer to place non-GAAP earnings more prominently in press releases relative to GAAP earnings, which is consistent with managerial opportunism in non-GAAP earnings disclosures (Brown et al., 2012).

Further, I examine the condition under which the effect of insider share pledging on the propensity and the quality of non-GAAP reporting is more pronounced. I find that the increase in the propensity and the frequency of non-GAAP earnings disclosures and the decline in non-GAAP reporting quality are concentrated mainly in firms that have less analyst coverage and fewer independent boards. These findings are consistent with the view that corporate governance plays a role in disciplining insiders and in constraining managerial opportunism in non-GAAP reporting (Christensen et al., 2021; Frankel et al., 2011).

I employ a DID design to compare the non-GAAP reporting changes surrounding the initiation of insider pledging. A DID analysis can mitigate concerns that unobservable time-invariant variables drive the observed changes in the dependent variable. The DID results show that pledging firms are more likely to report non-GAAP earnings and to make more frequent non-GAAP earnings disclosures one year after their pledging initiation relative to control firms during the same period.

This study makes several contributions to the literature. First, it contributes to the growing literature on share pledging. The existing literature documents that stock pledges negatively affect firms' value (Dou et al., 2019), deter firms' future innovation outputs and quality (Pang & Wang, 2020), motivate opportunistic activities such as share repurchases (Chan et al., 2018), earnings management (Singh, 2018), and tone management during earnings communication conference (Zhao et al., 2019). This study provides evidence that share pledging has an impact on the likelihood, the frequency and the quality of firms' voluntary non-GAAP earnings disclosures.

Second, this study adds to the literature on the determinants of non-GAAP earnings disclosures and contributes to the discussion on the motives (informative v. opportunistic) for non-GAAP reporting. Prior studies document that voluntary clawback adoption (Kyung et al., 2019), debt-covenant violation (Christensen et al., 2019), local political corruption (Chen et al., 2021) and analyst coverage influence managers' non-GAAP reporting decisions. Several studies find that non-GAAP disclosures are generally informative (Bentley et al., 2018; Black et al., 2021b). This study documents that share pledging is positively associated with the likelihood of non-GAAP reporting and negatively related to the quality of non-GAAP exclusions, which is consistent with the view that pledging managers are motivated to opportunistically bias investor perceptions through non-GAAP reporting.

Third, this study contributes to the literature on the relationship between managers' personal attributes and voluntary disclosures. Prior studies show that managerial risk-taking incentives (Bansal et al., 2013), CEO pay components (Black et al., 2021a) and CEO narcissism (Abdel-Meguid et al., 2021) are associated with non-GAAP earnings disclosures. This study shows that managers who pledge shares as collateral to secure loans also have incentives to disclose non-GAAP earnings opportunistically.

The remainder of this study is organised as follows. Section 3.2 reviews the existing literature on non-GAAP reporting and the effects of stock pledges. Section 3.3 develops the hypothesis. Section 3.4 describes the research design. Section 3.5 explains the sample selection and provides the descriptive statistics. Section 3.6 reports the primary results. Sections 3.7 and 3.8 discuss the cross-sectional analyses and the robustness tests. Section 3.9 concludes.

3.2 Literature Review

3.2.1 Non-GAAP Reporting

In addition to reporting GAAP-based earnings on income statements, some corporations provide alternative profitability metrics, non-GAAP earnings, in firms' earnings press releases. Non-GAAP earnings exclude certain items from the GAAP earnings and managers have discretion over which earnings components to exclude (Christensen et al., 2021). Prior studies find that managers normally exclude two types of items from their non-GAAP calculations: non-recurring items and recurring transactions. The exclusions of non-recurring items (e.g. restructuring and litigation charges) are generally considered a better reflection of firms' core operating performance whereas the exclusions of recurring items (e.g. research and development and tax-related expenses) are usually associated with managers' opportunistic behaviours, which can ultimately mislead investors (Black & Christensen, 2009; Doyle et al., 2003).

Analytical literature suggests that managers' decisions to provide non-GAAP earnings likely depends on trade-offs between the costs and the benefits of this type of disclosure (Verrecchia, 1983). Hirshleifer and Teoh (2003) model the two legitimate reasons for non-GAAP earnings disclosures and managers' strategic motives to manipulate investors' perceptions. They argue that investors tend to perceive non-GAAP earnings as maximally informative even when they are subjected to managers' manipulations because of limited attention. They conclude that although non-GAAP earnings positively bias investors' perceptions, they also help stock prices to represent the fundamental value of firms more accurately.

In line with the theoretical arguments, empirical studies find that informativeness and opportunism are two incentives for managers to provide non-GAAP reporting. On the one hand, managers use non-GAAP earnings to provide investors with a more informative measure of firms' core earnings by excluding one-time items when calculating non-GAAP earnings (Bhattacharya et al., 2003, 2007; Curtis et al., 2014). Specifically, Bhattacharya et al. (2003) demonstrate that investors regard non-GAAP earnings as more informative than GAAP earnings because of the analyses of short-window abnormal returns around earnings announcements. Curtis et al. (2014) find that investors can efficiently identify and price transitory gains from non-GAAP reporting, which suggests that investors are not misled by non-GAAP earnings in the presence of transitory gains. Leung and Veenman (2018) provide evidence that loss firms' non-GAAP disclosures are informative given that non-GAAP earnings can strongly predict these firms' future operating performance. Thus, this line of research concludes that managers provide non-GAAP reporting to accurately depict firms' core performance.

However, non-GAAP earnings reflect managerial opportunism and positively bias investors' perceptions (Brown et al., 2012; Doyle et al., 2013). Barth et al. (2012) argue that

although the exclusion of one-time items provides more informative core earnings, the exclusion of recurring items is not well justified. Numerous studies find that managers use non-GAAP adjustments to meet an earnings target that is not likely to be achieved according to the GAAP earnings (Doyle et al., 2003; Kolev et al., 2008). In addition, Doyle et al. (2013) find that firms are more likely to meet or to beat consensus analyst forecasts on a non-GAAP basis. Abdel-Meguid et al. (2021) provide evidence that narcissistic CEOs are more prone to increasing non-GAAP earnings by excluding income-decreasing items to project a more favourable image of their performance. Overall, these studies suggest that managers opportunistically use non-GAAP earnings to bias investors' perceptions.

Non-GAAP earnings are used by sophisticated and unsophisticated users. For example, although Allee et al. (2007) find that less (but not more) sophisticated investors rely on non-GAAP information, Bhattacharya et al. (2003) find that analysts also react to non-GAAP earnings when revising their future earnings forecasts. In addition to analysts, short sellers and creditors, who are argued to be sophisticated financial users, use non-GAAP earnings (Christensen et al., 2014, 2019).

Prior studies show that regulations (Kolev et al., 2008), board independence (Frankel et al., 2011), investor scrutiny (Christensen et al., 2019) and analysts' monitoring (Christensen et al., 2021) can constrain managerial opportunism in the providing of non-GAAP performance metrics. For example, Kolev et al. (2008) find that the quality of non-GAAP exclusions improved after the SEC's intensified scrutiny from mid-2001. Frankel et al. (2011) document that firms that have fewer independent boards are more likely to exclude recurring items from non-GAAP earnings, which suggests that board independence limits opportunism in voluntary non-GAAP disclosures. Christensen et al. (2021) examine the ways that an unanticipated decrease in analyst coverage influences the likelihood and the quality of non-GAAP disclosures. Their findings show that managers become more aggressive in non-GAAP

reporting after a reduction in analyst following, which indicates that analysts' monitoring could deter managerial opportunism.

3.2.2 Stock Pledges

Stock pledging refers to the practice of corporate insiders using shares in their firms as collateral to secure their personal loans. Most existing studies on share pledging apply data from China and Taiwan. These studies focus mainly on the effect of share pledging on firm value (Dou et al., 2019) and corporate decisions (Chan et al., 2018; Pang & Wang, 2020). Using an exogenous shock to share pledging in Taiwan, Dou et al. (2019) find that insider pledging negatively affects firm value by increasing crash risk and by decreasing risk-taking. However, Li et al. (2019) provide evidence that share pledging by controlling shareholders in China has a positive impact on firm value. Further, existing studies document that the existence and the percentage of shares that are pledged by controlling shareholders in China deter firms' future innovation outputs and quality (Pang & Wang, 2020).

Several studies document that insiders who pledge shares are motivated to influence investors' perceptions to avoid declines in the share price. Chan et al. (2018) find that controlling shareholders in Taiwan tend to repurchase the firms' shares to support the share price and to maintain control rights under margin-call pressure. Zhao et al. (2019) argue that insiders who pledge their shares are more likely to positively bias investors' perceptions. Using a sample of Chinese firms, they show that the tone of earnings communications in conference calls is more positive for firms that have share pledging than for firms that do not have share pledging. Similarly, Deren and Ke (2018) conclude that controlling shareholders that pledge shares in Chinese-listed firms are more likely to manage earnings upwards through real earnings management.

Huang and Xue (2016) provide evidence that firms with concentrated ownership have incentives to smooth earnings after share pledging. My research differs from Huang and Xue

(2016) in the following two aspects: First, I investigate the share pledging practice by corporate insiders (i.e. directors, executives) for a sample of U.S. firms whereas Huang and Xue (2016) use a sample of Chinese firms to examine controlling shareholders' pledging behaviours; second, I investigate the impact of share pledging on the frequency and quality of voluntary non-GAAP earnings disclosures whereas Huang and Xue (2016) focus on the effect of share pledging on the smoothness of GAAP earnings. Compared to GAAP earnings, non-GAAP earnings are unaudited and less legally binding yet give managers large discretion over when and how to report (Black et al., 2017). Moreover, the motives for non-GAAP reporting can be either informative or opportunistic, managers not necessarily report non-GAAP earnings opportunistically, while income smoothing is generally opportunistic.

Only a few studies examine share pledging in the US, probably because of data availability. Fabisik (2019) examines the underlying reasons that US CEOs pledge their own companies' stocks rather than selling part of their (unrestricted) shareholdings. She finds that CEOs use the funds from share pledging to obtain liquidity while maintaining ownership (90.5% of her observations), to leverage up their stock position (6.0% of her observations) or to hedge or to diversify their ownership (3.5% of her observations). Using a random sample of 500 firms listed on the S&P 1500, Anderson and Puleo (2020) find that executives' share pledging increases the equity risk of firms through the contingency-risk channel. Contingency risk means that outside investors bear downward pressure on stock prices and increased equity volatility when a forced sale of pledged shares is triggered. Their findings suggest that executives who pledge shares extract personal benefits at the expense of shareholders because shareholders bear increased risks but cannot receive any returns. Singh (2018) investigates the relationship between share pledging and earnings management. He finds that managers who pledge shares have the incentive to inflate earnings.

3.3 Hypothesis Development

The SEC's concern that pledged shares 'have the potential to influence management's performance and decisions' led to its enforcement of disclosures rules for share pledging in 2006 (SEC, 2006). In line with this view, prior studies find that when facing downward price pressures and the potential risk of losing control rights, managers initiate buyback programs (Chan et al., 2018), inflate reported earnings (Singh, 2018) and change to a more positive tone in earnings conference calls (Zhao et al., 2019) to support the share price and thus avoid a margin call.

I argue that managers could employ non-GAAP reporting to support the stock price when they pledge shares. This is because investors pay more attention to non-GAAP earnings than GAAP earnings (Bradshaw & Sloan, 2002) and regard non-GAAP earnings as a more informative measure of firm performance (Bhattacharya et al., 2003). Prior studies suggest that non-GAAP reporting is used by managers to positively influence investor perceptions about firms' performance (Brown et al., 2012; Doyle et al., 2013). Given that managers who pledge shares have incentives to counteract potential margin-call pressure by strategically disclosing non-GAAP earnings to support share prices, share pledging could positively affect firms' propensity and frequency of non-GAAP earnings reporting.

However, managers are less likely to report non-GAAP earnings if investor scrutiny increases after share pledging. Christensen et al. (2019) find that heightened shareholder monitoring after debt-covenant violations constrains managerial opportunism in non-GAAP reporting, which ultimately leads to a reduction in non-GAAP reporting. Given that share pledging exacerbates the agency problem (Anderson & Puleo, 2020), shareholders respond negatively when managers, directors or blockholders pledge more shares (Dou et al., 2019). Given that share pledging could signal agency problems to shareholders, the potentially heightened investor monitoring might increase the costs of opportunistic non-GAAP reporting.

As a result, managers are less likely to provide non-GAAP reporting after share pledging. In summary, whether share pledging has an impact on the likelihood and the frequency of non-GAAP reporting remains an empirical question. Therefore, I state my first hypothesis in null form as follows:

H1: *Share pledging has no effect on the propensity and the frequency of non-GAAP reporting.*

A well-established definition of non-GAAP reporting quality is the degree to which non-GAAP exclusions are transitory or recurring (Christensen et al., 2019; Kolev et al., 2008; Kyung et al., 2019; Leung & Veenman, 2018). Specifically, the existing literature documents that non-GAAP earnings are of a higher quality if the exclusions are transitory. Transitory items (e.g. special items) are considered non-predictive of firms' future performance so the exclusion of them can better reflect firms' core earnings. However, non-GAAP disclosures are of a lower quality if managers remove recurring items from non-GAAP earnings because these items are predictive of future performance (Doyle et al., 2003).

If managers opportunistically disclose non-GAAP earnings to support share prices, they might remove recurring items from non-GAAP earnings to portray a favourable performance of firms. As a result, the quality of non-GAAP reporting would deteriorate after insider share pledging. However, if increased investor scrutiny after share pledging curbs managerial opportunism in non-GAAP reporting, then the quality of non-GAAP reporting should improve after share pledging. Therefore, I state the second hypothesis in null form as follows:

H2: *Share pledging has no effect on the quality of non-GAAP reporting.*

3.4 Research Design

3.4.1 Likelihood and Frequency of Non-GAAP Earnings Disclosure

To investigate the relationship between share pledging and the propensity to report non-GAAP earnings, I estimate the following logit model:

$$\begin{aligned} Prob(Non\text{-}GAAP)_{it} = {} & \beta_0 + \beta_1 Pledge_{it} + \beta_2 Size_{it} + \beta_3 Leverage_{it} + \beta_4 BTM_{it} \\ & + \beta_5 Earnings\ Volatility_{it} + \beta_6 Intangible_{it} + \beta_7 Loss_{it} + \beta_8 High\ Tech_{it} \\ & + \beta_9 SPI_{it} + \beta_{10} Special\ Items_{it} + \beta_{11} Big\ Bath_{it} + \beta_{12} Sales\ Growth_{it} \\ & + Year\ Fixed\ Effects + Industry\ Fixed\ Effects + \varepsilon_{it} \end{aligned} \quad (1a)$$

where *i* and *t* denote firm and year, respectively. Following Black et al. (2017), the dependent variable *Non-GAAP* is an indicator variable that is coded 1 if a firm discloses non-GAAP earnings in any quarter during year *t* and 0 otherwise. *Pledge* represents one of the two measures of pledging in year *t* (*Pledge Dummy* and *Pledge Ratio*). *Pledge Dummy* is an indicator variable that is coded 1 if the named executives and directors in a firm pledge shares and 0 otherwise. *Pledge Ratio* is the ratio of the aggregated number of shares that is pledged relative to the aggregated number of shares that is owned by the named executives and directors. A significant and positive coefficient for *Pledge* (β_1) indicates that firms that have insider share pledging are more likely to report non-GAAP earnings.

I control for a set of variables that affects the likelihood of non-GAAP disclosures, as is suggested in the existing literature (Kolev et al., 2008; Kyung et al., 2019; Lougee & Marquardt, 2004). Specifically, given that larger firms are more likely to disclose non-GAAP earnings, firm size (*Size*) is included in the model. I control for financial leverage (*Leverage*) because highly leveraged firms tend to include non-GAAP figures in earnings releases (Kolev et al., 2008). High-growth firms are more likely to engage in non-GAAP disclosures (Lougee & Marquardt, 2004). Thus, I include book-to-market ratio (*BTM*) and sales growth (*Sales Growth*). Lougee and Marquardt (2004) argue that firms that have a high proportion of

intangible assets have incentives to report non-GAAP earnings. Thus, I control for intangible intensity (*Intangible*) and high-technology firms (*High Tech*). Earnings volatility (*Earnings Volatility*) is added to the model because firms that have highly volatile earnings are more likely to report non-GAAP earnings (Lougee & Marquardt, 2004). Following Kyung et al. (2019), I incorporate special items (*SPI* and *Special Items*) into the model because special items are positively associated with the likelihood of non-GAAP disclosures. Last, a loss indicator variable (*Loss*) and a big bath indicator variable (*Big Bath*) are included because firms that have losses are more likely to report non-GAAP earnings (Kyung et al., 2019). I control for year and industry (two-digit SIC industry codes) fixed effects and cluster the standard errors by firm to adjust for heteroskedasticity (Petersen, 2009) in all the regressions. All the continuous variables are winsorised at the 1% and 99% levels to reduce the effects of the extreme values. All the variables are defined in Appendix B.

To investigate whether share pledging is positively associated with the frequency of non-GAAP disclosures, I estimate the following OLS model:

$$\begin{aligned} FreqNonGaap_{it} = \quad & \beta_0 + \beta_1 Pledge_{it} + \beta_2 Size_{it} + \beta_3 Leverage_{it} + \beta_4 BTM_{it} \\ & + \beta_5 Earnings\ Volatility_{it} + \beta_6 Intangible_{it} + \beta_7 Loss_{it} + \beta_8 High\ Tech_{it} \\ & + \beta_9 SPI_{it} + \beta_{10} Special\ Items_{it} + \beta_{11} Big\ Bath_{it} + \beta_{12} Sales\ Growth_{it} \\ & + Year\ Fixed\ Effects + Industry\ Fixed\ Effects + \varepsilon_{it} \end{aligned} \qquad (1b)$$

where the dependent variable, *FreqNonGaap*, is the percentage of quarters in year *t* in which managers disclose non-GAAP earnings (Hsu et al., 2021). A significant and positive coefficient of *Pledge* (β_1) indicates that share pledging is associated with more frequent non-GAAP earnings disclosures. All the other variables are defined in Appendix B.

3.4.2 Quality of Non-GAAP Exclusions

H2 predicts that share pledging has no effect on the quality of non-GAAP reporting. Prior studies (Black et al., 2021b; Christensen et al., 2021; Doyle et al., 2003; Kyung et al., 2019) recognise non-GAAP exclusions as high-quality (low-quality) exclusions if they are transitory (recurring). To test H2, I estimate the following OLS model:

$$\begin{aligned} FOI_{i,t+1} = {} & \beta_0 + \beta_1\, Total\ Exclusion_{it} + \beta_2\, Pledge + \beta_3\, Total\ Exclusion_{it} \times Pledge_{it} \\ & + \beta_4\, Non\text{-}GAAP\ Earnings_{it} + \beta_5\, Size_{it} + \beta_6\, BTM_{it} + \beta_7\, Loss_{it} \\ & + \beta_8\, Earnings\ Volatility_{it} + \beta_9\, Sales\ Growth_{it} + \beta_{10}\, Age_{it} \\ & + Year\ Fixed\ Effects + Industry\ Fixed\ Effects + \varepsilon_{it} \qquad (2) \end{aligned}$$

where *FOI* is the earnings per share from operations in year $t + 1$ (Black et al., 2021b). *Total Exclusion* is the difference between the non-GAAP earnings per share and the GAAP earnings per share summed over the four quarters in year *t*. If the non-GAAP exclusions are of a high quality (transitory), they should have no impact on firms' future operating income and $\beta 1$ should be 0 (Doyle et al., 2003; Kyung et al., 2019). However, prior studies find that $\beta 1$ is negative, which suggests that the excluded items are not perfectly transitory (Kolev et al., 2008). In line with the existing literature, I expect $\beta 1$ to be negative.

The regression coefficient of the interaction term *Total Exclusion* × *Pledge* ($\beta 3$) captures the effect of share pledging on the quality of non-GAAP reporting. A significant and positive coefficient of $\beta 3$ indicates that share pledging can improve the quality of non-GAAP earnings disclosure whereas a negative coefficient indicates that share pledging would reduce the non-GAAP reporting quality. Following prior studies (Kolev et al., 2008; Kyung et al., 2019), I incorporate firm size (*Size*), book-to-market ratio (*BTM*), a loss indicator variable (*Loss*), earnings volatility (*Earnings Volatility*), sales growth (*Sales Growth*), firm age and non-GAAP earnings into the model. I include year and industry fixed effects and cluster the standard errors by firm to adjust for heteroskedasticity (Petersen, 2009). Consistently with the existing

literature (Black et al., 2021b; Christensen et al., 2021), the sample is restricted to firm-year observations that disclose only non-GAAP earnings so that the results are not affected by managers' decisions about non-GAAP disclosures.

3.5 Sample Selection and Descriptive Statistics

In August 2006, the SEC required footnote disclosure of share-pledging activities by named executive officers, directors and director nominees. My sample consists of the universe of all the firms in Compustat for the fiscal years 2006 to 2019. I obtain accounting and stock return data from Compustat and CRSP, respectively. The data on managers' non-GAAP earnings disclosures are retrieved from Bentley et al. (2018).[17]

I hand collect the stock-pledge data by using SeekEdgar to search through all the publicly listed firms' proxy statements (DEF 14A) between 1 January 2007 and 31 December 2019. I start from 2007 because it is the first calendar year in which all firms were required to disclose pledging activities. To locate information on share pledging, I search for the following keywords: 'pledge shares', 'pledged shares', 'pledging', 'shares pledging', 'pledge as security', 'pledge as collateral', 'pledge loan' and 'shares pledged'. These searches identify a total of 13,664 potential proxy statements. Then, I carefully read each of these proxy statements and obtain 2,536 firm-year observations that have insider share pledging. Observations that are missing identifiers and Compustat data are removed from my sample. The final sample includes 49,961 firm-year observations, within which 1,804 observations represent firm-years that have insider share pledging. The detailed sample-selection process is described in Table 3.1.

[17] Available from https://sites.google.com/view/kurthgee/data

Table 3.1: Description of Sample Selection

Panel A: Insider Share Pledging Sample

	Number of firm-year observations
Number of proxy statements (DEF 14A) that are available in the SeekEdgar Database that contain pledging-related keywords from 1 January 2007[a] to 31 December 2019	13,664
Less: Observations that do not have share pledging	(11,128)
Less: Observations that are missing identifiers	(531)
Less: Observations that do not have Compustat data	(201)
Share pledging firm-years	1804

[a]I start from 1 January 2007 because 2007 is the first calendar year in which all firms were required to disclose pledging activities. Therefore, the first fiscal year in my sample is 2006.

Panel B: Hypothesis Testing Sample

	Number of firm-year observations
All Compustat firm-year observations for fiscal years 2006 to 2019	157,028
Less: Observations that have insufficient data to estimate control variables	(85,736)
Less: Observations that do not have I/B/E/S data	(21,331)
Final sample	49,961

Table 3.2 reports the descriptive statistics for the variables that are used in the main analyses. The pledge indicator averages 0.036, which shows that 3.6% of the firms in Compustat have insider share pledging. Across the 1,804 pledging firm-years, the average pledge ratio, calculated as the aggregated number of shares that is pledged relative to the aggregated number of shares that is owned by the named executives and directors, is 0.352. This suggests that managers pledge, on average, 35.2% of their shares. I observe non-GAAP earnings disclosures in 38.1% of the firm-year observations. Across the sample, 24% of quarters during the fiscal year have non-GAAP reporting. Non-GAAP exclusion has a mean value of 0.03. The statistics of the non-GAAP measures are similar to those in prior studies (Hsu et al., 2021; Kolev et al., 2008). For example, Hsu et al. (2021) report that firms disclose non-GAAP

earnings in 27.5% of quarters during their sample period from 2003 to 2016 (Table 1, Hsu et al., 2021, p. 46).

Table 3.2: Descriptive Statistics

Variable	*N*	Mean	p50	SD	p25	p75
Non-GAAP	49,961	0.381	0.000	0.486	0.000	1.000
FreqNonGaap	49,961	0.240	0.000	0.355	0.000	0.500
Pledge Dummy	49,961	0.036	0.000	0.187	0.000	0.000
Pledge Ratio	1,804	0.352	0.287	0.255	0.154	0.495
Total Exclusion	14,810	0.030	0.010	0.083	0.001	0.034
Size	49,961	7.033	7.020	2.193	5.524	8.449
Leverage	49,961	0.559	0.543	0.291	0.350	0.737
BTM	49,961	0.291	0.249	0.239	0.132	0.399
Earnings Volatility	49,961	0.098	0.026	0.251	0.010	0.077
Intangible	49,961	0.164	0.070	0.201	0.005	0.269
Loss	49,961	0.330	0.000	0.470	0.000	1.000
High Tech	49,961	0.292	0.000	0.455	0.000	1.000
SPI	49,961	0.987	1.000	0.113	1.000	1.000
Special Items	49,961	0.015	0.001	0.053	0.000	0.010
Big Bath	49,961	0.197	0.000	0.397	0.000	0.000
Sales Growth	49,961	0.118	0.049	0.542	–0.015	0.201

Note. This table reports the descriptive statistics for the variables that are used in the main analyses. The sample period runs from 2006 to 2019. Detailed definitions of the variables are in Appendix B. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

3.6 Main Results

3.6.1 Share Pledging and Likelihood and Frequency of Non-GAAP Reporting

H1 predicts that share pledging will have no effect on the propensity and the frequency of non-GAAP reporting. Table 3.3 presents the impact of share pledging on the likelihood and the frequency of non-GAAP reporting from estimating Equations (1a) and (1b). The dependent variable in Columns (1) and (2) is the non-GAAP indicator variable and the dependent variable in Columns (3) and (4) is the percentage of quarters in a year in which managers report non-GAAP earnings.

Table 3.3: Share Pledging and Propensity and Frequency of Non-GAAP Reporting

	(1)	(2)	(3)	(4)
	Non-GAAP		*FreqNonGaap*	
Pledge Dummy	0.513***		0.066***	
	(5.55)		(4.36)	
Pledge Ratio		1.210***		0.156***
		(4.39)		(3.45)
Size	0.115***	0.116***	0.024***	0.024***
	(7.83)	(7.90)	(11.13)	(11.18)
Leverage	–0.076	–0.078	–0.021	–0.021
	(–0.74)	(–0.75)	(–1.57)	(–1.58)
BTM	–0.484***	–0.488***	–0.074***	–0.074***
	(–4.23)	(–4.27)	(–4.94)	(–4.98)
Earnings Volatility	–1.035***	–1.036***	–0.059***	–0.059***
	(–9.21)	(–9.21)	(–8.48)	(–8.48)
Intangible	1.768***	1.767***	0.284***	0.284***
	(14.30)	(14.29)	(13.99)	(13.98)
Loss	–0.674***	–0.675***	–0.060***	–0.060***
	(–12.02)	(–12.03)	(–8.70)	(–8.71)
High Tech	0.128	0.125	0.068***	0.068***
	(1.28)	(1.25)	(4.48)	(4.46)
SPI	0.331**	0.334**	0.061***	0.061***
	(2.50)	(2.53)	(3.37)	(3.39)
Special Items	2.506***	2.502***	0.315***	0.315***
	(9.75)	(9.74)	(8.36)	(8.35)
Big Bath	0.685***	0.684***	0.065***	0.065***
	(12.96)	(12.96)	(9.76)	(9.76)
Sales Growth	0.014	0.015	0.011***	0.011***
	(0.61)	(0.63)	(2.86)	(2.87)
Intercept	–3.397***	–3.400***	–0.196***	–0.197***
	(–9.64)	(–9.63)	(–6.62)	(–6.63)
Year fixed effect	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes
N	49,961	49,961	49,961	49,961
Pseudo R^2/Adj. R^2	0.128	0.128	0.167	0.167

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents an analysis of the association between share pledging and the propensity and the frequency of non-GAAP reporting. Columns (1) and (2) report the results for when the dependent variable *Non-GAAP* is an indicator variable that is coded 1 if the firm discloses non-GAAP earnings in any quarter during year *t* and 0 otherwise. Columns (3) and (4) present the results for when the dependent variable *FreqNonGaap* is the percentage of quarters in year *t* in which managers disclose non-GAAP earnings. Detailed definitions of the variables are in Appendix B. The robust standard errors are clustered by firms. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

The coefficient of *Pledge Dummy* in Column (1) is positive and significant at the 1% level ($\beta_1 = 0.513$, *z*-statistic = 5.55), which suggests that the existence of insider share pledging increases the propensity of non-GAAP reporting. In Column (3), the coefficient of *Pledge Dummy* is also significant and positive at the 1% level ($\beta_1 = 0.066$, *z*-statistic = 4.36), which suggests that share pledging is associated with more frequent non-GAAP earnings disclosures.

Prior studies conjecture that examining the effects of the percentage of shares that is pledged by insiders has more economic significance (Pang & Wang, 2020). Therefore, I present the findings for *Pledge Ratio* in Columns (2) and (4). The coefficients of *Pledge Ratio* are positive and significant at the 1% level in the two columns, which suggests that a higher percentage of shares that is pledged by managers is associated with a higher likelihood and frequency of non-GAAP reporting. Economically, the coefficient of *Pledge Ratio* in Column (4) suggests that a 1% increase in shares that are pledged by managers is associated with a 0.156% increase in the frequency of non-GAAP earnings disclosures.

The signs of the estimated coefficients of the control variables are generally consistent with the existing literature. For example, larger firms and firms that have higher levels of intangibles are more likely to disclose non-GAAP earnings. The existence and the dollar amount of special items increase the propensity to report non-GAAP earnings. *Big Bath* is positively associated with the likelihood of non-GAAP reporting.

Overall, the findings from Table 3.3 provide evidence that the existence of share pledging and the percentage of shares that is pledged by managers are positively associated with the propensity and the frequency of non-GAAP reporting.

3.6.2 Share Pledging and Quality of Non-GAAP Reporting

My second hypothesis predicts that share pledging would have no effect on the quality of non-GAAP reporting. I present the results from estimating Equation (2) in Table 3.4. Columns (1) and (2) report the results from using *Pledge Dummy* and *Pledge Ratio*, respectively.

Consistently with prior studies (Abdel-Meguid et al., 2021; Doyle et al., 2003; Kolev et al., 2008), the coefficients of *Total Exclusion* in the two columns are significant and negative (Column (1): $\beta_1 = -0.419$, *t*-statistic = −2.13; Column (2): $\beta_1 = -0.425$, *t*-statistic = −2.17), which indicates that the excluded items are more likely to be transitory than recurring. The estimated coefficients of the variables of interest, *Pledge Dummy* × *Total Exclusion* and *Pledge Ratio* × *Total Exclusion*, are significant and negative at the 5% and 1% levels, respectively. These findings suggest that the non-GAAP exclusions are of a lower quality (less transitory) for firms that have insider share pledging, which is consistent with the view that managers who have shares under pledge tend to report non-GAAP earnings opportunistically.

Table 3.4: Share Pledging and Quality of Non-GAAP Reporting

	(1) *FOI*	(2) *FOI*
Pledge Dummy	0.025	
	(0.25)	
Pledge Ratio		0.323
		(1.06)
Total Exclusion	–0.419**	–0.425**
	(–2.13)	(–2.17)
Pledge Dummy × *Total Exclusion*	**–3.636****	
	(–2.24)	
Pledge Ratio × *Total Exclusion*		**–13.54*****
		(–3.01)
Size	0.182***	0.181***
	(9.73)	(9.68)
BTM	–0.800***	–0.798***
	(–8.53)	(–8.51)
Earnings Volatility	–0.163	–0.159
	(–0.75)	(–0.73)
Loss	–0.777***	–0.774***
	(–14.12)	(–14.10)
Age	0.210***	0.210***
	(6.85)	(6.85)
Sales Growth	0.274***	0.274***
	(5.99)	(5.98)
NonGaapEPS	1.373***	1.374***
	(14.14)	(14.14)
Intercept	–1.649***	–1.636***
	(–5.26)	(–5.29)
Year fixed effect	Yes	Yes
Industry fixed effect	Yes	Yes
N	14,810	14,810
Pseudo R^2	0.467	0.467

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents an analysis of the association between share pledging and the quality of non-GAAP exclusions. The dependent variable *FOI* is the earnings per share from operations in year $t + 1$. The independent variable *Pledge Dummy* in Column (1) is an indicator variable that is coded 1 if the named executives and directors in a firm pledge shares and 0 otherwise. The independent variable *Pledge Ratio* in Column (2) is the ratio of the aggregated number of shares that is pledged relative to the aggregated number of shares that is owned by the named executives and directors. *Total Exclusion* is the non-GAAP earnings per share minus the GAAP earnings per share summed over the four quarters in year *t*. Detailed definitions of the variables are in Appendix B. The robust standard errors are clustered by firms. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

The coefficients of the control variables are generally consistent with prior studies. For example, larger and more mature firms and firms that have better growth opportunities are more likely to report higher levels of operating income in the future. *Non-GAAP Earnings* is significant and positively associated with future earnings.

3.6.3 Prominence of Non-GAAP Earnings

The prominence of non-GAAP earnings can reflect the informative and opportunistic incentives of managers (Bowen et al., 2005). Brown et al. (2012) posit that sentiment-driven managers tend to prioritise non-GAAP earnings over GAAP earnings because of their excessively positive beliefs about firm performance during optimistic periods. Their results show that managers place non-GAAP earnings before GAAP earnings when non-GAAP earnings can be used to meet earnings benchmarks whereas GAAP earnings cannot. This partially provides evidence of managerial opportunism in non-GAAP reporting.

Following prior studies, I measure prominence as an indicator variable that is coded 1 if non-GAAP earnings are reported before GAAP earnings in earnings press releases and 0 otherwise (Bowen et al., 2005; Brown et al., 2012; Chen et al., 2021). The data on prominence is available from Chen et al. (2021). Then, I replace the dependent variable *CONSENSUS* in Equation (3a) with the *PROMINENCE* indicator variable to explore whether share pledging has any impact on the prominence of non-GAAP reporting.

The results in Table 3.5 indicate a positive relationship between stock pledging and the prominence of non-GAAP reporting. In Column (1), the coefficient of *Pledge Dummy* is significant and positive at the 5% level (β_1 = 0.299, *z*-statistic = 2.38), which suggests that pledging firms are more likely to highlight non-GAAP earnings than non-pledging firms. Furthermore, I find that the percentage of shares that is pledged is positively associated with the prominence of non-GAAP earnings disclosures. Overall, these findings provide evidence that managers who have shares under pledge tend to place non-GAAP earnings prominently.

Table 3.5: Share Pledging and Non-GAAP Prominence

	(1) *PROMINENCE*	(2) *PROMINENCE*
Pledge Dummy	0.299**	
	(2.38)	
Pledge Ratio		0.671*
		(1.91)
Size	0.122***	0.123***
	(6.26)	(6.32)
Leverage	0.306**	0.303**
	(2.02)	(2.00)
BTM	–0.207	–0.211
	(–1.14)	(–1.17)
Earnings Volatility	–0.510**	–0.510**
	(–2.47)	(–2.47)
Intangible	1.558***	1.556***
	(9.76)	(9.76)
Loss	–0.572***	–0.574***
	(–5.54)	(–5.55)
High Tech	0.094	0.092
	(0.75)	(0.73)
SPI	0.283	0.285
	(1.10)	(1.11)
Special Items	2.238***	2.236***
	(7.14)	(7.13)
Big Bath	0.435***	0.435***
	(4.34)	(4.34)
Sales Growth	0.010	0.010
	(0.30)	(0.30)
RET	–0.153	–0.155
	(–1.22)	(–1.24)
Institutional Ownership	0.914***	0.915***
	(8.67)	(8.69)
Intercept	–5.364***	–5.369***
	(–10.88)	(–10.88)
Year fixed effect	Yes	Yes
Industry fixed effect	Yes	Yes
N	36,105	36,105
Pseudo R^2	0.092	0.091

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents the analysis of the association between share pledging and the prominence of non-GAAP earnings disclosures. The dependent variable *PROMINENCE* is an indicator variable that is coded 1 if non-GAAP earnings are mentioned before GAAP earnings in press releases in any quarter during year *t* and 0 otherwise. The independent variable *Pledge Dummy* in Column (1) is an indicator variable that is coded 1 if the named executives and directors in a firm pledge shares and 0 otherwise. The independent variable *Pledge Ratio* in Column (2) is the ratio of the aggregated number of shares that is pledged relative to the aggregated number of shares that is

owned by named executives and directors. Detailed definitions of the variables are in Appendix B. The robust standard errors are clustered by firms. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

3.6.4 Margin-Call Pressure Hypothesis

This study argues that pledging managers have incentives to counteract the potential margin-call pressure from pledged shares by strategically reporting non-GAAP earnings. To test the margin-call pressure hypothesis, I follow Chan et al. (2018) and use *Pledge under Pressure* as a proxy for the margin-call threat. Specifically, *Pledge under Pressure* is the pledge ratio if the firm's previous three-month return is below −15% and 0 otherwise.[18] Then, I replace the independent variables *Pledge Dummy* and *Pledge Ratio* in Equations (1a) and (1b) with *Pledge under Pressure* to investigate whether pledging managers under margin-call pressure are more likely to report non-GAAP earnings.

Table 3.6 shows that the coefficients of *Pledge under Pressure* are significant and positive at the 1% level in the two columns, which suggests that managers are more likely to report non-GAAP earnings when there is a considerable amount of margin-call pressure. These findings indicate that margin-call pressure from pledged shares encourages managers to disclose non-GAAP earnings to secure share prices.

[18] The findings are similar if I use −10% as the threshold.

Table 3.6: Share Pledging and Non-GAAP Reporting: Margin-Call Hypothesis

	(1) *Non-GAAP*	(2) *FreqNonGaap*
Pledge under Pressure	2.481***	0.363***
	(4.49)	(3.91)
Size	0.117***	0.0245***
	(7.99)	(11.28)
Leverage	–0.082	–0.022
	(–0.79)	(–1.61)
BTM	–0.495***	–0.075***
	(–4.33)	(–5.03)
Earnings Volatility	–1.038***	–0.059***
	(–9.22)	(–8.48)
Intangible	1.771***	0.285***
	(14.31)	(14.01)
Loss	–0.679***	–0.060***
	(–12.12)	(–8.79)
High Tech	0.121	0.0673***
	(1.21)	(4.43)
SPI	0.337**	0.062***
	(2.56)	(3.42)
Special Items	2.507***	0.315***
	(9.74)	(8.35)
Big Bath	0.686***	0.065***
	(12.98)	(9.77)
Sales Growth	0.016	0.011***
	(0.70)	(2.94)
Intercept	–3.409***	–0.198***
	(–9.63)	(–6.66)
Year fixed effect	Yes	Yes
Industry fixed effect	Yes	Yes
N	49,961	49,961
Pseudo R^2/Adj. R^2	0.127	0.166

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents the analysis of the association between share pledging under pressure and non-GAAP reporting. Column (1) reports the results for when the dependent variable *Non-GAAP* is an indicator variable that is coded 1 if the firm discloses non-GAAP earnings in any quarter during year *t* and 0 otherwise. Column (2) presents the results for when the dependent variable *FreqNonGaap* is the percentage of quarters in year *t* in which managers disclose non-GAAP earnings. The independent variable *Pledge under Pressure* is the pledge ratio if the firm's previous three-month return is below –15% and 0 otherwise. Detailed definitions of the variables are in Appendix B. The robust standard errors are clustered by firms. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

3.7 Cross-Sectional Analyses

3.7.1 Analyst Coverage

The main findings show that share pledging is positively associated with the likelihood of non-GAAP reporting and negatively related to the quality of non-GAAP exclusions. Next, I perform analyses to investigate the cross-sectional differences of share pledging on the likelihood and the quality of non-GAAP earnings disclosures. Exploiting an unanticipated decrease in analyst coverage, Christensen et al. (2021) show that managers are more aggressive in non-GAAP reporting and the quality of non-GAAP earnings is lower after a reduction in analyst following, which indicates that analyst coverage can constrain managerial opportunism in non-GAAP reporting. Therefore, I expect the effect of share pledging on the propensity and the quality of non-GAAP reporting to be more pronounced for firms that have low analyst coverage.

I partition the firms into two subsamples according to their analyst coverage. The analyst coverage is measured as the total number of unique analysts following the firm, which is obtained from the I/B/E/S database. Firms are regarded as low analyst coverage firms if their analyst following is lower than the median value otherwise they are considered high analyst coverage. The findings are presented in Columns (1) and (2) in Panels A and B of Table 3.7. The first two columns of Panel A show that the effect of stock pledges on the propensity of non-GAAP earnings disclosures is more pronounced for firms that have low analyst following because the coefficient of *Pledge Dummy* is significantly larger for firms that have low analyst coverage (β_1 = 0.593, z-statistic = 4.18) than for firms that have high analyst coverage (β_1 = 0.307, z-statistic = 2.77). The Chow test rejects the null hypothesis of no difference between these two coefficients at the 10% level (p-value = 0.088).

Table 3.7: Cross-Sectional Tests

Panel A: Propensity of non-GAAP earnings disclosure

	Analyst coverage		Board independence	
	High	Low	High	Low
	(1)	(2)	(3)	(4)
	Non-GAAP		*Non-GAAP*	
Pledge Dummy	**0.307*****	**0.593*****	**0.034**	**0.756*****
	(2.77)	**(4.18)**	**(0.25)**	**(6.57)**
Size	0.065***	–0.059***	0.332***	0.032*
	(2.81)	(–3.42)	(15.20)	(1.79)
Leverage	0.610***	–0.059	–0.177	0.154
	(4.11)	(–0.44)	(–1.21)	(1.21)
BTM	0.073	–0.146	–0.022	–0.591***
	(0.40)	(–1.10)	(–0.14)	(–4.04)
Earnings Volatility	–0.589***	–1.300***	–0.731***	–1.210***
	(–3.74)	(–7.60)	(–5.08)	(–7.90)
Intangible	1.688***	1.604***	1.281***	2.066***
	(9.91)	(9.86)	(7.37)	(13.41)
Loss	–0.696***	–0.696***	–0.617***	–0.622***
	(–8.37)	(–9.30)	(–7.77)	(–8.47)
High Tech	0.232*	–0.123	0.644***	–0.192
	(1.81)	(–0.95)	(5.04)	(–1.54)
SPI	0.105	0.360**	0.138	0.365*
	(0.61)	(1.98)	(0.77)	(1.93)
Special Items	4.423***	1.528***	2.586***	2.388***
	(7.63)	(5.14)	(7.00)	(6.86)
Big Bath	0.561***	0.835***	0.693***	0.608***
	(6.88)	(12.15)	(9.06)	(8.61)
Sales Growth	–0.029	0.049	–0.071*	0.064**
	(–0.92)	(1.37)	(–1.79)	(2.17)
Intercept	–3.087***	–2.597***	–4.286***	–3.261***
	(–6.28)	(–6.52)	(–9.98)	(–7.99)
Year fixed effect	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes
N	23,685	26,275	20,420	29,541
Pseudo R^2	0.115	0.113	0.169	0.135
Test of difference in Pledge Dummy (*p*-value): (1) − (2) or (3) − (4)	–0.286* (0.088)		–0.722*** (0.000)	

Panel B: Quality of Non-GAAP Exclusions

	Analyst coverage		Board independence	
	High	Low	High	Low
	(1)	(2)	(3)	(4)
	FOI		*FOI*	
Pledge Dummy	–0.028	0.116	–0.047	0.068
	(–0.21)	(0.88)	(–0.28)	(0.63)
Total Exclusion	–1.091**	–0.174	–0.235	–0.675**
	(–2.09)	(–1.07)	(–1.07)	(–2.15)
Pledge Dummy ×	**–1.913**	**–6.363*** **	**–2.389**	**–4.588****
Total Exclusion	**(–1.01)**	**(–3.29)**	**(–1.26)**	**(–2.28)**
Size	0.194***	0.132***	0.161***	0.198***
	(6.42)	(5.37)	(7.69)	(7.06)
BTM	–1.383***	–0.584***	–0.729***	–0.884***
	(–7.42)	(–6.19)	(–6.69)	(–6.56)
Earnings Volatility	–0.948***	–0.126	–0.144	–0.295
	(–2.74)	(–0.51)	(–0.47)	(–1.22)
Loss	–0.833***	–0.776***	–0.711***	–0.814***
	(–9.03)	(–12.14)	(–9.20)	(–10.94)
Age	0.239***	0.180***	0.185***	0.220***
	(4.71)	(5.22)	(4.52)	(5.38)
Sales Growth	0.025***	0.025***	0.030***	0.024***
	(3.95)	(4.47)	(3.96)	(4.47)
NonGaapEPS	1.459***	1.157***	1.512***	1.275***
	(11.71)	(7.72)	(10.97)	(10.00)
Intercept	–1.530***	–1.384**	–1.712***	–1.551***
	(–4.30)	(–2.51)	(–5.20)	(–3.18)
Year fixed effect	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes
N	7,390	7,420	7,405	7,405
Adj R^2	0.485	0.393	0.490	0.452
Test of difference in Pledge Dummy × Total Exclusion (*P*-value) (1) − (2) or (3) − (4)	4.450*** (0.000)		2.199*** (0.000)	

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents the results for the impact of share pledging on the propensity of non-GAAP earnings disclosures (Panel A) and the quality of non-GAAP exclusions (Panel B) by subsamples of analyst coverage and board independence. Firms are classified as low analyst coverage firms if their analyst following is lower than the median value. Firms are regarded as low board independence firms if their percentage of independent directors is below the median value. Detailed definitions of the variables are in Appendix B. The robust standard errors are clustered by firms. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

Columns (1) and (2) of Panel B present the impact of share pledging on the quality of non-GAAP exclusions in these two subsamples. Similarly, I find that the coefficient of *Pledge Dummy* × *Total Exclusion* for low analyst coverage firms (β_3 = −6.363, *t*-statistic = −3.29) in Column (2) is more negative than for high analyst coverage firms (β_3 = −1.913, *t*-statistic = −1.01) in Column (1). The Chow test rejects the null hypothesis of no difference between these two coefficients at the 1% level (*p*-value = 0.000). In summary, the findings from Columns (1) and (2) in Panels A and B indicate that share pledging has a negative impact on the quality of non-GAAP earnings disclosures for firms that have low analyst coverage only.

3.7.2 Board Independence

Strong corporate governance can deter managers' opportunistic use of non-GAAP earnings (Frankel et al., 2011). Specifically, Frankel et al. (2011) find that the non-GAAP exclusions are of a lower quality for firms that have fewer independent boards, which indicates that board independence can constrain opportunism in voluntary non-GAAP disclosures. Therefore, I conjecture that the effect of share pledging on the likelihood and the quality of non-GAAP reporting is stronger for firms that have fewer independent boards.

I obtain board of director independence information from BoardEx. Following Frankel et al. (2011), I measure board independence as the number of independent directors relative to the total number of directors. I split the sample into two groups according to board independence. Firms are classified as low board independence firms if their percentage of independent directors is below the median value. I re-estimate Equations (1) and (2) for these two subsamples and present the results in Columns (3) and (4) in Panels A and B of Table 3.7.

In line with my prediction, in the subsample of low board independence firms, the coefficient of *Pledge Dummy* in Column (4) of Panel A is significant and positive at the 1% level (β_1 = 0.756, *z*-statistic = 6.57) and the coefficient of *Pledge Dummy* × *Total Exclusion* in Column (4) of Panel B is significant and negative at the 5% level (β_3 = −4.588, *t*-

statistic = −2.28). However, the coefficients of the variables of interest are insignificant for firms that have higher independent boards. The Chow test rejects the null hypothesis of no difference between these two coefficients at the 1% level (p-value = 0.000). Overall, these findings show that the effect of share pledging on the likelihood and the quality of non-GAAP earnings disclosures is more pronounced for firms that have fewer independent boards.

3.8 Robustness Tests

3.8.1 Alternative Measures of Non-GAAP Reporting Quality

The main tests use the quality of excluded items as a proxy for the quality of non-GAAP earnings. In this section, I consider two alternative measures of non-GAAP reporting quality that have been widely documented in the existing literature and are considered to capture the aggressiveness of non-GAAP reporting (Black & Christensen, 2009; Christensen et al., 2019, 2021).

My first alternative measure of non-GAAP reporting quality is the likelihood that managers would use non-GAAP earnings to meet or to beat analysts' forecasts. Prior studies find evidence that non-GAAP earnings are used by managers to meet or to beat analysts' expectations when GAAP earnings fall short (Black et al., 2017; Christensen et al., 2021). Following this logic, I apply the following logit model to investigate whether pledging managers are more likely to use non-GAAP earnings to meet or to beat analysts' consensus forecasts when their GAAP earnings miss the forecasts.

$$\begin{aligned} Prob(CONSENSUS)_{it} = {} & \beta_0 + \beta_1 Pledge_{it} + \beta_2 Size_{it} + \beta_3 Leverage_{it} + \beta_4 BTM_{it} \\ & + \beta_5 Earnings\ Volatility_{it} + \beta_6 Intangible_{it} + \beta_7 Loss_{it} + \beta_8 High\ Tech_{it} \\ & + \beta_9 SPI_{it} + \beta_{10} Special\ Items_{it} + \beta_{11} Big\ Bath_{it} + \beta_{12} Sales\ Growth_{it} \\ & + \beta_{13} RET_{it} + \beta_{14} Institutional\ Ownership_{it} + Year\ Fixed\ Effects \\ & + Industry\ Fixed\ Effects + \varepsilon_{it} \end{aligned} \quad (3a)$$

where *CONSENSUS* is an indicator variable that is coded 1 if GAAP earnings fall short of the analysts' consensus forecast while non-GAAP earnings meet or beat the forecast in any quarter during year *t*. Following Christensen et al. (2021), I also control for stock return (*RET*) and institutional ownership (*Institutional Ownership*) in Equation (3a). Data on stock return and institutional ownership are obtained from CRSP and Thomson Reuters Institutional Shareholdings 13f file, respectively. *RET* is the cumulative monthly stock return during the past 12 months in fiscal year *t*. *Institutional Ownership* is calculated as the number of shares that is held by institutional shareholders divided by the total number of shares where the total number of shares is obtained from Compustat. All the other variables are as defined in Appendix B.

Table 3.8 shows a significant and positive association between the two measures of pledging (*Pledge Dummy* and *Pledge Ratio*) and *CONSENSUS*. Specifically, the coefficient of *Pledge Dummy* in Column (1) is positive and significant at the 5% level ($\beta_1 = 0.457$, *z*-statistic = 2.09), which indicates that managers who pledge shares are more likely to use non-GAAP earnings to meet or to beat analysts' consensus forecasts when firms' GAAP earnings miss the earnings benchmark. This evidence is consistent with my primary findings that share pledging is negatively associated with the quality of non-GAAP reporting.

Table 3.8: Robustness Test: Meeting/Beating Analyst Consensus Forecast

	(1) *CONSENSUS*	(2) *CONSENSUS*
Pledge Dummy	0.457**	
	(2.09)	
Pledge Ratio		1.102*
		(1.79)
Size	–0.309***	–0.307***
	(–6.66)	(–6.64)
Leverage	0.896***	0.892***
	(3.71)	(3.69)
BTM	0.775***	0.768***
	(2.91)	(2.88)
Earnings Volatility	–0.044	–0.041
	(–0.15)	(–0.14)
Intangible	–1.842***	–1.845***
	(–6.89)	(–6.90)
Loss	4.597***	4.595***
	(19.74)	(19.71)
High Tech	0.559***	0.558***
	(2.85)	(2.85)
SPI	–0.244	–0.238
	(–0.58)	(–0.57)
Special Items	–0.369	–0.372
	(–0.79)	(–0.79)
Big Bath	–0.013	–0.013
	(–0.13)	(–0.13)
Sales Growth	–0.484***	–0.481***
	(–4.18)	(–4.14)
RET	–0.005	–0.08
	(–0.02)	(–0.04)
Institutional Ownership	0.157**	0.155**
	(2.21)	(2.18)
Intercept	–3.019***	–3.039***
	(–4.46)	(–4.47)
Year fixed effect	Yes	Yes
Industry fixed effect	Yes	Yes
N	14,266	14,266
Pseudo R^2	0.444	0.444

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents the results for using alternative measures of non-GAAP reporting quality. The dependent variable *CONSENSUS* is an indicator variable that is coded 1 if the non-GAAP earnings meet or beat the analysts' consensus forecast while GAAP earnings fall short and 0 otherwise. The independent variable *Pledge Dummy* in Column (1) is an indicator variable that is coded 1 if the named executives and directors in a firm pledge shares and 0 otherwise. The independent variable *Pledge Ratio* in Column (2) is the ratio of the aggregated number of shares that is pledged relative to the aggregated number of shares that is owned by the named executives and

directors. Detailed definitions of the variables are in Appendix B. The robust standard errors are clustered by firms. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

My second alternative measure of non-GAAP reporting quality is managers' propensity to exclude recurring items. The exclusion of recurring items is generally regarded as aggressive non-GAAP reporting, which indicates a lower non-GAAP disclosure quality (Chen et al., 2021). I apply the following logit model to investigate whether managers that pledge shares are more likely to exclude recurring items when measuring non-GAAP earnings.

$$\begin{aligned} Prob(RECUR)_{it} = {} & \beta_0 + \beta_1 Pledge_{it} + \beta_2 Size_{it} + \beta_3 Leverage_{it} + \beta_4 BTM_{it} \\ & + \beta_5 Earnings\ Volatility_{it} + \beta_6 Intangible_{it} + \beta_7 Loss_{it} + \beta_8 High\ Tech_{it} \\ & + \beta_9 SPI_{it} + \beta_{10} Special\ Items_{it} + \beta_{11} Big\ Bath_{it} + \beta_{12} Sales\ Growth_{it} \\ & + \beta_{13} RET_{it} + \beta_{14} Institutional\ Ownership_{it} + Year\ Fixed\ Effects \\ & + Industry\ Fixed\ Effects + \varepsilon_{it} \end{aligned} \quad (3b)$$

Following Chen et al. (2021), I define *RECUR* as an indicator variable that is coded 1 if non-GAAP earnings exclude recurring items and 0 otherwise. Recurring items include research and development costs, depreciation and amortisation costs and stock-based compensation costs. The control variables are the same as those in Equation (3a).

Table 3.9 documents a significant and positive relationship between share pledging and *RECUR*. Specifically, the coefficients of *Pledge Dummy* (β_1 = 0.346, *z*-statistic = 3.39) and *Pledge Ratio* (β_1 = 0.866, *z*-statistic = 2.89) are significant and positive at the 1% level. These findings suggest that managers who have shares under pledge are more likely to exclude recurring items from non-GAAP earnings, which indicates that non-GAAP disclosure quality decreases after share pledging.

Table 3.9: Robustness Test: Likelihood of Excluding Recurring Items

	(1) *RECUR*	(2) *RECUR*
Pledge Dummy	0.346***	
	(3.39)	
Pledge Ratio		0.866***
		(2.89)
Size	0.122***	0.123***
	(7.78)	(7.84)
Leverage	0.090	0.0880
	(0.76)	(0.74)
BTM	–0.196	–0.198
	(–1.54)	(–1.56)
Earnings Volatility	–0.795***	–0.795***
	(–6.48)	(–6.48)
Intangible	1.488***	1.487***
	(10.82)	(10.81)
Loss	–0.540***	–0.541***
	(–8.41)	(–8.42)
High Tech	0.342***	0.340***
	(3.33)	(3.31)
SPI	0.109	0.111
	(0.72)	(0.74)
Special Items	2.492***	2.491***
	(8.21)	(8.20)
Big Bath	0.601***	0.602***
	(9.64)	(9.64)
Sales Growth	–0.035	–0.035
	(–1.35)	(–1.35)
RET	0.150*	0.149*
	(1.72)	(1.71)
Institutional Ownership	1.662***	1.663***
	(20.42)	(20.43)
Intercept	–3.944***	–3.951***
	(–10.98)	(–10.99)
Year fixed effect	Yes	Yes
Industry fixed effect	Yes	Yes
N	36,105	36,105
Pseudo R^2	0.147	0.147

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents the results for using alternative measures of non-GAAP reporting quality. The dependent variable *RECUR* is an indicator variable that is coded 1 if the non-GAAP earnings exclude recurring items and 0 otherwise. The independent variable *Pledge Dummy* in Column (1) is an indicator variable that is coded 1 if the named executives and directors in a firm pledge shares and 0 otherwise. The independent variable *Pledge Ratio* in Column (2) is the ratio of the aggregated number of shares that is pledged relative to the aggregated number of shares that is owned by the named executives and directors. Detailed definitions of the variables are in Appendix

B. The robust standard errors are clustered by firms. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

3.8.2 Controlling for Earnings Management

The existing literature documents that stock pledging is positively associated with earnings management (Singh, 2018). To investigate whether the relationship between share pledging and non-GAAP reporting still holds after controlling for earnings management, I use discretionary accruals (DTA) as the proxy for earnings management and incorporate the variable *DTA* into Equations (1) and (2). Discretionary accruals are estimated by using the modified Jones model (Kothari et al., 2005).

Table 3.10 shows that the coefficient estimates of *Pledge Dummy* and *Pledge Ratio* are positive and significant at the 1% level in all the columns, which suggests that managers who have shares under pledge are more likely to report non-GAAP earnings and to make more frequent non-GAAP disclosures. Thus, the inference about the significant and positive association between share pledging and the likelihood and the frequency of non-GAAP earnings disclosures remains the same.

Table 3.10: Robustness Test: Controlling for Earnings Management

	(1)	(2)	(3)	(4)
	Non-GAAP		*FreqNonGaap*	
Pledge Dummy	0.636***		0.087***	
	(5.35)		(4.57)	
Pledge Ratio		1.690***		0.237***
		(4.73)		(4.14)
Size	0.159***	0.160***	0.031***	0.031***
	(9.40)	(9.47)	(12.29)	(12.35)
Leverage	–0.100	–0.102	–0.025*	–0.026*
	(–0.94)	(–0.96)	(–1.84)	(–1.86)
BTM	–0.568***	–0.572***	–0.088***	–0.088***
	(–4.79)	(–4.83)	(–5.63)	(–5.66)
Earnings Volatility	–1.011***	–1.011***	–0.057***	–0.057***
	(–8.58)	(–8.58)	(–7.85)	(–7.85)
Intangible	1.605***	1.603***	0.260***	0.260***
	(12.51)	(12.49)	(12.29)	(12.28)
Loss	–0.664***	–0.664***	–0.057***	–0.057***
	(–10.71)	(–10.69)	(–7.22)	(–7.22)
High Tech	0.162	0.160	0.075***	0.075***
	(1.58)	(1.55)	(4.80)	(4.77)
SPI	0.322**	0.326**	0.062***	0.063***
	(2.38)	(2.41)	(3.55)	(3.58)
Special Items	2.474***	2.473***	0.305***	0.305***
	(9.21)	(9.21)	(7.75)	(7.75)
Big Bath	0.724***	0.723***	0.071***	0.071***
	(12.70)	(12.66)	(9.78)	(9.74)
Sales Growth	0.002	0.002	0.010**	0.011**
	(0.09)	(0.12)	(2.54)	(2.56)
DTA	0.005	0.005	0.001	0.001
	(0.42)	(0.41)	(0.73)	(0.73)
Intercept	–3.582***	–3.589***	–0.242***	–0.243***
	(–9.82)	(–9.81)	(–7.53)	(–7.53)
Year fixed effect	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes
N	40,460	40,460	40,460	40,460
Pseudo R^2/Adj. R^2	0.135	0.134	0.170	0.170

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table reports the robustness test of controlling for earnings management. Columns (1) and (2) report the results for when the dependent variable *Non-GAAP* is an indicator variable that is coded 1 if the firm discloses non-GAAP earnings in any quarter during year *t* and 0 otherwise. Columns (3) and (4) present the results for when the dependent variable *FreqNonGaap* is the percentage of quarters in year *t* in which managers disclose non-GAAP earnings. Detailed definitions of the variables are in Appendix B. The robust standard errors are clustered by firms. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

Consistently with the primary finding, the untabulated results show that share pledging negatively affects the quality of non-GAAP exclusions after controlling for earnings management. Specifically, the estimated coefficients of the interaction terms (*Pledge Dummy* × *Total Exclusion* and *Pledge Ratio* × *Total Exclusion*) are significant and negative at the 1% level.

3.8.3 Excluding Financial and Utility Firms

To investigate whether the primary findings are robust to the exclusion of firms in the financial and utility industries, I exclude financial (SIC 6,000–6,999) and utility companies (SIC 4,900–4,949) and re-estimate Equations (1) and (2). The untabulated results suggest that share pledging is positively associated with the propensity and the frequency of non-GAAP reporting and it negatively affects the quality of non-GAAP exclusions. These findings are similar to the main findings.

3.8.4 Propensity Score Matching Approach

Prior studies conjecture that share pledging arises from managers' own decisions (Anderson & Puleo, 2020) thus some confounding factors may affect the pledging activities and the disclosure of non-GAAP earnings. For example, CEO personality traits (e.g. overconfidence) can encourage them to become involved in share pledging and non-GAAP reporting. To improve the covariate balance between the pledging and control firms, I explore the PSM approach (Pang & Wang, 2020).

I perform a one-to-one nearest-neighbour matching without replacement. First, I estimate a logit model that has the pledging indicator as the dependent variable. The pledging dummy is then regressed on all the control variables in the baseline model and dividend yield and board independence because these two variables can explain insider share pledging (Anderson & Puleo, 2020). This matching process yields 1,753 pledging firm-year observations and 1,753

non-pledging firm-year observations. The covariate balances after matching are reported in Appendix C.

As is shown in Table 3.11, the coefficient of *Pledge Dummy* in Column (1) is positive and significant at the 1% level (β_1 = 0.376, *z*-statistic = 3.66), which suggests that firms that have insider share pledging are more likely to report non-GAAP earnings than control firms. Further, I observe a significant and positive coefficient for *Pledge Dummy* in Column (2), which provides evidence that pledging firms make more frequent non-GAAP earnings disclosures than control firms. These findings are consistent with the primary findings that share pledging can positively affect the likelihood and the frequency of non-GAAP reporting.

Table 3.11: Propensity Score Matching

	(1) *Non-GAAP*	(2) *FreqNonGaap*
Pledge Dummy	0.376***	0.052***
	(3.66)	(3.12)
Size	0.128***	0.025***
	(3.70)	(4.39)
Leverage	0.269	0.076
	(0.78)	(1.35)
BTM	0.262	0.081
	(0.58)	(1.17)
Earnings Volatility	–0.644*	–0.104*
	(–1.74)	(–1.87)
Intangible	2.000***	0.306***
	(5.57)	(5.18)
Loss	–0.317	–0.012
	(–1.35)	(–0.33)
High Tech	0.993***	0.199***
	(3.99)	(4.45)
SPI	0.527	0.070
	(0.85)	(0.81)
Special Items	4.204***	0.725***
	(2.69)	(3.28)
Big Bath	0.133	–0.009
	(0.56)	(–0.27)
Sales Growth	–0.043	–0.005
	(–0.48)	(–0.30)
Intercept	–3.050***	–0.254**
	(–3.68)	(–2.09)

Year fixed effect	Yes	Yes
Industry fixed effect	Yes	Yes
N	3,506	3,506
Pseudo R^2	0.142	0.178

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents the analysis of the association between share pledging and the propensity and the frequency of non-GAAP reporting by using a propensity score matched sample. Column (1) reports the results for when the dependent variable *Non-GAAP* is an indicator variable that is coded 1 if the firm discloses non-GAAP earnings in any quarter during year *t* and 0 otherwise. Column (2) shows the results for when the dependent variable *FreqNonGaap* is the percentage of quarters in year *t* in which managers disclose non-GAAP earnings. Detailed definitions of the variables are in Appendix B. The robust standard errors are clustered by firms. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

3.8.5 Difference-in-Differences Analysis

In this section, I apply a DID analysis to compare the changes in non-GAAP earnings disclosures for pledging firms and non-pledging firms from one year before the initiation of share pledging to one year after the pledging initiation.

First, I identify the pledging initiation year for each firm that has insider share pledging in my sample. Following Fabisik (2019), I remove observations from the calendar year 2007 because firms that had insider share pledging in 2007 might have initiated pledging long before this but disclosed it for the first time in 2007.[19] This process gives rise to 576 pledging initiations.

Then, I employ PSM to match each of the aforesaid pledging firms to a control firm in the same year and industry by using the one-to-one nearest-neighbour matching. The matching variables are the same as those in section 3.8.4. I apply the following model to estimate the changes in non-GAAP reporting for the pledging firms and the control firms after the initiation of share pledging.

[19] It was not until August 2006 that firms were required by the SEC to disclose share-pledging activities.

$$Prob(Non\text{-}GAAP)_{it} = \beta_0 + \beta_1\, Pledge\ Dummy_{it} + \beta_2\, Post_{it} + \beta_3\, Pledge\ Dummy_{it} \times Post_{it} + \beta_4\, Size_{it} + \beta_5\, Leverage_{it} + \beta_6\, BTM_{it} + \beta_7\, Earnings\ Volatility_{it} + \beta_8\, Intangible_{it} + \beta_9\, Loss_{it} + \beta_{10}\, High\ Tech_{it} + \beta_{11}\, SPI_{it} + \beta_{12}\, Special\ Items_{it} + \beta_{13}\, Big\ Bath_{it} + \beta_{14}\, Sales\ Growth_{it} + Year\ Fixed\ Effects + Industry\ Fixed\ Effects + \varepsilon_{it} \quad (4a)$$

$$FreqNonGaap_{it} = \beta_0 + \beta_1\, Pledge\ Dummy_{it} + \beta_2\, Post_{it} + \beta_3\, Pledge\ Dummy_{it} \times Post_{it} + \beta_4\, Size_{it} + \beta_5\, Leverage_{it} + \beta_6\, BTM_{it} + \beta_7\, Earnings\ Volatility_{it} + \beta_8\, Intangible_{it} + \beta_9\, Loss_{it} + \beta_{10}\, High\ Tech_{it} + \beta_{11}\, SPI_{it} + \beta_{12}\, Special\ Items_{it} + \beta_{13}\, Big\ Bath_{it} + \beta_{14}\, Sales\ Growth_{it} + Year\ Fixed\ Effects + Industry\ Fixed\ Effects + \varepsilon_{it} \quad (4b)$$

where *Non-GAAP* and *FreqNonGaap* represent the non-GAAP reporting indicator and the frequency of non-GAAP reporting, respectively; *Pledge Dummy* is an indicator variable that is coded 1 if a firm has initiated share pledging and 0 otherwise. Specifically, if the firm initiates share pledging in year t, this firm will also be classified as the pledging firm in year t-1 and t+1. *Post* is an indicator variable that is coded 1 if the year is after the firm's initiation of pledging and 0 otherwise.

The variable of interest, *Pledge Dummy* × *Post*, captures the incremental change in the non-GAAP reporting of pledging firms in the post-initiation period of one year after their first pledge relative to the change for control firms during the same period. A significant and positive coefficient of *Pledge Dummy* × *Post* (β_3) indicates that share pledging increases the likelihood and the frequency of non-GAAP reporting.

Table 3.12 shows that the coefficients of *Pledge Dummy* × *Post* (β_3) are positive and statistically significant at the 1% and 5% level, respectively. These findings are consistent with the main findings that share pledging can positively affect the propensity and the frequency of non-GAAP earnings disclosures.

Table 3.12: Difference-in-Differences Analysis

	(1) *Non-GAAP*	(2) *FreqNonGaap*
Pledge Dummy	0.149	0.024
	(0.97)	(1.29)
Post	–0.483***	–0.036*
	(–2.92)	(–1.80)
Pledge Dummy × Post	0.603***	0.054**
	(3.13)	(2.32)
Size	0.313***	0.039***
	(9.15)	(9.70)
Leverage	–0.340	–0.044
	(–1.03)	(–1.24)
BTM	–0.414	–0.067
	(–0.97)	(–1.45)
Earnings Volatility	0.064	0.015
	(0.22)	(0.87)
Intangible	2.188***	0.283***
	(5.37)	(5.14)
Loss	–0.985***	–0.074***
	(–3.64)	(–3.28)
High Tech	1.244***	0.187***
	(4.41)	(4.95)
SPI	–0.120	0.044
	(–0.18)	(0.66)
Special Items	3.318*	0.225**
	(1.91)	(2.26)
Big Bath	1.049***	0.098***
	(3.39)	(3.43)
Sales Growth	–0.008	–0.001
	(–0.85)	(–0.57)
Intercept	–4.312***	–0.252**
	(–3.93)	(–2.48)
Year fixed effect	Yes	Yes
Industry fixed effect	Yes	Yes
N	2,106	2,106
Pseudo R^2	0.203	0.218

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents the results of the DID analyses of the initiation of firms' pledging activities. Column (1) reports the results for when the dependent variable *Non-GAAP* is an indicator variable that is coded 1 if the firm discloses non-GAAP earnings in any quarter during year *t* and 0 otherwise. Column (2) shows the results for when the dependent variable *FreqNonGaap* is the percentage of quarters in year *t* in which managers disclose non-GAAP earnings. Detailed definitions of the variables are in Appendix B. The robust standard errors are clustered by firms. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

3.9 Conclusion

I examined the association between insider (i.e. directors and executives) share pledging and firms' non-GAAP reporting practices. My study was motivated by the SEC's concern that share pledging by insiders potentially affects managerial behaviour and by the increased use of non-GAAP earnings disclosure in the past two decades.

My main findings can be summarised as follows. I provided evidence that the existence and the percentage of shares that are pledged are positively correlated with the propensity and the frequency of non-GAAP reporting. Managers are more likely to report non-GAAP earnings and to make more frequent non-GAAP disclosures when there is a considerable amount of margin-call pressure. Further, I documented that share pledging is negatively associated with the quality of non-GAAP reporting in that non-GAAP exclusions are not transitory. The cross-sectional analyses show that the effect of share pledging on the propensity and the quality of non-GAAP earnings disclosures is more pronounced for firms that have lower analyst coverage and fewer independent boards.

Furthermore, I used a DID design and a PSM approach to examine whether the results are sensitive to the baseline methods and obtained similar results. Overall, I interpreted the findings as supporting the view that insiders who have shares under pledge opportunistically report non-GAAP earnings to manage investor perceptions in an attempt to relieve margin-call pressure and to maintain their control rights.

Chapter 4: Study 3: Does Share Pledging Affect Workplace Safety?

4.1 Introduction

Workplace safety is an essential element of employee welfare and has substantial ethical, economic and social implications (Amin et al., 2021; Cohn & Wardlaw, 2016; Haga et al., 2022). According to the International Labour Organization, 2.78 million employees die from workplace accidents and 374 million employees suffer non-fatal workplace injuries and illnesses yearly, which accounts for about 4% of the world's GDP.[20] Several studies investigate the determinants of workplace safety and document that financially constrained firms (Cohn & Wardlaw, 2016) and firms that have pressure to meet earnings targets (Caskey & Ozel, 2017) exhibit higher workplace injury rates whereas publicly listed firms (Liang et al., 2022) and firms that have more analyst coverage (Bradley et al., 2022) experience fewer workplace injuries. However, factors such as managers' personal behaviours have received much less academic attention. This study addresses this void by investigating whether managers' personal financing choices, share pledging, affects workplace safety.

The practice of corporate insiders pledging their shares as collateral for personal loans has become a worldwide trend (Dou et al., 2019; Pang & Wang, 2020). For example, Larcker and Tayan (2010) find that 982 directors or officers reported share pledging in proxy statements in the 2006 to 2009 period in the US. Approximately 35 to 50% of publicly traded companies in China and Taiwan have insider share pledging (Dou et al., 2019; Zhu et al., 2021). However, the SEC is concerned that pledged shares 'have the potential to influence management's performance and decisions'. The media and regulators have also voiced concerns about share

[20] Available at wcms_686645.pdf (ilo.org).

pledging because it might result in further stock-price declines if insiders are obliged to sell their pledged shares in response to a margin call (Chan et al., 2018; Dou et al., 2019). Prior studies find that margin-call pressure from share pledging encourages managers to perform opportunistic behaviours such as share repurchasing, accrual-based earnings management and tone management to inflate share prices (Chan et al., 2018; Singh, 2018; Zhao et al., 2019). One strategy that managers can use to secure share prices is to conduct real activity management, such as compromises in workplace safety (Bai et al., 2020). Therefore, I investigate the potential impact of share pledging on employee welfare.

Ex ante, it is unclear whether share pledging can affect workplace safety. On the one hand, pledging managers have incentives to conduct real activity management (compromises in workplace safety) to inflate earnings and thus secure stock prices. Multitask principal–agent theory suggests that managers allocate more time and attention to activities that are easy to assess and to reward. Given that safety issues are more difficult to measure and to reward than operating performance (Holmstrom & Milgrom, 1991; Lin et al., 2021; Wachter & Wright, 1990), managers may cut safety investments and allocate more time and attention to operating performance. The existing literature documents that to support share prices and to avoid a margin call, pledging insiders buy back shares (Chan et al., 2018), manipulate accruals (Singh, 2018) and pursue merger and acquisition activities (Zhu et al., 2021). It is well established that managers could increase share prices through accrual-based or real earnings management (Cheng & Warfield, 2005; Cohen et al., 2008). Given that managers can increase earnings through reductions in safety investments and increases in employee workloads (Bai et al., 2020; Caskey & Ozel, 2017), managers who pledge shares may sacrifice workplace safety to inflate earnings and thus safeguard share prices. As a result, share pledging could negatively affect workplace safety.

However, share pledging may positively affect workplace safety if pledging managers are concerned that the potential public revelation of employee injuries could negatively affect firm value. The anecdotal and empirical evidence shows that investors react negatively to disclosures of employee injuries. For example, Christensen et al. (2017) document a 155 basis point decline in stock returns towards mandatory disclosures of mining-related workplace injuries. Tesla shares decreased by 2.7% after the news of a second investigation into a workplace injury in which a skid carrier hit an employee's jaw (Bai et al., 2020). Given that pledging managers have incentives to secure share prices, they may improve workplace safety to avoid employee injuries because the revelation of injuries can reduce firm value. In this regard, share pledging may have a positive impact on workplace safety.

To investigate the impact of share pledging on workplace safety, I use establishment-level data on workplace injury and illness that were compiled by the OSHA between 2007 and 2011. The sample starts from 2007 because that is the first year in which all firms were required to disclose their insider pledging activities. The sample ends in 2011 because the OSHA does not provide workplace injuries data after 2011 because of funding cuts. The data that are provided by the OSHA have continued to be used in recent studies (e.g. Amin et al., 2021; Bradley et al., 2022). I hand collect information about stock pledges from firms' proxy statements (DEF 14A). Following Caskey and Ozel (2017), I use Poisson regression and negative binomial regression to model the count data (the total number of injury and illness cases) and test the hypothesis. I control for a set of establishment-level and firm-level attributes and for establishment and year fixed effects. The baseline regression results suggest that firms that have insider share pledging experience higher numbers of workplace injury and illness. Economically, the injury case rates for pledging firms are 15 to 26% higher than those for non-pledging firms. Furthermore, I find that the percentage of shares that is pledged by insiders is

positively associated with workplace injuries, which suggests that insiders who are more sensitive to a stock-price decline are more likely to compromise on workplace safety.

Having documented the positive relationship between share pledging and workplace injuries, I examine the plausible channels through which pledging may affect workplace safety. Prior studies show that increased employee workloads and decreased safety-related investments contribute to higher numbers of injuries (Bai et al., 2020; Caskey & Ozel, 2017). Following this line of research, I use production per employee as a proxy for employee workloads and measure safety-related investments as abnormal discretionary expenses per employee by using the model from Caskey and Ozel (2017). I find that the higher number of injuries in pledging firms is associated with cuts in safety investments but not with increased employee workloads.

The primary findings are robust to using injury rate as an alternative measure (the total case rate) of workplace safety, the control for accrual-based earnings management and firm-level analyses. Additional analysis suggests that pledging firms experience higher numbers of workplace injuries when they meet or beat analyst forecasts, which indicates that pledging insiders manage earnings to support share prices and the attempts to manage earnings are related to an increase in workplace injuries.

Prior studies document that the factors that affect managers' abilities and incentives to sacrifice workplace safety can lead to cross-sectional variations in the association between share pledging and workplace safety (Bai et al., 2020; Caskey & Ozel, 2017). Specifically, the documented relationship between share pledging and workplace injuries is weaker if managers' ability to compromise workplace safety is constrained. The cross-sectional analyses suggest that the positive relationship between share pledging and injuries is mitigated for firms that have more analysts following, operate in industries that have higher levels of union membership, are located in states that have higher workers' compensation premiums and are situated in counties that have higher levels of social capital. These findings are consistent with

the conjecture that the detrimental effect of share pledging on workplace safety can be mitigated when pledging managers' discretions in safety-related issues are constrained (Caskey & Ozel, 2017).

This study makes several contributions to the literature. First, it adds to the literature on the consequences of share pledging. Prior studies document that share pledging is detrimental to firm value (Dou et al., 2019), deters firms' future innovation outputs and quality (Pang & Wang, 2020) and promotes opportunistic activities such as income smoothing (Huang & Xue, 2016), share repurchases (Chan et al., 2018) and accrual-based earnings management (Singh, 2018). This study provides evidence that share pledging could be harmful to employee welfare because pledging managers may compromise workplace safety.

Second, this study contributes to the growing literature on workplace safety. Prior studies document that financially constrained firms (Cohn & Wardlaw, 2016), firms that are under pressure to meet earnings benchmarks (Caskey & Ozel, 2017) and firms that are under threat of short selling (Bai et al., 2020) invest less in employee safety and exhibit higher workplace injury rates. This study documents that firms that have insider share pledging experience higher numbers of workplace injuries.

Third, this study contributes to the literature on the relationship between managers' personal behaviours and characteristics and workplace safety. Prior studies document that powerful CEOs tend to improve workplace safety to obtain personal benefits from lower injury rates (Haga et al., 2022) whereas overconfident CEOs experience higher rates of workplace injuries (Chen et al., 2022). This study shows that managers who pledge their own company's shares as collateral to obtain loans are incentivised to compromise workplace safety.

The remainder of this study is arranged as follows. Section 4.2 reviews the existing studies on workplace safety and stock pledges. Section 4.3 develops the hypothesis. Section 4.4 presents the research design. Section 4.5 explains the sample selection and provides the

descriptive statistics. Section 4.6 reports the primary results and explores the underlying channels of the baseline results. Section 4.7 discusses the cross-sectional analyses and robustness tests. Section 4.8 provides an additional analysis. Section 4.9 concludes.

4.2 Literature Review

4.2.1 Workplace Safety

Multitask principal–agent theory suggests that if agents devote more time or attention to one task, the marginal cost of attention to other tasks will increase so that more (less) time and attention will be allocated to tasks that are easy (hard) to measure and to reward (Holmstrom & Milgrom, 1991). Given that the outcomes for safety issues are more difficult to evaluate and to reward than operating performance (Wachter & Wright, 1990), managers may have incentives to prioritise operating performance over employee safety. Consistently with this view, a 2015 survey that was conducted by the National Safety Council shows that 33% of American employees believe that productivity is of higher importance than workplace safety in their firms (Bradley et al., 2022).

Empirical studies find that managers tend to compromise workplace safety when facing pressure from the stock market and in the presence of financial constraints and managerial myopia. Cohn and Wardlaw (2016) show that financing constraints have a negative impact on workplace safety. Specifically, firms that have higher leverage and lower operating cash flow experience higher injury rates. Caskey and Ozel (2017) document that firms that face pressure to meet earnings targets exhibit higher injury rates because of the cuts in safety-related investments and the increases in employee workloads. Using Regulation SHO as an exogenous shock to short-selling activities, Bai et al. (2020) provide evidence that increased short-selling pressure leads to higher workplace injury rates, which suggests that managers who prioritise

short-term outcomes may sacrifice workplace safety to reduce the risk of stock-price declines that are induced by potential short sellers.

Prior studies document that heightened scrutiny from regulators, media and analysts improves the safety performance of firms. For example, Christensen et al. (2017) provide evidence that the mandatory disclosure of safety-related issues in mining firms' financial reports leads to a reduction in workplace injuries and mining-related citations, which is explained by the increased public awareness and attention on safety issues. Liang et al. (2022) find that publicly listed firms exhibit fewer workplace injuries than private firms because of more intensive media and regulatory monitoring, which suggests that public scrutiny can encourage firms to participate in socially responsible activities. Bradley et al. (2022) posit that analysts are external monitors who can effectively discipline managers and document that workplace injuries decrease with analyst coverage.

4.2.2 Share Pledging

The practice of corporate insiders using shares in their own companies as collateral for personal loans is referred to as share pledging. Several studies examine the determinants of insider pledging. Fabisik (2019) documents that US CEOs use the funds from share pledging to obtain liquidity, to leverage their stock position and to hedge or to diversify their firm-specific risks. Shen et al. (2021) find that the likelihood of insider pledging in the US increases with insiders' risk exposure and decreases with the corporate governance quality of firms.

A growing body of literature examines the economic consequences of insider pledging. Several studies document that stock pledges have adverse economic effects. Specifically, Dou et al. (2019) find that pledging in Taiwan destroys firm value by exacerbating crash risk and by reducing firm risk-taking. Pang and Wang (2020) show that the existence and the percentage of the shares that are pledged by controlling shareholders in China adversely affect firms' innovation outputs and quality. Jiang et al. (2021) provide evidence that share pledging by

controlling shareholders in Chinese-listed firms aggravates conflicts of interest between shareholders and suppliers. Further, they document that firms obtain less trade credit from suppliers when their controlling shareholders pledge a higher percentage of the company's shares.

Moreover, the existing literature provides evidence that share pledging may have positive economic effects. Li et al. (2019) document a positive association between controlling shareholders' share pledging and firm value. One possible explanation is that investors treat pledging as a positive signal of firms' future performance. Zhu et al. (2021) examine whether share pledging by controlling shareholders in China affects firms' merger and acquisition (M&A) activities and find that firms that have share pledging are more likely to initiate M&As to support their share prices. Further, their findings show that pledging firms have higher stock returns and better operating performance than non-pledging firms during the post-acquisition period, which indicates that they conduct value-enhancing M&A activities.

Closely related to this study are studies on managerial opportunism after share pledging. Empirical studies document that insiders that perform share pledging may conduct some opportunistic activities, such as earnings management and stock repurchasing, to avoid declines in share prices (Chan et al., 2018; Deren & Ke, 2018; Huang & Xue, 2016). Specifically, using a sample of Chinese-listed firms, Huang and Xue (2016) find that firms that have share pledging smooth their earnings more than control firms to increase the value of those pledged shares. Deren and Ke (2018) provide evidence that controlling shareholders in Chinese firms conduct real earnings management to inflate earnings. Similarly, in a US study, Singh (2018) documents a positive relationship between share pledging and accrual-based earnings management, which indicates that share pledging distorts insiders' incentives and motivates them to conduct opportunistic activities to support share prices. Last, Chan et al. (2018) show that controlling

shareholders in Taiwan are more likely to repurchase the firms' shares to increase share prices and to preserve their control rights.

4.3 Hypothesis Development

The concern that shares that are pledged by corporate insiders may 'influence management's performance and decisions' prompted the SEC to implement disclosure guidelines for insider share pledging in 2006. Empirical studies support this view, finding that pledging managers are motivated to behave opportunistically to safeguard stock prices and thereby avoid a margin call when faced with downward pressures on stock prices and the possibility of losing control rights (Chan et al., 2018; Singh, 2018; Zhu et al., 2021). Specifically, pledging insiders are found to carry out repurchase programs (Chan et al., 2018), manipulate accruals (Singh, 2018), conduct real earnings management (Deren & Ke, 2018) and pursue M&A activities to increase share prices (Zhu et al., 2021).

I contend that managers that perform share pledging also have incentives to compromise workplace safety to support stock prices. Multitask principal–agent theory indicates that managers tend to allocate more (less) time and attention to activities that are easy (hard) to assess and to reward. Given that safety issues are more challenging to measure than operating performance (Holmstrom & Milgrom, 1991; Wachter & Wright, 1990) and it is more difficult for managers to be rewarded for safety-related performance (Lin et al., 2021), managers may reduce safety investments and focus more on operating performance because existing studies provide evidence that earnings directly affect managers' compensation (Sloan, 1993). Empirical studies document that managers invest less than is optimal in the safety of their employees when faced with pressures from the capital market (Bai et al., 2020; Bradley et al., 2022; Caskey & Ozel, 2017). For example, Caskey and Ozel (2017) provide evidence that managers reduce safety-related investments and impose higher workloads on employees when under pressure to achieve earnings targets, which ultimately leads to increased numbers of

workplace injuries. It is well established that managers could increase share prices through accrual-based or real earnings management (Cheng & Warfield, 2005; Cohen et al., 2008). Given that managers can increase earnings through reductions in safety investments and increases in employee workloads (real earnings management), pledging managers have the potential to compromise workplace safety to inflate earnings and thus secure share prices. Therefore, share pledging could have a negative impact on workplace safety.

However, share pledging may positively affect workplace safety if pledging managers are concerned that workplace injuries might attract increased scrutiny and adversely affect firms' share prices. Anecdotal evidence suggests that major workplace accidents are likely to garner media attention and public scrutiny (Bai et al., 2020; Caskey & Ozel, 2017). For example, Tesla shares decreased by 2.7% after the news of a second investigation into a workplace injury in which a skid carrier hit an employee's jaw (Bai et al., 2020).[21] Empirical studies document that investors react negatively to disclosures of employee injuries. Specifically, Christensen et al. (2017) show that stock returns drop by 155 basis points when mining firms disclose workplace injuries. Furthermore, they find that the mandatory disclosure of mine safety-related issues reduces the number of mining firms' workplace injuries. If pledging managers are concerned that the potential public revelation of employee injuries could negatively affect firm value, they may increase safety investments to avoid a decline in share prices and thus a margin call. As a result, share pledging may positively affect workplace safety.

To conclude, whether share pledging affects workplace safety is still an empirical question. Therefore, I present the hypothesis in its null form, which is as follows:

Hypothesis: *Share pledging has no impact on workplace safety.*

[21] Available at https://www.cnbc.com/2018/04/20/tesla-shares-fall-on-news-of-new-probe-into-workplace-injury.html#:~:text=Tesla%20shares%20dip%20on%20report%20of%20second%20safety,and%20Health%20Administration%2C%20or%20Cal-OSHA%2C%20confirmed%20the%20investigation.

4.4 Research Design

This study investigates whether share pledging has an impact on workplace safety. The proxy for workplace safety is the number of injury and illness cases, which is a count variable. Existing studies use Poisson regression and negative binomial regression to model count data (Caskey & Ozel, 2017; Rock et al., 2000). Following Caskey and Ozel (2017), I estimate the following model to test my hypothesis:

$$\textit{Employee Injuries}_{it} \sim \textit{Poisson}\ (\lambda_{it}),$$

$$\begin{aligned}\lambda_{it} = \exp\{ & \beta_0 + \beta_1\, \textit{Pledge}_{it} + \beta_2\, \textit{Firm Size}_{it} + \beta_3\, \textit{Leverage}_{it} + \beta_4\, \textit{MTB}_{it} + \beta_5\, \textit{Turnover}_{it} + \beta_6\, \textit{PPE}_{it} \\ & + \beta_7\, \textit{Capital Expenditures}_{it} + \beta_8\, \textit{Establishment Size}_{it} + \beta_9\, \textit{Hours Per Employee}_{it} \\ & + \beta_{10}\, \textit{Seasonal}_{it} + \beta_{11}\, \textit{Strike}_{it} + \beta_{12}\, \textit{Shutdown}_{it} + \beta_{13}\, \textit{Disaster}_{it} + \textit{Exposure}_{it} + e_{it}\}\end{aligned} \quad (1)$$

where $e_{it\,=\,0}$ for a Poisson regression model and $e^{eit} \sim$ Gamma $(1/\alpha, \alpha)$ for a negative binomial regression model. This is because the negative binomial regression is a generalisation of the Poisson regression by incorporating a gamma noise variable.[22] The negative binomial regression relaxes the Poisson model's restrictive assumption that the independent variable's (*Employee Injuries*) variance is equal to its mean. Given that establishment fixed effects cannot be included in a negative binomial model, following Caskey and Ozel (2017), I control for only the year fixed effects in the negative binomial model but the establishment and year fixed effects in the Poisson model.

The dependent variable, *Employee Injuries*, is the total number of injuries and illnesses that results in days away from work or job transfers and other recordable cases for establishment *i* in year *t*. *Pledge* represents one of the two measures of pledging in year *t* (*Pledge Dummy* and *Pledge Ratio*). *Pledge Dummy* is an indicator variable that is coded 1 if the firm's named executives and directors pledge shares in year *t* and 0 otherwise. *Pledge Ratio* is the ratio of the

[22] Available at Negative Binomial Regression (netdna-ssl.com).

aggregated number of shares that is pledged relative to the aggregated number of shares that is owned by the firm's named executives and directors. A significant and positive coefficient of *Pledge* (β_1) indicates that firms that have insider share pledging exhibit higher rates of workplace injuries.

I control for a set of firm characteristics and establishment-level variables that may influence workplace safety, as is documented in the existing literature (Bradley et al., 2022; Caskey & Ozel, 2017; Cohn & Wardlaw, 2016). Firm characteristics include *Firm Size*, which is the natural logarithm of the firm's beginning total assets, *Leverage*, which is the ratio of the firm's total liabilities to total assets at the beginning of the year, *MTB*, which is calculated as the firm's beginning market value of assets scaled by the beginning book value of assets, *Turnover*, which is the ratio of the firm's sales in year *t* to the beginning total assets in year *t*, *PPE*, which is measured as the firm's net property, plant and equipment divided by the total assets at the beginning of year *t*, and *Capital Expenditures*, which is the ratio of the firm's current period capital expenditures to the beginning total assets.

Prior studies show that establishment-level variables may also have an impact on workplace safety (Caskey & Ozel, 2017). Thus, I control for the average number of employees in a given establishment (*Establishment Size*) and the average working hours per employee in a given establishment-year (*Hours Per Employee*). Further, I incorporate four indicator variables into the model to account for the influence of unusual events. Specifically, *Seasonal* is an indicator variable that is coded 1 if there are seasonal workers in a certain establishment-year and 0 otherwise. *Strike* is an indicator variable that is coded 1 if the establishment experiences a strike during the year and 0 otherwise. *Shutdown* is an indicator variable that is coded 1 if there is a shutdown in a given establishment-year and 0 otherwise. *Disaster* is an indicator variable that is coded 1 if the establishment is affected by natural disasters during the year and 0 otherwise. Following prior studies (Bradley et al., 2022; Caskey & Ozel, 2017; Cohn

& Wardlaw, 2016), the standard errors are clustered at the firm level to adjust for heteroskedasticity (Petersen, 2009) in all the regressions. All the continuous variables are winsorised at the 1% and the 99% level to reduce the effects of extreme values. Appendix D provides detailed variable definitions.

4.5 Sample Selection and Descriptive Statistics

My initial sample consists of all the establishments from the OSHA that can be matched to firms in Compustat annual files[23] between 2007 and 2011. The sample period starts from 2007[24] because this is the first year in which all firms that have insider share pledging were required to disclose the pledging activities of their named executives and directors. The sample ends in 2011 because the OSHA stopped providing data on workplace safety after 2011. The statistics from the OSHA have continued to be used by researchers. (e.g. Amin et al., 2021; Bradley et al., 2022).

Workplace safety data are obtained from the Data Initiative Program (ODI) of the OSHA. From 1996 to 2011, the ODI collected data on work-related injuries and illnesses from about 80,000 private-sector establishments each year. For each establishment, information about the injuries and the illnesses, the establishment name, the number of employees and the hours that they worked and whether the establishment experienced unusual events (i.e. strikes, natural disaster) were recorded.

The data on stock pledges are collected from firms' proxy statements (DEF 14A). I conduct keyword searches through proxy statements to identify the firms that have insider share

[23] Caskey and Ozel (2017) provide an OSHA–Compustat link table in which establishments from the OSHA are matched to their corresponding Compustat firms. They manually match establishments with Compustat firms according to their names. The link table is available at https://sites.google.com/view/bugraozel/data

[24] In August 2006, the SEC required footnote disclosure of share-pledging activities by named executive officers, directors and director nominees. Firms were not affected by the disclosure requirements in 2006 if their annual shareholder meetings were earlier than August 2006. In other words, 2007 is the first year that all firms that have insider pledging were affected by the disclosure.

pledging. The keywords include ‘pledging’, ‘shares pledging’, ‘pledge as security’, ‘pledge as collateral’, ‘pledge loan’, ‘pledge shares’, ‘pledged shares’ and ‘shares pledged’. After that, for the pledging firms, I hand collect information about the names of the executives and the directors who pledge shares, the number of shares that is pledged and the total amount of shares that is owned by these executives and directors.

My initial sample consists of 34,378 establishment-year observations from the OSHA that can be linked to Compustat firms from 2007 to 2011. I remove utility firms that have SIC codes in the range of 4,900 to 4,999 and financial firms that have SIC codes in the range of 6,000 to 6,999 to mitigate the potential impact of regulation on workplace safety. This requirement eliminates 1,602 establishment-year observations. Workplace injuries are measured over calendar years. To better align financial data with workplace injuries data, I follow Caskey and Ozel (2017) and exclude 15,232 observations that have non-December year-ends. Furthermore, I require that the number of employees in each establishment of a certain firm should be at least 5% of the total number of employees in that firm, as is suggested by Caskey and Ozel (2017). The sample is reduced by 3,865. Finally, I remove 315 observations that are missing values for the variables that are used in the main analyses. The final sample consists of 13,364 establishment-year observations that pertain to 388 unique firms. Table 4.1 describes the sample-selection process.

Table 4.1: Description of Sample Selection

	Number of establishment-year observations
All establishments from the OSHA that can be matched to firms in Compustat annual files between 2007 and 2011	34,378
Less: Observations in the financial and utility industries	(1,602)
Less: Observations that have non-December fiscal year-ends	(15,232)
Less: Observations that have insufficient employees	(3,865)
Less: Observations that are missing values	(315)
Final sample	13,364

Table 4.2 presents the descriptive statistics of the variables that are used in my main analyses. The pledge indicator averages 0.18, which indicates that 18% of establishments had insider share pledging during 2007 to 2011. Across the 2,408 establishment-year observations that have insider pledging, the average pledge ratio is 0.150, which suggests that managers, on average, pledge 15% of their companies' shares. The average number of injury and illness cases (*Employee Injuries*) per establishment-year is 23, which is comparable to the mean total illness and injury cases of 22, as is reported in Caskey and Ozel (2017).

Table 4.2: Descriptive Statistics

Variable	*N*	Mean	p50	SD	p25	p75
Employee Injuries	13,364	23	10	60	4	22
Pledge Dummy	13,364	0.180	0	0.384	0	0
Pledge Ratio	2,408	0.150	0.072	0.123	0.068	0.223
Firm Size	13,364	9.456	9.482	1.622	8.270	10.41
Leverage	13,364	0.292	0.275	0.160	0.153	0.404
MTB	13,364	1.704	1.403	0.769	1.133	2.109
Turnover	13,364	1.169	1.105	0.538	0.847	1.421
PPE	13,364	0.339	0.311	0.183	0.197	0.452
Capital Expenditures	13,364	0.051	0.044	0.034	0.026	0.067
Establishment Size	13,364	4.857	4.745	0.979	4.205	5.371
Hours Per Employee	13,364	1,947	1,999	335	1,698	2,161
Seasonal	13,364	0.023	0	0.151	0	0
Strike	13,364	0.001	0	0.035	0	0
Shutdown	13,364	0.091	0	0.287	0	0
Disaster	13,364	0.003	0	0.055	0	0
Production	13,342	195.8	151.1	240.3	96.25	232.3
EMP	13,342	119.5	59.50	136.8	25.82	154
Sales	13,307	30.60	13.59	42.87	4.270	43.25
DisExp	13,307	9.612	1.512	44.58	–6.513	26.45

Note. This table reports the descriptive statistics of the variables that are used in the main analyses. The sample period runs from 2007 to 2011. Detailed definitions of the variables are presented in Appendix D. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

For firm-level control variables, the average firm in my sample has total assets of approximately $12 billion. The debt ratio has a mean of 29.2% and the market-to-book ratio averages 1.704. The average ratio of sales to beginning total assets is $1.169. On average, property, plant and equipment accounts for 33.9% of a firm's total assets and capital

expenditures are 5.1% of a firm's beginning assets. Appendix E provides summary statistics on the number of observations across years and industries. Appendix F reports the correlation matrix.

4.6 Main Results

4.6.1 Share Pledging and Workplace Safety

This study investigates whether insider share pledging affects workplace safety. Table 4.3 reports the findings on the relationship between share pledging and workplace safety from estimating Equation 1. The results of the negative binomial regression are presented in Columns (1) and (2) and the results of the Poisson regression are presented in Columns (3) and (4). The dependent variable is the total number of injuries and illnesses that lead to days away from work or job transfers and other recordable cases in a given establishment-year. Following Caskey and Ozel (2017), I use the number of hours that is worked by employees (*Hours*) as the exposure variable. I control for year fixed effects in the negative binomial and Poisson models and for establishment fixed effects in the Poisson model. The robust standard errors are clustered by firm.

Table 4.3: Share Pledging and Workplace Safety

	(1)	(2)	(3)	(4)
	Negative binomial model		Poisson model	
Pledge Dummy	0.235**		0.144**	
	(2.08)		(2.45)	
Pledge Ratio		0.804***		0.454***
		(2.74)		(2.69)
Firm Size	0.019	0.029	0.142	0.157
	(0.45)	(1.20)	(1.09)	(1.19)
Leverage	0.835**	0.820***	–0.469	–0.391
	(2.04)	(2.96)	(–1.38)	(–0.98)
MTB	0.003	0.034	–0.165*	–0.168*
	(0.03)	(0.54)	(–1.86)	(–1.85)
Turnover	0.188**	0.213***	0.012	0.041
	(1.98)	(3.64)	(0.09)	(0.31)
PPE	0.598***	0.484	1.076**	1.228**
	(2.60)	(1.57)	(1.98)	(2.30)

Capital Expenditures	1.682	1.427**	–0.188	–0.305
	(1.45)	(2.32)	(–0.16)	(–0.26)
Establishment Size	–0.383***	–0.385***	0.019	0.019
	(–3.87)	(–5.01)	(0.49)	(0.49)
Hours Per Employee	–0.001***	–0.001***	–0.001***	–0.001***
	(–5.21)	(–7.37)	(–3.46)	(–3.48)
Seasonal	0.250***	0.251***	0.021	0.020
	(4.33)	(4.35)	(0.71)	(0.67)
Strike	–0.168	–0.147	0.023	0.023
	(–0.59)	(–0.56)	(0.27)	(0.27)
Shutdown	0.058	0.057	–0.171**	–0.173**
	(0.55)	(0.71)	(–2.34)	(–2.35)
Disaster	–0.168*	–0.192**	0.036	0.041
	(–1.70)	(–2.22)	(0.32)	(0.35)
Intercept	–6.603***	–6.707***		
	(–12.51)	(–17.86)		
Year fixed effect	Yes	Yes	Yes	Yes
Establishment fixed effect	–	–	Yes	Yes
N	13,364	13,364	13,364	13,364
Log likelihood	–54,660	–54,686	–63,169	–63,143
Exposure variable	Hours	Hours	Hours	Hours

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents the analysis of the relationship between share pledging and workplace safety. The models are estimated at the establishment level. The dependent variable *Employee Injuries* is the total number of injuries and illnesses cases in the establishment during the year. The independent variable *Pledge Dummy* in Columns (1) and (3) is an indicator variable that is coded 1 if the firm's named executives and directors pledge shares during the year and 0 otherwise. The independent variable *Pledge Ratio* in Columns (2) and (4) is the ratio of the aggregated number of shares that is pledged relative to the aggregated number of shares that is owned by firms' named executives and directors. Columns (1) and (2) report the results that are estimated by using a negative binomial model. Columns (3) and (4) present the results that are estimated by using a Poisson model. Detailed definitions of the variables are in Appendix D. The robust standard errors are clustered at the firm level. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

The coefficient estimates of *Pledge Dummy* in Columns (1) and (3) are positive and statistically significant at the 5% level, which suggests that firms that have insider pledging exhibit higher rates of workplace injuries. Economically, the coefficient of *Pledge Dummy* is 0.235 in Column (1) and 0.144 in Column (3), which corresponds to incidence rates of 0.26 ($e^{0.235}$–1) and 0.15 ($e^{0.144}$–1), respectively. In other words, the injury case rates for pledging firms are 15 to 26% higher than those for non-pledging firms. The differences in the regression coefficients of *Pledge Dummy* in Columns (1) and (3) are caused mainly by establishment fixed

effects. Negative binomial models do not allow establishment fixed effects, which could result in an upward bias in the coefficient estimates.

The existing literature suggests that studying the impact of the percentage of shares that is pledged by insiders has greater economic significance because insiders are more affected by declines in stock prices when they pledge a higher percentage of shares (Pang & Wang, 2020). I present the findings for *Pledge Ratio* in Columns (2) and (4). The coefficients of *Pledge Ratio* are positive and statistically significant at the 1% level (Column (2): $\beta 1 = 0.804$, *z*-statistic = 2.74; Column (4): $\beta 1 = 0.454$, *z*-statistic = 2.69), which indicates that the percentage of shares that is pledged by managers is positively associated with workplace injuries.

The signs of the estimated coefficients for the firm-level control variables are generally consistent with previous studies. For example, Columns (1) and (2) show that firms that have greater levels of leverage, asset turnover, PPE and capital expenditure experience higher rates of workplace injuries. This is consistent with the negative binomial regression results in Caskey and Ozel (2017).[25] For the Poisson regression results, similar to the findings from Caskey and Ozel (2017), most of these control variables in Columns (3) and (4) become statistically insignificant.[26] One possible explanation is that establishment fixed effects absorb the variance in highly sticky variables.

Further, the regression coefficients of the establishment-level control variables are in the same direction as those in prior studies. Specifically, establishments that have seasonal workers exhibit higher rates of workplace injuries and establishments that have fewer working hours and total numbers of employees have lower rates of workplace injuries. This suggests that experienced employees suffer fewer injuries.

[25] Caskey and Ozel (2017) report the negative binomial regression results in Column (2) of Table 3.

[26] Caskey and Ozel (2017) present the findings from the Poisson regression analysis in Column (5) of Table 3.

Overall, the findings from Table 4.3 show that the existence of insider pledging and the percentage of shares that is pledged by insiders are positively associated with workplace injuries, which suggests that managers that pledge shares may compromise workplace safety. Given that negative binomial models potentially lead to an upward bias in the estimated coefficients of the variables of interest (*Pledge Dummy* and *Pledge Ratio*), following Caskey and Ozel (2017), I use the Poisson model that uses year and establishment fixed effects for the rest of this study.

4.6.2 Channel Tests

The baseline results show that share pledging is positively associated with workplace injuries. In this section, I examine the plausible channels through which pledging may affect workplace safety. Prior studies document that workplace injuries can be attributed to increased employee workloads and to decreased safety-related investments (Amin et al., 2021; Caskey & Ozel, 2017). Caskey and Ozel (2017) conjecture that increasing employee workloads indirectly affects injuries because higher workloads can only increase the possibility of employee injuries whereas reducing investments in safety can directly affect safety conditions. Therefore, I investigate whether share pledging affects workplace safety through these two channels.

First, I examine whether the higher rates of workplace injuries in firms that have share pledging are related to an increase in employee workloads. Following Caskey and Ozel (2017), I use production per employee (*Production*) as a proxy for employee workloads. *Production* is calculated as the sum of the firms' cost of sales and the change in inventory scaled by the firms' total number of employees. The interaction terms (*Pledge Dummy* × *Production* and *Pledge Ratio* × *Production*) capture the impact of employee workloads on the relationship between share pledging and workplace safety. A positive and significant coefficient on the interaction term would indicate that excessive workloads contribute to the high rate of workplace injuries in firms that have share pledging.

Columns (1) and (2) of Table 4.4 present the findings of this analysis. The estimated coefficients of the interaction terms are statistically insignificant, which suggests that employee workloads cannot explain the relationship between share pledging and workplace safety. In other words, I find no evidence that increased workloads play a role in higher rates of workplace injuries in firms that have share pledging. One possible explanation is that increasing employee workloads is not efficient for improving earnings and thus supporting share prices for pledging firms. Managers may resort to a more direct mechanism (reductions in safety-related investments) to support stock prices when their shares are under pledge.

Table 4.4: Channel Tests

	(1)	(2)	(3)	(4)
	Output = Production		*Output = DisExp*	
Pledge Dummy × Output	0.001		**–0.005*** **	
	(0.66)		**(–3.27)**	
Pledge Ratio × Output		–0.002		**–0.018*** **
		(–1.05)		**(–3.13)**
Pledge Dummy	0.094		0.172**	
	(0.95)		(2.56)	
Pledge Ratio		0.889**		0.716***
		(2.21)		(2.59)
Output	–0.001*	–0.002*	–0.001	–0.001
	(–1.72)	(–1.78)	(–0.70)	(–0.70)
Firm Size	0.132	0.151	0.147	0.148
	(1.20)	(1.40)	(1.30)	(1.30)
Leverage	–0.379**	–0.209	–0.516	–0.517
	(–2.19)	(–1.15)	(–1.24)	(–1.26)
MTB	–0.152**	–0.149**	–0.173*	–0.173*
	(–2.23)	(–2.22)	(–1.94)	(–1.94)
Turnover	0.067	0.129	0.091	0.092
	(0.51)	(0.97)	(0.55)	(0.55)
PPE	0.875**	1.090***	1.353***	1.357***
	(2.08)	(2.88)	(2.78)	(2.77)
Capital Expenditures	–0.143	–0.273	–0.909	–0.916
	(–0.11)	(–0.22)	(–0.60)	(–0.60)
Establishment Size	0.019	0.019	0.045	0.045
	(0.50)	(0.50)	(0.47)	(0.47)
Hours Per Employee	–0.001***	–0.001***	–0.001***	–0.001***
	(–3.27)	(–3.29)	(–4.54)	(–4.54)
Seasonal	0.026	0.026	–0.078	–0.078
	(0.95)	(0.92)	(–1.50)	(–1.50)
Strike	0.011	0.007	0.054	0.054
	(0.13)	(0.08)	(0.57)	(0.57)
Shutdown	–0.169**	–0.173**	–0.177**	–0.177**
	(–2.33)	(–2.35)	(–2.48)	(–2.48)
Disaster	0.051	0.058	0.025	0.030
	(0.45)	(0.48)	(0.21)	(0.24)
Year fixed effect	Yes	Yes	Yes	Yes
Establishment fixed effect	Yes	Yes	Yes	Yes
N	13,342	13,342	13,307	13,307
Log likelihood	–62,975	–62,915	–63,558	–63,556
Exposure variable	Hours	Hours	Hours	Hours

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents the analysis of the channels by which share pledging may affect workplace safety. The Poisson models are estimated at the establishment level. The dependent variable *Employee Injuries* is the total number of injuries and illnesses cases in the establishment during the year. The independent variable *Pledge Dummy* in Columns (1) and (3) is a dummy variable that is coded 1 if the firm's executives and directors pledge shares and

0 otherwise. The independent variable *Pledge Ratio* in Columns (2) and (4) is the ratio of the aggregated number of shares that is pledged relative to the aggregated number of shares that is owned by firms' executives and directors. Detailed definitions of the variables are in Appendix D. The robust standard errors are clustered at the firm level. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

Next, I investigate whether reducing safety investments is a channel by which share pledging could affect workplace safety. I expect that the cut in safety investments will contribute to the high rate of workplace injuries in firms that have share pledging because pledging managers can inflate earnings and thus secure share prices through reductions in safety-related expenses. Following Caskey and Ozel (2017), abnormal discretionary expenses are calculated according to Equation (2).[27] The model is estimated cross-sectionally for each industry and year and industry uses the two-digit SIC codes. The regression residuals from Equation (2) are the proxy for discretionary safety expenses.

$$SGA_{i,t} / EMP_{i,t-1} = \alpha_0 + \alpha_1 [1 / EMP_{i,t-1}] + \alpha_2 [Sales_{i,t-1} / EMP_{i,t-1}] + \varepsilon_{it} \quad (2)$$

where *SGA* represents the total selling, general and administrative expenses, *EMP* measures firms' total number of employees and *Sales* is the sales revenue of the firms.

The interaction terms (*Pledge Dummy* × *DisExp* and *Pledge Ratio* × *DisExp*) capture the effect of reductions in safety investments on the relationship between workplace injuries and insider share pledging. A negative and significant coefficient of the interaction terms suggests that higher rates of workplace injuries in pledging firms can be partially attributed to reductions in safety-related expenses.

The last two columns in Table 4.4 show that the coefficient estimates of the interaction terms are negative and statistically significant at the 1% level (Column (3): $\beta 1 = -0.005$, *z*-statistic = −3.27; Column (4): $\beta 1 = -0.018$, *z*-statistic = −3.13), which suggests that reductions

[27] Caskey and Ozel (2017) calculate the abnormal discretionary expenses by using Roychowdhury's (2006) model however they scale *SGA* and *Sales* by the total number of employees instead of the total assets at the beginning of the year.

in safety investments contribute to the high rates of workplace injuries in firms that have share pledging. *Pledge Dummy* ($\beta 2$ = 0.172, *z*-statistic = 2.56) and *Pledge Ratio* ($\beta 2$ = 0.716, *z*-statistic = 2.59) remain significant and positive, which is consistent with the primary findings. I acknowledge that *DisExp* does not perfectly capture the safety-related expenses of firms. Nonetheless, to some extent, this study provides evidence that reducing safety investments is a potential channel by which share pledging affects workplace safety.

4.7 Cross-Sectional Analyses and Robustness Tests

4.7.1 Cross-Sectional Test: Analyst Coverage

The main findings show that share pledging is positively associated with workplace injuries. I now investigate the cross-sectional differences in the association between share pledging and workplace safety. Prior studies document that managers react to external monitoring by improving workplace safety (Amin et al., 2021; Bradley et al., 2022). In particular, Bradley et al. (2022) argue that analysts are influential in the disciplining of managers. Further, they provide evidence that workplace injury rates are lower for firms that have higher analyst coverage. If greater analyst coverage can effectively monitor pledging managers and encourage them to improve workplace safety, then the impact of share pledging on workplace injuries might be weaker in the presence of more analysts.

The analyst coverage data are obtained from the I/B/E/S database. I create an indicator variable (*High Analyst*) that is coded 1 if the parent company of establishments has an analyst following that is above the median value in a given year and industry and 0 otherwise, where industry is classified by using the two-digit SIC codes. I incorporate the indicator variable and the interaction terms (*Pledge Dummy* × *High Analyst* and *Pledge Ratio* × *High Analyst*) into Equation (1) and re-estimate the Poisson models. I expect the interaction terms to be negative if more analyst coverage limits pledging managers' ability to sacrifice workplace safety.

Panel A of Table 4.5 presents the findings. The coefficients of *Pledge Dummy* (*β2* = 0.206, *z*-statistic = 2.98) and *Pledge Ratio* (*β2* = 0.574, *z*-statistic = 3.10) are positive and statistically significant at the 1% level, which is consistent with those from the main analysis. Importantly, the estimated coefficients of the interaction terms are −0.208 and −0.774 (*z*-statistics are −2.20 and −4.28) in Columns (1) and (2), respectively. These findings suggest that the positive relationship between share pledging and workplace injuries is significantly weakened when more analysts are present. Overall, the findings indicate that external monitors constrain managers' capacity to sacrifice workplace safety to safeguard stock prices when managers' shares are under pledge.

Table 4.5: Cross-Sectional Tests

Panel A: Analyst Coverage

	(1)	(2)
	Employee Injuries	
Pledge Dummy × *High Analyst*	–0.208**	
	(–2.20)	
Pledge Ratio × *High Analyst*		–0.774***
		(–4.28)
Pledge Dummy	0.206***	
	(2.98)	
Pledge Ratio		0.574***
		(3.10)
High Analyst	0.050	0.048
	(1.31)	(1.20)
Firm Size	0.130	0.150
	(1.03)	(1.19)
Leverage	–0.471**	–0.368*
	(–2.37)	(–1.81)
MTB	–0.166*	–0.170*
	(–1.77)	(–1.76)
Turnover	0.014	0.050
	(0.11)	(0.39)
PPE	1.046*	1.228**
	(1.88)	(2.24)
Capital Expenditures	–0.145	–0.244
	(–0.13)	(–0.22)
Establishment Size	0.019	0.019
	(0.49)	(0.48)
Hours Per Employee	–0.001***	–0.001***

	(1)	(2)
	Employee Injuries	
	(–3.48)	(–3.50)
Seasonal	0.022	0.021
	(0.72)	(0.68)
Strike	0.039	0.033
	(0.46)	(0.38)
Shutdown	–0.168**	–0.171**
	(–2.38)	(–2.41)
Disaster	0.029	0.035
	(0.26)	(0.30)
Year fixed effect	Yes	Yes
Establishment fixed effect	Yes	Yes
N	13,364	13,364
Log likelihood	–63,132	–63,092
Exposure variable	Hours	Hours

Panel B: Union Membership

	(1)	(2)
	Employee Injuries	
Pledge Dummy × High Union	–0.131*	
	(–1.82)	
Pledge Ratio × High Union		–0.860***
		(–4.45)
Pledge Dummy	0.151***	
	(2.60)	
Pledge Ratio		0.505***
		(2.74)
High Union	–0.043	–0.035
	(–1.33)	(–1.08)
Firm Size	0.143	0.158
	(1.10)	(1.20)
Leverage	–0.476**	–0.397**
	(–2.44)	(–2.04)
MTB	–0.165*	–0.168*
	(–1.86)	(–1.86)
Turnover	0.009	0.038
	(0.07)	(0.30)
PPE	1.058*	1.225**
	(1.93)	(2.29)
Capital Expenditures	–0.140	–0.276
	(–0.12)	(–0.24)
Establishment Size	0.018	0.018
	(0.46)	(0.46)
Hours Per Employee	–0.001***	–0.001***
	(–3.51)	(–3.52)
Seasonal	0.020	0.018
	(0.69)	(0.62)
Strike	0.017	0.019
	(0.21)	(0.21)
Shutdown	–0.172**	–0.174**
	(–2.34)	(–2.35)
Disaster	0.039	0.044
	(0.35)	(0.37)
Year fixed effect	Yes	Yes
Establishment fixed effect	Yes	Yes
N	13,364	13,364
Log likelihood	–63,139	–63,096
Exposure variable	Hours	Hours

Panel C: Workers' Compensation Premium

	(1)	(2)
	Employee Injuries	
Pledge Dummy × High Premium	–0.057**	
	(–2.46)	
Pledge Ratio × High Premium		–0.232**
		(–2.39)
Pledge Dummy	0.169***	
	(2.96)	
Pledge Ratio		0.546***
		(3.34)
High Premium	–0.022	–0.031
	(–0.78)	(–1.14)
Firm Size	0.144	0.158
	(1.11)	(1.21)
Leverage	–0.453**	–0.385*
	(–2.27)	(–1.94)
MTB	–0.165*	–0.169*
	(–1.85)	(–1.87)
Turnover	0.017	0.046
	(0.13)	(0.36)
PPE	1.109**	1.245**
	(2.05)	(2.32)
Capital Expenditures	–0.180	–0.267
	(–0.15)	(–0.23)
Establishment Size	0.016	0.017
	(0.39)	(0.41)
Hours Per Employee	–0.001***	–0.001***
	(–3.48)	(–3.50)
Seasonal	0.018	0.019
	(0.62)	(0.63)
Strike	0.023	0.024
	(0.26)	(0.27)
Shutdown	–0.173**	–0.176**
	(–2.39)	(–2.40)
Disaster	0.034	0.043
	(0.31)	(0.36)
Year fixed effect	Yes	Yes
Establishment fixed effect	Yes	Yes
N	13,364	13,364
Log likelihood	–63,109	–63,092
Exposure variable	Hours	Hours

Panel D: Social Capital

	(1)	(2)
	Employee Injuries	
Pledge Dummy × High Norm	–0.277***	
	(–2.60)	
Pledge Ratio × High Norm		–1.083*
		(–1.82)
Pledge Dummy	0.181**	
	(2.53)	
Pledge Ratio		0.568***
		(2.67)
High Norm	0.143	0.156*
	(1.28)	(1.86)
Firm Size	0.132	0.150
	(1.44)	(1.46)
Leverage	–0.345*	–0.416**
	(–1.95)	(–2.39)
MTB	–0.113**	–0.150**
	(–2.18)	(–2.34)
Turnover	0.017	0.031
	(0.14)	(0.25)
PPE	0.820**	0.915**
	(1.99)	(2.19)
Capital Expenditures	–0.372	–0.251
	(–0.32)	(–0.22)
Establishment Size	0.019	0.020
	(0.48)	(0.50)
Hours Per Employee	–0.001***	–0.001***
	(–3.44)	(–3.45)
Seasonal	0.021	0.025
	(0.69)	(0.91)
Strike	0.011	0.019
	(0.13)	(0.22)
Shutdown	–0.172**	–0.173**
	(–2.26)	(–2.28)
Disaster	0.045	0.049
	(0.40)	(0.40)
Year fixed effect	Yes	Yes
Establishment fixed effect	Yes	Yes
N	13,364	13,364
Log likelihood	–62,951	–63,001
Exposure variable	Hours	Hours

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents the results of the cross-sectional analyses. Panels A, B, C and D report the effects of analyst coverage, union membership, workers' compensation premium and social capital on the association between share pledging and workplace safety. The dependent variable *Employee Injuries* is the total number of injuries and illnesses cases in the establishment during the year. Detailed definitions of the variables are in Appendix D. The

robust standard errors are clustered at the firm level. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

4.7.2 Cross-Sectional Test: Union Membership

The empirical literature finds that labour unions can help to promote safe working practices (Gillen et al., 2002; Morantz, 2013). For example, Morantz (2013) documents that union workers in mining firms experience significantly lower numbers of traumatic injuries and fatalities. If labour unions constrain pledging managers' abilities to compromise workplace safety, a greater union membership could mitigate the positive impact of share pledging on workplace injuries.

The Union Membership and Coverage Database has provided data on industry union membership annually since 1986.[28] The database reports the percentage of employees who are union members for each industry. I create a dummy variable *High Union* that equals 1 if an establishment's union membership is above the median value and 0 otherwise.[29] Then, I incorporate *High Union* and the interaction between the pledge variables (*Pledge Dummy* and *Pledge Ratio*) and *High Union* into Equation (1) and re-estimate the baseline models.

Consistently with my prediction, Panel B of Table 4.5 reports the negative and statistically significant coefficients of the interaction terms (Column (1): $\beta 1 = -0.131$, z-statistic = −1.82; Column (2): $\beta 1 = -0.860$, z-statistic = −4.45). Specifically, the negative coefficient of *Pledge Dummy* × *High Union* in Column (1) suggests that pledging firms that have more union presence exhibit fewer workplace injuries than those that have lower union membership. The coefficients of *Pledge Dummy* ($\beta 2 = 0.151$, z-statistic = 2.60) and *Pledge Ratio* ($\beta 2 = 0.505$, z-statistic = 2.74) remain positive and significant. To conclude, the association between share

[28] Available at Union Membership and Coverage Database (unionstats.com).

[29] Caskey and Ozel (2017) define *High Union* as a dummy variable that equals 1 if an establishment's union membership is in the sample's top quartile. I obtain similar results by using their definition of *High Union*.

pledging and workplace safety becomes weaker when establishments operate in industries that have high levels of union membership.

4.7.3 Cross-Sectional Test: Workers' Compensation Premium Rate

Caskey and Ozel (2017) posit that managers are less likely to sacrifice employee safety when they bear higher worker compensation costs. Specifically, companies must pay more compensation to injured employees if the workers' compensation premium rate is high. This study investigates whether higher compensation premium rates can weaken the relationship between share pledging and workplace injuries.

The Oregon Department of Consumer and Business Services (DCBS) provides data on workers' compensation premium rates.[30] The DCBS reports the baseline premium rates for each US state every two years. For example, the premium rates ranged from US$1.02 in North Dakota to US$3.33 in Montana in 2010. Following Caskey and Ozel (2017), I assume that the premium rates apply to the current year and the previous year (i.e. 2010 premium rates apply to 2009 and 2010).

High Premium is an indicator variable that equals 1 if an establishment is located in a US state that has a premium rate above the median value and 0 otherwise. Panel C of Table 4.5 presents the results from estimating Equation (1) using the interaction terms *Pledge Dummy* × *High Premium* and *Pledge Ratio* × *High Premium.* The results show that the interaction terms are significant and negative (Column (1): $\beta 1 = -0.057$, z-statistic = −2.46; Column (2): $\beta 1 = -0.232$, z-statistic = −2.39), which indicates that pledging firms that are located in US states that have high compensation premiums have fewer injuries than their counterparties. In summary, when establishments operate in US states that have high workers'

[30] Available at https://www.oregon.gov/dcbs/cost/Pages/report-resouces.aspx

compensation costs, the documented relationship between share pledging and injuries attenuates.

4.7.4 Cross-Sectional Test: Social Capital

Community solidarity, social networks and trust can promote better employee safety (Amin et al., 2021). In particular, Amin et al. (2021) document that the number of injuries that are sustained by employees in establishments that operate in more religious counties is lower than those in less religious counties because more religious US counties exhibit better social networking, trust and solidarity. I examine whether the effect of share pledging on workplace injuries is mitigated in establishments that are located in counties that have a high level of social capital.

Following Hasan et al. (2017), I measure county-level social capital as the first principal component of the factor analysis. The four factors are obtained from the Northeast Regional Center for Rural Development (NRCRD) and are as follows: the voter turnout in presidential elections, the response rate in US census surveys, the number of non-profit organisations and the number of 10 types of social organisations. Following Amin et al. (2021), I define *High Norm* as an indicator variable that equals 1 if the establishment is located in a county that has social capital above the median value.

I present the findings in Panel D of Table 4.5. The regression coefficients of the interaction terms are negative and statistically significant in the two columns (Column (1): $\beta 1 = -0.277$, z-statistic = −2.60; Column (2): $\beta 1 = -1.083$, z-statistic = −1.82). These findings show that the association between pledging and workplace injuries is weaker when establishments are located in counties that have high levels of social capital. This is consistent with the conjecture that social trust can encourage managers to behave socially responsibly.

4.7.5 Robustness Test: Alternative Measure of Workplace Injuries

The primary analyses use the total number of injury and illness cases as a proxy for workplace safety. In this section, I consider an alternative measure of workplace injuries that has been widely documented in prior studies (Amin et al., 2021; Bradley et al., 2022), that is, the total case rate (TCR), which represents the incident rate per 100 workers in an establishment-year. Following prior studies (Amin et al., 2021; Bradley et al., 2022), I measure TCR as the total number of injuries and illnesses divided by the number of working hours in a given establishment-year multiplied by 200,000.

Given that TCR is a continuous variable, I employ the following OLS model to examine the relationship between share pledging and injury rates:

$$\begin{aligned} TCR_{it} = {} & \beta_0 + \beta_1 Pledge_{it} + \beta_2 Firm\ Size_{it} + \beta_3 Leverage_{it} + \beta_4 MTB_{it} + \beta_5 Turnover_{it} + \beta_6 PPE_{it} \\ & + \beta_7 Capital\ Expenditures_{it} + \beta_8 Establishment\ Size_{it} + \beta_9 Hours\ Per\ Employee_{it} \\ & + \beta_{10} Seasonal_{it} + \beta_{11} Strike_{it} + \beta_{12} Shutdown_{it} + \beta_{13} Disaster_{it} + e_{it} \end{aligned} \quad (3)$$

where *Pledge* represents one of the two measures of pledging in year *t* (*Pledge Dummy* and *Pledge Ratio*). I control for year and establishment fixed effects. All the variables are as defined in Appendix D.

Table 4.6 presents the OLS regression results. Consistently with the primary findings in Table 4.3, the coefficient estimates of *Pledge Dummy* and *Pledge Ratio* are positive and significant (Column (1): $\beta 1$ = 0.722, *t*-statistic = 1.70; Column (2): $\beta 1$ = 3.705, *t*-statistic = 2.74), which suggests that share pledging is associated with higher injuries rates.

Table 4.6: Robustness Test: Alternative Measure of Workplace Injuries

	(1) *TCR*	(2) *TCR*
Pledge Dummy	0.722*	
	(1.70)	
Pledge Ratio		3.705***
		(2.74)
Firm Size	–0.393	–0.364
	(–1.13)	(–1.04)
Leverage	–3.390***	–3.023**
	(–2.71)	(–2.42)
MTB	–0.549*	–0.528
	(–1.68)	(–1.60)
Turnover	–1.014*	–0.905*
	(–1.89)	(–1.69)
PPE	4.637**	5.277**
	(2.26)	(2.57)
Capital Expenditures	8.346	7.544
	(1.55)	(1.40)
Establishment Size	–0.807***	–0.805***
	(–6.98)	(–6.98)
Hours Per Employee	–0.003***	–0.003***
	(–11.01)	(–10.97)
Seasonal	0.620	0.617
	(1.34)	(1.34)
Strike	–0.933	–0.935
	(–0.53)	(–0.53)
Shutdown	–0.086	–0.085
	(–0.40)	(–0.40)
Disaster	–0.209	–0.210
	(–0.45)	(–0.44)
Intercept	25.67***	24.96***
	(6.83)	(6.62)
Year fixed effect	Yes	Yes
Establishment fixed effect	Yes	Yes
N	12,445	12,445
Adj. R^2	0.491	0.491

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table reports the results of using an alternative measure of workplace safety. The OLS regression model is estimated at the establishment level. The dependent variable *TCR* is the total number of injuries and illnesses divided by the number of working hours in a given establishment-year multiplied by 200,000. The independent variable *Pledge Dummy* in Column (1) is a dummy variable that is coded 1 if the firm's executives and directors pledge shares and 0 otherwise. The independent variable *Pledge Ratio* in Column (2) is the ratio of the aggregated number of shares that is pledged relative to the aggregated number of shares that is owned by firms' executives and directors. Detailed definitions of the variables are in Appendix D. The robust standard errors are clustered at the firm level. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

4.7.6 Robustness Test: Controlling for Accrual-Based Earnings Management

This study posits that pledging managers may conduct real activity management (compromises in workplace safety) to inflate earnings and thus secure share prices. Singh (2018) documents a positive relationship between share pledging and accrual-based earnings management, which indicates that managers manipulate accruals to support share prices. To investigate whether the primary findings are robust to the control of accrual-based earnings management, I incorporate discretionary accruals (*DTA*) into Equation (1). The discretionary accruals are estimated according to the modified Jones model (Kothari et al., 2005).

The unreported results show that the coefficients of *Pledge Dummy* and *Pledge Ratio* are positive and significant, which is consistent with the primary findings.

4.7.7 Robustness Test: Firm-Level Analysis

This study conducts establishment-level analysis in the main tests because workplace injury data are at the establishment level. Following the existing literature (Bradley et al., 2022; Caskey & Ozel, 2017), I also report the results from the firm-level analyses.

I aggregate the firm's establishment-level injury and illness cases for the firm-level analyses. The dependent variable *Firm Cases* represents the total number of injuries and illnesses that is aggregated across all the establishments for the firm in a year. The exposure variable *Hours* is also aggregated across firms' establishments. Consistently with Caskey and Ozel (2017), I only control for firm characteristics in the firm-level analyses. The Poisson model includes year and firm fixed effects.

Table 4.7 reports the firm-level Poisson regression results. The estimated coefficients of *Pledge Dummy* and *Pledge Ratio* are positive and significant at the 5% level in the two columns (Column (1): $\beta 1$ = 0.179, *z*-statistic = 2.48; Column (2): $\beta 1$ = 0.480, *z*-statistic = 2.14), which is consistent with the establishment-level results.

Table 4.7: Robustness Test: Firm-Level Analyses

	(1)	(2)
	Firm Cases	
Pledge Dummy	0.179**	
	(2.48)	
Pledge Ratio		0.480**
		(2.14)
Firm Size	–0.076	–0.097
	(–1.10)	(–1.34)
Leverage	–0.438*	–0.351
	(–1.65)	(–1.44)
MTB	–0.046**	–0.048**
	(–1.98)	(–2.15)
Turnover	0.031	0.046
	(0.52)	(0.77)
PPE	0.284	0.476**
	(1.44)	(2.09)
Capital Expenditures	–0.041	–0.035
	(–0.18)	(–0.15)
Year fixed effect	Yes	Yes
Firm fixed effect	Yes	Yes
N	1,255	1,255
Log likelihood	–3,089	3,086
Exposure variable	Hours	Hours

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the p-values are from two-tailed tests. This table presents the results of the firm-level analyses. The dependent variable Firm Cases represents the total number of injuries and illnesses aggregated across all establishments for the firm in a year. The independent variable Pledge Dummy in Column (1) is a dummy variable that is coded 1 if the firm's executives and directors pledge shares and 0 otherwise. The independent variable Pledge Ratio in Column (2) is the ratio of the aggregated number of shares that is pledged relative to the aggregated number of shares that is owned by firms' executives and directors. Detailed definitions of the variables are in Appendix D. The robust standard errors are clustered at the firm level. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

4.7.8 Controlling for financial constraints

Financial constraints can trigger insiders' share pledges (Shi, Li, and Liu, 2023), the observed negative effect of share pledging on workplace safety might be endogenously determined by financial constraints (Cohn and Wardlaw, 2016). To investigate whether the primary findings are robust to the control of financial constraints, I incorporate financial constraints into the baseline models and re-estimated the main regressions. Following prior

studies (Chen and Wang, 2012; Ding et al., 2022), I use the KZ index to measure corporate financial constraints (see Ding et al., 2022 for more details).

Table 4.8 shows that the coefficients of *Pledge Dummy* and *Pledge Ratio* are positive and statistically significant, which is consistent with the baseline results.

Table 4.8: Robustness Test: Controlling for financial constraints

	(1) *Employee Injuries*	(2) *Employee Injuries*
Pledge Dummy	**0.158*** **	
	(2.61)	
Pledge Ratio		**0.476*** **
		(3.03)
Firm Size	0.141	0.156
	(1.07)	(1.18)
Leverage	–0.492**	–0.410**
	(–2.49)	(–2.07)
MTB	–0.173**	–0.175**
	(–1.96)	(–1.96)
Turnover	-0.012	0.019
	(-0.09)	(0.15)
PPE	1.087**	1.245**
	(2.04)	(2.39)
Capital Expenditures	–0.410	–0.536
	(–0.32)	(–0.42)
Establishment Size	0.021	0.020
	(0.53)	(0.52)
Hours Per Employee	–0.001***	–0.001***
	(–3.43)	(–3.44)
Seasonal	0.021	0.020
	(0.70)	(0.66)
Strike	0.019	0.019
	(0.23)	(0.23)
Shutdown	–0.167**	–0.169**
	(–2.22)	(–2.23)
Disaster	0.040	0.045
	(0.35)	(0.38)
Financial Constraints	–0.002	–0.002
	(–0.88)	(–0.90)
Year fixed effect	Yes	Yes
Establishment fixed effect	Yes	Yes
N	13,364	13,364
Log likelihood	–61,905	–61,879
Exposure variable	Hours	Hours

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the p-values are from two-tailed tests. This table presents the results of controlling for financial constraints. The models are estimated at the establishment level. The dependent variable *Employee Injuries* is the total number of injuries and illnesses cases in the establishment during the year. The independent variable *Pledge Dummy* is an indicator variable that is coded 1 if the firm's named executives and directors pledge shares during the year and 0 otherwise. The independent variable *Pledge Ratio* is the ratio of the aggregated number of shares that is pledged relative to the aggregated

number of shares that is owned by firms' named executives and directors. Detailed definitions of the variables are in Appendix D. The robust standard errors are clustered at the firm level. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

4.8 Additional Test

4.8.1 Pledging, Meeting Earnings Benchmarks and Workplace Safety

Firms' stock prices decline (increase) if managers miss (meet) earnings thresholds such as zero-earnings and analyst earnings forecasts (Bowen et al., 2005; Degeorge et al., 1999). Caskey and Ozel (2017) document a positive association between meeting earnings benchmarks and injury rates. Given that managers that pledge stocks support share prices through manipulating accruals (Singh, 2018) and conducting real earnings management (Deren & Ke, 2018; Singh, 2018), they can also increase share prices by meeting earnings targets. If pledging managers meet or beat earnings thresholds to secure share prices, the relationship between share pledging and workplace injuries is expected to be more pronounced for firms that meet earnings targets.

Following Caskey and Ozel (2017), I create an indicator variable (*Suspect*) that equals 1 if the parent company of the establishments meets or beats the mean analyst forecasts by 2 cents or less. I then re-estimate Equation (1) using the interaction terms for the pledge variables and *Suspect*.

Table 4.9 shows that the coefficients of the interaction terms *Pledge Dummy* × *Suspect* and *Pledge Ratio* × *Suspect* are significant and positive at the 1% level (Column (1): $\beta 1 = 0.147$, z-statistic = 3.41; Column (2): $\beta 1 = 0.582$, z-statistic = 2.66). This suggests that pledging firms experience higher numbers of workplace injuries when they meet or beat analyst forecasts. Overall, these findings are consistent with the conjecture that pledging insiders

manage earnings to support share prices and the attempts to manage earnings are related to an increase in workplace injuries.

Table 4.9: Share Pledging and Workplace Safety—Meet and Beat Earnings Benchmarks

	(1) *Employee Injuries*	(2) *Employee Injuries*
Pledge Dummy × *Suspect*	**0.147*****	
	(3.41)	
Pledge Ratio × *Suspect*		**0.582*****
		(2.66)
Pledge Dummy	0.128**	
	(2.15)	
Pledge Ratio		0.154
		(1.17)
Suspect	–0.018	–0.021
	(–0.31)	(–0.37)
Firm Size	0.160	0.167
	(1.25)	(1.30)
Leverage	–0.338	–0.310
	(–1.51)	(–1.37)
MTB	–0.162*	–0.165*
	(–1.82)	(–1.84)
Turnover	0.053	0.060
	(0.39)	(0.45)
PPE	1.360***	1.350***
	(2.66)	(2.62)
Capital Expenditures	–0.357	–0.326
	(–0.32)	(–0.29)
Establishment Size	0.019	0.019
	(0.49)	(0.49)
Hours Per Employee	–0.001***	–0.001***
	(–3.48)	(–3.48)
Seasonal	0.021	0.022
	(0.69)	(0.73)
Strike	0.015	0.018
	(0.17)	(0.21)
Shutdown	–0.175**	–0.174**
	(–2.41)	(–2.38)
Disaster	0.032	0.033
	(0.28)	(0.29)
Year fixed effect	Yes	Yes
Establishment fixed effect	Yes	Yes
N	13,364	13,364
Log likelihood	–63,090	–63,108
Exposure variable	Hours	Hours

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents the additional analysis of the effect of meeting earnings benchmarks on the association between share pledging and workplace safety. The dependent variable *Employee Injuries* is the total number of injuries and illnesses cases in the establishment during the year. *Pledge Dummy* in Column (1) is an indicator variable that is coded 1 if the named executives and directors in a firm pledge shares and 0 otherwise.

Pledge Ratio in Column (2) is the ratio of the aggregated number of shares that is pledged relative to the aggregated number of shares that is owned by the named executives and directors. *Suspect* is an indicator variable that equals 1 if the parent company of establishments meets or beats the mean analyst forecast by 2 cents or less. Detailed definitions of the variables are in Appendix D. The robust standard errors are clustered by firms. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

4.8.2 Margin call pressure

This study argues that pledging managers have incentives to counteract the potential margin call pressure from pledged shares by sacrificing workplace safety. To test the margin call pressure hypothesis, I follow Chan et al. (2018) and use *Pledge under Pressure* as a proxy for margin call threat. Specifically, *Pledge under Pressure* is the pledge ratio if the firm's previous three-month return is below -15%, and 0 otherwise. The independent variables *Pledge Dummy* and *Pledge Ratio* in baseline regressions are then replaced with *Pledge under Pressure* to investigate whether pledging)

Table 4.10 shows that the coefficients on *Pledge under Pressure* are significantly positive at the 10% and 1% levels (Column (1): $\beta 1$ = 0.544, z-statistic = 1.89; Column (2): $\beta 1$ = 0.461, z-statistic = 3.05), suggesting that managers are more likely to compromise workplace safety when there is considerable margin call pressure.

Table 4.10: Share Pledging and Workplace Safety—Margin call hypothesis

	(1) *Employee Injuries*	(2) *Employee Injuries*
Pledge under Pressure	**0.544***	**0.461****
	(1.89)	**(3.05)**
Firm Size	0.035	0.149
	(1.59)	(1.14)
Leverage	0.536***	–0.479**
	(3.31)	(–2.41)
MTB	0.044	–0.172*
	(0.74)	(–1.91)
Turnover	0.187***	0.010
	(3.40)	(0.08)
PPE	0.587***	1.054*
	(2.58)	(1.94)
Capital Expenditures	1.588***	–0.158
	(2.61)	(–0.14)
Establishment Size	-0.407***	0.019
	(-5.40)	(0.49)
Hours Per Employee	-0.001***	–0.001***
	(-6.82)	(–3.47)
Seasonal	0.258***	0.021
	(4.32)	(0.70)
Strike	-0.166	0.027
	(-0.60)	(0.32)
Shutdown	0.0204	–0.169**
	(0.30)	(–2.31)
Disaster	-0.216***	0.038
	(-2.69)	(0.33)
Year fixed effect	Yes	Yes
Establishment fixed effect	Yes	Yes
N	13,364	13,364
Log likelihood	–52,752	–63,176
Exposure variable	Hours	Hours

Note. ***, ** and * represent significance at the 1%, 5% and 10% level, respectively. All the *p*-values are from two-tailed tests. This table presents the additional analysis of the effect of margin call pressure on the association between share pledging and workplace safety. The dependent variable *Employee Injuries* is the total number of injuries and illnesses cases in the establishment during the year. *Pledge under Pressure* is the pledge ratio if the firm's previous three-month return is below -15%, and 0 otherwise. Detailed definitions of the variables are in Appendix D. The robust standard errors are clustered by firms. All the continuous variables are winsorised at the first and ninety-ninth percentiles.

4.9 Conclusion

This study investigated whether share pledging has an impact on workplace safety. Managers that have stock pledges may resort to earnings management (Singh, 2018), share repurchases (Chan et al., 2018) and M&A activities to safeguard share prices to avoid a margin call or a forced sale of shares (Chan et al., 2018). Prior studies document that managers may compromise workplace safety when facing pressure from earnings targets (Caskey & Ozel, 2017) and short sellers (Bai et al., 2020). This study provides evidence that share pledging is also positively associated with workplace injuries, which suggests that pledging managers who have incentives to support share prices may sacrifice workplace safety.

Using establishment-level data on workplace injuries and illnesses, this study shows that the existence of share pledging and the percentage of shares that is pledged are positively related to injury and illness cases. The higher rates of injuries in pledging firms are associated with reduced safety investments but not with increased employee workloads.

The cross-sectional analyses suggest that the effect of share pledging on workplace injuries is weaker for firms that have more analysts following, face higher pressure from labour unions, are located in US states that have higher workers' compensation premiums and operate in counties that have higher levels of social capital. The findings are robust to an alternative workplace safety measure (TCR), the control for accrual-based earnings management and firm-level analyses. Last, the additional analysis shows that pledging managers could inflate earnings to support share prices and the attempts to manage earnings are related to a cut in workplace safety.

Overall, the findings are consistent with the view that pledging managers conduct real activities management (compromises in workplace safety) to relieve margin-call pressure and to maintain their control rights and those real activates are harmful to employees.

Chapter 5: Conclusion

5.1 Introduction

This thesis explored whether managers behave opportunistically when conducting hedging activities. Study 1 investigated managerial behaviours in firm-level hedging activities, that is, the use of financial derivatives. In particular, the first study examined whether the implementation of mandatory derivative disclosures under FAS 161 affects the relationship between corporate derivative use and accruals management. Studies 2 and 3 focused on individual-level hedging activities, that is, insider share pledging. Specifically, I investigated the impact of share pledging on non-GAAP reporting in the second study and the relationship between share pledging and workplace safety in the third study. Section 5.2 presents a summary of these three studies. Section 5.3 discusses the limitations and makes suggestions for future research.

5.2 Summary of Three Studies

5.2.1 Study 1: Derivative Hedging and Earnings Management—Evidence from FAS 161

The first study of this thesis examined the impact of FAS 161 implementation on the relationship between corporate derivative use and accrual management. Prior studies document that derivatives and discretionary accruals are substitutes for income smoothing before 1996 (Barton, 2001; Pincus & Rajgopal, 2002). However, Choi et al. (2015) find that derivatives are positively associated with discretionary accruals in the post-FAS 133 period (2002–2006), which suggests a complementary relationship between these two income-smoothing methods. This study predicted that the positive relationship between derivatives and discretionary accruals in the pre-FAS 161 period would be attenuated after the implementation of FAS 161

because the costs of using derivatives for smoothing earnings increase and the effectiveness of using derivatives decreases.

The empirical results from estimating simultaneous equations show that derivatives are positively associated with discretionary accruals before FAS 161, which confirms Choi et al.'s (2015) finding that derivatives and discretionary accruals are complements before the FAS 161 implementation. Further, the analysis suggests that the positive relationship between derivatives and discretionary (current and working capital) accruals is weaker after the implementation of FAS 161, which is consistent with my prediction. Overall, this study provides evidence that FAS 161 altered the relative merit of using derivatives in contrast to discretionary accruals for income smoothing.

The primary findings are robust to alternative measures of discretionary accruals, to controlling for cash-flow volatility, to excluding speculators and restatement firms and to using a sample of continuing firms. To eliminate the alternative explanation that the results are driven by macroeconomic shocks in FX rates and IRs, I control for these two factors and obtain similar results. The additional analysis indicates that the weakening in the relationship between derivatives and accruals management is more pronounced for firms that have higher disclosure costs and for those in more competitive industries.

5.2.2 Study 2: Impact of Share Pledging on Non-GAAP Reporting

The second study investigated whether insider share pledging affects firms' non-GAAP reporting practices. This study was motivated by the SEC's concern that share pledging by insiders potentially affects managerial behaviour and by the fact that managers have increased their voluntary non-GAAP earnings disclosures in the past two decades.

The findings indicate that the existence of share pledging and the percentage of shares that is pledged are positively associated with the likelihood and the frequency of non-GAAP

reporting. Managers are more likely to report non-GAAP earnings and to make more frequent non-GAAP reports when there is a considerable amount of margin-call pressure.

The tests of the impact of share pledging on the quality of non-GAAP reporting suggest that managers disclose non-GAAP earnings opportunistically when their shares are under pledge because share pledging is negatively associated with the non-GAAP exclusion quality. The cross-sectional test results indicate that the effect of share pledging on the propensity and the quality of non-GAAP reporting is more pronounced for firms that have better corporate governance, which has analyst coverage and board independence as proxies.

Furthermore, I employed a PSM approach and performed a DID analysis to investigate whether the results are sensitive to the baseline models. These results are consistent with the primary findings. Overall, the findings support the view that insiders that pledge shares opportunistically report non-GAAP earnings to bias investor perceptions to relieve margin-call pressure and to maintain their control rights.

5.2.3 Study 3: Does Share Pledging Affect Workplace Safety?

The third study aimed to explore the impact of share pledging on workplace safety, which has workplace injuries and illnesses as a proxy. This study was motivated by prior studies that document that managers compromise workplace safety to inflate earnings (Caskey & Ozel, 2017). It is well established that managers can support share prices through earnings management (Cheng & Warfield, 2005; Cohen et al., 2008). Given that managers that have pledged shares face margin-call pressure and have incentives to secure share prices (Chan et al., 2018; Singh, 2018; Zhao et al., 2019), they could conduct real activity management (compromises in workplace safety) to support share prices.

The analyses use establishment-level workplace injury and illness data. The findings from estimating the negative binomial and Poisson regression models suggest that the existence of share pledging and the percentage of shares that is pledged are positively associated with

workplace injury and illness cases. The higher rates of injuries in pledging firms are attributed to reduced safety investments but not to increased employee workloads.

The findings from the cross-sectional analyses indicate that the effect of share pledging on workplace injuries is weaker for firms that have more analyst coverage, face higher pressure from labour unions, are located in US states that have higher workers' compensation premiums and operate in counties that have higher levels of social capital. These findings suggest that managers are less likely to sacrifice workplace safety when their abilities are constrained.

The primary findings are similar when using an alternative workplace safety measure, conducting firm-level analyses and incorporating accrual-based earnings management into the baseline model. The additional analysis indicates that pledging managers could inflate earnings to support share prices and the attempts to manage earnings are related to compromises in workplace safety.

Overall, I interpret the findings as supporting the view that pledging managers conduct real activities management (compromises in workplace safety) to support share prices and to relieve margin-call pressure. However, those real activities are detrimental to employee welfare.

5.3 Limitations and Suggestions for Future Research

The finding of this thesis must be interpreted in light of several limitations. First, FAS 161 was implemented at a single point and the usual concerns emerge about the possibility of the results arising from other events that occurred during the post-FAS 161 implementation period. Second, following Barton (2001) and Choi et al. (2015), I use the notional amount of derivatives as a proxy for corporate derivative use in Study 1. However, a recent study suggests that neither the notional amount nor the fair value of derivatives is a perfect measure of the extent of firms' derivative use (Campbell et al., 2019). Future studies may use available data on derivatives after the implementation of FAS 161, such as the gains or the losses on derivatives, to measure

derivative use. The third limitation of Study 1 is the inability to separate the derivatives that are used primarily for risk management from the derivatives that are used for smoothing earnings. Future research is needed, where data allow, to better understand manager preferences for smooth earnings streams and, specifically, the ways that managers use derivatives to hedge risk and to smooth earnings.

Fourth, the documented association between share pledging and non-GAAP reporting in Study 2 should be more pronounced if the market price of pledged shares is close to the margin-call threshold but no publicly listed firms in the US disclose margin-call data on their proxy statements. To mitigate this concern, I follow Chan et al. (2018) and measure margin-call pressure according to firms' previous three-month returns. Future studies may explore this in a setting in which the pledging contract is available because the margin-call threshold is generally specified in pledging contracts.

Last, Study 3 documents that reductions in safety investments, which has abnormal discretionary expenses as a proxy, contribute to the high rate of workplace injuries in firms that have share pledging. This line of research (Amin et al., 2021; Caskey & Ozel, 2017) and my study acknowledge that abnormal discretionary expenses do not perfectly capture the safety-related expenses of firms. Nonetheless, this study provides evidence that decreased safety investments is a potential channel by which share pledging affects workplace safety. Future studies can better measure firms' safety-related expenses when data are available.

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Appendices

Appendix A: Variable Definitions (Chapter 2)

Variable	Variable definition and measurement	Data source
DERIVATIVES (*notional ratio*)	Derivative use ratio calculated as the notional amount of the derivatives divided by the total assets.	EDGAR
DERIVATIVES (*fair value ratio*)	Derivative use ratio calculated as the fair value of the derivatives divided by the total assets.	EDGAR
HEDGING	Hedging derivative use ratio calculated as the notional amount of hedging derivatives divided by the total assets. Hedging derivatives are those that receive hedge accounting treatment under FAS 133. In the pre-FAS 161 period, firms generally did not disclose the accounting designation of derivatives; thus, derivatives are treated as hedging derivatives unless firms specify otherwise.	EDGAR
NON-HEDGING	Non-hedging derivative use ratio calculated as the notional amount of non-hedging derivatives divided by the total assets.	EDGAR
FAS 161	Indicator variable for post-FAS 161 that is coded 1 if the fiscal year is between 2009 and 2016 and 0 otherwise.	
User	Indicator variable for derivative users that is coded 1 if a firm uses financial derivatives and 0 otherwise.	
|*DTC*|	One of the three measures of discretionary accruals (|DTACC|, |DWCACC| or |DCACC|).	Compustat
|*DTACC*|	Discretionary total accruals (*TAC*).	Compustat
|*DCACC*|	Discretionary current accruals (*CAC*).	Compustat
|*DWCACC*|	Discretionary working capital accruals (*WCA*).	Compustat
TAC	Total accruals defined as the difference between earnings before extraordinary items (Compustat #18) and operating cash flows from continuing operations (Compustat #308–Compustat #124).	Compustat
CAC	Current accruals defined as the change in non-cash current assets (Compustat #4–Compustat #1) less the change in current liabilities excluding the change in short-term debt (Compustat #5–Compustat #34) less the depreciation and amortisation expenses (Compustat #14).	Compustat
WCA	Working capital accruals defined as the change in non-cash current assets (Compustat #4–Compustat #1) less the change in current liabilities excluding the change in short-term debt (Compustat #5–Compustat #34).	Compustat
$\Delta Debt_t$	The change in short-term debts.	Compustat

Variable	Variable definition and measurement	Data source
$\Delta Cash_t$	The change in cash holdings.	Compustat
$\Delta Sales_t$	The difference in sales revenues between years t–1 and t.	Compustat
ΔCA_t	The change in current assets between years t–1 and t.	Compustat
ΔCL_t	The change in current liabilities.	Compustat
DEP_t	The depreciation and amortisation expenses in year t.	Compustat
PPE_t	Gross property, plant and equipment at the end of year t–1.	Compustat
ROA_{t-1}	Net profit in year t–1 scaled by the average total assets.	Compustat
A_{t-1}	The total assets at the end of year t–1.	Compustat
CEO_Incen	One minus the ratio of CEO salary to CEO total compensation.	ExecuComp
Cash_Comp	CEO salary and bonus divided by CEO total compensation.	ExecuComp
CEO_Stock	The value of the stocks that are held by the CEO divided by CEO total compensation.	ExecuComp
CEO_Option	CEO option compensation divided by CEO total compensation.	ExecuComp
Tax Convexity	The excess of the marginal tax rate divided by the average tax rate.	Compustat
Leverage	The total debts divided by the total assets.	Compustat
Distress	Probability of bankruptcy estimated by using Ohlson's (1980) bankruptcy prediction model.	Compustat
R&D	The R&D expenses divided by the total assets.	Compustat
Diversification	The entropy index of business segment sales.	Compustat
Ana_Cov	The number of analysts following the firm in the fourth quarter of a calendar year.	I/B/E/S
Term Spread	The difference between the US 10-year Treasury constant maturity rate and the US two-year Treasury constant maturity rate.	
Dollar Strength	The broad US dollar index.	US Federal Reserve
Foreign_Sale	The sales from the foreign divisions divided by the total sales.	Compustat
Firm Size	The natural logarithm of the total assets.	Compustat
Short_Debt	The short-term debt divided by the total debt.	Compustat
Cash_Cycle	The cash-conversion cycle in number of days.	Compustat
Earning Volatility	The standard deviation of earnings before extraordinary items for the past five years divided by the average total assets.	Compustat
CF_Volatility	The variance of operating cash flows over the past five years scaled by the total assets.	
Cap_Intensity	The capital expenditures divided by the total assets.	Compustat
Segment	The number of operating segments.	Compustat

Variable	Variable definition and measurement	Data source
Div_Yield	The cash dividend divided by the market value of equity.	Compustat
Div_Pay	The cash dividend divided by the net income.	Compustat
LIFO_Reserve	The LIFO reserve divided by the lagged total assets.	Compustat
|CFO|	The absolute value of the ratio of cash flows from operations to the lagged total assets.	Compustat
BTM	The book value of equity divided by the market value of equity.	Compustat
Flexibility	The root mean squared error of total accruals regression by using Equation (1).	Compustat
HHI	The Herfindahl–Hirschman index (HHI) calculated as the sum of the squared market shares of firms within an industry (four-digit SIC).	Compustat

Appendix B: Variable Definitions (Chapter 3)

Variable	Definition	Data source
Dependent variable		
Non-GAAP	An indicator variable that is coded 1 if the firm discloses non-GAAP earnings in any quarter during year *t* and 0 otherwise.	Bentley et al. (2018)
FreqNonGaap	The percentage of quarters in year *t* in which managers disclose non-GAAP earnings.	Bentley et al. (2018)
CONSENSUS	An indicator variable that is coded 1 if the non-GAAP earnings meet or beat the analysts' consensus forecast while the GAAP earnings fall short and 0 otherwise.	Bentley et al. (2018), I/B/E/S
RECUR	Indicator variable that is coded 1 if the non-GAAP earnings exclude recurring items and 0 otherwise.	Bentley et al. (2018), Compustat
FOI	Future operating income, which is the earnings per share from operations in year $t + 1$.	Compustat
PROMINENCE	Indicator variable that is coded 1 if non-GAAP earnings are mentioned before GAAP earnings in press releases in any quarter during year *t* and 0 otherwise.	Chen et al. (2021)
Pledge under Pressure	The pledge ratio if the firm's previous three-month return is below –15% and 0 otherwise.	Bentley et al. (2018), CRSP
Variable of interest (*Pledge*)		
Pledge Dummy	Indicator variable that is coded 1 if the named executives and directors in a firm pledge shares and 0 otherwise.	DEF 14A
Pledge Ratio	The ratio of the aggregated number of shares that is pledged relative to the aggregated number of shares that is owned by the named executives and directors.	DEF 14A
Control variable		
Size	The natural logarithm of the total assets.	Compustat
Leverage	The total liabilities divided by the total assets.	Compustat
BTM	The book value of equity divided by the market value of equity.	Compustat
Earnings Volatility	The standard deviation of ROA (Compustat item *ibcom/at*) over the past three years.	Compustat

Variable	Definition	Data source
Intangible	The intangible assets divided by the total assets.	Compustat
Loss	An indicator variable that is coded 1 if the firm's earnings before extraordinary items (Compustat item *ibcom*) is less than zero and 0 otherwise.	Compustat
High Tech	An indicator variable that is coded 1 if the firm is in a high-tech industry and 0 otherwise.	Compustat
SPI	An indicator variable that is coded 1 if the firm reports special items (Compustat item *spi*) and 0 otherwise.	Compustat
Special Items	Special items (Compustat item *spi*) divided by the total assets multiplied by –1.	Compustat
Big Bath	An indicator variable that is coded 1 if the firm reports negative GAAP earnings and income-decreasing special items and 0 otherwise.	Compustat
Sales Growth	The annual percentage change in sales.	Compustat
Age	The natural logarithm of firm age.	Compustat
RET	The cumulative monthly stock return during the past 12 months.	CRSP
DTA	The discretionary accruals estimated by using a modified Jones (1991) model.	Compustat
Post	An indicator variable that is coded 1 if the year is after the firm's initiation of pledging and 0 otherwise.	
Institutional Ownership	The number of shares that is held by institutional shareholders divided by the total number of shares.	Thomson Reuters Institutional Shareholdings 13f file
Board Independence	The number of independent directors relative to the total number of directors.	BoardEx
Analyst Coverage	The number of analysts following.	I/B/E/S
NonGaapEPS	Non-GAAP earnings per share.	Bentley et al. (2018)
Total Exclusion	Non-GAAP earnings per share minus GAAP earnings per share summed over the four quarters in year *t*.	Bentley et al. (2018) Compustat

Appendix C: Covariate Balances After Propensity Score Matching

	Pledging firms (n = 1,753)			Control firms (n = 1,753)			Difference	Difference in mean
	Mean	Median	Std	Mean	Median	Std	in mean	(*p*-value)
Size	8.087	8.178	1.705	7.990	7.924	1.841	0.097	0.106
Leverage	0.630	0.609	0.239	0.634	0.629	0.256	–0.004	0.702
BTM	0.245	0.213	0.178	0.245	0.211	0.182	0.000	0.945
Earnings Volatility	0.038	0.013	0.130	0.042	0.015	0.126	–0.004	0.356
Intangible	0.164	0.067	0.200	0.155	0.050	0.198	0.009	0.178
Loss	0.184	0	0.387	0.167	0	0.373	0.017	0.198
High Tech	0.113	0	0.317	0.109	0	0.312	0.004	0.707
SPI	0.993	1	0.082	0.994	1	0.079	–0.001	0.834
Special Items	0.009	0.001	0.036	0.009	0	0.040	0.000	0.949
Big Bath	0.127	0	0.333	0.115	0	0.319	0.012	0.255
Sales Growth	0.191	0.089	0.608	0.125	0.077	0.602	0.066	0.044

Appendix D: Variable Definitions (Chapter 4)

Variable	Definition	Data source
Workplace safety variable		
Employee Injuries	The total number of injuries and illnesses in the establishment during the year.	OSHA
TCR	The total case rate measured as the total number of injuries and illnesses divided by the total number of working hours in a given establishment-year and multiplied by 200,000.	OSHA
Firm Cases (*Firm Level*)	The total number of injuries and illnesses in a firm aggregated across all establishments for the firm in a year.	OSHA
Stock pledges variable		
Pledge Dummy	An indicator variable that is coded 1 if the firm's named executives and directors pledge shares during the year and 0 otherwise.	DEF 14A
Pledge Ratio	The ratio of the aggregated number of shares that is pledged relative to the aggregated number of shares that is owned by the firm's named executives and directors.	DEF 14A
Control variable		
Firm Size	The natural logarithm of the firm's total assets at the beginning of the year.	Compustat
Leverage	The firm's total liabilities scaled by the total assets at the beginning of the year.	Compustat
MTB	The firm's beginning market value of assets divided by the beginning book value of assets.	Compustat
Turnover	The firm's current-year sales scaled by the beginning-of-year total assets.	Compustat
PPE	The firm's net property, plant and equipment divided by the total assets measured at the beginning of the year.	Compustat
Capital Expenditures	The firm's current-year capital expenditures scaled by the beginning total assets.	Compustat
Establishment Size	The natural logarithm of the average number of employees in the establishment.	OSHA
Hours Per Employee	The total number of working hours in the establishment during the year scaled by the total number of employees.	OSHA
Seasonal	An indicator variable that is coded 1 if there are seasonal workers in the establishment during the year and 0 otherwise.	OSHA

Variable	Definition	Data source
Strike	An indicator variable that is coded 1 if there is a strike in a certain establishment-year and 0 otherwise.	OSHA
Shutdown	An indicator variable that is coded 1 if there is a shutdown in a certain establishment-year and 0 otherwise.	OSHA
Disaster	An indicator variable that is coded 1 if the establishment is affected by natural disasters during the year and 0 otherwise.	OSHA
Hours	The number of hours worked by employees.	OSHA
Production	The firm's sum of cost of sales and the change in inventory scaled by the total number of employees.	Compustat
SGA	The total selling, general and administrative expenses of the firm.	Compustat
EMP	The total number of employees of the firm.	Compustat
Sales	The total sales of the firm.	Compustat
DisExp	The abnormal discretionary expenses calculated in line with Caskey and Ozel (2017).	Compustat
High Analyst	An indicator variable that is coded 1 if the parent company of the establishments has an analyst following that is above the median value and 0 otherwise.	I/B/E/S
High Union	An indicator variable that is coded 1 if the union membership of an establishment is above the median value and 0 otherwise.	Union membership and coverage
High Premium	An indicator variable that is coded 1 if an establishment is located in a state that has a premium rate that is above the median value and 0 otherwise.	DCBS
High Norm	An indicator variable that is coded 1 if the establishment is located in a county that has social capital that is above the median value and 0 otherwise.	NRCRD
DTA	The discretionary accruals estimated by using a modified Jones model.	Compustat
Suspect	An indicator variable that is coded 1 if the parent company of the establishments meets or beats the analyst forecast by 2 cents or fewer and 0 otherwise.	I/B/E/S

Appendix E: Summary Statistics (Chapter 4)

Panel A: Observations by Year

Year	Number of observations (establishments)
2007	3645
2008	3922
2009	2525
2010	2452
2011	818

This table presents the number of observations by year.

Panel B: Average Injury Cases by Industry

Industry	Number of establishments	Average employees	Average injury/illness cases
Mining	57	146	7
Construction	7	102	6
Manufacturing	8,037	236	21
Transportation & Public Utilities	2,440	281	40
Wholesale Trade	264	75	10
Retail Trade	42	295	38
Services	1703	146	16
Other	814	272	14

This table presents the average number of employees and injury/illness cases across industries for all establishments. Industry definitions are based on two-digit SIC codes.

Appendix F: Correlation Matrix (Chapter 4)

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
(1) *Employee Injuries*	1.000													
(2) *Pledge Dummy*	**0.099**	1.000												
(3) Firm Size	**0.146**	**0.144**	1.000											
(4) Leverage	0.001	**-0.160**	**0.066**	1.000										
(5) *MTB*	**0.062**	**0.417**	**0.199**	**-0.367**	1.000									
(6) Turnover	0.007	**0.110**	**-0.555**	**-0.298**	**0.119**	1.000								
(7) PPE	**0.024**	**0.208**	**-0.249**	**0.090**	0.011	**0.103**	1.000							
(8) Capital Expenditures	**0.044**	**0.140**	**-0.277**	**-0.226**	**0.227**	**0.457**	**0.378**	1.000						
(9) Establishment Size	**0.439**	**0.057**	**0.302**	**-0.121**	**0.108**	**-0.055**	**-0.177**	**-0.044**	1.000					
(10) Hours Per Employee	**-0.111**	**-0.304**	0.001	**0.070**	**-0.094**	**-0.167**	**-0.273**	**-0.140**	**-0.037**	1.000				
(11) Seasonal	**0.038**	**0.042**	**0.035**	**-0.032**	**0.095**	-0.004	-0.014	0.013	**0.020**	-0.002	1.000			
(12) Strike	0.010	-0.011	0.002	0.002	-0.010	-0.007	-0.011	-0.003	**0.032**	0.003	0.008	1.000		
(13) Shutdown	0.002	**-0.056**	**0.017**	**0.026**	**-0.118**	**-0.094**	**-0.109**	**-0.140**	**0.027**	**0.056**	0.012	0.009	1.000	
(14) Disaster	-0.011	**-0.018**	0.004	-0.005	-0.005	-0.010	-0.007	-0.003	0.005	**0.025**	**0.036**	-0.002	**0.032**	1.000

This table presents the Pearson correlation coefficients of variables used in this study. All the continuous variables are winsorised at the first and ninety-ninth percentiles. Correlations significant at the 5% level or better are in bold. All variables are defined in Appendix D.