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Dedication

I dedicate this dissertation to my parents, Lalitha Raman and K.S. Raman, and my uncle, K.S.S. Rao. I could not have completed this process with their continued support and encouragement. I also dedicate this work to the loving memory of my sister, Divya Pulkeshi.

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Table of Contents

Acknowledgements iv

List of Tables viii

List of Figures x

Abstract xi

Chapter 1: The Who, Why, and How Well of Order Revisions: An Analysis of Limit Order Trading 1

1. Introduction 1
2. Literature Review and Hypotheses 11
   - 2.1. Literature on Limit Order Revisions 11
   - 2.2. Contributions and Testable Hypotheses 12
3. Data 19
4. Trader categories, styles, and order revisions 22
5. Duration Analysis of Order Revisions 26
   - 5.1. Order Cancellations 29
   - 5.2. Order Modifications 33
6. Order Revisions and Performance 36
   - 6.1. Results 39
7. Conclusion 43

Chapter 2: Naked Short-Selling, Fails-to-Deliver and Market Quality: The Emperor's New Clothes? 46

1. Introduction 46
2. Development of hypotheses 55

2.1. Naked short selling and pricing efficiency 55

2.2. Naked short selling and liquidity 56

2.3. Manipulative naked short selling and the 2008 financial crisis 57

2.4. Other questions: impact of covered vs. naked short-sales and public traders vs. market makers 58

3. Fails to deliver and naked short selling 59

4. Definitions of other measures and variables 63

5. Empirical results 66

5.1. Data and samples 66

5.2. Preliminary descriptive analysis 67

5.3. Naked short-selling and market quality 68

5.4. The market quality impact of restrictions on naked short selling. 80

5.5. Naked short selling and the 2008 financial crisis. 81

6. Conclusions 90

Chapter 3: Is Hedging Bad News? Evidence from Corporate Hedging Announcements 94

1. Introduction 94

2. Empirical Hypotheses 99

2.1. Background 99

2.2. Market reaction to hedging announcements 102

3. Data and Methodology 106

4. Announcement Effects in the Gold Market 110

4.1. Gold returns and hedging announcements 110

4.2. Gold market reaction to central bank gold sales 113

5. Announcement Effects in the Equity Market 115

5.1. Equity returns and hedging announcements 115

5.2. Industry effects of hedging announcements 118

6. Are Hedging Changes Consistent with Announcements? 120

7. Conclusions 121

References 124

Appendix A: Tables 133

Appendix B: Figures 175

List of Tables

Table 1. Variable Definition, Chapter 1 134

Table 2. Characteristics of Sample Stocks 137

Table 3.Trader Categories 138

Table 4.Order Revisions and Trader Attributes 139

Table 5. Order Cancellations and Trader Inventories: Duration Analysis 140

Table 6. Positive Order Modifications and Trader Inventories: Duration Analysis..... 142

Table 7. Negative Order Modifications and Trader Inventories: Duration Analysis ... 144

Table 8. Order Revisions and Performance: Panel Regressions 146

Table 9. Variable Definitions, Chapter 2.. 151

Table 10. FTDs as a Proxy for Naked Short Selling 152

Table 11. Summary Statistics 153

Table 12. Naked Short Selling and Market Quality, Portfolio Approach 154

Table 13. OLS Regressions 155

Table 14. Summary Results, Impact of Naked and Covered Short Selling 156

Table 15. Impact of Naked Short-Selling for Securities with High/Low Proportion of Naked Short-Selling by Public Traders relative to Market Makers 159

Table 16. The Impact of Restrictions on Naked Short Selling imposed by the SEC between July 21, 2008 and August 12, 2008 160

Table 17. Behavior of the Outstanding Naked Short Ratio (ONSR) of Bear Stearns (Ticker: BSC) and of Lehman Brothers Holdings Inc. (Ticker: LEH) in 2008. 161

Table 18. Naked Short Selling around Credit Rating Downgrades and Large Price Drops 162

Table 19. Attributes of Corporate Hedging Announcements 164

Table 20. Descriptive Statistics of Firm and Event Characteristics 165

Table 21. Effect of Corporate Hedging Announcements on Gold Returns 166

Table 22. Gold Abnormal Returns and Event Characteristics 167

Table 23. Central Bank Announcements 168

Table 24. Effect of Hedging Announcements on Equity Returns 169

Table 25. Equity Abnormal Returns, and Event and Firm Characteristics (Two-Factor Model) 170

Table 26. Equity Abnormal Returns, and Event and Firm Characteristics (Five-Factor Model) 171

Table 27. Industry Effects of Corporate Hedging Announcements 172

Table 28. Industry Abnormal Returns, and Event and Firm Characteristics 173

Table 29. Implementation of Hedging Announcements 174

List of Figures

Figure 1. Order Revisions and Time from Submission 175

Figure 2. Impulse Response Functions, Naked Short Selling 177

Figure 3. Impulse Response Functions, Covered Short Selling 178

Figure 4. Naked Short Selling and Returns in 2008, Bear Sterns 179

Figure 5. Naked Short Selling and Returns in 2008, Lehman 180

Figure 6. Naked Short Selling and Returns in 2008, Merrill Lynch 181

Figure 7. Naked Short Selling and Returns in 2008, AIG 181

Abstract

This dissertation is a collection of three essays that investigates various issues related to market microstructure and risk management. Chapter 1 examines the determinants of traders' decisions to revise orders, and the profitability of traders' order revision strategies using a unique dataset which provides complete information on trades, orders, trader identification codes, and trader categories. The analysis provides three important results. One, informed traders and traders who function as voluntary market makers revise orders most intensely. Two, along with changes in market prices and other market conditions, changes in traders' inventories, including inventories of correlated stocks, influence order revision strategies. Three, informed traders reduce the execution costs of their order portfolios through active order revisions; the benefit is especially pronounced on earnings announcement days, when the value of private information is high. That traders employ revisions to mitigate their order submission, inventory, and adverse selection risks indicates that order revisions are a valuable feature of the rapidly proliferating electronic limit order markets. Chapter 2 examines the impact of the option to fail and the resultant naked short-selling on market quality. For a sample of 1,492 NYSE securities, the study finds that naked short-selling has the same beneficial impact on liquidity and pricing efficiency as covered short-selling. The study does not find any evidence that naked short-sellers engineered price declines or distortions, or triggered the demise of financial firms during the 2008 financial crisis. Hence, the study questions the removal of the option to fail, a potentially valuable tool for limiting settlement-related distortions and stock-borrowing costs of all short-sellers. Chapter 3 examines how the gold and stock markets react to corporate hedging announcements by gold mining firms. The gold market reaction is consistent with the market believing that firms have credible private information about future gold prices, which is puzzling in light of extant evidence that firms cannot successfully time the market when they hedge. After controlling for the gold market announcement effect, the study finds a strong negative (positive) reaction in the stock prices of the firm making the announcement and other gold mining firms to a hedging increase (decrease), suggesting that (a) the announcement also conveys information about a change in the expected cost of financial distress of both the firm and its industry; and (b) any shareholder benefit of a hedging increase is more than offset by the negative news conveyed to shareholders about the change in the firm's prospects, and *vice versa*. These findings provide new insights into the endogeneity associated with hedging policy changes and its confounding effect on measuring the relation between hedging and the value of the firm.

Chapter 1: The Who, Why, and How Well of Order Revisions: An Analysis of Limit Order Trading

1. Introduction

The largest exchanges around the world operate as electronic limit order book markets or at least allow for public limit orders.[1] A unique feature of electronic limit order book (LOB) markets is that liquidity is provided by a pool of voluntary market participants who strategically place limit orders, not by designated market makers. Consequently, examinations of limit order trading strategies employed by such voluntary liquidity providers are integral to our understanding of the evolution of prices and liquidity in LOB markets. This paper focuses on a prominent class of limit order strategies: order revisions. These are dynamic strategies that involve decisions about when and how to modify or cancel prevailing limit orders.[2] Few limit order strategies are as ubiquitous as order revisions. Almost half of all limit orders submitted on the NYSE, the London Securities and Derivatives Exchange, and the Australian Securities Exchange are revised.[3] Further, due to innovations in information technology, the incidence of order revisions has been increasing in recent years at an alarming rate. Hasbrouck and Saar (2009) document that, in 2004, the rate of 'fleeting orders' — order

[1] See Jain (2005) and Swann and Westerholm (2006)

[2] I collectively refer to order cancellations and modifications, wherein the limit price and/or quantity specifications of the order are changed, as order revisions. Order cancellation is effectively a revision of the quoted volume to zero.

[3] Coppejans and Domowitz (2002) , Yeo (2005), and Fong and Liu (2010).

cancellations within two seconds of submission — on INET was 36% of submitted limit orders, twice its value in 1999. [4] Similarly, Hendershott et al. (2011) find that when the NYSE automated the dissemination of the inside quote in 2003, the orders-to-trades ratio, which proxies for the intensity of order cancellations, increased manifold.

The increasing incidence of order revisions, especially cancellations, has recently attracted regulatory scrutiny. Market regulators such as the Commodity Futures Trading Commission (CFTC) and the Securities Exchange Commission (SEC) in the US, the Financial Services Authority (FSA) in the UK, and the Securities and Exchange Board of India (SEBI) have filed charges against numerous market participants for having employed manipulative order cancellation strategies.[5] Even the recently passed Dodd-Frank act specifically discusses manipulative order cancellations, and has added the same to the list of unlawful "Disruptive Practices".[6] Order cancellations are also suspected of having played a significant role during the infamous 'Flash Crash' of May 6, 2010 — when the Dow Jones Industrial Average lost and gained 9% within minutes. Consequently, regulators are considering various actions to discourage order revisions. The SEC is debating the introduction of an order cancellation fee, and the European Commission (EC) has proposed imposing a minimum resting period before an order can be revised and/or limiting traders' order cancellations rates to a pre-specified level. [7]

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[4] INET is an electronic communications network (ECN) LOB market. See Hasbrouck and Saar (2009) for further details.

[5] For example, see CFTC press release PR6007-11; SEC press release 2001-129; SEBI press release 254.

[6] ‘‘Dodd-Frank Wall Street Reform and Consumer Protection Act’’: www.sec.gov/about/laws/wallstreetreform-cpa.pdf

[7] "SEC chief looks to fix market structure", *Reuters* (March 1, 2011). Document titled "Consultation Document" dated December 8, 2010:http://ec.europa.eu/internal_market/consultations/2010/mifid_en.htm

Despite the prevalence of order revision strategies in LOB markets around the world and the recent regulatory concern, few studies have empirically analyzed limit order revisions. Liu (2009), Hasbrouck and Saar (2009), and Fong and Liu (2010) provide valuable characterizations of order revisions. However, probably due to data limitations, our understanding of the rationale for, and the profitability of, order revisions remains incomplete. This paper employs a unique database drawn from one of the largest electronic LOB markets, the National Stock Exchange, India (NSE). The database provides complete information on trades, orders, trader identification codes, and trader classifications for a sample of 50 stocks, which constitute the Standard & Poor's CNX Nifty index, between April 1 and June 30, 2006. [8] The richness of the database enables the paper to answer three important and hitherto unaddressed questions. First, what type of traders revise orders? Second, how do trader inventories and market characteristics affect trader's decision to revise an order? And, third, do traders profit from the active management of their order portfolios through order revisions? Apart from adding to our understanding of order revisions, answers to the aforementioned questions also provide vital insight into the determinants of limit order trading and the role of informed traders in the rapidly evolving LOB markets.

The empirical analysis provides a number of new results. Traders who are members of the exchange (the voluntary dealers at the NSE), traders who curtail the size of their over-night inventories to a small fraction of their daily trading volume, and traders who regularly post a network of buy and sell orders around the mid-quote revise a significantly greater proportion of their orders than others. In sum, the de facto market

[8] The index represents almost 60% of the exchange's market capitalization, and covers 21 sectors of the economy.

makers or middlemen in the market prominently employ order revisions. That the de facto market makers greatly utilize order revisions supports the general implication of inventory management models that dealers with finite capital actively adjust their quotes to manage inventories. [9] Further, traders belonging to financial institutions, frequent traders, and traders who generally place large orders revise a significantly greater proportion of their orders than other traders. This result underlines the role of informed traders[10] in limit order trading; while it contradicts the traditional assumption that informed traders participate only through market orders, it adds to the emergent view[11] that informed traders strategically provide liquidity in LOB markets.

Results from the proportional hazards duration models show that traders closely monitor their outstanding limit orders and strategically respond to changes in their inventories and in market conditions through order revisions. [12] Specifically, consistent with the inventory control models, traders are more likely to cancel or negatively modify — move the limit price away from the prevailing mid-quote — a buy (sell) order when their inventory in the stock increases (decreases) after submitting the order. Similarly, traders are less likely to positively modify a buy (sell) order when their inventory in the stock increases (decreases) post its submission. The results are also

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[9] See, for example, Amihud and Mendelson (1980), O'Hara and Oldfield (1986), Madhavan and Smidt (1993).

[10] Kumar et al. (2009), who employ the same dataset as the current paper, find that orders placed by institutional traders, especially by financial institutional traders, are significantly more informed than those placed by individual traders. Numerous other studies have also found similar evidence in different settings. See, for example, Bartov et al. (2000) and Campbell et al. (2007), Chakravarty (2001), Anand et al. (2005), Boehmer and Kelly (2008), and Boehmer et al. (2008).

[11] See, for example, Bloomfield et al. (2005) and Anand et al. (2005).

[12] Similar to Hasbrouck and Saar (2009), I use hazard models to accommodate time varying covariates. While in their models only the best quotes are time varying, here I also introduce time varying inventory variables.

consistent with the dynamic limit order models proposed by Harris (1998), Foucault et al. (2005), Goettler et al. (2005 and 2009), and Rosu (2011). These models imply that because inventory imbalances increase waiting costs (costs of delayed execution), traders, especially the de facto market makers, will place aggressive orders so as to correct their inventory imbalances. Furthermore, changes to trader inventories in correlated stocks also have a similar effect on order cancellations. For example, traders are more likely to cancel a buy order in stock *s* when their inventories in stocks that belong to the same industry (2 digit SIC) as stock *s* increase after submitting the buy order. This result supports the Ho and Stoll (1983) inventory management model, which implies that traders actively adjust their quotes in a stock to manage their 'equivalent' inventories — inventory in the stock corrected for inventory positions in all other stocks with correlated returns — and not just their ordinary inventory in the stock. The equivalent inventory effect is not statistically significant for order modifications.

Trader category matters. Hazards duration models also show that even after controlling for trader inventories, order characteristics ,and market conditions, an order is more likely to be revised if it is submitted by an institutional trader. This evidence adds further credence to the hypothesis that informed traders revise a greater proportion of their limit orders than the uninformed. Also, consistent with extant literature, aggressively priced orders (Hasbrouck and Saar ,2009 and Fong and Liu, 2010) and large orders (Liu, 2009) are more likely to be revised. I also find evidence in favor of the 'chasing' hypothesis posited by Hasbrouck and Saar (2009); order cancellations and positive order modifications are more likely when prices move away from an order, while negative modifications are less likely.

Finally, panel regression analysis of execution costs of traders' order portfolios show that institutional traders, especially those belonging to financial institutions, significantly benefit from order revisions. Specifically, controlling for market conditions, stock characteristics, and trader's skill (through trader fixed effects), I find a negative relation between the number of times an order in an institutional trader's portfolio is revised and the portfolio's execution cost — measured by a modified[13] version of the Perold (1988) implementation shortfall method. Results also show that institutional traders reduce the adverse selection costs of executed trades and the opportunity costs associated with unexecuted orders through order revisions. These results indicate that institutional traders use order revisions to 'time' the limit order book; when the mid-quote is, say, below the fundamental value, they positively modify buy orders — 'walk up' the book — to ensure executions, and/or they negatively modify or cancel sell orders — 'walk down' the book — to avoid executions. In contradistinction, these results do not hold for individual traders. Bloomfield et al. (2005) argue that informed traders have a competitive advantage in limit order trading because they can manage adverse selection risk better than other traders. The panel regression results show that order revisions are one of the strategies through which

[13]Since order revisions are prominently employed by intermediaries, who are not precommitted to orders, as suggested by Harris and Hasbrouck (1996), the modified measure accounts not only for the cost of order execution (Price Impact) and the opportunity cost of unexecuted orders (Opportunity Cost), but also for the adverse selection spread, measured by the movement in market prices subsequent to the execution of orders (Ex post performance).

informed traders actualize this competitive advantage. Further, financial institutional traders use order revisions to mitigate the incremental execution costs on earnings announcements days, when information uncertainty and the value of private information are high. Bloomfield et al. (2005) show that, in an experimental set-up, informed traders use market orders when the value of information is high. Results here show that informed traders use order revisions to mitigate the incremental costs of liquidity provision when the value of information is high.

This paper directly contributes to the small but growing literature on order revision strategies. To the best of my knowledge, this is the first study to show that informed traders and de facto market makers use order revisions the most, that changes in traders' ordinary and equivalent inventories influence their order revision strategies, and that informed traders use order revisions to reduce the execution costs of their order portfolios. These findings also contribute to our understanding of at least three more important aspects of limit order trading.

First, extant empirical studies on limit order trading have neglected the effect of inventory management on traders' limit order strategies. Current empirical papers have focused mostly on the influence of spreads (e.g., Harris,1998; Biais et al.,1995), depth (e.g., Beber and Caglio, 2002; Ranaldo, 2004), volatility (e.g., Ahn et al., 2001; Handa et al., 2003), and pre-trade transparency (e.g., Aitken et al., 2001; De Winne and D'Hondt, 2007;Bessembinder et al., 2009) on limit order strategies. Bloomfield et al (2005) find that large liquidity traders, when placed with a deadline, place market orders instead of limit orders. However, there results are based on an experimental setup, not an actual LOB market. This is the first paper to provide direct empirical evidence of the inventory effect in limit order books. Similarly, the result that limit order strategies in a stock are also dependent on traders' inventories in related stocks (equivalent inventory) is also a novel finding; this is the first study to find evidence consistent with the Ho and Stoll (1983) model of equivalent inventory management. The inventory effect documented here should be especially instructive because of the increasing role of high frequency traders (HFTs) in LOB markets. HFTs, who account for more than 50% of trading volume in the US and European markets, are generally the implicit market makers in modern LOB markets.[14] More importantly, their trading strategies invariably involve high trading volumes and low (intraday and overnight) inventories.[15] Similar to the exchange members at the NSE, HFTs' order submissions will be significantly influenced by their inventory imbalances. Also, since monitoring costs are negligible for HFTs, they trade simultaneously in multiple securities and markets. Hence, the equivalent inventory effects documented in this study should be particularly profound for such traders and for the LOB markets they trade in.

Second, few papers have examined the profitability of limit order strategies. Harris and Hasbrouck (1996) find that conditional on execution, limit orders are more profitable than market orders; Griffiths et al. (2000) examine the relation between order aggressiveness and performance; Bessembinder et al. (2009) find a negative relation between the use of hidden quantity and execution costs. These papers analyze the performance of individual orders, not of a traders' portfolio of orders. Given the frequent order cancellations and resubmissions in LOB markets around the world, the

[14] See, for example, Jovanovic and Menkveld (2010), Hendershott and Riordan (2009), and Brogaard (2011).

[15] Kirilenko et al. (2010).

net execution cost of a trader's portfolio of orders should be the more pertinent measure of performance. To that end, Handa and Schwartz (1996) analyze the profitability of placing a network of buy and sell orders. However, there examination is based on executions of hypothetical limit orders given actual price time series, not actual transactions. This paper adds to the literature on the profitability of limit order strategies by examining the relation between execution costs of a trader's portfolio of orders and an important aspect of limit order trading — order revisions.

Third, this study also adds to the literature on the role of informed traders in limit order books. The traditional models (e.g., Rock,1996; Glosten,1994; and Seppi,1997) that assumed that limit orders were submitted only by uninformed traders have been recently questioned. Kaniel and Liu (2006) posit that informed traders will prefer to submit limit orders more than market orders, especially when their information is persistent. Goetler et al. (2009) theorize that in a dynamic limit order market with asymmetric information, informed traders submit a large proportion of limit orders even when their information is short lived. Bloomfield et al. (2005) use an experimental electronic market to show that pre-identified informed traders use more limit orders than uninformed. Anand et al. (2005), similar to the current study, indentify institutional traders as informed traders. Consistent with Bloomfield et al. (2005), they find that informed traders shift from market to limit orders over the course of the trading day. However, extant empirical evidence is based either on an experimental market (Bloomfield et al., 2005) or on the two decade old TORQ dataset[16] (Anand et al., 2005). This paper adds to the literature by providing evidence of informed limit order trading

[16] Audit trial data on NYSE stocks between November 1990 and January 1991.

in a (relatively) modern pure LOB market. More importantly, unlike extant studies, this paper documents that informed traders significantly benefit from active limit order trading.

The findings in this paper have implications for market regulators as well. Importance of order revisions stems from the result that informed traders and voluntary market makers prominently employ them. These two, often overlapping, classes of traders dictate the evolution of prices and liquidity in LOB markets. Consequently, if the option to revise orders were to become costlier due a regulatory directive, pricing efficiency and liquidity could be adversely affected. Specifically, the de facto intermediaries use order revisions to mitigate their information and inventory risks. In the absence of order revisions, they will maintain larger limit order spreads as a compensation for the increased risks, resulting in higher transaction costs for liquidity demanders. Further, as argued by Handa and Schwartz (1996), "the viability of LOB markets depends on limit order trading being profitable for a sufficient number of public participants." The results here show that the order revisions enhance limit order profitability for informed traders. If order revisions were to become costlier, due to reduced profitability, informed traders may opt for alternative means of trading, such as trading in 'dark pools' or in upstairs markets (Bessembinder et al., 2009). Such a development could potentially impede price discovery in LOB markets.

This paper is organized as follows. Section II reviews the extant literature on limit order revisions, and presents the testable hypotheses. Section III describes the data and the institutional features of the NSE. Section IV examines the relation between trader categories, styles, and order revisions. Results from duration analysis of order cancellations and modifications are presented in Section V. In Section VI, I examine the relation between order revisions and performance of trader's order portfolios. Section VII presents concluding remarks.

2. Literature Review and Hypotheses

2.1. Literature on Limit Order Revisions

Literature on limit order revisions is still in its infancy, and has recently witnessed a spurt in interest. Liu (2009) theorizes and empirically tests the relation between limit order revisions, the management of 'free trading option' and non-execution risks, and monitoring costs. Empirical examination of 23 stocks from the Australian Stock Exchange finds evidence in favor of his theory that order revision activity is higher when order submissions risks are higher, when spreads are narrower, and when the concerned firm is larger. Fong and Liu (2010) also find evidence in line with that of the Liu (2009). More specifically, they document that order revision activity increases with free trading option and non-execution risks, size of the order, and decreases with costs of monitoring. They also find evidence that order revisions are succeeded by favorable mid-quote returns. Further evidence linking order cancellations and monitoring costs in found in Boehmer, Saar and Yu (2005). They document an increase in the intensity of limit order cancellations and a decrease in time-to-cancellations after the introduction of NYSE's OpenBook, which increased pre-trade transparency. Evidence on the relation between order revisions and free-option risk is also documented by Biais et al. (1995). They find that after large sales (buys), which convey negative (positive) information, rate of cancellations increases on the buy (sell) side of the book. They also find positive serial correlation in order cancellations in a sample of stocks trading on the Paris Bourse; Ellul et al. (2007) find a similar autocorrelation on the NYSE. An explanation for this autocorrelation is provided by Yeo (2005), who documents that majority of cancellations originate from split orders.

Hasbrouck and Saar (2009) study the phenomena of fleeting orders — orders that are cancelled within two seconds of submission — in a sample of 100 NASDAQ stocks traded on the INET platform. They find evidence indicating that fleeting orders are submitted by impatient traders chasing market prices and searching for latent/hidden liquidity. Further, they document that rapid order cancellations are a consequence of automation and fragmentation in markets. Theoretical explanations for fleeting orders have been put forth by Large (2004) and Rosu (2011). Large (2004) proposes a model wherein resolution of order flow uncertainty leads to fleeting orders. Rosu (2011) presents a dynamic model of limit order trading where agents are allowed to modify and cancel orders. This theory posits that when limit order books are full, traders cancel preexisting limit orders and place market order to expedite execution.

2.2. Contributions and Testable Hypotheses

While the aforementioned studies have examined some important determinants of order revisions, our understanding of this recent phenomena is far from complete. This paper is distinguished from the extant literature for at least three reasons. One, I examine the characteristics of traders that employ order revision strategies. Two, I relate order revision decisions to inventory management of traders. Finally, I also analyze the relation between order revisions and performance of traders' portfolio of orders. The next section develops the hypotheses relating to the unique contributions of the paper.

2.2.1. Inventory management and order revisions

The literature on dealer markets has extensively examined the role of inventory management in establishing a dealer's trading behavior and market liquidity. Starting from Garman(1976), inventory management models (e.g., Amihud and Mendelson, 1980; Ho and Stoll, 1981 and 1983; O'Hara and Oldfield, 1986; Madhavan and Smidt ,1993) theorize that since a dealer has access only to finite or limited capital, he must actively adjust his prices or quotes to manage inventory. As noted by Madhavan (2000), a general implication of these models is that when a dealer's inventory is above (below) its optimal level, the dealer is more (less) likely to sell rather than buy the security. Empirical studies find evidence mostly in favor of these inventory management models. Ho and Macris (1984) show that specialists quotes in the AMEX options market are significantly affected by their inventories; the specialist decreases his bid and ask quotes when his inventory is positive. Hasbrouck (1988) and Madhavan and Smidt (1991) document weak intraday effects of specialists inventory management in equity markets, and Madhavan and Smidt (1993) find that the specialist inventory adjustments are slow and have a half-life of 7.3 days. On the other hand, Lyons (1995) using intraday data on dealer positions finds a strong evidence in favor of the inventory-control effect on prices. Similarly, Manaster and Mann (1996) use data on locals' intraday inventory positions in the commodity markets and find strong support in favor of the inventory models: locals with long (short) positions are the most active sellers (buyers). Comerton-Forde et al (2010) document a positive relation between NYSE specialists' overnight inventories and market spreads.

More recently, models of dynamic limit order trading have formalized the effect of inventory imbalances on limit order submission strategies. Harris (1998), Foucault et al. (2005), and Rosu (2009) propose models wherein an inventory imbalance increases the waiting costs — costs of delayed or non-execution of orders — for a trader. Hence, the impatient trader finds it optimal to place orders aggressively so as to rebalance his portfolio, particularly when faced with a deadline. In Goettler et al. (2005 and 2009), traders with liquidity or inventory rebalancing motives have a predisposition (private value) to placing orders on one side of the book over the other. A trader with a positive inventory imbalance is more likely to be aggressive on the sell side rather than on the buy side. Although empirical studies are yet to examine inventory effects in LOMs, Bloomfield et al (2005) find evidence supporting the same in an experimental set-up. They find that large liquidity traders (traders constrained to meet a target by a deadline) place limit orders to begin with, but as the deadline approaches place market orders to ensure execution of their outstanding orders.

The implications of the inventory control models for order revisions are immediate. A liquidity provider should revise his preexisting limit orders in response to changes in his inventory; for example, he should respond to an increase in his inventory by cancelling or by negatively (positively) modifying his preexisting buy (sell) order. [17] Accordingly, I state my first set of hypotheses.

H1a: A trader's propensity to cancel a buy (sell) order increases (decreases) after his inventory in the same stock increases.

[17] A positive modification is one where an order's price is revised aggressively.

H1b: A trader's propensity to negatively modify a buy (sell) order increases (decreases) after his inventory in the same stock increases.

H1c: A trader's propensity to positively modify a buy (sell) order decreases (increases) after his inventory in the same stock increases.

Of particular importance to the current study is the model proposed by Ho and Stoll (1983). They solve the dealer's pricing problem by relaxing the assumption of dealer monopoly and accommodating multiple dealers, which is a primary attribute of limit order markets. They show that a dealer's reserve price depends, among other things, on his *equivalent* inventory in the stock. In other words, a dealer revises his quotes in stock '*s*' based not just on his inventories in '*s*' (ordinary inventory), but also based on his inventories in other stocks whose returns are correlated with those of stock '*s*'(equivalent inventory). However, Naik and Yadav (2003) find that trading behavior of dealer *firms* in the London Stock Exchange is governed by ordinary inventories rather than their equivalent inventories. They argue that due to limitations on real time communication between traders and complications in performance evaluation, dealer firms adopt a decentralized framework of market-making, wherein every individual trader manages his inventory in isolation without regard to firm-level equivalent inventories. Notwithstanding the Naik and Yadav (2003) study, the *dealer-level* pricing problem vis-à-vis equivalent inventories as theorized by Ho and Stoll (1983) remains untested. Unlike their study, the current one employs trader-level data that enables a direct examination of the theory.

To the extent that stocks in the same industry (2 digit SIC) are highly correlated, an implication of the Ho and Stoll proposition is that a liquidity provider's order revision behavior should also be guided by his inventory in stocks from the same industry as the concerned stock. For example, a liquidity provider will respond to an increase in his inventory in stocks from the same industry as the concerned stock by cancelling or negatively (positively) modifying his preexisting buy (sell) order in stock *s*. Accordingly, I state my next set of hypotheses.

H2a: A trader's propensity to cancel a buy (sell) order increases (decreases) after his inventory in stocks from the same industry as the concerned stock increases.

H2b: A trader's propensity to negatively modify a buy (sell) order increases (decreases) after his inventory in stocks from the same industry as the concerned stock increases.

H2c: A trader's propensity to positively modify a buy (sell) order decreases (increases) after his inventory in stocks from the same industry as the concerned stock increases.

We can also test the relation between order revisions and inventory management by examining the nature of traders who employ order revisions on a regular basis. Intermediaries are most concerned about inventory management. If order revisions are driven by inventory management (amongst other factors), we should find traders performing an intermediary function employing order revisions more than other type of traders.

H3a: Intermediaries revise a greater proportion of orders than other traders.

2.2.2. Order Revisions and Performance

Limit orders are ex ante commitments to trade a fixed quantity of shares at a specific price. Hence, Copeland and Galai (1983) treat them as free options written by limit order traders to other market participants. The limit order trader faces the risk of being 'picked off' when the market prices move adversely after he places the limit order. To ensure that limit orders do not go 'stale', traders monitor market events after placing the order. In the model proposed by Foucault et al. (2003), NASDAQ dealers choose to monitor market events after placing their quotes in order to minimize the risk of being picked off by professional day traders. Liu (2009) extends the Foucault et al. (2003) model to incorporate non-execution risk, and also allows traders to revise posted limit orders. In his model, limit order traders weigh the benefits of monitoring against the costs of non-execution and free-option risk while placing orders. Even in Goetler et al. (2009), traders revise unexecuted limit orders so as to reflect changes in market conditions. In their model, traders choose to revise orders when the benefit from adjusting the order's specifications to reflect changes in market factors is greater than the cost incurred from losing the order's time priority due to the revision. The emergent intuition from these models is that order revisions are a consequence of traders monitoring and strategically responding to changing market conditions. Further, the objective of a revision is to ensure that the revised order reflects the trader's new expectation of market conditions and other factors that affect the order's payoff.

Consequently, revised orders should contain more information and perform better than other orders. Accordingly, I state the following hypothesis:

H4a: Performance of an order is positively related to the number of times it is revised.

Extant literature has consistently found that institutional traders are more informed than individual traders. For example, Bartov et al. (2000) and Campbell et al. (2007) find that institutions take positions to arbitrage mispricing around earnings announcements; Chakravarty (2001) and Anand et al. (2005) find that institutional orders have a significantly greater price impact than orders placed by individuals; Boehmer and Kelly (2008) show that prices of stocks with greater institutional ownership are more efficiently prices and Boehmer et al. (2008) document that institutional short sales are more informed than short sales initiated by other traders. Kumar et al. (2009), who employ the same dataset as the current paper, conduct an examination of the informativeness of orders placed by different traders on the NSE. They find that the information level of institutional traders, especially financial institutional traders, is significantly greater than the information level of individual traders. Accordingly, if limit order revisions are indeed employed more by informed traders, we should expect institutional traders to revise orders more frequently than others.

H5a: Institutional traders employ order revisions more frequently than others.

Unlike other traders, informed traders can recognize mispricing in securities. There trading strategies are also a consequence of market prices straying away from fundamental values. Indeed, Bloomfield et al. (2005) show that informed traders strategically place aggressive orders to arbitrage mispricing in market prices. Since order revisions enable traders to dynamically respond to evolving market conditions, informed traders will employ them also to actualize their informational superiority. In contrast, other traders will be able to use order revisions only to manage information and inventory risks. Therefore, the relation between order revisions and order performance should be more positive for informed (institutional) traders than for other market participants.

H6a: Order performance is more positively related to the number of revisions for institutional traders than it is for other market participants.

3. Data

NSE was created in 1994 as part of major economic reforms in India. It operates as pure electronic limit order book market, and uses an automated screen based trading system called National Exchange for Automated Trading (NEAT), which enables traders from across India to trade anonymously with one another on a real-time basis using satellite communication technology. NSE was the first exchange in the world to use satellite communication technology for trading. In terms of total number of trades, NSE is the second largest pure electronic LOB market in the world, just behind

Shanghai Stock Exchange (SSE), and it is the fourth largest among all markets irrespective of market structure, behind NYSE, NASDAQ and SSE.[18] NSE 's order books accommodate all the standard types of orders that exist internationally in order-driven markets, including limit orders, market orders, hidden orders, stop-loss orders, etc. Limit orders can be continuously cancelled or modified without any incremental fees. NSE operates a continuous trading session from 9:55 am until 3:30 pm local time. The tick size is INR 0.05 (less than USD 0.01). Outstanding orders are not carried over to the next day. There is no batch call auction at the beginning of the trading day. The opening price is also determined by pure order matching.

The sample consist of all the 50 stocks in Standard & Poor's CNX Nifty index, which represents about 60% of the market capitalization on the NSE and covers 21 sectors of the economy. The sample period is from April 1 through June 30, 2006, covering 56 trading days. Table 2 presents summary statistics on the trading characteristics of the sample stocks over the sample period. There are, on average, 19,121 trades per day, or 57 trades per stock per minute. There are, on average, 24,907 order submissions per stock per day, or about 75 order submissions per stock per minute.[19] More importantly, on an average, 24% of all incoming limit orders and 45% of incoming limit order volume is cancelled. The same for modifications are 16% and 26%, respectively. Larger orders are more likely to be cancelled or modified. In sum, about 36% of all incoming limit orders and 61% of all limit order volume is revised.

[18] World Federation of Exchanges, Annual Report, 2011

[19] These statistics are as reported in Kumar et al. (2009)

The dataset provides complete information of trades and orders that enables the reconstruction of the order book to obtain best quotes and depth information. Further, the data also provides identification codes and classifications of traders for all the orders and trades in the dataset. I aggregate the 14 trader classifications flagged in the dataset into 4 broad categories: *Individuals, Financial Institutions, Dealers,* and *Other Institutions*. Table 3 presents summary statistics and descriptions of the four trader categories. While *Individuals* outnumber other trader categories, institutional traders, especially *Dealers*, are more active in terms of order submissions. Although the NSE is a pure electronic limit order book market with no designated intermediaries, *Dealers*, who are registered members of the NSE, trade on behalf of their clients and also trade for their proprietary accounts. These traders generally function as voluntary intermediaries at the exchange[20]. The table also presents order revision activity by different trader groups. Clearly, traders revise a substantial proportion of their limit order volume. Dealers cancel the greatest proportion of their limit order volume; they cancel about 68% of their limit order volume. Financial institutional traders modify the greatest proportion of limit order volume; they modify about 34% of their limit order volume. Interestingly, individual traders appear to be more revising a greater proportion of limit order volume than financial institutional traders. This apparent anomaly is driven by the fact the individuals are an extremely heterogeneous group of traders. That a small portion of individual traders are influencing the revision numbers reported here is further substantiated in the next section where I examine order revision activity of an average trader in each category.

[20] See www.nseindia.com/content/press/NSEbyelaws.pdf for further details.

4. Trader categories, styles, and order revisions

In this section, I examine the relation between different attributes of traders — such as their category, reliance on inventory management, and trading frequency — and their use of order revisions. I next define the variables that are employed in the analysis.

The intensity of order cancellations and modifications for trader *i* is measured using the entire sample of 50 stocks (*s*) and 56 days of trading (*t*):

$$Cancellation\ Ratio_i = \frac{\sum_{t=1}^{56}\sum_{s=1}^{50} \text{Number of Cancellations}_{i,s,t}}{\sum_{t=1}^{56}\sum_{s=1}^{50} \text{Number of Order Submissions}_{i,s,t}}$$

$$Modification\ Ratio_i = \frac{\sum_{t=1}^{56}\sum_{s=1}^{50} \text{Number of Modifications}_{i,s,t}}{\sum_{t=1}^{56}\sum_{s=1}^{50} \text{Number of Order Submissions}_{i,s,t}}$$

Note that unlike the *Cancellation Ratio*, the *Modification Ratio* can be greater than 1 because each order can be modified multiple times. *Revision Ratio* is defined as the sum of the two ratios.

$$Revision\ Ratio_i = Cancellation\ Ratio_i + Modification\ Ratio_i$$

The *Revision Ratio* measures the number of times trader *i* revises (either cancels or modifies) orders for every limit order he places. I also define the following indicator variables based on trader categories as given in the dataset.

$$Dealer_i = \begin{cases} 1 & \text{if trader } i \text{ is identified as a trading member of the NSE in the dataset} \\ 0 & \text{otherwise} \end{cases}$$

$$Indiviual_i = \begin{cases} 1 & \text{if trader } i \text{ is identified as an individual in the dataset} \\ 0 & \text{otherwise} \end{cases}$$

$$Fin_i = \begin{cases} 1 & \text{if trader } i \text{ is identified as belonging to a financial institution in the dataset} \\ 0 & \text{otherwise} \end{cases}$$

$$Others_i = \begin{cases} 1 & \text{if trader } i \text{ is identified as belonging to a non-financial institution in the dataset} \\ 0 & \text{otherwise} \end{cases}$$

Table 4, Panel A presents results from the analysis of trader categories, *Revision Ratio*, *Cancellation Ratio*, and *Modification Ratio.* Exchange members (*Dealers*) and financial institutions (*Fin*) use revisions the most. The median revision ratios show that they approximately revise (modify and/or cancel) once for every two limit orders they place. Exchange members (*Dealers*) use cancellations more than any other class of traders; about 50 exchange members (p90), cancel every second limit order they place. However, financial institutions (*Fin*) modify orders most frequently; about 580 financial traders (p90), modify every orders more than once. To the extent that dealers and financial institutions are mostly likely to function as intermediaries in an electronic limit order market, these results are in line with *H3a*. Individuals (*Individual*) use order revisions least frequently. The difference between the average revision, cancellation, and modification ratios of institutional and individual trades is positive and statistically significant (at 1% level). These results are consistent with *H5a*; institutional traders, who are more likely to be the informed traders in the market, revise orders with greater intensity than individual traders.

I next examine the relation between trader characteristics — *Closing Ratio, Network Trading Ratio, Trader Size,* and *Trader Frequency* — and order revision ratios. Following Kirilenko et al. (2010), *Closing Ratio* is calculated as the ratio of a trader's daily closing position and his daily total trading volume. The ratio is calculated for each trader *i,* initially for each of the *m* days that he traded in stock *s* and then averaged over the *n* stocks he traded during the sample period.

$$Closing\ Ratio_i = \sum_{s=1}^{n_i} \frac{\dfrac{\sum_{t=1}^{m_{i,s}} \dfrac{\left|\text{End of day position}\right|_{i,s,t}}{\text{Daily tradng volume}_{i,s,t}}}{m_{i,s}}}{n_i}$$

A low *Closing Ratio* implies that the trader liquidates most of his intraday position before the close of trading. Hence, *Closing Ratio* proxies for the frequency of inventory management related trades, and thereby the half-life of the trader's inventories. Further, traders who function as intermediaries generally carry only a small component of their daily trading volume as overnight inventory. Consequently, *Closing Ratio* also identifies de facto or voluntary intermediaries in the market. Lower the value of *Closing Ratio* for a trader, more the trader behaves as an intermediary. Similarly, I also estimate *Network Trading Ratio* to identify the implicit or de facto market makers. Market makers typically post multiple two-sided quotes (network of quotes) and in doing so create their own limit order spread. Handa and Schwartz (1996) refer to such limit order trading as 'network trading'. The *Network Trading Ratio* captures the intensity of network trading for each trader in the dataset. Snapshots of the order book are created for all the stocks at one-minute intervals. In each such interval, a trader is said to be *Network Trading* if he has multiple orders on both sides of the book.

$$Network\ Trading_{i,s,k} = \begin{cases} 1 & \text{if trader } i \text{ has a network of orders in the } k^{th} \text{ snapshot of stock } s \\ 0 & \text{otherwise} \end{cases}$$

Network Trading Ratio is calculated for each trader *i,* initially for each of the *m* snapshots of stock *s* that his orders are present in and then averaged over the *n* stocks he traded during the sample period.

$$Network\ Trading\ Ratio_i = \sum_{s=1}^{n_i} \frac{\sum_{k=1}^{m_k} \frac{Network\ Trading_{i,s,k}}{m_k}}{n_i}$$

Trader Size and *Trader Frequency* variables are defined as follows:

$$Trader\ Size_i = \frac{\sum_{s=1}^{n_i} \frac{\sum_{t=1}^{m_{i,s}} \text{Trade Volume}_{i,s,t} * \text{Average Price}_{i,s,t}}{m_{i,s}}}{n_i}$$

$$Trader\ Frequency_i = \frac{\sum_{s=1}^{n_i} \frac{\sum_{t=1}^{m_{i,s}} \text{Number of Trades}_{i,s,t}}{m_{i,s}}}{n_i}$$

Table 4, Panel B presents results from the regression analysis of trading styles and order revision ratios. As shown in Table 4, Panel B, large and active traders revise a greater proportion of orders. More importantly, I find a negative coefficient on *Closing Ratio* in both the *Cancellation Ratio* and *Modification Ratio* regressions. The coefficient is also statistically significant (at 1% level). This implies that traders who actively manage their inventory revise a greater proportion of orders. Further, *Networking Trading Ratio* is also positively related to the intensity of order cancellations, modifications, and revisions. Again, the coefficients are statistically significant (at 1% level). These results indicate that traders who function as market makers employ order reversion strategies more regularly than others; evidence is consistent with *H3a.*

In sum, large and frequent traders, exchange members/dealers, traders belonging to financial institutions, traders who most frequently manage their inventories, and network traders employ order revisions the most. These findings are consistent with the previously stated hypotheses that informed traders and intermediaries revise orders more regularly than others.

5. Duration Analysis of Order Revisions

In this section I employ hazard analysis techniques to examine the determinants of a trader's decision to revise an order.

Order revisions are dynamic strategies executed by traders at high frequencies after order submission. In order to examine the determinants of such a phenomena we need to relate traders' decisions to developments in market conditions and other factors of interest through the life of the order. In fact, the inventory hypotheses built in the previous section require an analysis of changes in a trader's inventory after order submission. For example, if a trader decided to cancel a limit order 1 minute after submission, how did changes in his inventory in the said 1 minute affect his decision to cancel the order? The question of interest here is not how a trader's inventory affects his order placement, but how a change in his inventory since order submission affects his decision to revise the previously submitted order. Hence, a standard duration analysis[21], wherein the conditioning variables are all established prior to the submission of the order, is not best suited for the purposes of this study. Instead, following Hasbrouck and Saar (2009), I employ a (Cox's) proportional hazards duration model with *time-varying* covariates[22] to analyze traders' strategic responses to the evolving market conditions and other time variant factors post order submission.

I first discuss the explanatory variables, and then present the parameter estimates from hazard analysis of cancellations, positive modifications, and negative modifications. In accordance with Hasbrouck and Saar (2009), I include *Lagged*

[21]For examples of such applications please see Lo, Mackinlay and Zhang (2002) and Boehmer, Saar and Yu (2005).

[22] See Allison (1995) for an excellent and detailed discussion of proportional hazard duration models.

Volume, *Lagged Volatility*, and *Spreads* to account for the general market conditions prevailing prior to the submission of the order.

$$LaggedVolume_{o,s} = \text{Log(total trading volume) in sotck } s \text{ over the 5 mins leading to the submission of order } o$$

$$LaggedVolatility_{o,s} = |\text{Return}| \text{ in sotck } s \text{ over the 5 mins leading to the submission of order } o$$

$$Spreads_{o,s} = \frac{(Best\ Ask - Best\ Bid)}{Midquote} \text{ in sotck } s \text{ prevailing 5 secs before the submission of order } o$$

Hasbrouck and Saar (2009) also find a positive relation between price aggressiveness and intensity of cancellation, which supports their (search) hypothesis that traders cancel orders after failing to find latent liquidity within the spread. Although the NSE does not permit complete hidden orders, which limits the extent of liquidity searching, price aggressiveness remains an important factor. Liu (2009) theorizes that traders revise orders to manage the "free-option" risk. Since this risk is positively related to price aggressiveness, I include the Hasbrouck and Saar measure of the order's price aggressiveness ($p^{Relative}$) in the analysis. The definitions of buy and sell orders are analogously defined, and the definition for a buy order is as follows:

$$p^{Relative}_{o,s} = \frac{(\text{Limit Price}_{o,s} - \text{Best Bid}_{s,t=0})}{\text{Best Bid}_{s,t=0}}; \text{ where t} = 0 \text{ is the time of order } o\text{'s submission in sotck } s$$

Further, Hasbrouck and Saar (2009) find a positive (negative) relation between the intensity of order cancellation and post-submission changes in quotes on the same (opposite) side as the submitted order. They interpret the positive relation as evidence of traders "chasing" market prices by cancelling stale orders and resubmitting more aggressive ones; the negative relation as evidence of traders cancelling orders and submitting market orders to exploit cheaper opposite quotes. Accordingly, I include two time-variant variables Δq_t^{same} and $\Delta q_t^{opposite}$. The definitions of buy and sell orders are analogously defined, and the definitions for a buy order is as follows:

$$\Delta q_{s,t}^{same} = \frac{\left(\text{Best Bid}_{s,t} - \text{Best Bid}_{s,t=0^+}\right)}{\text{Best Bid}_{s,t=0^+}}; \text{ where } t = 0^+ \text{ is the instant after order submission in stock } s$$

$$\Delta q_{s,t}^{opposite} = \frac{\left(\text{Best Offer}_{s,t} - \text{Best Offer}_{s,t=0^+}\right)}{\text{Best Offer}_{s,t=0^+}}; \text{ where } t = 0^+ \text{ is the instant after order submission in stock } s$$

Fong and Liu (2010) document that large orders are more likely to be revised due to fixed costs of monitoring. Hence, I include *Order Size* as a covariate.

$$\textit{Order Size}_{o,s} = \text{Log}(\text{Order Volume}_{o,s} \times \text{Limit Price}_{o,s})$$

Next, I define the time varying inventory related variables that are central to the hypotheses developed earlier. The following definitions are for buy orders; variables for the sell orders are defined analogously.

$$\Delta Inventory\_Stock_{s,t,i,buy} = \text{Change in trader } i\text{'s buy-side inventory in stock } s \text{ over the period } (t-5, t]$$

$$\Delta Inventory\_Stock_{s,t,i,sell} = \text{Change in trader } i\text{'s sell-side inventory in stock } s \text{ over the period } (t-5, t]$$

$$\Delta Inventory\_Stock_{s,t,i} = \text{Log}(\Delta Inventory\_Stock_{s,t,i,buy} - \Delta Inventory\_Stock_{s,t,i,sell})$$
$$\text{Change in trader } i\text{'s inventory in stock } s \text{ over the period } (t-5, t]$$

Similarly,

$$\Delta Inventory\_Related_{s,t,i} = \text{Log}(\Delta Inventory\_Related_{s,t,i,buy} - \Delta Inventory\_Related_{s,t,i,sell})$$
$$\text{Change in trader } i\text{'s inventory in all stocks that belong to the same industry (2 digit SIC) as stock } s \text{ over the period } (t-5, t]$$

$$\Delta Inventory\_Industry_{s,t,i} = \Delta Inventory\_Stock_{s,t,i} + \Delta Inventory\_Related_{s,t,i}$$

$$\Delta Inventory\_Unrelated_{s,t,i} = \text{Log}(\Delta Inventory\_Unrelated_{s,t,i,buy} - \Delta Inventory\_Unrelated_{s,t,i,sell})$$
$$\text{Change in trader } i\text{'s inventory in all stocks that don't belong to the same industry (2 digit SIC) as stock } s \text{ over the period } (t-5, t]$$

Further, I also employ trader classification dummy variables that were defined earlier. Finally, following Lo et al. (2002), I also include the logarithm of average stock prices as a covariate to capture the differences across stocks.

$$LPR_S = \text{Log}(\text{AveragePrice}_S)$$

5.1. Order Cancellations

The data creation and the following analysis are similar to that of Hasbrouck and Saar (2009).Using all the orders in 50 stocks for duration analysis is computationally costly and unwarranted. Hence, I randomly sample 10,000 limit orders from each of the 50 stocks. Since there are only 50 stocks in the cross section, following Lo et al (2002), I pool all the orders for the analysis. In order to address dependence among orders from the same stock, I cluster standard errors by stock. The data is organized in a "counting process" format[23]; at each 5 second interval from the time of order submission, it is recorded whether the interested event — order cancellation — occurred, and the corresponding values of all covariates are also recorded. As seen in Figure 1, more than 50% of order cancellations and more than 60% of order modifications happen within 2 minutes of order submission. Hence, I track all orders only through the first 2 minutes. Execution is viewed as a competing process.[24] All the stock specific variables are standardized.

5.1.1. Results

Results of the proportional hazard duration model are presented in Table 5. The estimates are generally consistent with the extant literature. The positive sign on the estimated coefficients on *Lagged Volume* and *Lagged Volatility* indicate that traders cancel orders to eliminate the 'free-option risk' in volatile periods. This finding is consistent with the model developed by Liu (2009). The coefficient on the *Spreads*

[23] See Hosmer et. al. (2008) and http://www.ats.ucla.edu/stat/sas/faq/survival_repeated_events.htm for a detailed explanation.

[24] Chakrabarty et al. (2006) also analyze order executions and cancellations as competing events.

variable is always positive, but never statistically significant. The pricing aggressive variable ($p^{Relative}$) is always positive and statistically significant. This result is consistent with the 'search' hypothesis of Hasbrouck and Saar (2009) and the free-option risk hypothesis of Liu (2009) and Fong and Liu (2009). However, the NSE does not permit completely hidden orders, which reduces the potency of the 'search' hypothesis in this market. Hence, I infer that traders are more likely to cancel aggressive orders to manage their free-option risk. The statistically significant coefficients on the time varying same-side quote change variable (Δq_t^{same}) are consistent with the 'chasing' hypothesis posited by Hasbrouck and Saar (2009). Traders appear to be cancelling orders to post more aggressive ones when the markets prices move away from the posted limit prices. However, unlike Hasbrouck and Saar (2009), I do not find evidence in favor of the 'cost-of-immediacy' hypothesis — when ask (bid) quotes increase, traders cancel preexisting buy(sell) orders and submit market orders to execute against favorable ask (bid) quotes — that predicts a negative coefficient on the $\Delta q_t^{opposite}$ variable.

The coefficient relating to the indicator variable *Dealer* is positive and statistically significant (at 1% level) in all the specifications. Clearly, dealers have a greater propensity to cancel orders than the rest of the market. The hazard or intensity of cancellation for dealers is about 185% of the hazard for other traders. Since dealers are most likely to make markets, these results add further credence to the inventory hypotheses. The hazard for individuals is only about 47% of the hazard for other traders. To place the results in a better perspective, I restate the relevant results in terms of probabilities. Probability estimates are obtained through the survivor function that is non-parametrically estimated from the fitted hazard model[25]. When the order is submitted by a dealer (*Dealer* = 1 and *Individual* = 0), and all the other variables in specification 3 are held at the respective sample means, the probability of order cancellation(within 2 minutes of submission) is 33.74%; the same when the order is placed by an individual trader is 9.96%. The category of the trader has a non-trivial impact on the probability of order cancellation. This finding is consistent with the inventory hypotheses (*H3a*): de facto intermediaries such as the trading members/dealers, who manage inventory on a regular basis, utilize order cancellations more frequently than other traders. These results are also consistent with the descriptive analysis and the results from the OLS regressions, which showed that dealers use order cancellations the most and individuals the least. Further, the result that individual traders employ order cancellations with a lower intensity than institutional traders supports the hypothesis that informed traders employ more order revisions.

More important to this study are the coefficients on the inventory variables. The coefficient relating to the change in a trader's same-stock inventory ($\Delta Inventory\_Stock_t$) is always positive and statistically significant (at 1% level). These results imply that, after controlling for the category of the trader, price aggressiveness, order size, market volatility and volume and changes in quotes, an increase in a trader's inventory increases (decreases) his propensity to cancel preexisting buy (sell) orders; this evidence strongly supports *H1a.* The same-stock inventory effect is also economically significant. The estimated percentage change in hazard for each unit increase in covariate *x1* is given by $(e^{\beta_{x1}} - 1)$. Therefore, a unit increase in $\Delta Inventory\_Stock_t$,

[25] See Allison (1995) for an excellent and detailed discussion of proportional hazard duration models.

increases the intensity of cancellation by 1.3%. Or, the intensity of cancellation for a preexisting buy (sell) order in stock '*s*' increases (decreases) by 1.3% after the corresponding trader, in the previous 5 seconds, has bought 2.72 more units of '*s*' than he has sold. As before, I restate the results in terms of probabilities: when a trader increases his inventory in the stock by 1000 units (approximately 1 standard deviation in trade size), and all other variables are held at their respective sample means, the probability of a buy (sell) order cancellation increases (decreases) by 1.7 percentage points or 8.2%.

The coefficient on $\Delta Inventory\_Related_t$ (change in a trader's inventory in related or correlated stocks) is also significantly positive (at 1% level). This implies that traders' order cancellation decisions are not driven only by their inventory of the corresponding stock, but also by their inventory in stocks of the same industry as the said stock. Traders appear to be managing their *equivalent* inventory; this evidence is consistent with *H2a.* Although the coefficient on $\Delta Inventory\_Related_t$ is smaller than it is for $\Delta Inventory\_Stock_t$, the related-stock inventory effect is not trivial. A unit increase in $\Delta Inventory\_Related_t$, increases the intensity of order cancellation by 0.80%. Or, the intensity of cancellation for a preexisting buy (sell) order in stock '*s*' increases (decreases) by 0.80% after the corresponding trader, in the previous 5 seconds, has bought 2.72 more units of stocks in the same industry (2 digit SIC) as '*s*' than he has sold. Or, a 1 standard deviation increase in a trader's inventory in related stocks, increases (decreases) the probability of cancellation for a buy (sell) order by 1.00 percentage point or 4.8%. Not surprisingly, the coefficient on $\Delta Inventory\_Industry_t$ is also positive and statistically significant (at 1% level).

To further examine the validity of the inventory results, I introduce *ΔInventory_Unrelated*$_t$ — trader's inventory in unrelated stocks or stocks that don't belong to the same industry as the concerned stock — in place of *ΔInventory_Related*$_t$. The results are shown in the fifth column of Table 5. In accordance with the *equivalent* inventory hypothesis (*H2a*), the coefficient, although positive, is statistically insignificant from zero. Lower the (absolute)correlation between two stocks, lower the impact inventory in one has on order cancellation decisions in the other.

5.2. Order Modifications

The empirical design is identical to the one employed for analyzing order cancellations, except for the following differences. One, the events of interest are positive and negative modifications, not order cancellations. Two, while analyzing modifications I consider order cancellations and executions as competing events. Finally, since modifications are repetitive events, I cluster standard error by order to mitigate the effects of dependency amongst repeated events.[26]

5.2.1. Results

The results of the analysis are presented in Tables 6 and Tables 7I. Wider *Spreads* increase the intensity of positive modifications; the effect on negative modifications, although negative, is statistically insignificant. This implies that, ceteris paribus, traders positively modify their orders when the returns for providing liquidity are higher. The coefficients on *Lagged Volatility* is again consistent with the free-option risk hypothesis of Liu (2009). It appears that active markets (*Lagged Volume)*

[26]See Hosmer, Stanleu and May (2008) and http://www.ats.ucla.edu/stat/sas/faq/survival_repeated_events.htm for a detailed explanation.

discourage negative revisions and encourage positive revisions. The coefficient on price aggressiveness ($p^{Relative}$) is positive for both positive and negative modifications in all specifications; similar to order cancellations, traders employ order modifications to manage their 'free-option' risk. Intensity of order modifications is again positively related to *Order Size*; evidence supports the monitoring hypothesis of Liu (2009).

The coefficients on quote changes (Δq_t^{same} and $\Delta q_t^{opposite}$) provide strong support to the 'search' hypothesis of Hasbrouck and Saar (2009). Further the best quotes get from the limit order price, higher (lower) the intensity of positive (negative) order modifications. A one standard deviation increase in the Δq_t^{same} increases (decreases) the intensity of positive (negative) modifications by 5.5% (8.0%). Similarly, a one standard deviation increase in $\Delta q_t^{opposite}$ increases (decreases) the intensity of positive (negative) modifications by 10.4% (8.5%).

Similar to the order cancellation results, the coefficient relating to the indicator variable *Dealer* is positive and significant , while the coefficient relating to the variable *Individual* is negative and significant for both positive and negative modifications. That individuals modify orders with a lower intensity than institutional traders is consistent with the hypothesis that informed traders revise a greater proportion of their limit orders than the uninformed. That dealers most actively modify orders is consistent with the hypotheses that de facto intermediaries, who are most concerned about inventory levels, employ order revisions the most.

Results also show that an increase in same-stock inventory ($\Delta Inventory\_Stock_t$), increases the hazard of negative modifications and decreases the hazard of a positive modification. These results support inventory hypotheses *H1b* and *H1c.* When a trader increases his inventory in stock '*s'* by 2.72 units, his propensity to positively (negatively) modify a buy order reduces by 6.1% (2.5%). In terms of probabilities: 1 standard deviation increase in a trader's inventory in the stock, while all other variables are held at their respective sample means, increases the probability of a buy order positive (negative) modification by 4.41 (0.41)percentage points. Once again, after controlling for all the other factors, changes in inventory have a substantial impact on traders' order revision strategies. These results strongly support the inventory hypotheses.

The coefficient on $\Delta Inventory\_Related_t$ is statistically insignificant in both positive and negative modifications regressions. This evidence does not support hypotheses *H2b and H2c*. However, change in the total industry inventory ($\Delta Inventory\_Industry_t$) still affects order modification decisions. A 2.72 units increase in same-industry inventory, increases (decreases) the intensity of negative (positive) modifications by 4% (1.9%). The results indicate that traders employ order modifications to manage their ordinary inventory, not their equivalent inventory.

5.2.2. Summary

I find that limit order revisions (cancellations and modifications) are a function of various market and trader related factors. The results are consistent with extant literature that has documented that traders revise their orders to manage their free-option and non-execution risk. More important to this study is the finding that even after controlling for price aggressiveness, order size, market volatility and volume, and changes in best quotes, order revision decisions in a stock are governed by changes in traders' inventory in the same stock and ,to a lesser extent, in correlated stocks. Also, the category of the trader surfaces as an important determinant of the probability of order revisions: institutional traders employ order revisions strategies more regularly than individual traders do.

6. Order Revisions and Performance

Having shown that traders use order revisions to dynamically respond to changes in market prices/conditions and their own inventories, and that, even after controlling for all relevant factors, certain type of trades have a greater propensity to revise orders, I now try to answer the natural follow-up question: what is the net effect of such rampant and frenetic order management on the performance of traders' order portfolios?

Typically, the implementation shortfall measure (Perold,1988), which incorporates the cost of order execution (Price Impact) and the opportunity cost of unexecuted orders (Opportunity Cost) is used to evaluate the performance of orders.[27] However, as noted by Harris and Hasbrouck (1996), this method is better suited to analyze the performance of precommitted orders as it imputes a (substantial) penalty for non-execution. Evidence in the previous section implies that traders behaving as market intermediaries or middlemen use order revisions the most. Such traders are not precommitted to orders, but execute them opportunistically. Therefore, their performance evaluation should also incorporate the movement in market prices subsequent to the execution of their orders to account for the adverse selection

[27] See, for example, Griffith et al. (2000) and Bessembinder et al. (2008).

component of the trade. Accordingly, I also employ the 'ex post' measure proposed by Harris and Hasbrouck (1996) to examine the relation between order revisions and performance. The ex post performance measure is as follows:

$$Ex\ post_o = \begin{cases} bid\ price_{t+60\min} - fill\ price & \text{for a buy order } o \text{ executed at time } t \\ fill\ price - offer\ price_{t+60\min} & \text{for a sell order } o \text{ executed at time } t \\ 0 & \text{if the order is unexecuted} \end{cases}$$

Further, to facilitate a trader-level aggregation of performance measures, the ex post measure is standardized by the price of the stock an instant before order submission ($Price_{t^-}$).

$$Ex\ post\_Ratio_o = \frac{Ex\ post_o}{Price_{t^-}}$$

The price impact and opportunity cost variables are defined as in Bessembinder et al (2009):

$$Price\ Impact_o = \begin{cases} midquote_t - fill\ price & \text{for a buy order } o \text{ submitted at time } t \\ fill\ price - midquote_t & \text{for a sell order } o \text{ submitted at time } t \\ 0 & \text{if the order is unexecuted} \end{cases}$$

$$Opportunity\ Cost_o = \begin{cases} closing\ price - midquote_t & \text{for a buy order } o \text{ submitted at time } t \\ midquote_t - closing\ price & \text{for a sell order } o \text{ submitted at time } t \\ 0 & \text{if the order is completly executed} \end{cases}$$

Price Impact and *Opportunity Cost* variables are also standardized by the price of the stock an instant before order submission ($Price_{t^-}$).

$$Price\ Impact\_Ratio_o = \frac{Price\ Impact_o}{Price_{t^-}}$$

$$Opportunity\ Cost\_Ratio_o = \frac{Opportunity\ Cost_o}{Price_{t^-}}$$

Finally *Total Cost* of implementation for an order *o* is obtained as the weighted sum of *Ex Post* performance, *Price Impact*, and *Opportunity Cost*.

$$Total\,Cost_o = \frac{VolumeExecuted_o * (Price\,Impact_o - Ex\,Post) + VolumeUnexecuted_o * (Opportunity\,Cost_o)}{Total\,Volume_o}$$

$$Total\,Cost\_Ratio_o = \frac{Total\,Cost_o}{Price_{t^-}}$$

Variable *Total Revisions*$_O$, which measures the total number times order o has been revised, is used to measure revision activity. Other order related and stock related control variables, following Bessembinder et al. (2009), are defined as follows:

$$Hidden_o = \begin{cases} 1 & \text{if a proportion of order } o\text{'s quantity is hidden} \\ 0 & \text{if no proportion of order } o\text{'s quantity is hidden} \end{cases}$$

$$Buy_o = \begin{cases} 1 & \text{if order } o \text{ is a buy order} \\ 0 & \text{if order } o \text{ is a sell order} \end{cases}$$

$$Log\,Quantity_o = \text{Logarithm of order } o\text{'s total quantity}$$

$$Past\,Volatility_{o,s} = \text{Volatility of stock } s\text{' returns during the hour prior to the submission of order } o$$

$$Past\,Trading\,Frequency_{o,s} = \text{Number of trades in stock } s \text{ during the hour prior to the submission of order } o$$

We have seen in the previous section that traders' order revision strategies are driven, amongst other variables, by changes in their total inventory in the stock; traders use order revisions to manage their portfolio of orders in a stock. Consequently, a trader's performance for the analysis of order revisions should focus on his entire portfolio of orders in a stock rather than on the performance of individual orders. Further, unexecuted limit orders at the NSE are terminated at the end of the trading day. Hence, the analysis is conducted at a daily frequency. In lieu of these issues, the previously defined variables of interest are aggregated to the trader level in each stock and on each day of the sample. The aggregation is done on a value weighted basis, where value is calculated as the product of the order's quoted quantity and limit price. Next, I illustrate the aggregation procedure for the variable *Total Revisions*$_O$.

$$Total\,Revisions^{P}_{i,s,t} = \frac{\sum_{j=1}^{n}(value_j * Total\,Revisions_j)}{\sum_{j=1}^{n} value_j}$$

$$\text{Where,} value_j = \text{Order quantity}_j * \text{Order Price}_j$$
$$n = \text{total number of orders placed by trader } i \text{ in stock } s \text{ on day } t$$

$Total\,Revisions^{P}_{i,s,t}$ is the portfolio (*P*) value weighted average of the number of times trader *i* revised each order he placed in stock *s* on day *t*. Similarly, other variables are also aggregated. Such an aggregation results in a panel dataset of variables for approximately 1.2 million traders and 50 stocks over 56 days. Consequently, I employ panel (OLS) regressions with trader and stock fixed effects. Trader fixed effects are included to ensure that the regression coefficients depicting the relation between order revisions and order performance are not corrupted by the generic relation between a trader's skill level and the performance of his orders. To the best of my knowledge, this is the first study on limit order strategies to control for trader fixed effects. Similarly, stock fixed effects control for latent stock specific factors. Further, the standard errors of the regression coefficients are clustered by time (day) to control for the contemporaneous cross-correlation in residuals.[28]

6.1. Results

Table 8 presents results obtained from panel regressions of ex post performance ratio, price impact ratio, opportunity cost ratio, and total cost ratio on revision intensity, other order characteristics, and market conditions. Panels A, B, C and D report results

[28]See Peterson (2010) for an excellent discussion.

relating to financial institutions, other institutions, dealers, and individuals, respectively. In all panels, columns 1a-3a include all trader portfolios; columns 1b and 2b consider only trader portfolios with either partial or complete execution; coefficients in column 3b are obtained by including only trader portfolios with zero or partial executions.

Ex post performance of trader portfolios is positively related to the number of order revisions. As seen in column 1a in all panels, *Total Revisions* is positively and significantly related to ex post performance.[29] When I discard trader portfolios with no executions, the story changes. The relation between ex post performance and order revisions is positive and significant (at 10% levels) only for institutions, and is strongest for financial institutions; it is not statistically significant for individual traders. To the extent that institutional traders are more informed than other market participants, we can infer that order revisions are beneficial to ex post performance only when traders are informed. This evidence is in line with hypothesis *H6a*: order revisions are one of the strategies through which informed traders capitalize their informational advantage. Interestingly, price aggressiveness ($p^{Relative}$) is negatively related with ex post performance. This result is in accordance with the theory and evidence provided by Kaniel and Liu (2006) that limit orders perform better than market and marketable limit orders.

Price impact ratio of a trader's portfolio of orders is positively related to the average number times an order in the same portfolio is revised. The result is statistically significant (at 10%) for all classes of traders, except for dealers (*dealer*). The relation is

[29] Results are robust to the duration over which ex post performance is measured. Analyses of ex post performance measured over 5 minute, 15 minute, and 30 minute durations provide qualitatively similar results.

even stronger for trader portfolios with at least partial execution (column 2b). This finding is consistent with positively revised orders walking up the limit order book and incurring higher price impact costs. However, for institutional traders, this increase in price impact cost is compensated by superior ex post performance; they incur higher price impact costs to ensure execution of informed orders. Consistent with Bessembinder et al (2009), trader portfolios with a greater proportion of hidden orders (*Hidden*) incur lower price impact costs. Price impact costs are positively related to the average price aggressiveness ($p^{Relative}$) and size of portfolios ($Log\ Quantity$) . Further, price impact costs increase with market volatility ($Past\ Volatility$) and decrease with market activity ($Past\ Trading\ Frequency$).

Order revision activity is negatively and significantly (at 10%) related to opportunity costs in all panels. This relation is not merely because positively revised orders are less likely to remain unexecuted. Even when I consider trader portfolios with only partial or zero executions (column 3b), the relation between order revisions and opportunity cost ratio is negative and significant (at 5%). This result is consistent with negatively revised orders facing less adverse movements till close of day; even negatively revised orders appear to be informed. Opportunity costs are also significantly higher in volatile markets. Amongst other findings, the hidden order results are especially noteworthy. Portfolios with a greater proportion of hidden orders incur higher opportunity costs. This result implies that hidden orders are informed traders, and hence incur higher opportunity costs when they go unexecuted. On the contrary, Bessembinder et al. (2009) find that hidden orders are negatively related to opportunity costs, and hence infer that they are posted by uninformed traders. However, the current finding supports the theory posited by Moinas (2006). She argues that informed liquidity providers use hidden orders to "camouflage" their orders as uninformed orders to increase the probability of execution. Further, Kumar et al. (2009) find that an overwhelming proportion of hidden orders at the NSE are posted by institutional (informed) traders wanting to mask the information content of their orders.

The net effect of order revisions is presented in columns 4 and 5. The results in column 4 greatly depend on the category of the trader. While financial and other institutional traders (*Fin* and *Others*) significantly (at 10%) benefit from order revisions, the relation, although negative, is not significantly different from zero for dealers (*Dealer*) and individual (*Indi*) traders. Also, the coefficient on *Total Revisions* in Panel A (financial institutions) is significantly greater (at 5% level) than same coefficient in the remaining three panels. To the extent that financial institutions are more informed than other market participants, we can infer that the more informed a trader, the greater is the benefit of order revisions. This result supports hypothesis *H6a*. For traders belonging to financial and other institutions, the relation between order revisions and performance is also economically significant. When the average number of total revisions (*Total Revisions*) of a financial trader's portfolio increases by one unit (approximately 1 standard deviation), the portfolio's total execution cost (*Total Cost Ratio*) reduces by 8.23% of its mean value; the same for a trader belonging to other institutions is 8.49%.

Furthermore, financial institutions benefit more from order revisions on days with earnings announcements. As shown in column 5 of Panel A, the interaction between *Total Revisions* and *Earnings Day* is negative and statistically significant (at

10% level).The interaction is statistically insignificant for the other class of traders. Understandably, total cost ratio is significantly (at 10% level) higher on day with earnings announcements. But financial institutional traders employ order revisions to mitigate the increased execution costs around earnings announcements. This result indicates that informed traders (financial institutions) benefit more from order revisions, especially when the value of information is high. The evidence obtained here strongly supports hypothesis *H6a*. Total cost ratio is generally higher for portfolios that are more aggressively priced, larger in size and number, and for those that include a greater proportion of hidden orders. The results on hidden orders appear to be mainly driven by opportunity costs. Also, total cost ratio increases with market volatility and decreases with market activity.

7. Conclusion

While numerous studies have analyzed order submission strategies, few have focused on order revisions, which are post-submission strategies involving decisions on when and how to cancel or modify preexisting limit orders. We know relatively little about the type of traders who revise orders, the factors that govern their order revision strategies, and the profitability of actively managing order portfolios through order revisions. This is especially surprising given the predominance of order revisions in limit order book markets around the world. The current study fills this void in literature.

Analysis of order revision activity in the sample of 50 stocks, which constitute 60% of NSE's market capitalization, between April 1 and June 30, 2006, shows that about 36% of all incoming limit orders or 61% of all limit order volume is revised.

More importantly, analysis of different trader categories and trading styles shows that large and frequent traders, financial institutional traders, exchange members (voluntary dealers at the NSE),traders who most frequently manage their inventories, and traders who regularly post a network of two-sided quotes employ order revisions the most. In general, results indicate that informed traders and traders who function as voluntary market makers employ order revisions most prominently.

I employ a (Cox's) proportional hazards duration model with time-varying covariates to analyze traders' strategic responses to changing in market conditions, inventories, and other time variant factors after the orders are submitted. Results show that traders revise orders not only in response to changes in market prices, but to manage their inventories as well. I find that, after controlling for price aggressiveness of the order, order size, market volatility and volume, and changes in best quotes, a trader's decision to revise a preexisting order is substantially driven by changes in his inventory in the stock and ,to a lesser extent, in correlated stocks. Further, consistent with the hypothesis that informed traders dominate revision activity, I find that an order is more likely to be revised if it is submitted by an institutional trader rather than by an individual trader.

I also investigate the relation between order revisions and performance of traders' order portfolios. Results from panel regressions indicate that institutional traders, especially financial institutional traders, significantly reduce the execution costs of their order portfolios through order revisions. Institutional traders reduce the adverse selection costs of executed trades (i.e. obtain favorable price changes for executed orders) and the opportunity costs associated with unexecuted orders(i.e. obtain favorable price changes for unexecuted and/or cancelled orders) through order revisions; they seem to be using order revisions to 'time' the limit order book. In contrast, these results do not hold for individual traders. Results also show that financial institutional traders benefit the most from order revisions on earnings announcement days, when the value of private information is high.

These findings are of interest to market regulators as well. The proliferation of automated trading has heightened regulatory concern that traders manipulate order flow and market prices through nefarious order cancellations. Consequently, market regulators in Europe and the US are debating regulatory measures to curtail order revisions, especially order cancellations. The results presented in this paper should be especially instructive in this regulatory context. That traders, especially informed traders and implicit market makers, revise orders to manage information and inventory risks implies that order revisions are a valuable feature of modern limit order trading. A regulatory intervention that constrains order revisions could have an adverse effect on market liquidity and pricing efficiency.

Chapter 2: Naked Short-Selling, Fails-to-Deliver and Market Quality: The Emperor's New Clothes?

1. Introduction

Naked shorting harms the market and market participants… -- Harvey Pitt, former Chairman of the Security and Exchange Commission (Welborn, 2008).

A lot of those companies are gone. A lot of them died. This was a fatal attack. Now, some of them were weak when they were attacked. Some of them would have failed anyway. Others wouldn't have. Again, it's not up to the naked short sellers to decide. It's up to the investors that play by the rules. -- Robert Shapiro, former Under Secretary of Commerce in a 2007 Bloomberg Television special report.

Generally, short selling is "covered", i.e., adequate borrowing arrangements are made to ensure availability of the security for delivery at settlement. On the other hand, as per the Securities and Exchange Commission ("SEC") ("Key Points about Regulation SHO", 2005/2008), in "*a naked short sale, the seller does not borrow or arrange to borrow the securities in time to make delivery to the buyer within the standard three-day settlement period. As a result, the seller fails to deliver securities to the buyer when delivery is due (known as a 'failure to deliver' or 'fail')*". Our study investigates the impact of the presence of the option to fail and the resultant naked short-selling (hereafter "naked shorting") on market distortions, pricing efficiency, and liquidity.

There has been an enormous amount of discussion in the media, and serious concerns have been repeatedly voiced by regulators and market participants about the potentially adverse impact of naked shorting. Naked short-sellers have been widely alleged to have inflated the supply of shares by creating 'phantom shares' and contributed to the financial crisis by precipitating sharp price declines of financial firms.

Several investor associations and high-profile CEOs have lobbied aggressively against naked shorting, and the huge volume of litigation alleging naked-shorting-based stock price manipulation has led to naked shorting being called the "Holy Grail ….bigger than tobacco" for lawyers (Stokes, 2009).[30] The litigation has also often created credible doubts about potential motives for strongly emotive opinions of certain public figures, academics, and lawyers.[31] As per the Wall Street Journal (Asia, March 20, 2009), the SEC received over 5,000 complaints alleging price manipulation through naked shorting over the two years leading up to the 2008 financial crisis. And, a *Factiva* search shows over 4,600 printed articles on naked shorting in English-language outlets over that period.

Settlement failures are neither uncommon nor necessarily illegal in financial markets. From a legal perspective, Culp and Heaton (2007) emphasize that naked shorting has not been *malum in se* (wrong in itself), but *malum prohibitum* (wrong when and because prohibited).[32] On one hand, it has always been acceptable for *bonafide*

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[30] Examples of such investor associations include *The Movement for Market Reform*, the *National Coalition Against Naked Short Selling* (NCANS) and the *Coalition for the Reform of Regulation SHO*. Examples of high-profile CEOs crusading against naked shorting include Patrick Byrne, CEO of Overstock.com, whose corporation also filed lawsuits against both naked-shorters and financial institutions accused by them of facilitating naked shorting; and Richard Fuld, then CEO of Lehman Brothers who, in his testimony before the US House of Representatives Committee on Oversight and Reform on October 6, 2008, alleged that "naked short selling .... dealt a critical, if not fatal, blow to Bear Stearns", and also contributed significantly to the collapse of Lehman Brothers. Examples of lawsuits include *The Biovail lawsuit* against Stephen Cohen, Gradient, and others; *the Overstock lawsuit* against Rocker Partners, Gradient, and others; and the *The NFI lawsuit* against *Bank of America* (the Specialist) and the Prime Brokers.

[31] For example, the Depository Trust and Clearing Corporation ("DTCC") issued a long press release on March 15, 2006 to say that the report of (former Under-Secretary) Robert Shapiro "is intended to confuse reality ….deliberately" because "he is a paid consultant for ….legal firms, which have been suing DTCC with respect to naked short selling".

[32] The Regulation SHO requirement to "locate" stock prior to a short trade was not effective legal "prohibition" of all naked shorting, since the rules specifically allowed "Easy to Borrow" lists to be used to provide "reasonable grounds" for believing that the security is available for borrowing without the need to contact the source of the borrowed securities to locate a specific bloc of shares (Welborn, 2008).

market-making. On the other, it has always been illegal (under section 10(b) of the 1934 Securities Exchange Act and SEC Rule 10b-5) if deemed to be fraudulent misrepresentation or manipulation undertaken with the *intent* to deceive, manipulate or defraud. However, even intensive individual case analysis in the courts has been ineffective in successfully proving such *ex-ante intention*; and in spite of the flood of lawsuits, Stokes (2009) documents that no private plaintiff had won a final judgment with damages based on naked shorting.

From an economic perspective, in the presence of an option to fail, *ex ante* intention about going naked does not even need to definitively exist at the time of the short trade. This is because all short sales should only locate but not *actually* borrow the shares on the trade date since, as argued by Geczy et al (2002), it is not economically rational for short-sellers to pay extra borrowing fees by borrowing prior to the due date of delivery three days *after* the trade. The trader need not know at the time of the trade whether or not s/he will decide to fail and be naked, because that decision need not be taken on trade day *t,* but only by day *t+3*. From a purely economic perspective, this will depend on the cost of stock-borrowing. Irrespective of intention, the short will borrow and deliver if the cost of stock-borrowing is less than the interest earned on the cash from the short sale and "go naked" when that stock-borrowing cost exceeds that interest earning, i.e., when rebate rates in the OTC stock-borrowing "market" become negative; and Evans, et al. (2008) empirically link fails to rebate rates hitting zero. The resultant naked shorting should then exert a (potentially beneficial) downward pressure on stock borrowing costs.

The "close-out" requirement imposing additional delivery obligations on broker-dealers, albeit effective, was applicable only for "threshold-list" securities with a relatively high number of FTDs.

Another relevant and important institutional feature is that the *Depository Trust and Clearing Corporation* (DTCC) electronic settlement system ordinarily triggers immediate delivery of the stock to the buyer on settlement day through automatic borrowing of the stock from a voluntary pool of lenders; and when this pool is empty, the buying broker in an unrequited fail has two choices - either immediately force delivery through a buy-in of the stock (and without being identified since the DTCC system also delinked the buyer and seller after the trade), or remain as a (voluntary) lender of the security at zero rebate rates, responsible for mark-to-market margin cash flows on the open position to the DTCC, and earning full interest on the proceeds of the short-sale.[33] Accordingly, not only is the distinction between naked and covered short sales *not* necessarily definable or objectively observable prior to settlement day, covered and naked short sales are also functionally indistinguishable (Culp and Heaton, 2007). Hence, given the extensive literature that short-selling in general (without distinguishing between naked and covered) is beneficial for pricing efficiency and liquidity (Diamond and Verrecchia (1987); Abreu and Brunnermeier (2002, 2003); Miller (1977); Bris et. al. (2007); Diether et al. (2009); Boehmer et al. (2008)), *both* covered and naked short sales should contribute to the price discovery process by enabling value-traders to more quickly and easily bring the prices of overpriced securities in line with their "true value" and financial intermediaries and other liquidity

[33] In the case of a buyer becoming a voluntary lender at zero rebate rates, the stock lending was from the broker, and the buyer's security account was often credited with the security. This did create "phantom shares", but these were <u>not</u> "phantom shares" <u>*in the context of market trading*</u>, but "phantom shares" only in the context of corporate voting: a genuine but more general problem with stock lending practices, not naked shorting *per se*, arising from brokers lending client stock without adequate control systems on loss of client voting rights.

suppliers should be able to provide liquidity more effectively and expeditiously in the presence of *both* covered and naked short-selling.

While one would expect naked shorting to be at least as beneficial from the perspective of pricing efficiency and liquidity as covered short-selling (hereafter "covered shorting"), and while naked shorting should also exert a beneficial downward pressure on stock borrowing costs when these costs are high, the SEC *relaxed* covered-shorting restrictions by removal of the uptick rule, but *increased* naked-shorting restrictions: first, through the "locate" and "close-out" requirements introduced under Regulation SHO in January 2005; second, by banning naked shorting for select financial institutions between July 21 and August 12, 2008 in the wake of the heavy and rapid price-falls in financial sector stocks; and finally, by removing the option to fail (except for market-makers and under specified conditions) immediately after the Lehman collapse in September 2008 by mandating a presumption of deceptive intent from a failure to deliver (through Rule 10b-21), and by requiring a compulsory "close-out" of a fail through borrowing or purchase by the broker by the morning following the day of the fail (through Rule 204T, later made permanent).[34]

Notwithstanding the extensive concerns voiced by market participants, and the effective SEC ban from September 2008, the SEC (in Report No. 450, March 2009) acknowledges that *"there is hardly unanimity in the investment community or the financial media on either the prevalence, or the dangers, of "naked" short selling…. Despite its assertions regarding the potential of danger of "naked" short selling and*

[34] German regulators have also called for a pan-European ban on "naked short-selling" since around 2010, but their concerns about naked short-selling have often been directed at credit default swaps, and the issues involved in that context are qualitatively different from the issues addressed in this paper.

the growing interest in the subject, the [SEC] Report can cite to no bona fide studies or empirical data regarding the practice's market impact." The financial press (for example, *The Economist,* "Naked Fear", July 24, 2008) has also occasionally expressed concern about severe restrictions being placed on naked shorting with little concrete evidence linking it to market manipulation.

We first provide empirical evidence that shows, independent of our main analyses, that failures-to-deliver (hereafter "FTDs") have been mainly caused by naked shorting rather than routine settlement failures arising from human errors and processing delays, and our FTD-based measure is an excellent proxy for naked shorting. Using data on a sample of 1,492 NYSE securities over 2005 to 2008, we estimate that naked shorting has affected about 95% of NYSE securities, though naked shorting volume is order(s) of magnitude lower than covered shorting volume, accounting on average for only about 1% of total short selling.

As a first test of causation, we form daily portfolios on changes in naked shorting and show that increases in naked shorting lead to decreases in pricing errors, pricing error volatility, return volatility, bid-ask spreads and order imbalances, even after controlling for changes in covered shorting. We find also a slight decline in prices. We extend our testing framework by implementing panel OLS regressions in which we control for both covered shorting and for systemic market effects, and arrive at the same findings. Given the complex and endogenous interrelationships between market quality metrics, we further estimate a vector autoregressive model and find that an increase in naked shorting equivalent to 10 basis points of the number of outstanding shares leads to approximately a 1% reduction in spreads, a 2% reduction in order imbalances, a 10%

reduction in the magnitude of positive pricing errors, a 13% decline in pricing error volatility and a 1% reduction in stock price volatility. These results are consistent with traders employing naked shorting to provide liquidity when it is particularly needed, and value arbitrageurs enhancing pricing efficiency through naked shorting. As we are more likely to find negative effects associated with naked shorting when it is heaviest, we also focus on a sample of the most naked shorted securities. Even in this sample, we find a similar, positive impact on market quality. And, consistent with our expectations, we find that the market impact of covered and naked shorting is similar in all cases. Finally, we estimate impulse response functions and find that positive shocks in naked shorting lead to lower pricing error volatility, lower spreads, lower order imbalances and lower returns' volatility over the following day, without significant subsequent corrections.

The SEC September 2008 Rule 204T (discussed earlier in this section) forced day *(t+4)* close-out of FTDs by market participants other than market-makers, since the concerns of “abusive” naked shorting related to these “other” public traders, not to market-makers. We use the average level of naked shorting post-204T to benchmark the relative proportion of (potentially speculative) naked shorting that originated pre-204T from public traders (i.e., traders other than market makers). We re-estimate our VAR models separately for two groups with low and high levels of public trader naked shorting, and find that our results hold also for each group separately. We conclude that the beneficial impact of naked shorting on pricing efficiency and liquidity is not driven just by market makers, but also by naked short sales of public traders.

Since naked short sellers have been widely alleged in the media to have contributed to the financial crisis by precipitating *manipulative* price declines of financial firms in 2008, we analyze a few high-profile financial firms that experienced dramatic stock price declines: in particular, *Bear Stearns Companies Inc.* (hereafter Bear Stearns), *Lehman Brothers Holdings Inc.* (hereafter Lehman), *Merrill Lynch & Co. Inc.* (hereafter Merrill), and *American Insurance Group* (hereafter AIG). We find that, most of the time, naked shorting volume was too low to reasonably "cause" significant stock price distortions, and when naked shorting did become abnormally heavy, it was *after* dramatic price declines, *not before*, indicating that naked short sellers were *responding to* public domain information about the firms, rather than being responsible for triggering the observed precipitous price decline. We further analyze how naked shorting changes around public news of credit rating downgrades, and again find that naked shorting increases *after* rather than before the announcement. Our findings are consistent with naked short sellers *responding to* public information, rather than being responsible for triggering price declines.

We also analyze the market impact of the SEC naked shorting ban on 19 financial securities between July 15 and August 12, 2008. During the ban, we find higher absolute pricing errors and lower trading volumes, indicating that the naked shorting ban hampered price discovery and reduced liquidity. Returns, albeit negative, were not significantly affected, indicating that the ban failed to slow the price decline of the related securities.

Overall, our results have important implications for regulation and public policy. It is true that we should have legitimate concerns about naked shorting because of the disruptions that can be created by delivery fails. We cannot also condone potentially willful and blatant disregard for the rules that provide the framework for orderly markets. However, it is also true that the vast majority of FTDs are only delays rather than "failures" of delivery since the median age of fails is only 3 days (Boni, 2006), and the beneficial impact of naked shorting on pricing efficiency and liquidity is the same as that of covered shorting. There is also no empirical support for naked shorting having market-destabilizing effects. Hence, totally removing the right to fail for the majority of market participants is debatable, since we know from Merrick, et al. (2005) that the right to fail is an important release valve for settlement related pressures and manipulative distortions, and particularly valuable when borrowing is too expensive for covered short sellers, which is exactly when liquidity is most needed in the stock-borrowing market (Evans, et al., 2008). Naked shorting protects traders from the extreme vagaries of this less regulated stock borrowing market. Progressive fines for settlement delays may be more expedient than blanket bans; and irrespective, it is essential to have greater focus on removing the economic incentives for naked shorting by improving the liquidity, transparency and regulation of the stock borrowing market.

Our research also adds to the vast literature on short selling, which has not hitherto distinguished between naked and covered shorting. First, we construct effective metrics that enable accurate identification of the extent of each of the two practices. Second, we underscore the functional equivalence of the practices in the framework of stock borrowing mechanisms. Third, we estimate and compare the impact of covered and naked shorting separately in the context of market liquidity and pricing efficiency. Fourth, we provide strong evidence that focusing regulatory efforts on curbing naked shorting instead of improving the practices and the efficiency of the stock borrowing market is questionable, since most naked shorting has the same beneficial impact on market quality as covered shorting; and there is no evidence that naked shorting has contributed to (potentially manipulative) price distortions in any way.

The remainder of the paper is structured as follows. Section 2 develops our hypotheses. Section 3 motivates our naked shorting measure, and provides empirical evidence to show that it as an effective proxy for naked shorting. Section 4 defines the other measures and variables we use. Section 5 documents our core empirical methods and results. Finally, Section 6 presents concluding remarks.

2. Development of hypotheses

2.1. Naked short selling and pricing efficiency

Fails to deliver in the US equity market have exacerbated the sharp declines in share prices of financials. -- Helen Avery in "Short selling: The naked truth", *Euromoney* (December, 2008).

False rumors …. may be further exacerbated by "naked" short selling…. [and if] significant financial institutions are involved, this chain of events can threaten disruption of our markets. -- Securities Exchange Act of 1934 Release no. 58166 / July 15, 2008.

As discussed in Section 1, while there is no empirical evidence in this regard specifically on naked shorting, there is reasonably strong consensus that covered and naked short sellers collectively enhance price efficiency. Since covered and naked short sales are functionally and observationally indistinguishable at the time of the trade, our first hypothesis is *H1: Naked short selling improves pricing efficiency.*

To test Hypothesis *H1,* we empirically investigate several aspects of pricing efficiency.

1. First, naked short sellers will contribute to pricing efficiency if they enter the market when securities are over-priced: then, their trades will arguably reduce the positive pricing errors of these overpriced securities. Hence, we test whether naked shorting leads to a reduction in positive pricing errors.
2. Second, a reduction in positive pricing errors should make the market more informationally efficient, and such a market should display a lower dispersion of pricing errors (Hasbrouck, 1993). Hence, we test whether naked shorting leads to reduced volatility of pricing errors.
3. Third, higher pricing efficiency should translate into more orderly markets, and hence we test whether naked shorting leads to a reduction in stock price volatility.
4. Finally, if naked shorting contributes to pricing efficiency, we should observe that the SEC ban on naked shorting in the wake of the 2008 financial crisis leads to reduced pricing efficiency. Accordingly, we test whether the July/August 2008 and the September 2008 SEC bans on naked shorting lead to higher volatility of pricing errors, higher spreads and lower trading volume.

2.2. Naked short selling and liquidity

In certain circumstances, naked short selling contributes to market liquidity…. Because it may take a market maker considerable time to purchase or arrange to borrow the security, a market maker engaged in bona fide market making, particularly in a fast-moving market, may need to sell the security short without having arranged to borrow shares…. SEC Report "Key Points about Regulation SHO", April 11, 2005, updated 2008.

The Commission has repeatedly stressed the fact that the practice [of naked short-selling] can provide needed market liquidity in certain circumstances …. SEC Report No. 450, March 18, 2009.

The SEC has clearly acknowledged that naked shorting can be employed by market makers and other liquidity providers to quickly and efficiently fulfill orders. Naked shorting offers an alternative to short sellers when the cost of borrowing in security-lending markets is too high (Evans, et al., 2009), which is more likely when liquidity is most needed. Hence, our second hypothesis is *H2: Naked short selling improves liquidity.*

We test Hypothesis *H2* in two ways. First, we test whether naked shorting leads to lower spreads and reduced order imbalances. Second, if naked shorting contributes to liquidity, we should observe that the SEC ban on naked shorting introduced in the wake of the 2008 financial crisis leads to a reduction in liquidity. Accordingly, we also test whether the July/August 2008 and the September 2008 SEC bans on naked shorting lead to lower trading volumes and higher spreads.

2.3. Manipulative naked short selling and the 2008 financial crisis

We have been concerned about 'naked' short selling and, in particular, abusive 'naked' short selling, for some time. -- SEC Report No. 450, March 18, 2009.

Illegal naked short selling, according to Robert Shapiro, a former Under Secretary of Commerce, has cost investors $100 billion and driven 1,000 companies into the ground. -- "Watch Out, They Bite!", Time Magazine (November 9, 2005).

As discussed in the introduction, extensive concerns have been articulated in the media, and by investor groups and company CEOs, about naked short-sellers undertaking "bear raids", causing stock prices to decline, particularly during the 2008 financial crisis. Regulators have also accused naked short sellers of manipulatively depressing stock prices. Accordingly, our third hypothesis is *H3: Naked short selling* *contributed adversely to the 2008 financial crisis.* We empirically investigate Hypothesis *H3* in several different ways.

1. First, commentators have blamed naked short sellers for the price crashes of Bear Stearns, Lehman, AIG and Merrill. Accordingly, we test for the presence of high levels of naked shorting prior to the large price declines in the stock prices of those companies.
2. Second, we note that naked short-sellers are typically thought of as undertaking "bear raids" to trigger downward price spirals with the aim of achieving credit downgrades so as to also profit from potentially simultaneous positions in the CDS market. In this context, we test whether naked shorting intensifies *prior to* credit rating downgrades. In a similar spirit, we investigate whether naked shorting intensifies prior to large stock price declines, particularly for securities issued by highly levered firms.

2.4. Other questions: impact of covered vs. naked short-sales and public traders vs. market makers

We address two other overarching questions though we do not formally frame them as numbered hypotheses. First, in view of the functional and observational equivalence of covered and naked shorting, we examine whether the (beneficial) impact of covered shorting on our pricing efficiency and liquidity metrics is different from that of naked shorting. Second, in the context of the widespread pejorative (or at least negative) association of naked shorting with potentially parasitic "speculators", we analyze whether the proportion of naked shorting undertaken by public traders other than registered market-makers changes our inferences on the impact of naked shorting on pricing efficiency and liquidity.

Table 9 summarizes the totality of variables we use all through the paper. Liquidity measures, and the other variables in Table 9, are defined and estimated as commonly done in the literature. The other variables are discussed in the next two sections.

3. Fails to deliver and naked short selling

… the majority of these failures-to-deliver are not the result of honest mistakes or bad processing -- Former SEC commissioner Roel Campos in an interview reported in "Short Sellers Squeezed All Around", The Wall Street Journal (April 7, 2009)

FTDs result from three reasons: honest mistakes or delays in processing, potentially illegal ("abusive") naked shorting done with *ex-ante* intent to deceive, defraud, or manipulate, and *bonafide* legal naked shorting. As discussed earlier in the introduction, even though it may be tempting to think about the *ex-ante* intention of the short-seller at the time of the trade, such *ex-ante* intention is neither observable nor even definitively exists at the time of the trade. Any attempt to infer *ex-ante* intention will have to be anchored in highly subjective and restrictive modeling frameworks. As emphasized earlier, even the courts drilling down into detailed evidence in individual cases have not been able to adjudicate damages against a single short-seller in spite of extensive litigation on naked shorting (Stokes, 2009). Hence, in constructing our measure of naked shorting, we estimate aggregate naked shorting, without trying to distinguish between illegal and legal naked shorting. Our results reflect the overall impact of all naked shorting. Given that a naked short sale cannot be objectively defined at the time of the trade t, the only way a trade can be credibly defined or classified as naked or covered is on the basis of the actual *ex-post* failures as of settlement day *t+3*, i.e., in accordance with the definition hitherto used by the SEC. Accordingly, we construct a measure of naked shorting that is based on FTDs actually observed *ex-post* as of settlement day, daily data on which has been made available by the SEC under the Freedom of Information Act (FOIA) since March 22, 2004.

We proxy naked shorting by the *Outstanding Naked Short Ratio* (*ONSR*) defined for each day *t* as the number of shares that represent outstanding fails to deliver as of day *t* scaled by the total number of shares of the firm. Since failure is recorded only on day *t*+3, we calculate cumulative naked short sales till day *t* by adjusting the outstanding failures to deliver (*Outstanding FTDs*) from the SEC data by adding the naked short sales that have already taken place but have not yet been observed (*New FTDs*) because they will show up only after settlements are duly completed over days *t+1*, *t+2*, and *t+3.* For each day *t*, the difference between cumulative FTDs on day *t* and cumulative FTDs on day *t-1* is equal to the number of new naked shorted shares minus the number of previously outstanding FTDs closed on day *t*. We approximate the number of previously outstanding FTDs settled on a particular day on the basis of the SEC *Office for Economic Analysis* memorandum dated August 21, 2006, "Fails to Deliver Pre- and Post- Regulation SHO", and the assumption of constant settlement rates. Our results are robust to different settlement rate assumptions. Accordingly:

$$ONSR_{i,t} = \frac{Outstanding\ FTDs_{i,t} + \sum_{j=1}^{3} New\ FTDs_{i,t+j}}{Shares\ Outstanding_i}$$

Extant literature provides good support for FTDs being a good basis for proxying for naked shorting. In particular, Evans et al. (2008) find that the number of

FTDs is strongly related to rebate rates, indicating that FTDs originate largely from (naked) short transactions; and Boni (2006) shows that the number of FTDs is related to the number of short sales, and offers evidence that market makers 'strategically' fail to deliver when borrowing costs are high, again pointing to FTDs being governed by (naked) shorting. However, Edwards and Hanley (2010) suggest that FTDs "in price supported IPOs may arise [also] from the mechanism of the offering process." To avoid the possibility of IPO-related FTDs, we exclude from our analysis securities that started trading during our sample interval. In addition, to prevent the possibility of similar FTDs in conjunction with other share issues, we exclude securities for which we observe significant changes in the number of shares outstanding from our sample.

However, our FTD-based *ONSR* measure is still a proxy and not a perfect measure of aggregate naked shorting because FTDs generated by human errors or processing delays will always add some random noise. In this section, we report the results of our own empirical analysis that provides strong support for our FTD-based *ONSR* measure being an excellent proxy for naked shorting.

First, in results not formally reported in tables for brevity, we find that the number of new FTDs on day t+3 is significantly (p-value<<0.01) *and positively* related only to 'short volume', i.e. the daily trading volume arising from short sales on day t, rather than 'non-short' volume, the daily trading volume arising from regular sales on day t. This indicates that FTDs arise overwhelmingly from short-sales and not from settlement-related delays arising from regular sales.

Second, we examine time-series changes in *ONSR* during a period in which we have good economic reason to independently expect those changes to be driven ***only*** by naked shorting, that is, during the SEC ban on naked shorting of the stocks of 19 publicly traded financial institutions from July 21 to August 12, 2008. During this ban period, the SEC order required that "*no person may effect a short sale in these securities …. unless such person or its agent has borrowed or arranged to borrow the security or otherwise has the security available to borrow in its inventory prior to effecting such short sale.*" (SEC Release 58166, 2008). Accordingly, we employ event study methodology to investigate the variation in FTDs around the SEC ban period from July 21 to August 12, 2008. Data for 17 of the 19 affected securities, listed in Table 10, were available to us from CRSP. We construct a matched sample as follows. We start from the universe of firms listed on CRSP as of January 1, 2008. For each of our target securities, we identify common equity of the firm sharing the same 4-digit SIC code with the closest market capitalization to that of our targets (as of January 1, 2008) and not affected by the ban. Then, for each of the 34 securities (the 17 affected securities and the 17 unique matches) and for each day in the interval January 1 to August 12, 2008, we compute the *Outstanding Naked Short Ratio* (*ONSR*). We then compute *Mean ONSR* for both event and control samples over a pre-ban period (January 1 to July 20, 2008), for each week in the ban period (July 21 to August 12, 2008), and for the three-week period following the ban (August 13 to September 2, 2008), finishing well before the tumultuous period starting in the second week of September 2008. We report these results in Table 10.

Importantly, for the sample of securities that are affected by the ban, we find an extremely significant and monotonic *reduction* in *Mean ONSR* of more than 40% over the first week of the ban, more than 93% by the second week of the ban, and more than

96% by the third week of the ban. In fact, in the second and the third weeks of the ban period, *Mean ONSR* is statistically indistinguishable from zero for the sample of securities that are affected by the ban. We believe that *Mean ONSR* took a few days to reduce to virtually zero in the ban period because it is derived from a measure of outstanding rather than new FTDs, and some time is required for the old outstanding fails to clear (Boni, 2006). As soon as the naked shorting ban ends, *Mean ONSR* again increases monotonically and very significantly to several multiples of its end-of-ban value. In contrast, *Mean ONSR* increased over the ban period for control firms.[35] The difference between *Mean ONSR* for the event and control firms is positive and significant prior to the ban, negative and significant during the ban, and again positive and significant after the ban is lifted. Assuming that the SEC ban was effective in curtailing naked shorting, our results clearly indicate that the vast majority of FTDs (> 95%) originate from naked shorting, rather than processing delays or human errors, and at the very least, FTDs provide an excellent basis for a proxy for naked shorting.[36]

4. Definitions of other measures and variables

Our main proxy for naked shorting, the *Outstanding Naked Short Ratio* (*ONSR*) has been defined in the preceding section. In order to control for the effects of covered

[35] While we do not formally investigate this increase, it would appear reasonable for both covered and naked shorting to increase over July and August 2008 for financial firms, due to news of deteriorating financial performance and liquidity.

[36] The SEC Office for Economic Analysis (OEA) has analyzed the number of FTDs around the introduction of their October 2008 rule removing the right to fail except for market-makers and under certain conditions (OEA Memoranda "Impact of Recent SHO Rule Changes on Fails to Deliver" dated November 26, 2008, "Impact of Recent SHO Rule Changes on Fails to Deliver" dated March 20, 2009, and "Impact of Recent SHO Rule Changes on Fails to Deliver" dated April 16, 2009). They also report a significant drop in FTDs though, as expected, much less of a drop than was seen in July/August 2008 when all naked shorting was banned. Irrespective, their reported results again show that it is naked shorting, and not human errors or processing delays, which primarily generates FTDs.

shorting, we similarly estimate the *Outstanding Covered Short Ratio* (*OCSR*), the daily outstanding covered shorted shares scaled by the number of shares outstanding. The number of daily outstanding covered shorted shares is computed by subtracting the outstanding naked shorted shares from the contemporaneously outstanding total shorted shares, estimated using total short-interest data and the total volume of daily short sales from the NYSE short sales dataset.

In order to examine the effect of naked shorting on pricing efficiency, we construct, for each sample security, a daily estimate of the information-efficient “random-walk” or "fundamental" price of the security; and accordingly define the “pricing error” on the day as the difference between the observed price that day and the estimated information-efficient price. The unobservable information-efficient price, a "latent" stochastic variable, is estimated using a Kalman-filter methodology as in Hamilton (1985). The procedure involves establishing two equations. The first equation dictates the evolution of the latent variable, and in our case we assume, in the spirit of Hasbrouck (1993), that the logarithm of the stock’s underlying or information-efficient value, $F(t)$, follows a random walk with a drift, μ, and a white noise innovation, $\varepsilon(t)$, with mean zero and variance σ_{ε}^{2}:

$$F(t) = \mu + F(t-1) + \varepsilon(t), \quad \varepsilon \sim N(0, \sigma_{\varepsilon}^{2})$$
$$\Delta Y(t) = -\alpha Y(t-1) + \varphi(t), \quad \varphi \sim N(0, \sigma_{\varphi}^{2})$$

The second equation relates the observed and latent variables, i.e., specifies the pricing error process. In our case, we assume that the pricing error *Y(t)* follows a mean-reverting process around zero, with *α,* the rate of mean-reversion, ranging between 0 and 1. Pricing errors correct fully in one period when α is equal to one, and not correct at all when α is equal to zero.

The observed log of stock price $S(t)$ is the sum of the fundamental price and pricing error:

$$S(t) = F(t) + Y(t)$$

Hence, $S(t) = \mu + (1-\alpha)S(t-1) + \alpha F(t-1) + \theta(t)$, $\quad \theta(t) = \phi(t) + \varepsilon(t)$

The *Expectation Maximization* (EM) algorithm (Dempster, et al., 1977) is employed to compute the Maximum Likelihood (ML) estimate of the unobservable variable, *F(t)*, based on data relating to the observed variable, *S(t)*. Hamilton (1985) employs such an approach to estimate expected quarterly inflation, the latent variable, based on observed actual inflation. In exactly the same way, we utilize the observed daily stock prices to infer the daily unobserved "fundamental price", and hence the daily pricing error, using daily closing price data from CRSP. The state-space representation of the system is as follows.

Measurement Equation:

$$[S(t)] = [1 \quad 0 \quad 0]\begin{bmatrix} S(t) \\ F(t) \\ \varepsilon(t) \end{bmatrix}$$

Transition Equation

$$\begin{bmatrix} S(t) \\ F(t) \\ \varepsilon(t) \end{bmatrix} = \begin{bmatrix} (1-\alpha) & \alpha & \mu \\ 0 & 1 & 0 \\ 0 & 0 & 0 \end{bmatrix}\begin{bmatrix} S(t-1) \\ F(t-1) \\ 1 \end{bmatrix} + \begin{bmatrix} \theta(t) \\ \varepsilon(t) \\ 0 \end{bmatrix}$$

To assure ourselves of the unbiasedness and efficacy of our pricing error estimation process, we run 500 simulations of both fundamental price and pricing error for a hypothetical stock over 252 trading days assuming a range of volatility parameters and mean reversion parameters. In each case, we add the fundamental price and the pricing error to arrive at the equivalent of a simulated "observed" price. Then, we run our Kalman-filter estimation procedure on this "observed" price series to determine our Kalman-filter estimate of the originally simulated fundamental price. Finally, we run a regression of changes in the originally simulated fundamental price on changes in our Kalman-filter estimate of that fundamental price. In each and every case, the regression intercept is not significantly different from zero, and the regression slope is not significantly different from one; and the root mean square error in the estimated fundamental price is economically small in magnitude.

5. Empirical results

5.1. Data and samples

Data regarding the number of fails-to-deliver is available from late March 2004 onwards. To calculate and control for covered shorting, we use short interest data from www.shortsqueeze.com and short sales data available from January 2005 onwards from the NYSE. We obtain the number of shares outstanding from CRSP. Our market quality measures are based on NYSE TAQ data. Our main sample consists of all common shares of US-listed securities (CRSP share codes 10 and 11) for which we have complete data across the various databases we use and which are listed on the NYSE. However, we restrict our main sample to securities that are listed for at least six months (as we require at least six months of data to estimate the vector autoregressive model at the heart of our empirical analysis) and that have been trading for at least one year prior to our period of interest. Having a year of trading data allows us to estimate pricing errors as discussed in section 4. It also leads to excluding shares of recent IPOs, thus controlling for the unusual volumes of FTDs around IPOs documented by Edwards and Hanley (2010). In the same spirit, we further require that the number of shares outstanding does not vary by more than 10% over any single day to further eliminate the potential for confounding effects on market quality due to share issuance or repurchases. That said, we also re-sample without this last constraint and find that our results are robust.

A series of restrictions on both covered and naked shorting were initiated with a temporary naked shorting ban on 19 securities in July/August 2008 and culminated in severe restrictions on naked shorting in all stocks from September 2008 onwards (through SEC Rules 204T) by mandating delivery against an FTD by the clearing agency member on the morning of the day after settlement day. Extending our joint analysis of naked and covered shorting beyond June 2008 could not offer additional insight in view of these major restrictions on naked shorting. Our main sample for the comparative analysis of naked and covered shorting consists of 1,492 NYSE securities over the period January 2005 to June 2008.

5.2. Preliminary descriptive analysis

Table 11 reports descriptive statistics relating to our main variables of interest. We find that mean *ONSR* is about 6 basis points. By construction, *ONSR* varies by decile: the mean is less than 1 basis point in decile 1 and about 43 basis points in decile 10. Notably, though not formally in the table, *ONSR* is higher prior to the introduction of Regulation SHO in January 2005 by an average of about 9 basis points. The mean *Naked to Total Shorts* ratio is about 1% for the overall sample, 2.8% for decile 10 and 0.1% for decile 1. We find that *OCSR* is similarly higher in decile 10 (13.4%) than in decile 1 (3.95%), which is indicative of a positive correlation between *OCSR* and *ONSR*. Securities in deciles 1 and 10 do not differ significantly in terms of mean pricing errors. However, we find that securities with high naked shorting differ significantly from securities with low naked shorting in several other ways. Securities with higher naked shorting display significantly higher positive pricing errors and pricing error volatility, which is consistent with naked shorting intensifying when securities are overpriced. Similarly, higher levels of naked shorting are associated with higher, positive order imbalances and higher stock price volatility but lower spreads. Our analysis also reveals that naked shorting is *significantly* greater for relatively smaller firms, as average market capitalization for firms in decile 1 (USD 12.5 billion) is almost ten times larger than for firms in decile 10 (USD 1.48 billion). We recognize that the direction of causality cannot be inferred from such univariate analyses and, hence, refrain from further interpretation of these findings. Instead, we focus in the following section on several methodologies – portfolio analysis, OLS panel regressions and VAR analysis – which allow us to draw some inferences about causation.

5.3. Naked short-selling and market quality

In this section, we test Hypotheses *H1* and *H2* by investigating the relationship between naked shorting and measures of pricing efficiency and liquidity. We formally test for causality by employing three different modeling frameworks: (1) a portfolio approach, (2) panel OLS regressions employing controls for systemic effects, and (3) a vector autoregressive (VAR) modeling framework to control for both systemic effects and endogenous interrelationships. Our naked shorting data is available only at a daily frequency, and therefore, our analyses are based on data at daily frequency.

5.3.1. Portfolio approach

As a first test of the impact of naked shorting on market quality, on each day *t* in our sample spanning January 2005 to June 2008, we group our sample of 1,492 securities into nine portfolios of securities based on changes in *ONSR* and *OCSR*. We start by estimating the time-series standard deviation in *ONSR* by security. We include securities with a one-standard deviation or greater decrease in *ONSR* into an '*ONSR* Decrease' portfolio and, similarly, securities with a one-standard deviation or greater increase in *ONSR* into an '*ONSR* Increase' portfolio. We include all remaining securities into an '*ONSR* No Change' portfolio. In order to control for the extent of covered shorting, we replicate the same procedure on the basis of covered shorting, proxied by changes in *OCSR*, thus forming the portfolios '*OCSR* Decrease', '*OCSR* No Change' and '*OCSR* Increase'. Finally, we intersect those groups of securities, forming nine final portfolios ('*ONSR* Decrease and *OCSR* Decrease', '*ONSR* Decrease and *OCSR* No Change', '*ONSR* Decrease and OCSR Increase' and so forth). For each portfolio, we then compute average changes in our metrics of price levels, return volatility, market liquidity, pricing errors, pricing error volatility and order imbalances for the following day (t+1). All variables are standardized and winsorized (at three standard deviations) by security. We compute next-day average changes for the portfolios '*ONSR* Decrease, *OCSR* No Change' and '*ONSR* Increase, *OCSR* No Change'; and we further test for differences between these averages.

The results of this analysis are presented in Table 12: for ease of interpretation, we convert standardized average changes into base units. For the portfolio '*ONSR* Increase, *OCSR* No Change', we find, on the following day, a 16 bps decrease in pricing error, an 82 bps reduction in pricing error volatility, a 1 bp decrease in spreads, a 24 bp decrease in order imbalances and a 6 bps decrease in stock price volatility – all results being statistically significant at the 1% level; we also find a decrease in prices, but the result is not statistically significant. For the portfolio '*ONSR* Decrease, *OCSR*

No Change', the only result we document that is statistically significant (at the 5% level) is a 2 bps increase in volatility and an increase in prices on the following day. We further investigate the differences in next-day metrics between the portfolios '*ONSR* Increase, *OCSR* No Change' and '*ONSR* Decrease, *OCSR* No Change'. The increase portfolio displays higher next-day returns, lower prices, pricing errors, pricing error volatilities, spreads, order imbalances and volatility, with all results being statistically significant at the 1% level, with the exception of the change in prices, significant at the 5% level. Further, we compute the difference between then changes in market quality metrics estimated for the '*ONSR* Increase, *OCSR* No Change' and the '*ONSR* Decrease, *OCSR* No Change' portfolios. Consistent with the above findings, all results are statistically significant.

Clearly, this initial analysis offers evidence consistent with Hypotheses *H1* and *H2*: naked shorting is associated with improvements in both market liquidity and pricing efficiency. Given the limitations of this univariate framework, we conduct multivariate analysis in the following section, employing OLS regressions to investigate the relationship between naked shorting and market quality while controlling for covered shorting and systemic factors.

5.3.2. Panel OLS regressions

In this section, we estimate panel regressions to investigate the impact of naked shorting on market quality metrics: pricing errors, pricing error volatility, prices, return volatility, spreads and order imbalances. Accordingly, we estimate six separate regressions, with changes in market quality metrics as responses. Our main explanatory variables are lagged (previous-day) changes in *ONSR* and *OCSR*. As controls, we include, in each regression, lagged changes of the dependent variable, to account for possible autocorrelations. In addition, to control for systemic effects, we include market averages of the changes in the same quality metrics. Finally, when modeling changes in pricing errors, we add interaction variables between changes in *ONSR* and *OCSR* and a binary variable identifying positive pricing errors to investigate the asymmetric impact of shorting on pricing errors. All regressions contain security fixed-effects and standard errors are time-clustered. The models we estimate are as follows (variables are as defined in Table 9 and the *'M'* subscript indicates a market-wide equally weighted average).

$$\begin{aligned}\Delta PE_{i,t} = {} & \lambda_i + \lambda_1 \Delta ONSR_{i,t-1} + \lambda_2 \Delta OCSR_{i,t-1} + \lambda_3 \Delta ONSR_{i,t-1} * Positive\_PE\_Dum_{i,t-1} + \\ & \lambda_4 \Delta OCSR_{i,t-1} * Positive\_PE\_Dum_{i,t-1} + \lambda_5 \Delta PE_{M,t} + \lambda_6 \Delta PE_{i,t-1} + \varphi_{i,t}\end{aligned}$$

$$\Delta PE\_Volatiltiy_{i,t} = \gamma_i + \gamma_1 \Delta ONSR_{i,t-1} + \gamma_2 \Delta OCSR_{i,t-1} + \gamma_3 \Delta PE\_Volatility_{M,t} + \gamma_4 \Delta PE\_Volatiltiy_{i,t-1} + \vartheta_{i,t}$$

$$\Delta Volatility_{i,t} = \delta_i + \delta_1 \Delta ONSR_{i,t-1} + \delta_2 \Delta OCSR_{i,t-1} + \delta_3 \Delta Volatiltiy_{M,t} + \delta_4 \Delta Volatility_{i,t-1} + \theta_{i,t}$$

$$\Delta Spread_{i,t} = \beta_i + \beta_1 \Delta ONSR_{i,t-1} + \beta_2 \Delta OCSR_{i,t-1} + \beta_3 \Delta Spread_{M,t} + \beta_4 \Delta Spread_{i,t-1} + \varepsilon_{i,t}$$

$$\Delta OIB_{i,t} = \kappa_i + \kappa_1 \Delta ONSR_{i,t-1} + \kappa_2 \Delta OCSR_{i,t-1} + \kappa_3 \Delta OIB_{M,t} + \kappa_4 \Delta OIB_{i,t-1} + \nu_{i,t}$$

$$\Delta Logmid_{i,t} = \mu_i + \mu_1 \Delta ONSR_{i,t-1} + \mu_2 \Delta OCSR_{i,t-1} + \mu_3 \Delta Logmid_{M,t} + \mu_4 \Delta Logmid_{i,t-1} + \sigma_{i,t}$$

Results for the overall sample are presented in Table 13. As reported, an increase in *ONSR* is associated with a next-day decrease in spreads (significant at 1%), return volatility (at 10%) and pricing error volatility (at 1%). *ONSR* is also associated with a decrease in pricing errors when those are positive and an increase in pricing errors when those are negative significant (both significant at 1%). Prices are not significantly affected.

This second set of results is highly consistent with our previous findings: naked shorting is related to improvements in liquidity and pricing efficiency. In the next section, we discuss results from a vector autoregressive framework that duly controls for endogeneity and interrelations between market quality metrics.

5.3.3. Vector autoregressive framework

In our third methodology, we test for causality while controlling for endogenous interrelationships using three vector autoregressive (VAR) models, with additional exogenous variable(s) added as controls. The system of equations underlying each of these models is formally specified in Table 14.

In VAR Model 1, our VAR variables are changes in *Outstanding Naked Short Ratio*, *Covered Short Ratio*, *Pricing Error*, *Volatility*, *Spread* and *Order Imbalance*. In the pricing error equation, we add, as a predictor, an interaction between lagged changes in *Outstanding Naked Short Ratio* and a lagged binary variable set equal to one when pricing error is positive. Accordingly, we are able to separately estimate the impact of naked shorting on pricing error when the pricing error is positive in contrast to when the pricing error is negative. We add two more predictors to each of the equations in which the change in *ONSR and OCSR* are the response variables: a lagged binary variable set equal to one when order imbalance is positive and zero otherwise and a lagged binary variable set equal to one when pricing error is positive and zero otherwise. This allows us to check whether positive order imbalances and positive pricing errors lead to higher naked and covered shorting. Finally, in each equation we add, as an exogenous variable, the market-wide equally-weighted average of the dependent variable, to control for possible systematic effects. In VAR Model 2, we replace *Pricing Error* by *Pricing*

Error Volatility, or effectively the absolute value of the *Pricing Error*, and in VAR Model 3, we replace *Pricing Error* by *Price*.

We estimate VAR Models 1, 2 and 3 separately for each security. The results we report in Table 14 are based on estimating the models by first standardizing all variables by subtracting the security-specific mean and dividing by the security-specific standard deviation, and then winsorizing at three standard deviations from the mean.[37] Based on an analysis of the Schwarz information criteria (SIC), we determine that, for all models, it is most appropriate to use a VAR of order one. In order to draw inferences about the true population parameters, we average coefficient estimates obtained for each security as in Fama and MacBeth (1973). Similarly, we use cross-sectional estimates of standard errors. To account for possible underestimation of those standard errors due to cross-correlations, we correct standard errors as in Chordia, Roll and Subrahmanyam (2001).

We estimate VAR Models 1, 2 and 3 for both our 'entire market' sample and for a subset of our sample containing only the 10% of most-naked-shorted securities based on mean *ONSR* over the sample period, January 2005 to June 2008. We focus on the subset of securities with most naked shorting to find whether the impact of naked shorting differs when it is heaviest. We find that the sign and the significance of the various inter-relationships involved are economically reasonable for both samples. For compactness, ease of interpretation and to preserve the focus on the specific equations of interest to us, we report in Table 14 only those relationships that are relevant to the issues addressed in the paper. In particular, we report, for all three models and for both

[37] For robustness, we also estimate all three models without standardizing variables, but we sometimes run into problems with the convergence of our maximum likelihood estimation algorithm, likely driven by the extreme differences in magnitude across our variables. For those securities for which we manage to obtain results without standardizing variables, our results are qualitatively similar to those reported.

samples, the effects of both covered and naked shorting, i.e. how covered and naked shorting impact each of our market quality metrics after controlling for endogenous interactions. We present estimated coefficients and related statistical significance in Table 14, Panels A and B.

First, interestingly, we do *not* find statistically significant evidence of a causal relationship from naked shorting to stock prices. On the other hand, in our overall market sample, we do find that covered shorting is followed by significantly lower stock prices in the following period.

Second, irrespective of the measure used as proxy for pricing efficiency or liquidity, naked shorting has a significantly beneficial effect on pricing efficiency and liquidity, strongly supporting Hypotheses *H1* and *H2*. In particular, we reach the following conclusions:

1. When pricing errors are positive, naked shorting significantly reduces pricing errors, consistent with naked short-sellers functioning as value arbitrageurs (statistically significant at 1%).
2. Naked shorting significantly reduces the volatility of pricing errors, which is also consistent with an increase in pricing efficiency (statistically significant at 1%).
3. An increase in naked shorting significantly reduces stock return volatility, consistent with improved market stability (significant at 1% in five models and at 5% in the sixth).

4. An increase in naked shorting significantly reduces order-imbalances, consistent with naked short-sellers contributing to improvement in liquidity (significant at 1%).
5. Coefficient estimates uniformly indicate that an increase in naked shorting reduces spreads but results are not statistically significant.
6. Coefficient estimates uniformly indicate that an increase in naked shorting reduces prices, but results are not statistically significant.

Finally, the direction and significance of all results relating to the impact of naked shorting are qualitatively similar to the direction and significance of all results relating to the impact of covered shorting. The exceptions are that the impact of covered shorting on prices and spreads is statistically significant in some models, while not significant for naked shorting.

To further investigate the impact of naked shorting on pricing efficiency and liquidity, we compute impulse response functions based on the vector autoregressive models of Table 14. We estimate accumulated impulse response function parameters for each security and then present cross-sectional means of the parameter estimates. Standard errors employed in the computation of confidence intervals are cross-sectional. We present the results related to accumulated impulse response functions depicting the impact of one-standard deviation increase in naked shorting on pricing error volatility, return volatility, spreads and order imbalances in Figure 2.[38] For comparison, we present a similar set of impulse response functions for the impact of

[38] We opt to utilize accumulated, rather than orthogonalized, impulse response functions because, consistent with what is expected on the basis of the general econometric literature in this regard, the latter are extremely sensitive to the ordering of variables in the model, thus adding a level of arbitrariness to the estimation procedure. In unreported analysis, we estimate orthogonalized impulse response functions for models differing in order of the variables and find the results not to be robust to the ordering of variables.

covered shorting in Figure 3. For brevity, we only report impulse response functions related to the VAR models including pricing error volatility, omitting the models including pricing errors and prices; given the similarities in estimated and reported VAR coefficients, the omitted impulse response function sets are extremely similar.

As the impulse response functions indicate, the impact of naked shorting on market quality metrics – pricing errors, pricing error volatility, prices, return volatility, spreads, order imbalances – documented in Table 14 occur over the day following a shock in naked shorting. On subsequent days, accumulated responses are mostly flat, indicating no further correction or reversal.

5.3.4. Naked vs. covered short-selling

Overall, when qualitatively comparing the impact of naked and covered shorting, the direction and significance of our results are virtually identical, indicating that the two practices have very similar impacts. That said, we note that the coefficients measuring the impact on market quality in Table 14, Panels A and B are, in most cases, much larger in magnitude for covered shorting than for naked shorting. This is arguably what we should expect given that the vector autoregressive models we employ are based on standardized variables, and the average standard deviation of covered shorting is much greater than that of naked shorting. Hence, a one standard deviation change in covered shorting clearly results in a much greater change in the level of overall short-selling relative to a one standard deviation change in naked shorting, and a greater level of overall short-selling should arguably have a greater beneficial impact on market quality. In this context, we next examine the economic significance of the impact of naked and covered shorting by estimating the market-quality impact of a given *amount* of naked and covered shorting in terms of the incremental number of shares sold short (or proportion sold short relative to the total number of outstanding shares). The results are reported in Table 14, Panels C and D for the impact of naked and covered shorting respectively.

Based on the 'entire market' sample, we find that an increase in naked shorting corresponding to 10 basis points of the total number of outstanding shares leads to about a 1% reduction in spreads, a 1.7% reduction in order imbalances, a 9.8% reduction in the magnitude of positive pricing errors, a 12.9% decline in pricing error volatility and a 1% reduction in stock price volatility. The impact on prices, aside from not being statistically significant, is tiny, with a reduction of 0.01%. In comparison, again based on the 'entire market' sample, we find that an increase in covered shorting corresponding to the same 10 basis points of the total number of outstanding shares leads to about a 0.7% reduction in spreads, a 1.3% reduction in order imbalances, a 4.9% reduction in the magnitude of positive pricing errors, a 7.4% decline in pricing error volatility and a 0.7% reduction in stock price volatility. The impact on prices is similarly tiny, at 0.01%. Overall, the economic significance of the estimated impact is of roughly similar magnitude, but naked shorting has a slightly stronger beneficial impact than covered shorting. Inferences based on the most-naked shorted sample are weaker. This could potentially be because a certain amount of naked shorting has a weaker impact when existing levels of naked shorting are higher.

The bottom-line is that the market-quality impact of both covered and naked shorting is economically significant and very similar, which is in line with our expectations, since naked and covered shorting are indistinguishable at the time of the trade. The impact is also in the direction of being clearly beneficial for market quality. Our results, quite unequivocally, also provide strong support to Hypotheses *H1* and *H2*. Naked short-sellers appear to have a considerably positive effect on market quality, first by enhancing pricing efficiency through correction of security overvaluation and reduction of volatility, and second by providing and improving liquidity through reduction of order-imbalances and spreads.

5.3.5. Naked Short-Selling: market makers vs. public traders

Market-makers have routinely employed naked shorting to cheaply and quickly provide liquidity. It is therefore conceivable that the beneficial effects of naked shorting on market quality arise entirely due to such market-making activities, and that there are no beneficial effects from any naked shorting done by public traders, where we define the term "public trader" in this context to mean all market participants other than registered market-makers. Notably, it is these public traders that have been the primary focus of negative media and regulatory attention. To estimate the impact of naked shorting done specifically by public traders (relative to the impact of naked shorting by market-makers), we need to classify securities based on the proportion of naked shorting originating from public traders. In order to accomplish that, we employ an exogenous event – a ban that essentially affected only naked shorting by public traders but not market-makers. Clearly, if there are no beneficial effects of naked shorting done by public traders, our estimated naked-shorting related improvement in market quality in the pre-ban period will be relatively weaker for securities that have a relatively greater proportion of naked shorting by public traders.

Our investigation of the issue is based on the SEC temporary rule 204T enacted in September 2008 (and later made permanent) that required all market participants *other than registered market makers* to purchase or borrow securities to close out their FTD position by the beginning of day *t*+4, effectively banning naked shorting for anyone other than a market maker. Hence, we compute average naked shorting across our entire sample of NYSE securities for two periods: pre-204T from January 1, 2005 to June 30, 2008; and post-204T from January 1, 2009 to December 31, 2010. We intentionally omit the period July to October 2008 because of multiple short-selling and naked-short-selling rule changes, and omit November and December 2008 to allow outstanding naked short positions to fully clear and thereby enable accurately gauging the impact of post-204T naked shorting. Across all securities, we find a drop in average *ONSR* of approximately 71%, suggesting that less than 30% of all naked shorting was initiated by market-makers prior to September 2008.

For each security in our sample, we use the proportional change in mean *ONSR* between the two sub-periods as our proxy for naked shorting by public traders. We accordingly rank securities on the basis of this proportional decline and allocate securities to terciles. We then re-estimate our VAR for January 2005 to June 2008 for the two terciles "Low Proportion of Public Traders" (i.e., securities with the lowest decline in mean *ONSR*) and "High Proportion of Public Traders" (i.e., securities with the highest decline in mean *ONSR*). Our underlying assumption here is that naked shorting originating from market makers is fairly constant over time and that the observed decline indicates the quantity of naked shorting originating from public traders.

Our results are reported in Table 15. For both samples, our coefficient estimates are of the same sign as those of the overall sample. For the "High Proportion of Public Traders" sub-sample, all previously documented results continue to remain strong and statistically significant for all market quality metrics even though the sample size is much smaller than the overall sample. The results for the "Low Proportion of Public Traders" subsample are also statistically significant for magnitude of pricing error, returns volatility, and order imbalances, but are not statistically significant for spreads and the sign and level of pricing errors. Clearly, our results indicate that, if anything, the impact of naked shorting is stronger for the subset of securities with a relatively high proportion of naked shorting from public traders, rather than the subset with a high proportion of market maker initiated naked shorting. Hence, at the very least, our results are inconsistent with the hypothesis that the beneficial effects of naked shorting arise entirely from naked shorting by market-makers. They show that naked shorting by public traders also leads to improvement in market quality.

5.4. The market quality impact of restrictions on naked short selling.

In this sub-section, we report the effect on market quality of restrictions on naked shorting. We employ a natural experiment: the one-time SEC ban on naked shorting in July/August 2008. Given our previous results that naked shorting positively impacts market quality, we expect deterioration in market quality in the presence of a ban on naked shorting. Our sample is constructed as in our analysis of the same event in Section 3. For each of our 17 sample securities affected by the ban and the 17 unique control sample matches, and for each day in the interval January 1, 2008 to September 9, 2008, we compute *ONSR*, *Pricing Error Volatility*, *Return*, *Volatility*, *Spread* and

Order Imbalance, as in Table 9. We average each of these six variables across securities to obtain a daily mean for the affected and control samples. We then run six separate OLS regressions: in each regression, the response variable is the affected sample mean of either *Outstanding Naked Short Ratio*, *Pricing Error Volatility*, *Return*, *Volatility*, *Spread* or *Order Imbalance*, while the explanatory variables include an intercept, the control sample mean of the variable of interest and a binary variable, *Event*, set equal to 1 on all days during which restrictions were in place and equal to 0 on all other days. Our results, presented in Table 16, indicate that the number of naked shorted shares outstanding declines heavily during the ban (as expected and as discussed in section 3). Further, we observe significantly higher return volatility and pricing error volatility, both significant at the 1% level. Finally, we document higher spreads, significant at 10%. The impact of the ban on returns and order imbalances is not statistically significant. Overall, our results are in line with expectations, indicating that a ban on naked shorting adversely impacts the stock's liquidity,[39] and hampers the price discovery process, providing further strong support to Hypotheses *H1* and *H2*. Interestingly, our results also indicate that the ban failed to slow the price decline of the affected securities.

5.5. Naked short selling and the 2008 financial crisis.

Bear Stearns failed because it went bankrupt. However, the pace of the collapse of the stock price was clearly accelerated by the enormous naked short-sale activity. -- Robert

[39] Our results are consistent with contemporaneous work of Kolasinski, et al. (2009). While their main focus is on the different impact of restrictions vs. bans on short selling, they analyze the same event (interpreting it as a temporary short selling restriction) and find that the ban negatively impacted liquidity of the affected securities. Our results on this particular issue are also consistent with contemporaneous work of Boulton and Braga-Alves (2009).

Shapiro, former Under Secretary of Commerce quoted in “Short selling: The naked truth”, *Euromoney* (December, 2008).

Like all the great merchants of the bubble economy, Bear and Lehman were leveraged to the hilt and vulnerable to collapse. Many of the methods that outsiders used to knock them over were mostly legal: credit markers were pulled, rumors were spread through the media, and legitimate short-sellers pressured the stock price down. But when Bear and Lehman made their final leap off the cliff of history, both undeniably got a push — especially in the form of a flat-out counterfeiting scheme called naked short-selling. -- "Wall Street's Naked Swindle", *Rolling Stone* (October 2009).

Fails to deliver in the US equity market have exacerbated the sharp declines in share prices of financials. Although the SEC is clearing up the mess caused by naked short-selling, more drastic measures might be needed to restore confidence. -- “Short selling: The naked truth”, Euromoney (December, 2008).

In the wake of the financial crisis of 2008, the media has consistently pointed an accusing finger at naked short-sellers, blaming them for having caused, or at least accelerated, sharp declines in stock prices, particularly of financial firms. Naked short-sellers have often been described as villains who deliberately undertake “bear raids” to drive prices down, create conditions that trigger credit downgrades, and profit from the downward price spiral and the eventual collapse of the financial institutions involved. In this context, we test Hypothesis *H3* by specifically examining whether significant naked shorting *preceded* (and hence potentially triggered) the price crashes associated with the four major casualties of the 2008 financial crisis, i.e., Bear Stearns, Lehman, AIG and Merrill, and similarly *preceded* credit downgrades and large price decline episodes in other financial firms; or did significant naked shorting take place *after* these price crashes and *in response to* negative public news.

5.5.1. Bear Stearns

The first large financial casualty of the 2008 financial crisis was Bear Stearns, the fifth largest investment banking firm in the nation at the time of its demise. We analyze naked shorting on the days preceding and immediately following the dramatic loss of market value that led to the demise of the firm. Figure 4 provides a time-line about the crisis. Outstanding suspicions about liquidity problems at Bear Stearns were reported in the media from March 10 onwards, along with news that the company's management was repeatedly denying rumors about such problems. The first major price-crash took place on Friday, March 14, when the price per share dropped from \$57 to \$30 after a 9 a.m. announcement that Bear would receive an unprecedented loan from the Federal Reserve System; and two days later, on Sunday March 16, JP Morgan Chase proposed buying Bear Stearns for \$2 per share.[40] When markets opened on March 17, a second major price crash materialized, and the price dropped to a close of \$4.81.

We compute *ONSR* for Bear Stearns on each trading day from January 1 to March 28, 2008. We also compute a control variable for the same period - an equal weighted average *ONSR* for four other financial institutions with the same primary SIC code as Bear Stearns, and with the closest market value as of the end of the fiscal year 2007 - and test for the statistical significance each day of the difference, i.e. the "abnormal" naked shorting.[41] Our results are presented in Table 17, Panel A.

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[40] See, for example, "Fed Races to Rescue Bear Stearns in Bid to Steady Financial System Storied Firm", *The Wall Street Journal* (March 15, 2008) and "JP Morgan Chase to Acquire Bear Stearns", *J.P. Morgan News Release* (March 16,2008).

[41] The four control stocks are: *Raymond James Financial Corporation, Ameritrade Holdings Corporation, Ameriprise Financial Inc.* and *Charles Schwab Corporation*. To construct a t-statistic for the difference in means: we compute the mean and standard error of this difference over the period January 1 to February 15, 2008; we subtract this historic mean from the daily difference and divide the result by the historic estimate of the standard deviation of the difference.

Even though negative media attention started on March 10, Table 17, Panel A and Figure 4 show that abnormal naked shorting was statistically insignificant or significantly negative up to March 11. While naked shorting did increase significantly on March 12 and 13, the increase was still relatively tiny from an economic perspective since it was tiny relative to the total number of shares outstanding, tiny relative to the typical overall short volume, and tiny relative to what took place on or after March 14: the outstanding naked shorted shares were only 1.06% of shares outstanding until market close on March 13. Naked shorting increased to 2.24% (t-stat. 20.7) on March 14, but increased massively only on March 17, reaching 12.18% (t-stat. 137.3). Importantly, given that the Fed announcement was at the start of trading on March 14, even the (relatively small) increase in naked shorting on March 14 was clearly *subsequent to* the public release of tangibly negative news in the form of the announcement and the consequent immediate precipitous price-drop. By the time naked shorting really spiked on March 17, the company was already in an open distress sale. The evidence clearly shows that the abnormal incidence of naked shorting did not precede the price decline but followed it; and the decline in stock price was triggered by other well-identified negative economic news.[42]

Even in this extreme scenario, often cited as a glaring example of the negative role of naked short sellers in financial markets, we fail to find any evidence of naked short sellers engaging in manipulative "bear-raid" type activity. Rather, they appeared to be following strategies *in response to* public information. The decline in stock price

[42] However, media journalists have aggressively reinforced rumors that the price collapse happened because of naked-shorting. See, for example, "Wall Street's Naked Swindle", *Rolling Stone* (October 2009). See also opinions cited in "Short Sellers Aren't Jackals, They're Bears, Fleckenstein Says", *Bloomberg.com* (October 29, 2008).

appears motivated by unrelated and clearly identifiable factors; and consistent with our previous results, naked short sellers appear to be facilitating price discovery, rather than increasing pricing errors. Overall, we find no support for Hypothesis *H3*.

5.5.2. Lehman

The second notable casualty of the financial crisis of 2008 was Lehman. To investigate the link between naked shorting and the stock price crash, we employ the same method we used for Bear Stearns. We analyze naked shorting on the days surrounding the dramatic loss of market value of the firm on September 9, 2008, estimating "abnormal" naked shorting and associated t-statistics using a methodology similar to that employed above for Bear Stearns. Our results are in Table 17, Panel B. In Figure 5A we present the relationship between *ONSR* and stock price for the period from January 2008 to Lehman's bankruptcy on September 15, 2008. We present a closer look at the period surrounding Lehman's bankruptcy in Figure 5B.

The above table and figures indicate abnormally low naked shorting in the days leading to September 9, with abnormal *ONSR* at only around 0.01%. [43] But, by September 9, the firm's stock price had already lost approximately 87% of its value as of the beginning of the year. The biggest single-day price drop, about 45%, occurred on September 9, following news that talks with the *Korea Development Bank* (previously rumored to be considering a 25% stake in Lehman) had failed. While *ONSR* increases on that day, outstanding naked short shares still represent less than 0.16% of shares outstanding. Abnormal *ONSR* increased more dramatically only *after* September 10,

[43] A recent article "Naked Short Sales Hint Fraud in Bringing Down Lehman", Bloomberg (March 19, 2009), notes that a rumor about Barclays Plc buying Lehman for a 25% discount to market value was responsible for a 11% fall in Lehman's stock price on June 30. We find that *ONSR* spikes significantly on June 27, the day preceding the rumor, but the spike is still just 0.06%, far too miniscule to conclude that naked shorting, rather than negative information, was responsible for the price decline.

well after widespread coverage of negative news about Lehman and the associated price crash. On September 11, as shareholders rejected a management rescue plan, the stock price fell by an additional 42% and *ONSR* further increased to 3.3%. Over the following days, talks of a possible acquisition by Bank of America and Barclays failed, triggering further declines in stock price and an increase in *ONSR* to 4.9%. Lehman announced its bankruptcy on September 15, and *ONSR* increased beyond 8% on September 17.

In sum, our analysis shows that, first of all, the incidence of naked shorting, even at its peak, was too low to justify the decline in price that took place. Second, our analysis indicates that naked shorting intensified *not before but after* the stock had lost most of its value and in reaction to negative news about the company, which is inconsistent with stock price manipulation. Again, we find no support for Hypothesis *H3*.

5.5.3. Merrill and AIG

We report the relationship between *ONSR* and the stock price for Merrill and AIG in Figure 6 and Figure 7 respectively. In both cases, the stock price declines were fairly gradual through the year and were not accompanied by any significant increase in naked shorting. For Merrill, *ONSR* reached its highest value of *only* 0.18% on October 14, 2008, well after the Merrill had lost most of its value. AIG had also lost about 40% of its market value by the end of August 2008, and the largest single-day price drop was on September 15, 2008 when Standard & Poor's cut AIG's credit rating. Following the announcement, the company's stock price dropped by about 60%. Yet, naked shorting remained low, and *ONSR* reached its highest value only a fortnight later on September 29, 2008, and even this highest value was *only* 0.32%. Given that naked shorting remained so extremely small all through the financial crisis period for both Merrill and AIG, we do not engage in any further statistical testing. There is clearly no evidence of naked-shorting linked manipulation, or any support for Hypothesis *H3*.

5.5.4. Naked short selling and credit rating downgrades

Naked short-sellers have been alleged to engage in (manipulative) naked shorting by creating conditions that trigger credit downgrades specifically to profit not just from the downward price spiral but also from linked credit default swap positions on the associated stock. In this context, we examine naked shorting around credit rating downgrades for a sample of the most affected financial securities. As our sample of financial firms, we use Bear Stearns and the 17 securities used earlier in section 3 for which the SEC had temporarily banned naked shorting in mid-2008, and for which data was available; but we exclude Lehman, as its credit rating downgrade was soon followed by a bankruptcy. For this sample of companies, we identify 21 long term issuer downgrades by S&P over the year 2008.

For each downgrade, we compute *ONSR* for the security of interest for 40 trading days preceding and following the announcement. We then compute average *ONSR* for each day in the event day calendar, where day 0 is the day of the downgrade. We estimate abnormal daily *ONSR* and its significance using a standard mean-adjusted event study methodology with an estimation window of 100 trading days ending 20 days prior to the credit rating downgrade. The results are reported in Table 18, Panel A.

If it were true that naked short-sellers were manipulatively creating conditions that triggered the downgrade, we would expect to find abnormally high naked shorting in the days preceding the credit rating downgrade. However, what we do find is the polar opposite: naked shorting is actually abnormally low in the days preceding the credit rating downgrade. Naked shorting becomes abnormally high only in the days following the downgrade. This abnormal naked shorting is sustained for approximately one month following the rating downgrade. This evidence is again consistent with naked short sellers *reacting* to negative news regarding the company, rather than engaging in naked shorting with a manipulative intent, and is hence inconsistent with our hypothesis *H3*.

5.5.5. Naked short selling and large price drops

Similar to our analysis of naked shorting around credit rating downgrades, we analyze whether naked shorting intensifies prior to large price drops. We start with a sample including all NYSE common stocks of US-based firms (CRSP share codes 10 and 11) included in the CRSP and TAQ databases over the interval January 1, 2008 to July 20, 2008, with no large changes (>10%) in the number of shares outstanding during the same period.[44] For each security, we standardize daily returns by subtracting the mean and dividing by the standard deviation. Our "event" days of large abnormal negative stock price returns are the 83 security-days with standardized returns smaller than -2.

For each security-day in the interval between day -20 and day +20 (where day 0 is the previously-identified day with the large negative abnormal return), we compute daily *Abnormal ONSR*, by subtracting mean *ONSR*, estimated over a split interval

[44] We aim at investigating the role of naked short sellers during the financial crisis, hence we focus on the year 2008. We restrict our analysis to the period ending July 20, 2008, as the SEC introduced various restrictions on both covered and naked shorting over the subsequent period, hence making it difficult to draw inferences regarding the relationship between naked shorting and stock returns. In unreported results, we find that including the subsequent period (until December 31, 2008) yields qualitatively similar results, with no evidence of intense naked shorting prior to the largest stock price declines.

containing the 50 trading days ending 21 trading days prior to the identified event date and the 50 trading days starting 21 trading days after the identified event date. We cumulate *Mean Abnormal ONSR* over various event-windows in the interval between day -20 and day +20 and test for significance using a Brown-Warner (1980, 1985) adjustment in the computation of standard errors, to account for the clustering of event dates. As indicated by the results presented in Table 18, Panel B, *Mean Cumulative Abnormal ONSR* is significantly negative at the 1% level over the 20, 10 and 5 day windows preceding the event date. *Mean Abnormal ONSR* is positive but statistically insignificant on the event date. In contrast, *Mean Cumulative Abnormal ONSR* is positive and statistically significant over the 5, 10 and 20 day intervals following the event. Once again, naked shorting does not precede large price drops but follows them.

To further investigate whether naked short sellers specifically target companies with a view to triggering credit rating downgrades, we investigate whether there is greater evidence of manipulative naked shorting among securities of highly levered firms, as those would be more vulnerable to credit downgrades. Accordingly, we obtain *Total Assets* and *Long Term Debt* as of the end of the fiscal year 2006 from the Compustat database and compute *Leverage* as the ratio of *Long Term Debt* to *Total Assets*. We then rank securities on *Leverage* and assign those with leverage below the sample median to a 'low leverage' group and those with leverage above the sample median to a 'high leverage' group. We then repeat our analysis on these two subsets and present results in Table 18, Panel B. The results for the two subsets do not differ qualitatively from those of the overall sample (except for a lack of significance of the abnormal return on the 20-day post-event window for the low leverage sample).

Overall, our results show that naked short sellers do *not* intensify their activity prior to the largest stock price declines, specifically hold also for the subset of high leverage firms, which are the ones that are most likely to be targeted by potential manipulators.

6. Conclusions

There has been intense regulatory and media concern about manipulative distortions associated with naked shorting even though naked shorts are functionally indistinguishable from covered shorts at the time of trade, and should arguably have the same beneficial impact on liquidity and pricing efficiency as documented for short-selling in general. In this paper, we empirically investigate the impact of the option to fail and the resultant naked shorting on market distortions, pricing efficiency and liquidity. Our focus is on aggregate naked shorting without trying to isolate "abusive" naked shorting since the intent to deceive, manipulate or defraud is very difficult to ascertain *ex-ante*, and since *ex ante* intention about going naked does not even need to definitively exist at the time of the short trade.

We first demonstrate empirically that the overwhelming majority of FTDs originate from naked short sales and not from human errors or processing delays. We accordingly construct and analyze an accurate FTD-based proxy for naked shorting. We find that the impact on market quality of naked shorting is very similar to that of covered shorting and, overall, beneficial for both pricing efficiency and liquidity. On average, naked short sellers function as liquidity providers reducing order imbalances and as value arbitrageurs who stabilize markets and reduce the mispricing of overvalued securities. An increase in naked shorting of 10 basis points of the number of outstanding shares leads to approximately a 1% reduction in spreads, a 2% reduction in order imbalances, a 10% reduction in the magnitude of positive pricing errors, a 13% decline in pricing error volatility, and a 1% reduction in stock price volatility. Further, we find that the beneficial impact of naked short sales on market-quality is not driven just by the naked-shorting of market makers, but even more strongly by the naked shorting by public traders. And, consistent with naked shorting having a beneficial impact on pricing efficiency and liquidity, we find that the SEC ban on naked shorting in select financial securities between July and August 2008 led to higher absolute pricing errors, higher spreads and lower trading volumes.

We analyze naked shorting in Bear Stearns, Lehman, Merrill and AIG around the days surrounding their dramatic declines in market value and find that abnormal naked shorting in these flagship victims of the 2008 financial crisis took place *after* and *not* before their major stock price declines and associated negative news; and hence their demise or other fate was not triggered by naked short sellers. We also analyze naked shorting around credit rating downgrades of financial firms in 2008, and again find that abnormal naked shorting takes place only in response to these downgrade announcements, and not prior to them. Similarly, we analyze naked shorting around the steepest stock price declines of financial firms during our 2008 financial crisis sample-period, and find yet again that abnormal naked shorting lags price declines, and that the direction of significant causality is from steep price falls to naked shorting, not vice-versa. Overall, our results indicate that, contrary to media and regulatory perceptions, naked short sellers did not precipitate the collapse of major financial firms in 2008, nor did they trigger credit rating downgrades or large stock price declines of financial firms.

Our findings are in sharp contrast with the extremely negative pre-conceptions that appear to exist among media commentators and market regulators in relation to the impact of naked short-sellers on market quality. They indicate that naked and covered short-sellers are not polar opposites in this context, and naked short-sellers are not necessarily the unadulterated villains in the marketplace. Our results have important implications for regulatory policy. While there clearly needs to be zero tolerance for individual cases of abusive naked shorting, we need to recognize that there is no evidence whatsoever that indicates that, in aggregate, naked short-sellers systematically and manipulatively precipitated price declines, or otherwise contributed adversely to any market distortions, even in the extreme situation of the 2008 financial crisis. Instead, the gently regulated naked shorting that existed after Regulation SHO up to mid-2008 was net beneficial for pricing efficiency and market liquidity, and this beneficial impact of naked shorting was similar to the well-accepted benefits of covered shorting. In this context, the rigid inflexible removal of the option to fail for the vast majority of market participants appears questionable, since the option to fail arguably reduces stock borrowing costs at the time when such costs are the highest, and thereby protects traders from the extreme lack of liquidity sometimes seen in the less regulated stock borrowing market. This option to fail, through naked shorting, is valuable for markets, also because it can prevent (potentially manipulative) distortions in the stock borrowing and lending market from getting transformed into serious pricing and liquidity distortions in the mainstream stock market. We believe that regulators can alternatively consider progressive fines for settlement delays rather than blanket removal of the option to fail; and should also significantly increase their focus on generating liquidity and transparency in the stock borrowing market.

Our empirical analysis is limited to NYSE stocks. We recognize that stock price manipulation, through naked shorting or other means, is more likely in less regulated markets populated by smaller capitalization firms. Possible directions for future research would be an examination of other markets – especially OTC markets – and an investigation of the intraday impact of naked shorting on market liquidity and pricing efficiency. In both cases, the challenge would lie in being able to find data that can enable construction of a good proxy for naked short sales within the day and across different markets.

Chapter 3: Is Hedging Bad News? Evidence from Corporate Hedging Announcements[45]

1. Introduction

Gold prices soared 8% yesterday, its biggest gain in four months, after Placer Dome Inc. said it was winding down its hedging position in expectation of an improved gold market…. Shares of Placer Dome (PDG/TSE) rose 24%, up $3.05 to $15.70. -- Reuters, Friday, 02/05/2000.

Despite a growing literature, researchers have failed to arrive at a consensus on whether corporate hedging creates value for shareholders.[46] Theory argues that it should, by mitigating the costs of financial distress, financial constraints, taxes, underinvestment, etc.[47] However, the empirical evidence in support of these hedging rationales is mixed.[48] Additionally, recent studies have documented that a majority of firms use derivatives not only to hedge their risk exposure but also to incorporate market timing into their hedging programs,[49] which suggests that these firms believe they have valuable private information. Yet, very little is known about the informational

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[45] This chapter is based on collaborative work with Chitru Fernando.

[46] See, for example, Allayannis and Weston (2001), Chidambaran, Fernando and Spindt (2001), Guay and Kothari (2003), Adam and Fernando (2006), Jin and Jorion (2006), and Mackay and Moeller (2007).

[47] See, for example, Smith and Stulz (1985), Bessembinder (1991), Froot, Scharfstein and Stein (1993), and Leland (1998).

[48] See, for example, Nance, Smith and Smithson (1993), Tufano (1996), Mian (1996), Géczy, Minton and Schrand (1997), Haushalter (2000) and Graham and Rogers (2002).

[49] See, for example, Dolde (1993), Stulz (1996), Bodnar, Hayt and Marston (1998), Glaum (2002), Faulkender (2005), Adam and Fernando (2006), Brown, Crabb and Haushalter (2006), Beber and Fabbri (2006), and Géczy, Minton and Schrand (2007).

superiority of firms, although casual empiricism as reflected in the above press report would suggest that markets do react strongly to firms' hedging announcements. However, the literature has failed to document any benefit to shareholders from such selective hedging activity,[50] suggesting that firms may not have as much private information as they think they do.

In this paper, we address these puzzles by studying the information content of hedging announcements, an approach that is new to the corporate risk management literature as far as we are aware but well established in other areas of corporate finance.[51] The hedging announcements in our study are made by a sample of gold mining companies. Our study permits us to re-examine three questions that are currently unresolved in the literature: (a) whether commodity firms have private information about the underlying commodity market; (b) whether changes in hedging policies also reveal private information about the firm and its industry; and (c) whether hedging decisions affect shareholder value. The answer to the first question would shed new light on the pervasive practice of selective hedging while the answers to the second and third questions would provide new evidence on the motives for corporate hedging and the extent to which it is consistent with theoretical rationales especially from the standpoint of maximizing shareholder wealth.

We hand-collect a sample of 153 announcements pertaining to changes in hedging policies made by gold mining companies between 1991 and 2008. Using this sample we test for the informational effects of hedging. First, we study the effect of

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[50] See Adam and Fernando (2006) and Brown, Crabb and Haushalter (2006).

[51] See, for example, Smith (1986), Eckbo and Masulis (1995) and Ritter (2003).

hedging announcements on the underlying commodity market. To the extent there is a reaction in the commodity market to hedging announcements, it could be taken as evidence that the market believes the firm is informed. Stulz (1996) argues that firms that are informed can enhance shareholder wealth by hedging selectively, i.e., by incorporating their market views into their hedging programs. Therefore, by examining the informational superiority of commodity firms, we can contribute to our understanding of the potential effects of selective hedging on shareholder wealth.

We find evidence to support the hypothesis that changes in hedging policies provide valuable gold price information to other market participants. Specifically, we find that announcements about decreases in hedging are associated with strong positive abnormal returns in the gold market, with increases in hedging eliciting a considerably weaker negative gold price response. We rule out the possibility that our results are due to the spot market impact of hedging changes on gold supply by cross-sectional analysis and also by studying the gold market reaction to 103 announcements of central bank gold sales during the same time period. We find that the gold market reaction to central bank announcements is neither statistically nor economically significant, despite average central bank gold sales exceeding average corporate hedging changes by a factor of three. As central bank announcements are unlikely to be driven by market timing objectives, we regard this finding as further evidence in support of the information hypothesis, i.e., that markets deduce private gold price information from the hedging announcements of gold mining firms. This conclusion is surprising in light of the evidence provided by Adam and Fernando (2006) and Brown, Crabb and

Haushalter (2006) that firms do not create any value for shareholders when they incorporate market timing practices into their hedging programs.

Second, we study the effect of hedging announcements on stock prices. A change in hedging policy could, all else equal, signal a change in the value of the firm as predicted by hedging theory. Specifically, keeping all else equal, an increase in hedging should increase stock prices and *vice versa*. However, if a firm changes its hedging policy in response to a change in its forecast of future gold prices, an increase in hedging could signal that the firm expects a deterioration in its financial condition due to a lower gold price forecast and we would expect to observe a decrease in the stock price, and *vice versa*. By examining the stock price reaction to firm-level information revealed by hedging policies, we add a new dimension to the debate on the effect of hedging on shareholder wealth.

We document several interesting results that strongly support our hypothesis that a change in hedging policy reveals private information about a change in the firm's financial condition. First, stock prices react negatively to an increase in hedging, and *vice versa*, after controlling for the fact that the gold price response to the announcement also affects gold company stocks directly. Second, firms with higher leverage react more negatively (positively) to increases (decreases) in hedging than lower levered firms. Third, changes in hedging policies by individual firms also cause an industry-wide contagion. Stock prices of other gold mining firms, after controlling for changes in gold prices, react negatively (positively) to announcements of increases (decreases) in hedging by individual firms.

A negative (positive) stock price reaction for firms that announce increases (decreases) in hedging could also arise in situations where managers and shareholders disagree about the firm's optimal hedging policy.[52] However, this possibility is effectively ruled out by our finding of a similarly strong industry-wide reaction to the announcement. While our firm and industry results are not inconsistent with shareholder value maximizing rationales for hedging, our findings show that any shareholder benefit of a hedging increase is more than offset by the negative news conveyed by the hedging increase about the change in the firm's prospects, and *vice versa*. Thus, our findings provide new insights into the endogeneity associated with hedging policy changes and the confounding effect of this endogeneity on measuring the relation between hedging and the value of the firm.

In the next section we discuss the relevant literature and develop our empirical hypotheses. In Section 3, we describe the data and discuss our empirical methodology. We present and discuss our empirical findings pertaining to the gold market impacts in Section 4. We present and discuss our empirical findings pertaining to the equity market impacts in Section 5. In Section 6, we examine whether the hedging changes following announcements by gold mining firms are consistent with the announcements. Section 7 concludes.

[52] See, for example, DeMarzo and Duffie (1995), Tufano (1996), Tufano (1998b), Knopf, Nam and Thornton (2002), and Kumar and Rabinovich (2010).

2. Empirical Hypotheses

2.1. Background

The extant literature provides numerous theoretical arguments in support of the notion that corporate hedging creates value for shareholders by mitigating market imperfections that cause departures from a Modigliani-Miller world. First, hedging can reduce firms' expected costs of financial distress (Smith and Stulz, 1985). This argument also suggests that hedging will help firms increase their debt capacity and the tax shields they can realize from debt (Leland, 1998; Graham and Rogers, 2002). Furthermore, by reducing the cost of financial distress, hedging can also enhance credit quality and reduce the cost of debt financing (Chidambaran, Fernando and Spindt, 2001). Second, when a firm faces a convex tax function, lowering the volatility of earnings by hedging can help reduce a firm's expected tax burden (Smith and Stulz, 1985; Graham and Smith, 1999). Finally, growth firms that find external financing to be more expensive than internally generated funds could employ hedging practices to reduce the underinvestment problem by ensuring that they have sufficient internal funds available to take advantage of attractive investment opportunities (Froot, Scharfstein, and Stein, 1993) and by reducing the cost to equity holders of financing these investment opportunities externally (Bessembinder, 1991).

Empirical studies that examine these theories provide mixed evidence. Nance, Smith and Smithson (1993) provide evidence suggesting that firms with more convex tax functions, more financial constraints and more growth opportunities hedge more. In contrast, Tufano (1996) finds little evidence in favor of shareholder value maximization theories, instead finding support for the idea that a firm's hedging practices are related to managerial incentives, specifically stock and option compensation. Géczy, Minton and Schrand (1997) find that firms with greater growth opportunities and firms with greater financial constraints are more likely to employ currency derivatives to hedge their foreign exchange exposure, Haushalter (2000) finds a positive relation between the extent to which a firm hedges and its financial leverage. Neither of the latter two studies find support for notion that hedging is tied to managerial utility or for the tax convexity hypothesis. Graham and Rogers (2002) provide evidence suggesting that although tax convexity does not seem to be a factor in hedging decisions, firms appear to be hedging to increase their debt capacity and thereby increase tax shields. They also find that firms hedge to reduce expected distress costs.

Recent studies have also examined the relation between firm value and hedging. Allayannis and Weston (2001) find that the market value of firms using foreign currency derivatives is 4.87% higher on average than for nonusers. Chidambaran, Fernando and Spindt (2001) show that hedging is associated with an enhancement in a firm's credit quality, thereby lowering financing costs. Graham and Rogers (2002) argue that derivatives-induced debt capacity increases firm value by 1.1% on average. Adam and Fernando (2006) find that their sample of gold mining firms consistently realized abnormal positive cash flows from their derivative transactions due to positive risk premia. They do not find a corresponding increase in systematic risk and hence infer that these derivative transactions increased shareholder value. Mackay and Moeller (2007) find that by hedging concave revenues and leaving concave costs exposed, their sample of 34 oil refiners could have increased their market values between 2% and 3%.

On the other hand, Guay and Kothari (2003) find that for most of their sample firms, the cash flow and market value sensitivities to their derivative portfolios are small relative to the magnitude of sensitivities to traditional measures of economic exposures. Jin and Jorion (2006) study 119 oil and gas companies and find that hedging does not affect market value of these companies although it does lower their stock price sensitivity to oil and gas prices.

Furthermore, recent studies have documented that many firms not only hedge but also speculate with derivatives by varying the size and timing of their derivatives transactions based on their market views, a practice known as “selective hedging.” For example, Dolde (1993) reports that almost 90% of firms in his survey of 244 Fortune 500 firms at least sometimes base the size of their hedges on their views of future market movements. Bodnar, Hayt and Marston (1998) survey derivatives policies by 399 U.S. non-financial firms and find that about 50% of their sample firms admit to sometimes (and 10% frequently) altering the size and/or the timing of a hedge based on their market views. Glaum (2002) surveys the risk management practices of the major non-financial firms in Germany and finds that the majority follows forecast-based, profit-oriented risk management strategies. These findings suggests that the practice of hedging departs considerably from the underlying assumption in the theoretical literature that firms use derivatives to reduce risk, not to speculate.

For selective hedging to be value increasing, Stulz (1996) argues that firms would need to possess private information about future market prices and the ability to act on this information without jeopardizing their core businesses. Adam and Fernando (2006) find considerable evidence of selective hedging in their sample of gold mining firms but find no economically significant cash flow gains on average from selective hedging. Brown, Crabb, and Haushalter (2006) also study selective hedging in the gold mining industry and arrive at a similar conclusion. While these findings do not rule out the possibility that some gold mining firms are privately informed about future gold prices,[53] they suggest that the firms that do engage in speculation are unlikely to be privately informed. Private information possessed by firms about future gold prices could potentially influence other aspects of their hedging behavior and the overall gold market as well.

2.2. Market reaction to hedging announcements

The literature reviewed in the previous subsection suggests that researchers have failed to arrive at a consensus on whether firms have private information about the underlying commodity market and whether hedging decisions affect shareholder value. In addition, the question of whether changes in hedging policies also reveal private information about the firm and its industry has not been previously addressed in the literature. In this subsection we develop a series of empirical hypotheses that are aimed at examining these questions based on how the gold and stock markets react to hedging announcements.

If a commodity firm announces a change in its hedging policy, it is possible that in doing so it will reveal (a) any private information it has about future gold prices and/or (b) private information about changes in the firm's financial condition that precipitated the hedging change, while also allowing for the possibility that a change in

[53] Indeed, Brown, Crabb and Haushalter (2006) report that of the 13 gold producers who responded to their survey, only two respondents believed that gold prices were "never predictable," also noting that the two most important factors determining the extent to which the companies in their survey hedged were "a long-term market view on gold prices" and a "short term market view on gold prices."

hedging policy could, all else equal, signal a change in the value of the firm as predicted by hedging theory. We develop our first set of hypotheses based on "(a)" to examine the gold price impact of the announcement and our second set of hypotheses based on "(b)" to examine the firm and industry impact of the announcement.

2.2.1. Announcement effect of hedging change on the gold price

If a commodity firm that has private information about the price of the commodity announces a change in its hedging policy and other market participants believe that the firm's information is of value, they can be expected to draw inferences about the firm's expectation of future commodity prices. Consider, for example, a commodity company that has just announced that it is closing out all its hedge positions. The market could construe this change in policy as a credible signal of the firm's greater confidence in the future prospects for the commodity. If so, we expect an announcement of an increase (decrease) in hedging by a commodity firm to be associated with a negative (positive) abnormal return in the corresponding commodity market.

However, the commodity market may also react to changes in hedging policies because hedging transactions can affect the supply in the gold markets and therefore the spot price of the commodity, and not necessarily because of the signaling power of the announcement.[54] For example, if a gold mining firm initiates a hedging program by selling gold forward, the counterparty to the transaction will typically hedge its exposure by borrowing gold and selling it in the spot market, thereby increasing the spot supply and depressing prices. While both the information hypothesis and the

[54] See Cross (2000) and Brown, Crabb and Haulhalter (2006) for further discussion on the potential market impact of hedging changes.

market impact hypothesis predict the same directional change in response to the announcement, any spot price reaction caused by a market supply impact will depend only on the quantity of gold involved and not on any other attribute of the hedging announcement or of the company making the announcement. Additionally, we can separate out information and market impact driven gold price movements by examining the gold sales of entities that are unlikely to be in the market for information reasons, in our case central banks that periodically rebalance their treasury portfolios by selling gold reserves.[55]

2.2.2. *Announcement effect of hedging change on equity prices*

In addition to revealing information about the firm's expectation of gold prices, a change in hedging policy could also signal either a move designed to increase (or decrease) the benefits of hedging predicted by hedging theory and/or a readjustment in the level of hedging in response to a change in the firm's financial condition.[56] Under the former argument, hedging theory would lead us to expect a stock price increase (decrease) if the firm increased (decreased) its hedging. Under the latter argument, an increase in hedging could signal that a firm's probability of financial distress has increased (thereby increasing the need for hedging) and we would expect to observe a decrease in the stock price (albeit partially mitigated by any benefits of an increase in hedging), whereas a decrease in hedging could signal that the firm's probability of financial distress has decreased (thereby decreasing the need for hedging) which would

[55] For example, Russia sold significant quantities of gold in the early 1990s to repay its international debt and the United Kingdom reduced its gold reserve by half between 1998 and 2004, in order to diversify and reduce risk.

[56] See Smith (1986) for a discussion of the latter argument in the context of capital structure changes.

lead us to expect an increase in the stock price (albeit partially suppressed by the adverse effects of a decrease in hedging).

Furthermore, if firms change their hedges to move closer to optimal hedge levels, firms with higher leverage will react more positively to an increase in hedging and more negatively to a decrease in hedging. In contrast, a market readjustment to firms' financial condition would predict that firms with higher leverage will react more negatively to an increase in hedging and more positively to a decrease in hedging.

A negative (positive) stock price reaction for firms that announce increases (decreases) in hedging could also arise in situations where managers and shareholders disagree about the firm's optimal hedging policy. DeMarzo and Duffie (1995) show how manager-shareholder conflicts can arise regarding a firm's optimal hedging policy in the context of information disclosure, while Tufano (1996) and Knopf, Nam and Thornton (2002) find evidence of such conflicts in the context of managerial compensation. Tufano (1998b) and Kumar and Rabinovich (2010) argue that in the presence of agency conflicts between managers and shareholders, cash-flow hedging strategies can be used to reduce shareholder wealth, since they remove the valuable discipline that obtaining new external financing imposes on managers.

However, any stock price reaction due to manager-shareholder agency conflict will be confined only to the firm making the announcement. In contrast, to the extent that changes in hedging policy by a firm due to expected gold price changes also implies a change in the probability of financial distress due to the gold price change, we should expect to find industry wide contagion effects of hedging announcements over and above the direct effect of changes in gold prices on the firm making the announcement.[57] Furthermore, unlike the agency conflict hypothesis, the information hypothesis also implies that announcements that are identified as solely being motivated by the companies' view of market prices should cause a greater reaction in the commodity and equity markets.

3. Data and Methodology

We use a *Factiva* guided search to hand-collect announcements made by individual firms related to changes in their hedging policies.[58] These announcements include hedging program initiations, closures, and changes, between January 1991 and February 2008. Furthermore, we search for firm-specific news in the Factiva database and categorize hedging announcement events as *contaminated* when the related company has other news/events in the interval between days -1 and +1 and as *uncontaminated* otherwise. We obtain 153 hedging announcements made by 26 different gold mining firms during our sample period. In addition to the date of the announcement, we also record data on the announced change in the quantity of hedging, reason for the change in hedging policy and whether the change is to be implemented or has already been implemented. An event is categorized as *Market View* when the firm making the hedging announcement explicitly states that the change in hedging policy is a result of its expectations about future gold prices. Beatty, Chen and Zhang (2008) find that changes in corporate hedging policies that are consequences of debt covenants are

[57] Jorion and Zhang (2007) find that credit events, such as Chapter 11 and 7 bankruptcies and large jumps in credit default swap spreads, are associated with industry wide contagion effects rather than competition effects.

[58] We include numerous sources such as Reuters news, PR Newswire, Business Wire, Dow-Jones Newswires, Wall Street Journal and Major English Dailies in the United States. We search for stories which include any word starting with "Hedg" in the headline or in the first 200 words.

mostly devoid of market timing intentions. Accordingly, we segregate hedging policy changes that firms attribute to loan related transactions and categorize them as *Bank Loans.* We further verify hedging transactions that may be driven by debt covenants by studying 10-K filings and annual reports to verify the classification. The remaining events are categorized as *Others*. Events are categorized as *Ex ante* (*Ex post*) when the change in hedging policy is announced before (after) its implementation.

Table 19 provides further details of the sample of hedging announcements. Announcements about decreases in hedging significantly outnumber announcements about hedging increases. Stricter disclosure and accounting regulations such as the Statement of Financial Accounting Standards No. 133 (SFAS 133) were implemented between 1999 and 2001. These regulations were aimed at providing shareholders with a greater clarity on the use of derivative instruments in corporate risk management. Accordingly, we find more announcements in the post-2000 period.

We obtain financial data from Compustat. Stock market return data is obtained from the CRSP database. Gold production figures are obtained from firms' financial statements. NYMEX near-month contract prices are used as a proxy for daily gold prices.[59] We obtain data on gold prices from Bloomberg. We estimate *Firm Size, Leverage (Distress), Quick Ratio* and *Tax Savings* in accordance with Tufano (1996). *Information Asymmetry* is calculated as the percentage error in analyst's forecasts of earnings. We calculate *Hedge Proportion* as the ratio of change in quantity of hedging to the following year's total gold production. Summary statistics of these variables are

[59] Although it can be argued that futures prices are more appropriate for testing the information hypothesis, it must be noted that the spot-future arbitrage relationship ensures that changes in spot prices reflect changes in the entire term-structure of gold prices. Furthermore, lack of data on the maturity of forward contracts associated with our hedging announcements makes using futures prices impractical.

presented in Table 20. As seen from Table 20, hedging policies are not trivial corporate decisions. As a consequence of a change in hedging policy, derivative positions can be changed on as much as 87.5% of next year's gold production. The scale of the impact of changes in hedging policies adds further credence to the notion that these policy changes must be a consequence of changes in expectations of future cash flows and hence, should convey significant incremental information to the market about firm value.

We test our empirical hypotheses using standard event study methodologies (Brown and Warner, 1985) and OLS regressions. To estimate the abnormal returns in the commodity market, we employ a mean-adjusted methodology. The mean or expected return is calculated based on returns from day -110 to -10, where the day of the hedging announcement is identified as day zero. Since we are able to accurately establish the timing of the hedging announcements, we use a one-day event window (day zero) for the study.

As shown by Tufano (1998a), gold mining firms have a significant exposure to gold price risk. Also, Tufano (1998a) documents that other variables such as interest rates and exchange rates do not enter as significant factors in the gold mining firm market model. Accordingly, we employ a two-factor model to measure the abnormal returns in the equity market. The expected return model for firm *i* on day *t* is shown below:

$$R_{i,t} = \alpha_i + \beta_{i,Mkt} R_{Mkt,t} + \beta_{i,Gold} R_{Gold,t} + \varepsilon_{i,t} \qquad (1)$$

where $R_{i,t}$ is the total daily return on stock *i* from *t*-1 to *t*, $R_{Mkt,t}$ is the daily return on CRSP value-weighted index, and $R_{Gold,t}$ is the return on the near-month gold contract traded on the NYMEX.[60] For robustness, we also employ a five-factor model that augments the two-factor model with the Fama-French and momentum factors. The five-factor model is as follows:

$$R_{i,t} = \alpha_i + \beta_{i,Mkt} R_{Mkt,t} + \beta_{i,Gold} R_{Gold,t} + s_i SMB_t + h_i HMB_t + u_i UMD_t + \varepsilon_{i,t} \tag{2}$$

where SMB_t, HML_t, and UMD_t are the returns to the Small-Minus-Big, High-Minus-Low, and Up-Minus-Down portfolios meant to capture size, book-to-market, and return momentum effects, respectively.[61]

We employ an estimation period of 255 days ending 45 days prior to the event date. The day of the hedging announcement is identified as day zero. For reasons mentioned earlier, we use a one-day event window.

Since some of our hedging announcements are clustered in calendar time, it is imperative to control for the cross-sectional correlation in abnormal returns. To this effect, we compute t-statistics using the "crude dependence adjustment" (CDA)[62] of Brown and Warner (1980, 1985). This methodology is robust to cross correlation of event day abnormal returns because the standard error used in the statistic is based on the time-series standard deviation of average (portfolio) excess returns from the

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[60] We only use daily spot rates and do not consider daily lease rates when calculating the daily gold return. Tufano (1998a) reports that gold betas calculated using only spot gold prices are statistically equivalent to those calculated by considering both spot price returns and daily lease rates.

[61] The daily factor returns for the SMB, HML, and UMD portfolios are generously provided on Ken French's website.

[62] For examples of other applications see Campbell, Cowan and Salotti (2010) , Kothari and Warner (2007) and Rau and Vermaelen (1998).

estimation period. Furthermore, as a test of the median CAR, we also report a non-parametric statistic -- Generalized sign Z (Cowan, 1992).[63]

We use unsigned event-day CARs for all cross-sectional regressions:

$$\text{UnSigned CAR}_i = \text{CAR}_i * \text{Direction}$$
$$\text{Where Direction} = \begin{Bmatrix} 1 \text{ for Hedging Increases} \\ -1 \text{ for Hedging Decrease} \end{Bmatrix} \tag{3}$$

Employing unsigned event-day CARs alleviates some of the problems associated with using a smaller sample by enabling us to combine observations from the hedging increase and decrease samples into a single cross-sectional regression, especially since we have no reason to expect the relation between CARs and other firm and event variables to be conditional on the direction of change in hedging policies.

4. Announcement Effects in the Gold Market

4.1. Gold returns and hedging announcements

We first examine the effect of hedging announcements on abnormal returns in the gold market. Results for the event studies of hedging decreases and increases are presented in Table 21.

Panel A of Table 21 indicates that the mean abnormal gold return for the event day in response to a hedging decrease is positive and significantly different from zero at the 1% level. The gold CARs of 0.5% and 0.6% on the event day and 3-day interval

[63] For examples of other applications see Harvey, Lins and Roper (2004), Karpoff, Lee and Vendrzyk (1999) and Singh (1997).

around it, respectively, are also economically significant. There is some evidence of a gold price run-up prior to the announcement and no evidence of a reversal of the announcement return in the ten days following the event.

Panel B of Table 21 reports the event study results for announcements of increases in hedging. Although the abnormal return associated with hedging increases is negative on the announcement date, it is not statistically different from zero. However, the cumulative abnormal returns between days +1 and +5 (-0.3%) and days +1 and +10 (-1.4%) are negative and significantly different from zero based on the Generalized Sign Z and CDA test statistics, respectively. This evidence indicates that announcements about hedging increases have a weak negative impact on the gold market. The lack of statistical significance for the event-day abnormal return may be an artifact of the smaller sample size for hedging increases (35) compared to hedging decreases (117) or because the gold market interprets hedging increases differently from hedging decreases.[64]

The market impact hypothesis also predicts that announcements regarding increases (decreases) in hedging will be associated with decreases (increases) in gold abnormal returns. We conduct two tests to distinguish between the information and market impact hypotheses: a cross-sectional regression of unsigned abnormal returns and a comparison of the impact of announcements made by commodity companies with the impact of announcements made by central banks.

4.1.1. Cross-Sectional regressions of unsigned gold abnormal returns

[64] As we show later, we observe strong stock price reactions for the firm and for the gold mining industry to both decreases and increases in hedging.

We use the combined (decrease and increase) hedging sample to examine the relationship between abnormal returns in the gold market, and different attributes of the event and the firm making the announcement. The overall idea is that if the market impact hypothesis is to prevail, the abnormal returns should be related only to the expected change in quantity of hedging and not to any other attribute of the event or the firm. The results of the regressions are presented in Table 22. It is important to note that the dummy variable *Market View* is consistently significant and negative across different specifications. This implies that the reaction in the gold market is much stronger when the firm making the announcement explicitly claims that the change in hedging policy is a result of its expectation about future gold prices. The use of the *Market View* dummy helps us differentiate between the information and the market impact hypotheses, and our findings strongly support the former. While a negative coefficient on the hedging quantity change variable could be interpreted as evidence in support of either the market impact or the information hypothesis (the latter because bigger adjustments in private information may be associated with larger hedging changes), we find that the hedging quantity change coefficient is not statistically significant.

To the extent that the proportion of change in hedging can be considered as a proxy for the strength of the signal, the information hypothesis would predict a negative sign on the variable. The results indicate that although the variable has a negative sign it is not statistically different from zero. We employ a dummy variable *Big* Five to proxy for firm size. The dummy variable is equal to 1 when the firm making the announcement is one of the five largest firms in the sample. As argued by Stulz (1996) and Brown, Crabb and Haushalter (2006), large firms are more likely to acquire valuable private information about the gold market. Although firm size (*Big Five*) is negatively related to abnormal returns, it is not statistically significant in any of the specifications. The *Inc_Dec* dummy variable that indicates whether the announcement pertained to a hedging increase or decrease is also insignificant. This implies that, all else equal, announcements about hedging increases and hedging decreases have impacts of similar magnitude on gold returns. This observation adds credence to our previous suggestion that the lack of statistical significance for event-day abnormal gold returns in response to hedging increases is a consequence of our small sample. An insignificant *Ex post* dummy implies that the gold market reacts similarly to announcements made prior to and following the execution of the hedging policy change, which may be indicative of the opaqueness of the gold market. In one of our three specifications, the 20-day historical gold CAR, *Car_Pre_Gold* is weakly negatively related to announcement returns in the gold market. Finally, the contamination of hedging announcements by other announcements does not have a significant effect on the abnormal returns in the gold market.

4.2. Gold market reaction to central bank gold sales

In this subsection, we examine how the gold spot market reacts to announcements of central bank gold sales to further separate out information driven and market supply impact driven gold price movements. As we have noted, central bank gold sales are unlikely to be motivated by private information about future gold prices, which is confirmed by the reasons given in the announcements. Moreover, the fact that the only gold transactions by central banks during our 18-year sample period are gold sales also suggests the absence of market timing.

We hand-collect central bank announcements related to the sale or purchase of gold reserves using a *Factiva* guided search. We find 103 central bank gold sale announcements between January 1991 and February 2008.[65] Our sample includes one announcement by the European Central Bank and one announcement by the International Monetary Fund. Panel A of Table 23 provides further details on the central bank announcement sample. Although 54% of the observations are from the Canadian Central Bank, we have verified in unreported work that the gold market reactions to Canadian announcements are not significantly different from reactions to the rest of the announcements. We carry out an event study of the gold market reaction to central bank gold sales and the results are presented in Panel B of Table 23.

The event day CAR of -0.1% is neither statistically not economically significant, despite average central bank gold sales exceeding average corporate hedging changes by a factor of three. This finding stands in stark contrast to our finding for the gold market impact of corporate hedging announcements (hedging decreases in particular) and we regard this finding as further evidence against the market impact hypothesis and in support of the information hypothesis, i.e., that markets deduce private gold price information from the hedging announcements of gold mining firms.

To summarize, our findings in this section provide strong evidence in favor of the information hypothesis, which implies that gold market participants credibly infer a firm's change in expectation of future gold prices from hedging decreases. While our

[65] We find no central bank gold purchase announcements during our study period.

findings in support of the information hypothesis for hedging increases are considerably weaker, this could be due either to our smaller sample or to an asymmetry in the ways the gold market deduces hedging decreases and increases. Our findings in support of the information hypothesis are further strengthened by the insignificant gold market price reaction to central bank gold sales, which provides no support for the alternative hypothesis that the observed price response is due to the spot market supply impact of hedging changes.

5. Announcement Effects in the Equity Market

5.1. Equity returns and hedging announcements

We next examine the effect of hedging announcements on the equity market and analyze the relation between abnormal returns in the equity market and characteristics of the firm making the announcement and of the announcement itself.

Table 24 presents results from event studies on hedging decreases and increases, respectively. In Panels A and C, abnormal returns are calculated using a two-factor model to control for daily changes in gold prices and hence, cannot be attributed to any information related to gold prices. As reported in Panel A of Table 24, the mean abnormal return on the event day in response to a hedging decrease is +1.49%, which is highly significant both economically and statistically (at the 1% level). Further, there is no evidence of mean reversion in abnormal returns. As reported in Panel C of Table 24, the mean abnormal return on the event day in response to a hedging increase is negative (-2.05%) and highly significant. Similar to the hedging decreases sample, there is no evidence of mean reversion in abnormal returns. As reported in Panels B and D, the results are substantively identical when using a five-factor model that includes the gold factor together with the Fama-French and momentum factors.[66] In sum, increases (decreases) in hedging result in strong negative (positive) abnormal returns that are statistically and economically significant. These results are in accordance with the hypothesis that firms change their hedging policy in response to a change in their financial condition (especially their cost of financial distress), perhaps tied to their new gold price forecast. Our finding suggests that changes in hedging policies reveal hitherto private information about the firm's financial condition and hence signal changes in firm value.

5.1.1. Cross-Sectional regressions of unsigned equity abnormal returns

Next, we examine the relationship between equity abnormal returns, and firm and event characteristics. The results of the cross-sectional regression are presented in Tables 25 and 26. Unsigned event-day abnormal return is the dependent variable in all the specifications, and in all our analysis we control for pre-announcement abnormal returns in both the gold and equity markets. In Table 25 the abnormal returns are obtained from the two-factor model and in Table 26 they are obtained from the five-factor model. Tables 25 and 26 report several noteworthy results, and the results in the two tables are substantively identical.

First, the *Market View* variable is again negative and significant in all the models. This implies that the greater the content of information regarding future gold prices, the stronger is the reaction in the equity market. This finding is not predicted by

[66] A change in hedging policy by itself can change a firm's gold beta. In such a case, abnormal returns obtained from gold betas estimated using pre-event data might be inaccurate. To address this concern, we re-run our event studies using gold betas estimated from post-event data. In unreported results, we find that the event-day reactions are substantively similar to those reported here.

the market impact and the agency conflict hypotheses, and provides strong support for the information hypothesis. In contrast, the size of the hedging change is not significant in any of the specifications.

Second, leverage (*Distress*) is negatively related to unsigned abnormal returns in all the specifications, which implies that highly levered firms react more negatively (positively) to increases (decreases) in hedging than other firms. This finding is robust to controlling for information asymmetry (*Info_Assy*), measured by the percentage error in analysts' annual earnings forecasts. Smith and Stulz (1985) argue that firms with more leverage are expected to derive greater benefits from hedging than their counterparts with less leverage. Our finding suggests that the signal about the change in a firm's financial condition implied by the hedging change is amplified by leverage despite more levered firms deriving higher benefits from hedging.

While information asymmetry (*Info_Assy*) is not statistically significant, we find weak evidence that larger firms react less negatively (positively) than smaller firms to hedging increases (decreases). We find no evidence that firms with more tax savings (*tax savings*, measured by the total tax loss carry forward to proxy for the convexity of the firm's tax schedule as in Tufano (1996)) react differently to hedging changes than firms with more tax savings.

In all our specifications, *Car_Pre_Gold* is strongly positively related to the firm's announcement return, suggesting that the equity market reacts less negatively (positively) to hedging increases (decreases) when the 20-day historical gold CAR is positive. We find somewhat weaker evidence suggesting that the firm's stock price also reacts less negatively (positively) to hedging increases (decreases) when the 20-day historical equity CAR for the firm making the announcement, *Car_Pre_Firm*, is positive. These findings suggest that conditions in the gold market and the equity market for the firm making the announcement can either moderate or exacerbate the equity announcement effect.

5.2. Industry effects of hedging announcements

Finally, we examine the industry-wide contagion effects of hedging announcements made by commodity firms by studying the impact of the announcement on all firms in the gold mining industry (SIC = 1041) excluding the firm making the announcement. The results for industry effects of hedging decreases and increases are presented in Table 27. In line with the industry contagion hypothesis, we find that hedging increases (decreases) by individual firms are associated with strong and highly significant negative (positive) abnormal returns for the rest of the industry. Furthermore, there is no evidence of mean reversion in abnormal returns in either of the cases. As in our previous analysis for the announcing firms, it should also be noted that because abnormal returns are calculated using both two- and five-factor models that account for daily changes in gold prices, these reported contagion effects cannot be attributed to changes in gold prices. This evidence adds further credence to the argument that changes in hedging policies convey information about changes in the expected financial condition of commodity firms, associated with the revised gold price forecast implied by the hedging announcement.

Next, we examine the relationship between day 0 abnormal returns experienced by gold mining firms (excluding the firm making the announcement), and their characteristics and event attributes. The results of this cross-sectional analysis are presented in Table 28. Unsigned event-day abnormal return obtained from the two-factor model is the dependent variable in all the specifications.[67] Although the industry-wide cross-sectional regression results are weaker than those obtained for firms making the hedging announcements, they are still largely consistent with the readjustment to financial condition hypothesis. Specifically, we find that leverage (*Distress*) is negative and significant in three of the four model specifications. As in the case of individual firms, we find that the leverage result holds even after controlling for the level of information asymmetry (*Info*_Asy). The *Market View* variable is negative and statistically significant in two of our four model specifications. Moreover, unlike for individual firms, we find weak evidence that the proportion of change in hedging, which can be considered as a proxy for the strength of the announcing firm's information, is also statistically significant at the 10% level. The industry-wide effects of the 20-day historical performance in the gold and equity markets are essentially the same as the previously documented effects of these variables on the firm making the announcement. In all our specifications, *Car_Pre_Gold* is strongly positively related to announcement returns in the gold industry sample (excluding the firm making the announcement), suggesting that the gold industry sample firms also react less negatively (positively) to hedging increases (decreases) when the 20-day historical gold CAR is positive. As before, we find somewhat weaker evidence suggesting that the gold industry sample firms react less negatively (positively) to hedging increases (decreases) when the 20-day historical equity CAR for the gold industry sample firms (minus the firm making the announcement), *Car_Pre_Industry*, is positive.

[67] In unreported results, we find substantively identical results when the unsigned abnormal returns are obtained from the five-factor model.

To summarize, our findings in this section provide strong evidence in favor of the hypothesis that changes in hedging policies reveal private information to the market about a change in the expected cost of financial distress of the firm and its industry, presumably arising from the revision in the gold price forecast. While a similar stock price reaction for firms that announce hedging changes also could arise in situations where managers and shareholders disagree about the firm's optimal hedging policy, this possibility is effectively ruled out by our finding of a strong industry-wide reaction to the announcement. While our firm and industry results are not inconsistent with shareholder value maximizing rationales for hedging, our findings show that any shareholder benefit of a hedging increase is more than offset by the negative news conveyed by the hedging increase about the change in the firm's prospects, and *vice versa*.

6. Are Hedging Changes Consistent with Announcements?

The market reactions we document in the previous sections suggest that the hedging announcements are credible in the sense that the market believes firms put their money where their mouth is by following through on their announced hedging changes. In this section, we examine the validity of this premise. The data on hedge positions of gold mining firms is obtained from quarterly reports compiled by the VM Group[68] between the second quarter of 2002 and the first quarter of 2008. The quarterly reports provide information on all outstanding gold derivatives positions, their size, maturities, and the respective delivery prices for each instrument. The derivatives positions are

[68] Publicly available on their website http://www.virtualmetals.co.uk/index.php?inc=products&id=cp8.

composed of forward instruments (forwards, spot-deferred contracts, and gold loans) and options (put and call). Tufano (1996) and Adam and Fernando (2006) employ an earlier version of the data that we use.

We use the *Total Committed Hedge* measure – the theoretical maximum a producer might have to sell due to derivative positions – to track the changes in a firm's hedge book. Since our data on hedging positions starts only in the second quarter of 2002, we are unable to conduct the analysis for hedging announcements prior to Q2 of 2002. Also, since all the announcements after Q2 of 2002 are hedging decreases, we are able to conduct this analysis only for announcements of hedging decreases. In total, we are able to obtain data on changes in hedging positions for 34 announcements. The results are presented in Table 29. We find that *Total Committed Hedge* as a proportion of yearly production drops by 10.42% in one quarter and 36.52% in one year after the announcement of a hedging decrease. Both these changes are statistically significant at the 1% level. This result helps to confirm the credibility of corporate hedging announcements and is consistent with our finding that the gold and equity markets react to hedging announcements. Gold mining firms follow up their announcements with actions that are consistent with the announcements, thereby reinforcing the credibility of their announcements as signals of changes in expectations of future gold prices and bankruptcy risks.

7. Conclusions

We examine the informational effects of hedging changes on gold prices and shareholder wealth. We use a hand-collected sample of 153 announcements related to changes in hedging policies made by gold mining companies between 1991 and 2008. We hypothesize that changes in hedging policies provide incremental information to other market participants about changes in expected gold prices and corresponding changes in a firm's expected financial condition. We find strong evidence in favor of these hypotheses. Specifically, we find that announcements about decreases in hedging are associated with strong positive abnormal returns in the gold market, with increases in hedging eliciting a considerably weaker negative gold price response. To differentiate between the market impact hypothesis – that hedging announcements move spot market prices due to the implied change in market supply – and the information hypothesis, we examine the relation between gold market abnormal returns and event characteristics. We find that gold market reactions are stronger when firms change their hedging policies because of their stated view of the future market price of gold. Also, we find that the gold market reacts more strongly to announcements made by gold mining companies than to announcements made by central banks. Indeed, the market reaction to central bank announcements is neither statistically nor economically significant despite the much larger size of average central bank sales. As central bank announcements are unlikely to be driven by market timing objectives, we regard this as further evidence in support of the information hypothesis.

Further, we argue that changes in hedging policies reveal private information about changes in a firm's expected financial condition (expected costs of financial distress), presumably associated with the revised gold price forecast imputed by the market from the hedging change. In support of this hypothesis we find that, after controlling for gold market returns, announcements of increases (decreases) in hedging are associated with negative (positive) abnormal market returns in the corresponding firm's equity, and we also find similar industry-wide contagion effects of hedging announcements.

In summary, we add to the literature on corporate risk management by being the first to systematically study the informativeness of corporate hedging announcements and by showing that hedging announcements reveal private information about both the underlying commodity market and the expected financial condition of firms and the industry. Our findings on the gold market reaction to hedging changes is consistent with the market believing that firms have credible private information about future gold prices. This is puzzling in light of extant evidence that firms cannot successfully time the market when they hedge, and points to the need for further research to address the inconsistency between our findings and previous studies. Our findings also shed valuable new insights on the endogeneity associated with hedging policy changes. Firms increase hedging in anticipation of adverse future conditions and decrease hedging in anticipation of favorable future conditions. Moreover, their hedging announcements convey this information to the market, thereby confounding the measurement of the effect of hedging on the value of the firm. Consequently, our study highlights the need to better control for this endogeneity in studies that examine the relation between hedging and the value of the firm. Since hedging practices are widespread across many industries and economic segments, the implications of our study extend well beyond the gold industry.

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Appendix A: Tables

Table 1. Variable Definition, Chapter 1

Variable	Description
	Trader Analysis
Modification Ratio	Ratio of total number of order modifications (positive and negative) and order submissions. It is calculated for each trader, across all stocks, and through all the trading days in the sample.
Cancellation Ratio	Ratio of total number of order cancellations and order submissions. It is calculated for each trader *i*, across all stocks, and through all the trading days in the sample.
Revision Ratio	Sum of *Cancellation Ratio* and *Modification Ratio*.
Closing Ratio	Average of the ratio of a trader's daily closing position and his daily total trading volume. It is calculated for each trader, first for all trading days in each stock, and then averaged across all stocks.
Trader Frequency	Average number of times a trader trades in a day through the sample. It is calculated for each trader, first by stock, and then averaged across all stocks in the sample.
Trader Size	Average size of trades placed by a trader through the sample. It is calculated for each trader, first by stock, and then averaged across all stocks in the sample.
Network Trading Ratio	Percentage number of times a trader has multiple orders on both sides of the book in one minute snapshots of a stock's order book. It is calculated for each trader, first by stock, and then averaged across all stocks in the sample.
	Hazard Analysis
Order Size	Natural logarithm of the product of the total quantity and price of the order.
Spreads	Ratio of the difference between the best buy and sell prices and the midquote prevailing 5 seconds before order submission.
Lagged Volatility	Absolute value of returns over the five minutes leading to order submission.
$P^{Relative}$	For a buy order, it is the difference between the limit price and best bid prevailing at the time of order submission, expressed as a percentage of the latter; it is analogously defined for a sell order.
Lagged Volume	Natural logarithm of the total trading volume over the five minutes leading to order submission.
LPR	Natural logarithm of the average price of the stock over the entire sample period.
Dealer	Binary variable equal to 1 when the trader is identified as a member of the exchange in the dataset.
Individual	Binary variable equal to 1 when the trader is identified as an individual trader in the dataset.

Hazard Analysis

Δq_t^{same}	For a buy order, it is the change in the best bid between time t and an instant after order submission, expressed as a percentage of the latter; it analogously defined for a sell order.
$\Delta q_t^{Opposite}$	For a buy order, it is the change in the best ask between time t and an instant after order submission, expressed as a percentage of the latter; it analogously defined for a sell order.
$\Delta Inventory\_Stock_t$	Natural logarithm of the change in a trader's net inventory over the period (*t*-5secs, *t*]. Net inventory is defined as the difference in buy side and sell side inventories.
$\Delta Inventory\_Related_t$	Natural logarithm of the change in a trader's net inventory in stocks belonging to the same industry (2 digit SIC) as the concerned stock, over the period (*t*-5secs, *t*]. Net inventory is defined as the difference in buy side and sell side inventories.
$\Delta Inventory\_Industry_t$	Sum of $\Delta Inventory\_Stock_t$ and $\Delta Inventory\_Related_t$.
$\Delta Inventory\_Unrelated_t$	Natural logarithm of the change in a trader's net inventory in stocks not belonging to the same industry (2 digit SIC) as the concerned stock, over the period (*t*-5secs, *t*]. Net inventory is defined as the difference in buy side and sell side inventories.

Panel Regressions

Total Revisions^P	Value weighted average of the number of revisions trader *i* employed on the orders he placed in stock *s* on day *t*.
Price Agg^P	Value weighted average of the price aggressiveness of the orders trader *i* placed in stock *s* on day *t*. Price aggressiveness, for a buy order, is the difference between the limit price and best bid prevailing at the time of order submission, expressed as a percentage of the latter; it is analogously defined for a sell order.
Past Volatility^P	Value weighted average of the volatility of returns prevailing 1 hour prior to the submission of trader *i*'s orders in stock *s* on day *t*.
Log Quantity^P	Natural logarithm of the value weighted average of the total quoted quantity of the orders submitted by trader *i* in stock *s* on day *t*.
Buy^P	Value weighted proportion of buy orders submitted by trader *i* in stock *s* on day *t*.
Hidden^P	Value weighted proportion of hidden orders submitted by trader *i* in stock *s* on day *t*.
Past Trading Frequency^P	Value weighted average of the number of trades prevailing 1 hour prior to the submission of trader *i*'s orders in stock *s* on day *t*.
Number of Orders^P	Natural logarithm of the number of orders submitted by trader *i* in stock *s* on day *t*.
Ex post Ratio^P	Value weighted ex post performance of all orders submitted by trader *i* in stock *s* on day *t*. Ex post performance, for a buy order, is calculated as the ratio of the difference between the best bid 60 mins after execution and the execution price of the order, and the price of the stock an instant before order submission; it is zero for unexecuted orders.

Panel Regressions

Price Impact Ratio^P	Value weighted price impact ratio of all orders submitted by trader *i* in stock *s* on day *t*. Price impact ratio, for a buy order, is calculated as the ratio of the difference between the midquote at the time of order submission and the execution price of the order, and the price of the stock an instant before order submission; it is zero for unexecuted orders.
Opportunity Cost Ratio^P	Value weighted opportunity cost ratio of all orders submitted by trader *i* in stock *s* on day *t*. Opportunity cost ratio, for a buy order, is calculated as the ratio of the difference between the closing price of the stock and the midquote at the time of order submission, and the price of the stock an instant before order submission; it is zero for fully executed orders.
Total Cost Ratio^P	Value weighted total cost ratio of all orders submitted by trader *i* in stock *s* on day *t*. Total cost for each order is calculated as weighted sum of the difference between price impact and ex post performance , and opportunity cost, where the weights are volume of the order executed and unexecuted, respectively. Total cost ratio is the ratio of Total Cost and the price of the stock an instant before order submission.
Earnings Day	Binary variable equal to 1 when stock *s* has an earnings announcement on day *t*.

Table 2. Characteristics of Sample Stocks

This table presents trading characteristics of the 50 stocks that make up the Standard & Poor's CNS Nifty index at the National Stock Exchange (NSE), India. The various characteristics are calculated for each of the 50 stocks using the entire sample data of 56 trading days — April to June, 2006. These Nifty constituent stocks cover 21 sectors of the economy including, and represent about 60% of market capitalization on the NSE.

	Mean	Median	Max	Min	Q1	Q3
Market Capitalization (USD Billions)	7	4	38	1	3	7
Daily Turnover per stock (USD Millions)	21	13	159	1	6	25
Effective Spread in basis points	3	3	8	2	3	4
Daily Number of Trades per stock	19,121	12,710	70,129	2,870	6,597	24,390
Daily Order Submissions per stock	24,907	18,334	94,355	4,210	9,142	35,345
Number of Order Cancellations (% of Total Number of Limit Orders)	24.24%	25.04%	30.17%	17.10%	21.61%	27.10%
Volume of Order Cancellations (% of Total Volume of Limit Orders)	44.83%	46.35%	58.93%	22.05%	41.91%	50.12%
Number of Order Modifications (% of Total Number of Limit Orders)	15.51%	15.28%	20.62%	12.73%	14.37%	16.37%
Volume of Order Modifications (% of Total Volume of Limit Orders)	26.29%	27.19%	35.48%	18.26%	22.72%	29.65%
Number of Order Revisions (% of Total Number of Limit Orders)	35.71%	36.13%	41.49%	30.18%	32.79%	37.84%
Volume of Order Revisions (% of Total Volume of Limit Orders)	61.30%	62.62%	69.47%	42.97%	58.99%	65.10%

Table 3.Trader Categories

This table describes the different trader categories identified in the data. Their share of total limit order volume submitted in the sample, and the proportions of their limit order volume that are cancelled, modified and revised (cancelled or modified) are also presented. The proprietary data from the NSE identifies 14 different trader clienteles, which are further classified into 4 broader categories: Individuals, Financial Institutions, Dealers and Other Institutions.

Trader Category	Description	Number of Traders	Percentage of Total Limit Order Volume Submitted	Percentage of Limit Order Volume		
				Cancelled	Modified	Revised
Individuals	Individual Non-Residential Indians HUF (Families)	1,070,125	32.18%	32.33%	22.68%	49.12%
Financial Institutions	Mutual Fund Bank Insurance Other Domestic Financial Institutions Foreign Financial Institutions	5,771	16.45%	10.00%	34.06%	41.96%
Dealers	Exchange Members	509	40.68%	67.94%	16.32%	73.84%
Others Institutions	Public and Private companies Partnership Firms Trusts and Societies Other Corporate Bodies Statutory Bodies	153,894	10.69%	38.23%	23.94%	54.67%

Table 4.Order Revisions and Trader Attributes

These tables present results from the analysis of trader categories, trader styles and intensity of order revisions. Panel A reports results relating to trader categories and Panel B reports the same for trader styles. Measures of order revision activity and trader styles are calculated for each trader i using the entire sample of 50 stocks and 56 trading days — April to June, 2006. Please refer to Table 1 for variable definitions. Two tailed p-values are reported (within parentheses).

Panel A: Order Revisions and Trader Categories

		Individual Traders	Institutional Traders			Institutional - Individual	*p*- value
			Financial Institutions	Dealers	Other Institutions		
Revision Ratio	Median	0.167	0.552	0.480	0.106		
	Mean	0.257	0.705	0.534	0.242	0.237	*(<0.001)*
	P90	0.667	1.375	0.916	0.667		
Cancellation Ratio	Median	0.000	0.006	0.196	0.000		
	Mean	0.061	0.065	0.222	0.055	0.053	*(<0.001)*
	P90	0.208	0.198	0.462	0.200		
Modification Ratio	Median	0.083	0.498	0.224	0.000		
	Mean	0.196	0.640	0.312	0.187	0.184	*(<0.001)*
	P90	0.500	1.267	0.634	0.500		
N		1,070,125	5,771	509	153,894		

Panel B: Order Revisions and Trader Styles

$$Revision\ Ratio_i = \alpha + \beta_1 Closing\ Ratio_i + \beta_2 Trader\ Frequency_i + \beta_3 Trader\ Size_i + \beta_4 Network\ Trading\ Ratio_i + \varepsilon_i$$
$$Cancellation\ Ratio_i = \alpha + \beta_1 Closing\ Ratio_i + \beta_2 Trader\ Frequency_i + \beta_3 Trader\ Size_i + \beta_4 Network\ Trading\ Ratio_i + \varepsilon_i$$
$$Modification\ Ratio_i = \alpha + \beta_1 Closing\ Ratio_i + \beta_2 Trader\ Frequency_i + \beta_3 Trader\ Size_i + \beta_4 Network\ Trading\ Ratio_i + \varepsilon_i$$

Variable	Revision Ratio	Cancellation Ratio	Modification Ratio
Intercept	14.292%	12.091%	2.201%
	(<0.001)	*(<0.001)*	*(<0.001)*
Closing Ratio	-16.399%	-10.265%	-6.134%
	(<0.001)	*(<0.001)*	*(<0.001)*
Trader Frequency	7.414%	0.840%	6.574%
	(<0.001)	*(<0.001)*	*(<0.001)*
Trader Size	1.903%	0.080%	1.823%
	(<0.001)	*(0.001)*	*(<0.001)*
Network Trading Ratio	11.593%	10.434%	1.159%
	(<0.001)	*(<0.001)*	*(<0.001)*
N	1,170,355	1,170,355	1,170,355

Table 5. Order Cancellations and Trader Inventories: Duration Analysis

This table presents results from the analysis of limit order cancellations using Cox's proportional hazard duration models. The cancellation hazard is modeled as follows:

Specification 1:

$$h_{o,s,i}(t) = \lambda_0(t)\exp\begin{Bmatrix} \beta_1 Order\ Size_{o,s} + \beta_2 Spreads_{o,s} + \beta_3 LaggedVolatility_{o,s} + \beta_4 p_{o,s}^{Relative} + \beta_5 LaggedVolume_{o,s} \\ \beta_6 LPR_s + +\beta_7 Dealer_i + \beta_8 Individual_i \end{Bmatrix}$$

Specification 2:

$$h_{o,s,i}(t) = \lambda_0(t)\exp\begin{Bmatrix} \beta_1 Order\ Size_{o,s} + \beta_2 Spreads_{o,s} + \beta_3 LaggedVolatility_{o,s} + \beta_4 p_{o,s}^{Relative} + \beta_5 LaggedVolume_{o,s} \\ \beta_5 LPR_s + \beta_6 \Delta q_{o,s,t}^{same} + \beta_7 \Delta q_{o,s,t}^{opposite} + \beta_8 Dealer_i + \beta_9 Individual_i + \beta_{10} \Delta Inventory\_Stock_{s,t,i} \end{Bmatrix}$$

Specification 3:

$$h_{o,s,i}(t) = \lambda_0(t)\exp\begin{Bmatrix} \beta_1 Order\ Size_{o,s} + \beta_2 Spreads_{o,s} + \beta_3 LaggedVolatility_{o,s} + \beta_4 p_{o,s}^{Relative} + \beta_5 LaggedVolume_{o,s} \\ \beta_6 LPR_s + \beta_7 \Delta q_{o,s,t}^{same} + \beta_8 \Delta q_{o,s,t}^{opposite} + \beta_9 Dealer_i + \beta_{10} Individual_i + \beta_{11} \Delta Inventory\_Stock_{s,t,i} + \\ \beta_{12} \Delta Inventory\_Related_{s,t,i} \end{Bmatrix}$$

Specification 4:

$$h_{o,s,i}(t) = \lambda_0(t)\exp\begin{Bmatrix} \beta_1 Order\ Size_{o,s} + \beta_2 Spreads_{o,s} + \beta_3 LaggedVolatility_{o,s} + \beta_4 p_{o,s}^{Relative} + \beta_5 LaggedVolume_{o,s} \\ \beta_6 LPR_s + \beta_7 \Delta q_{o,s,t}^{same} + \beta_8 \Delta q_{o,s,t}^{opposite} + \beta_9 Dealer_i + \beta_{10} Individual_i + \beta_{11} \Delta Inventory\_Industry_{s,t,i} \end{Bmatrix}$$

Specification 5:

$$h_{o,s,i}(t) = \lambda_0(t)\exp\begin{Bmatrix} \beta_1 Order\ Size_{o,s} + \beta_2 Spreads_{o,s} + \beta_3 LaggedVolatility_{o,s} + \beta_4 p_{o,s}^{Relative} + \beta_5 LaggedVolume_{o,s} \\ \beta_6 LPR_s + \beta_7 \Delta q_{o,s,t}^{same} + \beta_8 \Delta q_{o,s,t}^{opposite} + \beta_9 Dealer_i + \beta_{10} Individual_i + \beta_{11} \Delta Inventory\_Stock_{s,t,i} + \\ \beta_{12} \Delta Inventory\_Unrelated_{s,t,i} \end{Bmatrix}$$

Where $h_{o,s,i}$ is the estimated hazard of cancellation for order o of stock s, submitted by trader i, at time t. λ is the unspecified baseline hazard rate and time t is measured from the moment of order o's submission. Please refer to Table 1 for variable definitions. A random sample of 10,000 orders are selected from each stock. Orders are tracked through their first 2 minutes. Order executions are treated as competing events. Orders from all the stocks are stacked, and a pooled analysis is conducted. The standard errors are clustered by stock. Two tailed p-values are reported (within parentheses) below the parameter estimates.

Variable	(1)	(2)	(3)	(4)	(5)
Order Size	0.287	0.286	0.286	0.286	0.286
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*	*(<.001)*
Spreads	0.003	0.003	0.003	0.003	0.003
	(0.708)	*(0.726)*	*(0.725)*	*(0.726)*	*(0.726)*
Lagged Volatility	0.088	0.059	0.059	0.059	0.059
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*	*(<.001)*
$p^{Relative}$	0.185	0.185	0.185	0.185	0.185
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*	*(<.001)*
Lagged Volume	0.027	0.029	0.029	0.029	0.029
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*	*(<.001)*
LPR	-0.036	-0.035	-0.035	-0.035	-0.035
	(0.162)	*(0.175)*	*(0.172)*	*(0.170)*	*(0.170)*
Dealer	0.615	0.616	0.616	0.615	0.616
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*	*(<.001)*
Individual	-0.757	-0.751	-0.751	-0.752	-0.751
	(<.001)	*(<.001)*	*<.0001*	*(<.001)*	*(<.001)*
Δq_t^{same}		0.037	0.037	0.037	0.037
		(<.001)	*(<.001)*	*(<.001)*	*(<.001)*
$\Delta q_t^{opposite}$		0.005	0.005	0.005	0.005
		(0.688)	*(0.689)*	*(0.690)*	*(0.690)*
$\Delta Inventory\_Stock_t$		0.013	0.013		0.013
		(<.001)	*(<.001)*		*(<.001)*
$\Delta Inventory\_Related_t$			0.008		
			(0.008)		
$\Delta Inventory\_Industry_t$				0.011	
				(<.001)	
$\Delta Inventory\_Unrelated_t$					0.002
					(<.463)

Table 6. Positive Order Modifications and Trader Inventories: Duration Analysis

This tables presents results from the analysis of positive order modifications using Cox's proportional hazard duration models. The hazard of positive modifications is modeled as follows:

Specification 1:

$$h_{o,s,i}(t) = \lambda_0(t)\exp\left\{\begin{array}{l}\beta_1 Order\ Size_{o,s} + \beta_2 Spreads_{o,s} + \beta_3 LaggedVolatility_{o,s} + \beta_4 p_{o,s}^{Relative} + \beta_5 LaggedVolume_{o,s} \\ \beta_5 LPR_s\end{array}\right\}$$

Specification 2:

$$h_{o,s,i}(t) = \lambda_0(t)\exp\left\{\begin{array}{l}\beta_1 Order\ Size_{o,s} + \beta_2 Spreads_{o,s} + \beta_3 LaggedVolatility_{o,s} + \beta_4 p_{o,s}^{Relative} + \beta_5 LaggedVolume_{o,s} \\ \beta_5 LPR_s + \beta_6 \Delta q_{o,s,t}^{same} + \beta_7 \Delta q_{o,s,t}^{opposite} + \beta_8 Dealer_i + \beta_9 Individual_i + \beta_{10}\Delta Inventory\_Stock_{s,t,i}\end{array}\right\}$$

Specification 3:

$$h_{o,s,i}(t) = \lambda_0(t)\exp\left\{\begin{array}{l}\beta_1 Order\ Size_{o,s} + \beta_2 Spreads_{o,s} + \beta_3 LaggedVolatility_{o,s} + \beta_4 p_{o,s}^{Relative} + \beta_5 LaggedVolume_{o,s} \\ \beta_5 LPR_s + \beta_6 \Delta q_{o,s,t}^{same} + \beta_7 \Delta q_{o,s,t}^{opposite} + \beta_8 Dealer_i + \beta_9 Individual_i + \beta_{10}\Delta Inventory\_Stock_{s,t,i} + \\ \beta_{10}\Delta Inventory\_Related_{s,i,t}\end{array}\right\}$$

Specification 4:

$$h_{o,s,i}(t) = \lambda_0(t)\exp\left\{\begin{array}{l}\beta_1 Order\ Size_{o,s} + \beta_2 Spreads_{o,s} + \beta_3 LaggedVolatility_{o,s} + \beta_4 p_{o,s}^{Relative} + \beta_5 LaggedVolume_{o,s} \\ \beta_5 LPR_s + \beta_6 \Delta q_{o,s,t}^{same} + \beta_7 \Delta q_{o,s,t}^{opposite} + \beta_8 Dealer_i + \beta_9 Individual_i + \beta_{10}\Delta Inventory\_Industry_{s,t,i} \\ \end{array}\right\}$$

Where $h_{o,s,i}$ is the estimated hazard of positive order modifications for order o of stock s, submitted by trader i, at time t. λ is the unspecified baseline hazard rate and time t is measured from the moment of order o's submission. Please refer to Table 1 for variable definitions. A random sample of 10,000 orders are selected from each stock. Orders are tracked through their first 2 minutes. Order executions and cancellations are treated as competing events. Orders from all the stocks are stacked, and a pooled analysis is conducted. The standard errors are clustered by order (Lee, Wei, and Amato, 1992). Two tailed p-values are reported (within parentheses) below the parameter estimates.

Variable	(1)	(2)	(3)	(4)
Order Size	0.372	0.376	0.376	0.375
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*
Spreads	0.082	0.112	0.112	0.112
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*
Lagged Volatility	0.082	-0.021	-0.021	-0.021
	(<.001)	*(0.060)*	*(0.061)*	*(0.060)*
$p^{Relative}$	0.188	0.188	0.188	0.188
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*
Lagged Volume	0.028	0.035	0.035	0.035
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*
LPR	0.066	0.064	0.064	0.064
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*
Dealer	0.460	0.446	0.446	0.449
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*
Individual	-0.152	-0.155	-0.155	-0.151
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*
Δq_t^{same}		0.054	0.054	0.054
		(<.001)	*(<.001)*	*(<.001)*
$\Delta q_t^{opposite}$		0.099	0.099	0.099
		(<.001)	*(<.001)*	*(<.001)*
$\Delta Inventory\_Stock_t$		-0.063	-0.063	
		(<.001)	*(<.001)*	
$\Delta Inventory\_Related_t$			~0.000	
			(0.995)	
$\Delta Inventory\_Industry_t$				-0.041
				(<.001)

Table 7. Negative Order Modifications and Trader Inventories: Duration Analysis

This tables presents results from the analysis of negative order modifications using Cox's proportional hazard duration models. The hazard of negative modifications is modeled as follows:

Specification 1:

$$h_{o,s,i}(t) = \lambda_0(t)\exp\begin{Bmatrix} \beta_1 Order\ Size_{o,s} + \beta_2 Spreads_{o,s} + \beta_3 LaggedVolatility_{o,s} + \beta_4 p_{o,s}^{Relative} + \beta_5 LaggedVolume_{o,s} \\ \beta_5 LPR_s \end{Bmatrix}$$

Specification 2:

$$h_{o,s,i}(t) = \lambda_0(t)\exp\begin{Bmatrix} \beta_1 Order\ Size_{o,s} + \beta_2 Spreads_{o,s} + \beta_3 LaggedVolatility_{o,s} + \beta_4 p_{o,s}^{Relative} + \beta_5 LaggedVolume_{o,s} \\ \beta_5 LPR_s + \beta_6 \Delta q_{o,s,t}^{same} + \beta_7 \Delta q_{o,s,t}^{opposite} + \beta_8 Dealer_i + \beta_9 Individual_i + \beta_{10} \Delta Inventory\_Stock_{s,t,i} \end{Bmatrix}$$

Specification 3:

$$h_{o,s,i}(t) = \lambda_0(t)\exp\begin{Bmatrix} \beta_1 Order\ Size_{o,s} + \beta_2 Spreads_{o,s} + \beta_3 LaggedVolatility_{o,s} + \beta_4 p_{o,s}^{Relative} + \beta_5 LaggedVolume_{o,s} \\ \beta_5 LPR_s + \beta_6 \Delta q_{o,s,t}^{same} + \beta_7 \Delta q_{o,s,t}^{opposite} + \beta_8 Dealer_i + \beta_9 Individual_i + \beta_{10} \Delta Inventory\_Stock_{s,t,i} + \\ \beta_{10} \Delta Inventory\_Related_{s,i,t} \end{Bmatrix}$$

Specification 4:

$$h_{o,s,i}(t) = \lambda_0(t)\exp\begin{Bmatrix} \beta_1 Order\ Size_{o,s} + \beta_2 Spreads_{o,s} + \beta_3 LaggedVolatility_{o,s} + \beta_4 p_{o,s}^{Relative} + \beta_5 LaggedVolume_{o,s} \\ \beta_5 LPR_s + \beta_6 \Delta q_{o,s,t}^{same} + \beta_7 \Delta q_{o,s,t}^{opposite} + \beta_8 Dealer_i + \beta_9 Individual_i + \beta_{10} \Delta Inventory\_Industry_{s,t,i} \\ \end{Bmatrix}$$

Where $h_{o,s,i}$ is the estimated hazard of negative order modifications for order *o* of stock *s*, submitted by trader *i*, at time *t*. λ_0 is the unspecified baseline hazard rate and time *t* is measured from the moment of order *o*'s submission. Please refer to Table 1 for variable definitions. A random sample of 10,000 orders are selected from each stock. Orders are tracked through their first 2 minutes. Order executions and cancellations are treated as competing events. Orders from all the stocks are stacked, and a pooled analysis is conducted. The standard errors are clustered by order (Lee, Wei and Amato, 1992). Two tailed *p-values* are reported (within parentheses) below the parameter estimates.

Variable	(1)	(2)	(3)	(4)
Order Size	0.317	0.316	0.316	0.317
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*
Spreads	~ -0.001	-0.002	-0.002	-0.002
	(0.987)	*(0.856)*	*(0.857)*	*(0.726)*
Lagged Volatility	0.019	0.004	0.004	0.004
	(0.167)	*(0.768)*	*(0.769)*	*(0.773)*
$p^{Relative}$	0.173	0.172	0.172	0.172
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*
Lagged Volume	-0.085	-0.081	-0.081	-0.081
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*
LPR	-0.047	-0.046	-0.046	-0.047
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*
Dealer	0.412	0.417	0.417	0.416
	(<.001)	*(<.001)*	*(<.001)*	*(<.001)*
Individual	-0.248	-0.256	-0.256	-0.257
	(<.001)	*(<.001)*	*<.0001*	*(<.001)*
Δq_t^{same}		-0.083	-0.083	-0.083
		(<.001)	*(<.001)*	*(<.001)*
$\Delta q_t^{opposite}$		-0.089	-0.089	-0.089
		(<.001)	*(<.001)*	*(<.001)*
$\Delta Inventory\_Stock_t$		0.024	0.024	
		(<.001)	*(<.001)*	
$\Delta Inventory\_Related_t$			0.009	
			(0.2821)	
$\Delta Inventory\_Industry_t$				0.019
				(<.001)

Table 8. Order Revisions and Performance: Panel Regressions

This tables presents results from panel regressions of different measures of performance on order revisions, other order characteristics and market variables. The different specifications employed in the analysis are as follows:

Specifications 1a and 1b:

$$\begin{aligned} Ex\ Post\ Ratio^P_{i,s,t} &= \alpha_i + \gamma_s + \beta_1 Total\ Revisions^P_{i,s,t} + \beta_2 Price\ Agg^P_{i,s,t} + \beta_3 Log\ Quantity^P_{i,s,t} + \beta_4 Buy^P_{i,s,t} \\ &+ \beta_5 Hidden^P_{i,s,t} + \beta_6 Past\ Volatility^P_{i,s,t} + \beta_7 Past\ Trading\ Frequency^P_{i,s,t} + \beta_8 NumberOf\ Orders^P_{i,s,t} + \varepsilon_{i,s,t} \end{aligned}$$

Specifications 2a and 2b:

$$\begin{aligned} Price\ Impact\ Ratio^P_{i,s,t} &= \alpha_i + \gamma_s + \beta_1 Total\ Revisions^P_{i,s,t} + \beta_2 Price\ Agg^P_{i,s,t} + \beta_3 Log\ Quantity^P_{i,s,t} + \beta_4 Buy^P_{i,s,t} \\ &+ \beta_5 Hidden^P_{i,s,t} + \beta_6 Past\ Volatility^P_{i,s,t} + \beta_7 Past\ Trading\ Frequency^P_{i,s,t} + \beta_8 NumberOf\ Orders^P_{i,s,t} + \varepsilon_{i,s,t} \end{aligned}$$

Specifications 3a and 3b:

$$\begin{aligned} Opportunity\ Cost\ Ratio^P_{i,s,t} &= \alpha_i + \gamma_s + \beta_1 Total\ Revisions^P_{i,s,t} + \beta_2 Price\ Agg^P_{i,s,t} + \beta_3 Log\ Quantity^P_{i,s,t} + \beta_4 Buy^P_{i,s,t} \\ &+ \beta_5 Hidden^P_{i,s,t} + \beta_6 Past\ Volatility^P_{i,s,t} + \beta_7 Past\ Trading\ Frequency^P_{i,s,t} + \beta_8 NumberOf\ Orders^P_{i,s,t} + \varepsilon_{i,s,t} \end{aligned}$$

Specifications 4:

$$\begin{aligned} Total\ Cost\ Ratio^P_{i,s,t} &= \alpha_i + \gamma_s + \beta_1 Total\ Revisions^P_{i,s,t} + \beta_2 Price\ Agg^P_{i,s,t} + \beta_3 Log\ Quantity^P_{i,s,t} + \beta_4 Buy^P_{i,s,t} \\ &+ \beta_5 Hidden^P_{i,s,t} + \beta_6 Past\ Volatility^P_{i,s,t} + \beta_7 Past\ Trading\ Frequency^P_{i,s,t} + \beta_8 NumberOf\ Orders^P_{i,s,t} + \varepsilon_{i,s,t} \end{aligned}$$

Specification 5:

$$\begin{aligned} Total\ Cost\ Ratio^P_{i,s,t} &= \alpha_i + \gamma_s + \beta_1 Total\ Revisions^P_{i,s,t} + \beta_9 Total\ Revisions^P_{i,s,t} * Earnings\ Day_{S,t} + \beta_{10} Earnings\ Day_{s,t} \\ &+ \beta_2 Price\ Agg^P_{i,s,t} + \beta_3 Log\ Quantity^P_{i,s,t} + \beta_4 Buy^P_{i,s,t} + \beta_5 Hidden^P_{i,s,t} + \beta_6 Past\ Volatility^P_{i,s,t} \\ &+ \beta_7 Past\ Trading\ Frequency^P_{i,s,t} + \beta_8 NumberOf\ Orders^P_{i,s,t} + \varepsilon_{i,s,t} \end{aligned}$$

All the variables with superscript P are value weighted averages of trader i's portfolio (P) of orders in stock s on day t. Please refer to Table 1 for variable definitions. The panel regressions are conducted with trader and stock fixed effects — α and γ, respectively. Further, to control for contemporaneous cross-sectional correlation in residuals, the standard errors are cluster by day (t). Panels A, B, C and D report results relating to financial institutions (FIN), other institutions (Others), dealers (Dealer) and individuals (Individuals). In all panels, columns 1a-3a include all trader portfolios; columns 1b and 2b consider only trader portfolios with either partial or complete execution; coefficients in column 3b are obtained by including only trader portfolios with zero or partial executions. Two tailed p-values are reported (within parentheses) below the parameter estimates.

Panel A – Order Revisions and Performance: Financial Institutions

Variable	Ex post ratio		Price impact ratio		Opportunity cost ratio		Total cost ratio	
	All Orders	If fill rate > 0%	All Orders	If fill rate > 0%	All Orders	If fill rate < 100%	All Orders	All Orders
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4)	(5)
Total Revisions^P	0.057%	0.057%	0.069%	0.080%	-0.012%	-0.059%	-0.010%	-0.009%
	(<0.001)	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(0.009)*	*(<0.001)*	*(0.097)*	*(0.141)*
Total Revisions^P * *Earnings Day*								-0.050%
								(0.068)
Earnings Day								0.090%
								(0.082)
Price Agg^P	-1.791%	-4.824%	4.114%	12.544%	-3.703%	2.044%	1.721%	1.725%
	(<0.001)	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(0.054)*	*(0.087)*	*(0.086)*
Log Quantity^P	0.006%	0.006%	-0.009%	-0.007%	0.025%	0.052%	0.016%	0.016%
	(0.400)	*(0.472)*	*(<0.001)*	*(0.002)*	*(<0.001)*	*(0.001)*	*(0.022)*	*(0.022)*
Buy^P	-0.072%	-0.078%	-0.025%	-0.023%	-0.061%	-0.156%	-0.006%	-0.005%
	(0.328)	*(0.330)*	*(0.197)*	*(0.241)*	*(0.216)*	*(0.332)*	*(0.927)*	*(0.935)*
Hidden^P	-0.012%	-0.014%	-0.019%	-0.025%	0.039%	0.128%	0.042%	0.042%
	(0.568)	*(0.566)*	*(0.021)*	*(0.009)*	*(0.017)*	*(0.009)*	*(0.066)*	*(0.066)*
Past Volatility^P	18.421%	20.956%	4.654%	7.680%	27.128%	91.517%	14.949%	14.681%
	(0.048)	*(0.047)*	*(0.143)*	*(0.048)*	*(<0.001)*	*(<0.001)*	*(0.164)*	*(0.174)*
Past Trading Frequency^P	0.019%	0.021%	-0.003%	-0.002%	-0.007%	-0.004%	-0.025%	-0.026%
	(0.080)	*(0.095)*	*(0.249)*	*(0.460)*	*(0.297)*	*(0.860)*	*(0.017)*	*(0.014)*
Number of Orders^P	-0.004%	-0.005%	-0.002%	0.001%	-0.009%	-0.095%	-0.009%	-0.009%
	(0.402)	*(0.405)*	*(0.228)*	*(0.537)*	*(0.001)*	*(<0.001)*	*(0.112)*	*(0.113)*
Stock Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trader Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	92,083	84,100	92,083	84,100	92,083	25,689	92,083	92,083

Panel B – Order Revisions and Performance: Other Institutions

Variable	Ex post ratio		Price impact ratio		Opportunity cost ratio		Total cost ratio	
	All Orders	If fill rate > 0%	All Orders	If fill rate > 0%	All Orders	If fill rate < 100%	All Orders	All Orders
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4)	(5)
Total Revisions^P	0.007%	0.008%	0.012%	0.024%	-0.016%	-0.025%	-0.008%	-0.008%
	(<0.001)	*(0.022)*	*(<0.001)*	*(<0.001)*	*(0.001)*	*(0.001)*	*(0.025)*	*(0.023)*
*Total Revisions^P * Earnings Day*								~0.000%
								(0.991)
Earnings Day								0.113%
								(0.051)
Price Agg^P	-0.944%	-2.316%	2.102%	11.828%	-3.073%	0.636%	-0.153%	-0.155%
	(<0.001)	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(0.005)*	*(0.565)*	*(0.904)*	*(0.902)*
Log Quantity^P	-0.002%	-0.004%	0.009%	0.021%	-0.005%	-0.024%	0.008%	0.008%
	(0.495)	*(0.501)*	*(<0.001)*	*(<0.001)*	*(0.060)*	*(<0.001)*	*(0.065)*	*(0.068)*
Buy^P	-0.115%	-0.158%	-0.045%	-0.054%	-0.146%	-0.312%	-0.073%	-0.073%
	(0.072)	*(0.080)*	*(0.170)*	*(0.205)*	*(0.260*	*(0.301)*	*(0.592)*	*(0.592)*
Hidden^P	-0.006%	-0.008%	-0.035%	-0.049%	0.059%	0.114%	0.038%	0.039%
	(0.701)	*(0.677)*	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(0.073)*	*(0.071)*
Past Volatility^P	-0.108%	0.279%	-21.244%	-19.870%	41.120%	98.183%	21.738%	21.188%
	(0.988)	*(0.978)*	*(<0.001)*	*(<0.001)*	*(0.002)*	*(<0.001)*	*(0.033)*	*(0.038)*
Past Trading Frequency^P	0.002%	0.003%	-0.010%	-0.016%	-0.003%	0.015%	-0.013%	-0.014%
	(0.798)	*(0.741)*	*(0.002)*	*(<0.001)*	*(0.748)*	*(0.463)*	*(0.117)*	*(0.084)*
Number of Orders^P	0.001%	0.001%	-0.005%	0.002%	0.001%	-0.037%	-0.005%	-0.005%
	(0.767)	*(0.865)*	*(0.002)*	*(0.347)*	*(0.788)*	*(0.006)*	*(0.254)*	*(0.271)*
Stock Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trader Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	480,806	384,244	480,806	384,244	480,806	203,470	480,806	480,806

Panel C – Order Revisions and Performance: Dealers

Variable	Ex post ratio		Price impact ratio		Opportunity cost ratio		Total cost ratio	
	All Orders	If fill rate > 0%	All Orders	If fill rate > 0%	All Orders	If fill rate < 100%	All Orders	All Orders
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4)	(5)
Total Revisions^P	0.003%	0.004%	0.004%	0.006%	-0.004%	-0.006%	-0.001%	-0.001%
	(0.083)	*(0.117)*	*(0.201)*	*(0.298)*	*(0.088)*	*(0.012)*	*(0.656)*	*(0.591)*
Total Revisions^P * *Earnings Day*								0.018%
								(0.186)
Earnings Day								-0.006%
								(0.849)
Price Agg^P	-0.860%	-2.035%	1.899%	6.179%	-1.868%	-1.238%	0.506%	0.506%
	(<0.001)	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(0.013)*	*(0.123)*	*(0.570)*	*(0.570)*
Log Quantity^P	0.002%	0.002%	0.008%	0.013%	-0.014%	-0.021%	-0.008%	-0.008%
	(0.002)	*(0.038)*	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(0.002)*	*(0.002)*
Buy^P	-0.077%	-0.108%	-0.023%	-0.037%	-0.313%	-0.459%	-0.256%	-0.256%
	(0.116)	*(0.117)*	*(0.367)*	*(0.308)*	*(0.105)*	*(0.101)*	*(0.154)*	*(0.154)*
Hidden^P	0.005%	0.002%	-0.026%	-0.029%	0.043%	0.042%	0.023%	0.024%
	(0.538)	*(0.843)*	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(0.010)*	*(0.092)*	*(0.090)*
Past Volatility^P	2.033%	2.154%	-0.547%	2.068%	29.897%	47.054%	28.506%	28.465%
	(0.605)	*(0.659)*	*(0.800)*	*(0.460)*	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(<0.001)*
Past Trading Frequency^P	-0.005%	-0.005%	-0.001%	-0.002%	0.006%	0.014%	0.013%	0.012%
	(0.194)	*(0.251)*	*(0.764)*	*(0.478)*	*(0.352)*	*(0.147)*	*(0.033)*	*(0.036)*
Number of Orders^P	0.002%	0.001%	~0.000%	0.003%	0.002%	-0.007%	-0.001%	-0.001%
	(0.122)	*(0.224)*	*(0.748)*	*(<0.001)*	*(0.209)*	*(0.023)*	*(0.781)*	*(0.783)*
Stock Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trader Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	163,160	147,048	163,160	147,048	163,160	109,647	147,048	147,048

Panel D – Order Revisions and Performance: Individuals

Variable	Ex post ratio		Price impact ratio		Opportunity cost ratio		Total cost ratio	
	All Orders	If fill rate > 0%	All Orders	If fill rate > 0%	All Orders	If fill rate < 100%	All Orders	All Orders
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4)	(5)
Total Revisions^P	0.007%	-0.002%	0.062%	0.105%	-0.079%	-0.220%	-0.014%	-0.013%
	(0.070)	*(0.505)*	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(0.327)*	*(0.353)*
Total Revisions^P ** Earnings Day*								-0.046%
								(0.354)
Earnings Day								0.183%
								(0.026)
Price Agg^P	-0.777%	-1.712%	2.149%	11.965%	-3.058%	2.113%	-0.257%	-0.260%
	(<0.001)	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(0.007)*	*(0.695)*	*(0.695)*
Log Quantity^P	-0.011%	-0.013%	0.007%	0.015%	0.005%	-0.020%	0.024%	0.024%
	(0.005)	*(0.009)*	*(<0.001)*	*(<0.001)*	*(0.253)*	*(0.062)*	*(<0.001)*	*(<0.001)*
Buy^P	-0.140%	-0.202%	-0.041%	-0.041%	-0.172%	-0.358%	-0.071%	-0.072%
	(0.013)	*(0.013)*	*(0.215)*	*(0.341)*	*(0.214)*	*(0.283)*	*(0.613)*	*(0.609)*
Hidden^P	0.014%	0.019%	-0.045%	-0.050%	0.059%	0.071%	0.008%	0.008%
	(0.067)	*(0.042)*	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(<0.001)*	*(0.576)*	*(0.566)*
Past Volatility^P	-4.986%	-6.424%	-20.373%	-20.362%	32.379%	90.750%	17.653%	16.583%
	(0.402)	*(0.441)*	*(<0.001)*	*(<0.001)*	*(0.002)*	*(0.001)*	*(0.017)*	*(0.024)*
Past Trading Frequency^P	-0.003%	-0.004%	-0.009%	-0.017%	-0.006%	0.006%	-0.011%	-0.014%
	(0.563)	*(0.625)*	*(0.004)*	*(<0.001)*	*(0.458)*	*(0.753)*	*(0.193)*	*(0.105)*
Number of Orders^P	0.005%	0.008%	-0.004%	-0.007%	-0.005%	-0.052%	-0.013%	-0.013%
	(0.256)	*(0.134)*	*(0.075)*	*(0.015)*	*(0.436)*	*(0.004)*	*(0.049)*	*(0.057)*
Stock Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trader Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	13,962,603	11,206,706	13,962,603	11,206,706	13,962,603	5,182,729	13,962,603	13,962,603

Table 9. Variable Definitions, Chapter 2

This table defines the variables used in our analysis. All variables are estimated daily, unless otherwise specified.

Short Selling	
Outstanding Naked Short Ratio (ONSR)	Ratio of the estimated number of outstanding fails to deliver over total shares outstanding.
Outstanding Covered Short Ratio (OCSR)	Ratio of the estimated number of outstanding covered-shorted shares over total shares outstanding.
Naked to Total Shorts Ratio	Ratio of the estimated number of outstanding naked shorted shares to estimated total shorted shares.
Pricing Error	
Pricing Error (PE)	The non-random walk component of a daily return series estimated using a Kalman filter methodology.
Negative Pricing Error (Negative PE)	A variable set equal to the pricing error when pricing error is negative, to zero otherwise.
Positive Pricing Error (Positive PE)	A variable set equal to the pricing error when pricing error is positive, to zero otherwise.
Pricing Error Volatility (PE Volatility)	The absolute value of the pricing error.
Positive PE Dum	A binary variable set equal to one if pricing error is positive, to zero otherwise.
Liquidity Related Metrics	
Order Imbalance (OIB)	The daily sum of the 5-minute difference between the market value of shares traded in buyer initiated trades and the market value of shares traded in seller initiated trades, divided by total daily dollar trading volume.
Positive OIB Dum	A binary variable set equal to 1 if *OIB* is positive and to zero otherwise.
Spread	The daily average of the ratio of the difference between bid and ask and the mid-quote.
Volume	Daily number of shares traded.
Other	
Price	The natural log of the daily average of the 5-minute midquote.
Volatility	The daily standard error of the 5-minute stock price return.
Market Value	The number of shares outstanding multiplied by the closing price for the day.

Table 10. FTDs as a Proxy for Naked Short Selling

This table presents the behavior of *ONSR* during different naked short selling regimes in 2008. "Event Securities" are our sample of 17 stocks that were subject to the ban on naked short selling between July 21, 2008 and August 12, 2008, i.e., the "Ban Period" (while the ban affected 19 securities, we have data for 17 of these: Bank of America Corporation, Barclays, Bear Stearns Companies Inc., Citigroup Inc., Credit Suisse Group, Deutsche Bank Group AG, Allianz SE, Goldman, Sachs Group Inc, Royal Bank ADS, HSBC Holdings PLC ADS, J. P. Morgan Chase & Co., Merrill Lynch & Co., Inc., Mizuho Financial Group, Inc., Morgan Stanley, UBS AG, Freddie Mac, and Fannie Mae). "Control Securities" are the sample of 17 market capitalization and industry matched stocks that were not subject to any increased restrictions on naked short selling during the "Ban Period". The "Pre-Ban Period" refers to the interval January 1, 2008 to July 20, 2008. The "Post-Ban Period" refers to the interval August 13, 2008 to September 2, 2008. *ONSR* is computed for the Event and Control stocks on a daily basis over the interval January 1, 2008 to August 12, 2008. Reported p-values are for a test of whether *ONSR* has changed significantly in relation to the pre-ban period. "*", "**", and "***" indicate significance at the 10%, 5% and 1% level respectively.

	Event Securities		Control Securities		Event-Control	
	Mean	**p value**	**Mean**	**p value**	**Mean**	**p value**
Pre Ban	0.106%		0.015%		0.091%	<0.01 ***
Ban, 1st Week	0.063%		0.054%		0.010%	1.34
Change from Pre Ban	-0.043%	<0.01 ***	0.039%	<0.01 ***	-0.081%	<0.01 ***
Ban, 2nd Week	0.007%		0.024%		-0.017%	-2.38 **
Change from Pre Ban	-0.099%	<0.01 ***	0.009%	<0.01 ***	-0.108%	<0.01 ***
Ban, 3rd Week	0.004%		0.014%		-0.009%	-1.30
Change from Pre Ban	-0.102%	<0.01 ***	-0.001%	0.24	-0.100%	<0.01 ***
Post Ban	0.028%		0.006%		0.023%	<0.01 ***
Change from Ban (3rd Week)	0.024%	<0.01 ***	-0.008%	0.25	0.032%	<0.01 ***

Table 11. Summary Statistics

All variables are as defined in Table 9. The sample is built as follows: *ONSR* is computed for all NYSE common stock of US-based firms (CRSP share codes 10 and 11) listed for at least 18 months over the interval January 1, 2005 to June 30, 2008, and with no daily large changes (>10%) in the number of shares outstanding and for which we had all required data (n= 1,492). We rank each security by mean *ONSR* and allocate securities on that basis to decile 1 (lowest) through 10 (highest). Daily statistics are computed by security, security averages are further averaged over deciles and for the entire sample. This table reports mean, median and standard deviation for the sample, means for deciles 1 and 10 and the difference between those, along with results of a t-test for differences in means across deciles 1 and 10. "*", "**", and "***" indicate significance at the 10%, 5% and 1% level respectively.

Variables	Sample Mean	Sample Median	Sample STD	Decile 1 Mean	Decile 10 Mean	Decile 10 - Decile 1	p-value
ONSR	0.06%	0.01%	0.22%	<0.01%	0.43%	0.40%	<0.01 ***
OCSR	5.45%	4.34%	5.30%	3.95%	13.38%	9.43%	<0.01 ***
Naked to Total Shorts Ratio	0.97%	0.43%	5.09%	0.10%	2.77%	2.67%	<0.01 ***
Pricing Error	-0.15%	0.00%	4.33%	0.02%	-0.50%	-0.52%	0.35
Positive Pricing Error	0.51%	0.10%	1.47%	0.38%	1.11%	0.73%	<0.01 ***
Pricing Error Volatility	1.18%	0.21%	4.96%	0.74%	2.75%	2.01%	<0.01 ***
Order Imbalance	6.72%	6.92%	5.71%	3.84%	7.98%	4.14%	<0.01 ***
Spread	0.18%	0.11%	0.27%	0.38%	0.26%	-0.12%	0.01 **
Volatility	0.21%	0.20%	0.08%	0.21%	0.29%	0.08%	<0.01 ***
***Market Value* (US$ M)**	$8,223	$2,088	$2,421	$12,500	$1,480	($11,020)	<0.01 ***
Number of Obs.	1,492	1,492	1,492	149	150		

Table 12. Naked Short Selling and Market Quality, Portfolio Approach

All variables are as defined in Table 9. All variables are standardized and winsorized by security. The reported results are converted to base units for ease of interpretation. The sample is built as follows: we include all NYSE common stock of US-based firms (CRSP share codes 10 and 11) available in the CRSP and TAQ databases listed for at least 18 months over the interval January 1, 2005 to June 30, 2008, and with no large changes (>10%) in the number of shares outstanding. The resulting sample includes 1,492 securities. On each day *t* between January 2005 and June 2008, we allocate securities from the sample with a one-standard deviation or greater decrease in *ONSR* into an '*ONSR* Decrease' portfolio and, similarly, securities with a one-standard deviation or greater increase in *ONSR* into an '*ONSR* Increase' portfolio. We include all remaining securities into an '*ONSR* No Change' portfolio. In order to control for the extent of covered short selling, we replicate the same procedure on the bases of the intensity of covered short selling, proxied by changes in OCSR, thus forming the portfolios '*OCSR* Decrease', '*OCSR* No Change' and '*OCSR* Increase'. Finally, we intersect those groups of portfolios, forming nine final portfolios. For each portfolio, we then compute averages of changes in our measures of returns, return volatility, market liquidity, pricing errors and order imbalances for the following day (*t*+1). Table 12 presents results for the 'Naked Increase, Covered No Change' and the 'Naked Decrease, Covered No Change' portfolios, along with results of a test for differences between them. p-values are from two-sided t-tests. "*", "**", and "***" indicate significance at the 10%, 5% and 1% level respectively.

Response Variable	***ONSR* Increase, *OCSR* No Change**	**p-value**	***ONSR* Decrease, *OCSR* No Change**	**p-value**	**Difference**	**p-value**
$\Delta PE(t+1)$	-0.16%	<0.01 ***	0.05%	0.28	-0.20%	<0.01 ***
$\Delta PE\ Volatility(t+1)$	-0.82%	<0.01 ***	0.03%	0.60	-0.84%	<0.01 ***
$\Delta Price(t+1)$	-0.18%	0.34	0.44%	0.02 **	-0.62%	<0.01 ***
$\Delta Volatility(t+1)$	~0.01%	<0.01 ***	~0.01%	0.05 **	~0.01%	<0.01 ***
$\Delta Spread(t+1)$	-0.01%	<0.01 ***	0.00%	0.61	-0.01%	<0.01 ***
$\Delta OIB(t+1)$	-0.24%	<0.01 ***	0.07%	0.34	-0.31%	<0.01 ***
N (days)	842		842			

Table 13. OLS Regressions

This table presents results for panel regressions with firm fixed effects and time-clustered standard errors. All variables are standardized and winsorized by security, and are as defined in Table 9. The sample is built as follows: we include all NYSE common stocks of US-based firms (CRSP share codes 10 and 11) available in the CRSP and TAQ databases listed for at least 18 months over the interval January 1, 2005 to June 30, 2008, and with no large changes (>10%) in the number of shares outstanding. The resulting sample includes 1,492 securities. The Market variable for a given predictor on day *t* is obtained by averaging the related variable across all securities on day *t*. All variables are differenced (subtracting the previous day's value). The *p-values* are in italics. "*", "**", and "***" indicate significance at 10%, 5% and 1% level respectively.

Predictors	*Pricing Error(t)*	*Pricing Error Volatility(t)*	*Price(t)*	*Volatility(t)*	*Spread(t)*	*OIB(t)*
ONSR(t-1)	0.015 *<0.01* ***	-0.020 *<0.01* ***	~ 0.001 *0.96*	-0.005 *0.05** *	-0.011 *<0.01* ***	-0.022 *<0.01* ***
OCSR(t-1)	0.195 *0.02* ***	-0.275 *0.03* **	-0.004 *0.21*	-0.070 *0.31*	-0.098 *0.08* *	-0.261 *<0.01* ***
Pricing Error (t-1)	-0.349 *<0.01* ***					
*ONSR(t-1)*Positive PE(t-1)*	-0.033 *<0.01* ***					
*OCSR(t-1)*Positive PE(t-1)*	0.195 *0.02* **					
Pricing Error Volatility(t-1)		-0.376 *<0.01* ***				
Price(t-1)			-0.025 *<0.01* ***			
Volatility(t-1)				-0.194 *<0.01* ***		
Spread(t-1)					-0.321 *<0.01* ***	
OIB(t-1)						-0.448 *<0.01* ***
Market Pricing Error(t)	0.856 *<0.01* ***					
Market Pricing Error Volatility(t)		0.836 *<0.01* ***				
Market Price(t-1)			0.997 *<0.01* ***			
Market Volatility(t)				0.915 *<0.01* ***		
Market Spread(t)					0.875 *<0.01* ***	
Market OIB(t)						0.802 *<0.01* ***
Number of Firm-Days	1,001,656	1,001,656	1,001,656	1,001,656	1,001,656	1,001,656
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table 14. Summary Results, Impact of Naked and Covered Short Selling

This table provides results for the estimation of three vector autoregressive models of order one: VAR with *Pricing Error* (Model 1), VAR with *Pricing Error Volatility* (Model 2) and VAR with *Price* (Model 3). All variables are standardized and winsorized by security, and are as defined in Table 9. The 'entire market' sample is built as follows: we include all NYSE common stock of US-based firms (CRSP share codes 10 and 11) available in the CRSP and TAQ databases listed for at least 18 months over the interval January 1, 2005 to June 30, 2008, and with no large changes (>10%) in the number of shares outstanding. For the 'most naked-shorted' sample, we rank each security by mean *ONSR,* and allocate securities on that basis to decile 1 (lowest) through 10 (highest); the securities included in decile 10 constitute the 'most naked-shorted' sample. The VAR equation for Model 1is defined below; the subscripts *M*, *i* and *t* indicate, respectively, the market-wide average of the variable of interest, stock *i* and day *t*. The other VAR models are analogously defined.

$$\mathbf{\Delta Y_{i,t}} = \mathbf{c_i} + \varphi_\mathbf{i}\mathbf{\Delta Y_{i,t-1}} + \boldsymbol{\psi}_\mathbf{i}\mathbf{X_{i,t}} + \boldsymbol{\varepsilon}_\mathbf{i,t} \qquad \mathbf{\Delta Y_{i,t}} = \mathbf{Y_{i,t}} - \mathbf{Y_{i,t-1}} \qquad \boldsymbol{\varepsilon}_\mathbf{i,t} \sim \text{i.i.d.}N(\mathbf{0}, \mathbf{\Omega_i})$$

$$\mathbf{Y_{i,t}} = \begin{pmatrix} ONSR_{i,t} \\ OCSR_{i,t} \\ PE_{i,t} \\ Volatility_{i,t} \\ Spread_{i,t} \\ OIB_{i,t} \end{pmatrix} \qquad \boldsymbol{\psi}_\mathbf{i} = \begin{pmatrix} \psi_{i,1,1} & \psi_{i,1,2} & 0 & \psi_{i,1,4} & 0 & 0 \\ \psi_{i,2,1} & \psi_{i,2,2} & 0 & \psi_{i,2,4} & 0 & 0 \\ 0 & \psi_{i,3,2} & \psi_{i,3,3} & \psi_{i,3,4} & 0 & 0 \\ 0 & 0 & 0 & \psi_{i,4,4} & 0 & 0 \\ 0 & 0 & 0 & \psi_{i,5,4} & 0 & 0 \\ 0 & 0 & 0 & \psi_{i,6,4} & 0 & 0 \end{pmatrix}$$

$$\mathbf{X_{i,t}} = \begin{pmatrix} x_{i1,1} & x_{i1,2} & 0 & 0 & 0 & 0 \\ x_{i2,1} & x_{i2,2} & x_{i2,3} & 0 & 0 & 0 \\ 0 & 0 & x_{i3,3} & 0 & 0 & 0 \\ ONSR_{M,t} & OCSR_{M,t} & PE_{M,t} & Volatility_{M,t} & Spread_{M,t} & OIB_{M,t} \\ 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 \end{pmatrix}$$

$$\text{where} \begin{cases} x_{i1,1} = \text{Positive}\,OIB\ Dum_{i,t-1} \\ x_{i2,1} = \text{Positive}\,PE\ Dum_{i,t-1} \\ x_{i1,2} = \text{Positive}\,OIB\ Dum_{i,t-1} \\ x_{i2,2} = \text{Positive}\,PE\ Dum_{i,t-1} \\ x_{i2,3} = \text{Positive}\,OIB\ Dum_{i,t-1} * \Delta ONSR_{i,t-1} \\ x_{i3,3} = \text{Positive}\,OIB\ Dum_{i,t-1} * \Delta OCSR_{i,t-1} \end{cases}$$

Panels A and B are extracts of estimates of the parameters in Models 1, 2 and 3 above and report results pertaining to the impact of, respectively, ONSR and OCSR. Reported parameter estimates are averages of parameters estimated by security. Significance is tested employing a cross-sectional estimate of the standard error of the parameter estimate, as in Chordia, Roll and Subrahmanyam (2000). The p-values from two-sided t-tests are in italics below the parameter estimate. "*", "**", and "***" indicate significance at the 10%, 5% and 1% level respectively.

Panel A - Impact of Naked Short Selling

Sample	Response – Change						
	PE	*PE* (incremental effect when lag *PE* > 0)	*PE Volatility*	*Price*	*Volatility*	*Spread*	*OIB*
A: Overall	0.046	-0.123			-0.064	-0.016	-0.046
1,402 Securities	*<0.01 ****	*<0.01 ****			*0.01 ***	*0.19*	*<0.01 ****
B: Most Naked Shorted	0.165	-0.288			-0.124	-0.008	-0.084
150 securities	*<0.01 ****	*<0.01 ****			*<0.01 ****	*0.79*	*<0.01 ****
A: Overall			-0.069		-0.070	-0.015	-0.046
1,418 Securities			*<0.01****		*0.02 ***	*0.24*	*<0.01 ****
B: Most Naked Shorted			-0.159		-0.138	-0.005	-0.083
150 securities			*<0.01****		*<0.01 ****	*0.88*	*<0.01 ****
A: Overall				~-0.001	-0.064	-0.014	-0.046
1,418 Securities				*0.86*	*<0.01 ****	*0.36*	*<0.01 ****
B: Most Naked Shorted				-0.008	-0.131	-0.010	-0.087
150 securities				*0.28*	*<0.01 ****	*0.73*	*<0.01 ****

Panel B - Impact of Covered Short Selling

Sample	Response – Change						
	PE	*PE* (incremental effect when lag *PE* > 0)	*PE Volatility*	*Price*	*Volatility*	*Spread*	*OIB*
A: Overall	0.590	-1.494			-0.996	-0.237	-0.819
1,402 Securities	*<0.01 ****	*<0.01 ****			*0.01 ***	*0.19*	*<0.01 ****
B: Most Naked Shorted	1.006	-1.739			-1.230	-0.072	-0.758
150 securities	*<0.01 ****	*<0.01 ****			*<0.01 ****	*0.37*	*<0.01 ****
A: Overall			-0.927		-1.040	-0.222	-0.808
1,418 Securities			*<0.01 ****		*<0.01 ****	*<0.01 ****	*<0.01 ****
B: Most Naked Shorted			-1.027		-1.290	-0.065	-0.754
150 securities			*<0.01 ****		*<0.01 ****	*0.43*	*<0.01 ****
A: Overall				-0.023	-0.991	-0.219	-0.811
1,418 Securities				*<0.01 ****	*<0.01 ****	*<0.01 ****	*<0.01 ****
B: Most Naked Shorted				-0.025	-1.254	-0.081	-0.765
150 securities				*0.21*	*<0.01 ****	*0.30*	*<0.01 ****

Panel C: Impact of a Change in *Outstanding Naked Short Ratio* **(*ONSR)* equal to 10 Basis Points of the number of outstanding shares**

	Overall sample			Most Naked Shorted Sample		
Response Variable	Model 1 (*PE*)	Model 2 (*PE Volatility*)	Model 3 (*Price*)	Model 1 (*PE*)	Model 2 (*PE Volatility*)	Model 3 (*Price*)
Positive PE	**-9.81%**			**-6.74%**		
PE Volatility		**-12.92%**			**-9.43%**	
Price			-0.01%			-0.04%
Volatility	**-1.04%**	**-1.14%**	**-1.04%**	**-0.84%**	**-0.94%**	**-0.89%**
Spread	-1.03%	-0.99%	-0.92%	-0.14%	-0.08%	-0.17%
OIB	**-1.75%**	**-1.72%**	**-1.73%**	**-1.80%**	**-1.79%**	**-1.87%**

Panel D: Impact of a Change in *Outstanding Covered Short Ratio* **(*OCSR)* equal to 10 Basis Points of the number of outstanding shares**

	Overall sample			Most Naked Shorted Sample		
Response Variable	Model 1 (*PE*)	Model 2 (*PE Volatility*)	Model 3 (*Price*)	Model 1 (*PE*)	Model 2 (*PE Volatility*)	Model 3 (*Price*)
Positive PE	**-4.90%**			**-2.98%**		
PE Volatility		**-7.37%**			**-4.53%**	
Price			**-0.01%**			**-0.01%**
Volatility	**-0.69%**	**-0.72%**	**-0.69%**	**-0.62%**	**-0.65%**	**-0.63%**
Spread	**-0.65%**	**-0.61%**	**-0.60%**	-0.09%	-0.09%	-0.11%
OIB	**-1.31%**	**-1.30%**	**-1.30%**	**-1.20%**	**-1.20%**	**-1.22%**

Table 15. Impact of Naked Short-Selling for Securities with High/Low Proportion of Naked Short-Selling by Public Traders relative to Market Makers
This table provides results for the estimation of three vector autoregressive models of order one: VAR with *Pricing Error* (Model 1), VAR with *Pricing Error Volatility* (Model 2) and VAR with *Price* (Model 3), as described in Table VI. All variables are standardized and winsorized by security, and are as defined in Table 9. The sample is built as follows: we include all NYSE common stock of US-based firms (CRSP share codes 10 and 11) available in the CRSP and TAQ databases listed for at least 18 months over the interval January 1, 2005 to June 30, 2008, and with no large changes (>10%) in the number of shares outstanding. The resulting sample includes 1,492 securities. Mean *ONSR* is computed for all securities included in the sample for the period preceding the introduction of Rule 204T (January 1, 2005 to June 30, 2008) and for a period following (January 1, 2009 to December 31, 2010). Securities are ranked according to the proportional change in Mean *ONSR*. Securities are then allocated to three quantiles – those with the highest decline in Mean *ONSR* are allocated to the "High Proportion of Public Traders" sample, while those with the lowest decline in Mean *ONSR* are allocated to the "Low Proportion of Public Traders" sample. Reported parameter estimates are averages of parameters estimated by security. Significance is tested employing a cross-sectional estimate of the standard error of the parameter estimate, as in Chordia, Roll and Subrahmanyam (2000). p-values from two-sided t-tests are in italics below the parameter estimates. "*", "**", and "***" indicate significance at the 10%, 5% and 1% level respectively.

	Response – Change						
Sample	*PE*	*PE* (incremental effect when lag *PE* > 0)	*PE Volatility*	*Price*	*Volatility*	*Spread*	*OIB*
A: Low Proportion of Public Traders	0.028	-0.125			-0.041	-0.009	-0.027
338 Securities	*0.24*	*0.34*			*<0.01 ****	*0.45*	*0.04 ***
B: High Proportion of Public Traders	0.054	-0.117			-0.073	-0.020	-0.063
345 Securities	*0.03 ***	*<0.01 ****			*<0.01 ****	*0.09 ***	*<0.01 ****
A: Low Proportion of Public Traders			-0.038		-0.045	-0.010	-0.027
345 Securities			*<0.01 ****		*<0.01 ****	*0.41*	*0.06 **
B: High Proportion of Public Traders			-0.073		-0.079	-0.020	-0.063
352 Securities			*<0.01 ****		*<0.01 ****	*0.10*	*<0.01 ****
A: Low Proportion of Public Traders				-0.001	-0.043	-0.011	-0.029
345 Securities				*0.79*	*<0.01 ****	*0.32*	*0.07 **
B: High Proportion of Public Traders				-0.003	-0.076	-0.022	-0.061
352 Securities				*0.19*	*<0.01 ****	*0.07 **	*<0.01 ****

Table 16. The Impact of Restrictions on Naked Short Selling imposed by the SEC between July 21, 2008 and August 12, 2008

The following table presents parameter estimates and related two-sided p-values (in italics, grey font) from five OLS regressions, one for each variable of interest: *ONSR*, *Pricing Error Volatility*, *Price*, *Volatility*, *Spread* and *Order Imbalance*. All variables are computed daily over the interval January 1, 2008 to September 9, 2008. In each k^{th} ($1 <= k <= 6$) regression, the response variable is the mean value of the k^{th} variable of interest for the sample of the 17 stocks that were subject to restrictions on naked shorting. The ban affected 19 securities, but we have data for 17 of these: Bank of America Corporation, Barclays, Bear Stearns Companies Inc., Citigroup Inc., Credit Suisse Group, Deutsche Bank Group AG, Allianz SE, Goldman, Sachs Group Inc, Royal Bank ADS, HSBC Holdings PLC ADS, J. P. Morgan Chase & Co., Merrill Lynch & Co., Inc., Mizuho Financial Group, Inc., Morgan Stanley, UBS AG, Freddie Mac, and Fannie Mae. Explanatory variables include, in each regression, an intercept, the mean value of the k^{th} variable of interest for the control sample, *Control*, and a binary variable, *Event*, equal to 1 between July 21, 2008 and August 12, 2008. All variables are standardized and winsorized by security, and are as defined in Table 9. "*", "**", and "***" indicate significance at the 10%, 5% and 1% level respectively. The OLS regression equation is as follows:

$$Response\,Variable_{k,t} = \alpha_k + \beta_{1,k} Event + \beta_{2,k} Control_{k,t} + \varepsilon_{k,t}$$

Predictors	*ONSR*	*PE Volatility*	*Return*	*Volatility*	*Spread*	*OIB*
Intercept	0.001	0.197	-0.002	-0.038	-0.001	0.017
	*<0.01 ****	*<0.01 ****	*0.16*	*<0.01 ****	*<0.01 ****	*<0.01 ****
Control	2.014	0.625	1.179	1.353	0.941	0.496
	*<0.01 ****	*0.33*	*<0.01 ****	*<0.01 ****	*<0.01 ****	*<0.01 ****
Event	-0.001	0.074	-0.006	0.007	~0.001	-0.017
	*<0.01 ****	*<0.01 ****	*0.11*	*<0.01 ****	*0.10 **	*0.11*

Table 17. Behavior of the Outstanding Naked Short Ratio (ONSR) of Bear Stearns (Ticker: BSC) and of Lehman Brothers Holdings Inc. (Ticker: LEH) in 2008.

ONSR is computed as the ratio of our estimate of outstanding fails to deliver and shares outstanding. *Index ONSR* is calculated as the equal weighted average of *ONSR* of common stock of 4 firms that are matched on primary SIC and market capitalization as of the end of the fiscal year 2007 to BSC in Panel A and to LEH in Panel B. We construct a t-statistic using the mean and standard error of the *ONSR* difference over the time interval January 1, 2008 to February 15, 2008 for BSC and over the time interval January 1, 2008 and ending 20 trading days prior to September 9, 2008 for LEH; p-values are two-sided. "*", "**", and "***" indicate significance at the 10%, 5% and 1% level respectively.

Panel A: Bear Stearns

Date	*BSC Stock Price*	*BSC ONSR*	*Index ONSR*	*Difference in ONSR*	p-value	
3/3/2008	77.32	0.30%	<0.01	0.30%	0.21	
3/4/2008	77.17	0.14%	<0.01	0.14%	<0.01	***
3/5/2008	75.78	0.14%	0.02%	0.12%	<0.01	***
3/6/2008	69.9	0.24%	0.02%	0.22%	<0.01	***
3/7/2008	70.08	0.12%	0.02%	0.10%	<0.01	***
3/10/2008	62.3	0.12%	0.02%	0.10%	<0.01	***
3/11/2008	62.97	0.28%	<0.01	0.28%	0.16	
3/12/2008	61.58	1.16%	0.06%	1.10%	<0.01	***
3/13/2008	57	1.06%	0.06%	1.00%	<0.01	***
3/14/2008	30	2.24%	0.08%	2.16%	<0.01	***
3/17/2008	4.81	12.18%	0.08%	12.10%	<0.01	***
3/18/2008	5.91	11.74%	0.04%	11.70%	<0.01	***
3/19/2008	5.33	11.74%	0.04%	11.70%	<0.01	***
3/20/2008	5.96	11.68%	0.08%	11.60%	<0.01	***
3/24/2008	11.25	12.26%	0.04%	12.22%	<0.01	***
3/25/2008	10.94	14.38%	0.08%	14.30%	<0.01	***
3/26/2008	11.21	10.92%	0.08%	10.84%	<0.01	***
3/27/2008	11.23	11.68%	0.06%	11.62%	<0.01	***
3/28/2008	10.78	12.36%	0.06%	12.30%	<0.01	***
3/29/2008	10.49	12.36%	0.06%	12.30%	<0.01	***

Panel B: Lehman Brothers Holdings Inc.

Date	*LEH Price*	*LEH ONSR*	*Index ONSR*	*Difference in ONSR*	p-value	
8/25/2008	13.45	0.31%	<0.01	0.31%	<0.01	***
8/26/2008	14.03	0.16%	<0.01	0.16%	<0.01	***
8/27/2008	14.78	0.16%	<0.01	0.16%	<0.01	***
8/28/2008	15.87	0.02%	<0.01	0.02%	<0.01	***
8/29/2008	16.09	0.02%	<0.01	0.02%	<0.01	***
9/2/2008	16.13	0.02%	<0.01	0.02%	<0.01	***
9/3/2008	16.94	0.02%	<0.01	0.02%	<0.01	***
9/4/2008	15.17	0.01%	<0.01	0.01%	<0.01	***
9/5/2008	16.20	0.01%	0.02%	-0.01%	<0.01	***
9/8/2008	14.15	0.01%	0.02%	-0.01%	<0.01	***
9/9/2008	7.79	0.16%	0.03%	0.13%	<0.01	***
9/10/2008	7.25	0.85%	0.04%	0.81%	<0.01	***
9/11/2008	4.22	3.29%	0.04%	3.25%	<0.01	***
9/12/2008	3.65	4.86%	0.03%	4.83%	<0.01	***
9/15/2008	0.21	4.86%	0.05%	4.81%	<0.01	***
9/16/2008	0.30	5.21%	0.16%	5.05%	<0.01	***
9/17/2008	0.13	8.16%	0.18%	7.98%	<0.01	***
9/18/2008						
9/19/2008			DELISTED			

Table 18. Naked Short Selling around Credit Rating Downgrades and Large Price Drops

Panel A: Naked Short Selling around Credit Rating Downgrades

We analyze long-term issuer credit rating downgrades by S&P over the year 2008 for 17 financial firms: Bank of America Corporation, Barclays, Bear Stearns Companies Inc., Citigroup Inc., Credit Suisse Group, Deutsche Bank Group AG, Allianz SE, Goldman, Sachs Group Inc, Royal Bank ADS, HSBC Holdings PLC ADS, J. P. Morgan Chase & Co., Merrill Lynch & Co., Inc., Mizuho Financial Group, Inc., Morgan Stanley, UBS AG, Freddie Mac, and Fannie Mae. We compute *ONSR* for each firm's common stock (when the primary exchange is not in the US, we use the corresponding ADR). In all, we identify 21 downgrades, and define day 0 as the day of the downgrade. We compute abnormal daily *ONSR* by subtracting the *Mean ONSR* from daily *ONSR*. *Mean ONSR* is computed over 100 trading days ending 20 days prior to the credit rating downgrade. We report results for various event windows. *Cumulative Abnormal ONSR* is the sum of daily *ONSR* for all days in the event window. The significance of the mean is computed making use of the historic estimate of the standard error (computed over the estimation period of 100 trading days ending 20 days prior to the credit rating downgrade), adjusted for date clustering. p-values are reported next to the estimates "*", "**", and "***" indicate significance at the 10%, 5% and 1% level respectively.

Event Window	***N***	***Mean Cumulative Abnormal ONSR***	**p-value**	
(-20,-1)	21	-0.36%	0.06	*
(-10,-1)	21	-0.27%	0.05	**
(-5,-1)	21	-0.15%	0.11	
(0,0)	21	-0.02%	-0.57	
(+1,+5)	21	0.32%	<0.01	***
(+1,+10)	21	0.57%	<0.01	***
(+1,+20)	21	0.67%	<0.01	***

Panel B: Naked Short Selling around Large Drops in Returns

The sample includes all NYSE common stock of US-based firms (CRSP share codes 10 and 11) included in the CRSP and TAQ databases over the entire interval January 1, 2008 to July 20, 2008, with no large changes (>10%) in the number of shares outstanding during the same period. For each security, we compute daily returns over the entire period and estimate the mean and standard deviation of the daily stock price return. We standardize daily returns by subtracting the mean return and by dividing by the standard deviation of the security return. We identify days with large abnormal negative stock price returns as security-days for which the standardized return is less than -2. We obtain a sample of 83 security-days with extreme negative return and we refer to those as 'event days'. For each security-day in the interval between day -20 and day +20 (where day 0 is the 'event day'), we compute daily *Abnormal ONSR*, by subtracting *Mean ONSR*, estimated over a split interval containing the 50 trading days ending 21 trading days prior to the identified event date and the 50 trading days starting 21 trading days after the identified event date. We obtain *Mean Abnormal ONSR* by averaging *Abnormal ONSR* across securities. We then cumulate *Mean Abnormal ONSR* over various event windows. We compute *Leverage* as the ratio of *Long Term Debt* to *Total Asset,* rank securities on *Leverage* and assign those with leverage below the sample median to a 'low leverage' group and those with leverage above the sample median to a 'high leverage' group. We test for significance using a Brown-Warner (1980, 1985) adjustment in the computation of standard errors, to account for the clustering of event dates. "*", "**", and "***" indicate significance at the 10%, 5% and 1% level respectively.

Event Window	**All Companies**				**High Leverage Companies**				**Low Leverage Companies**			
	N	***Mean Cumulative Abnormal ONSR***	**p-value**		*N*	***Mean Cumulative Abnormal ONSR***	**p-value**		*N*	***Mean Cumulative Abnormal ONSR***	**p-value**	
(-20,-1)	83	-1.90%	<0.01	***	41	-2.48%	<0.01	***	42	-1.32%	<0.01	***
(-10,-1)	83	-1.25%	<0.01	***	41	-1.70%	<0.01	***	42	-0.81%	<0.01	***
(-5,-1)	83	-0.79%	<0.01	***	41	-1.20%	<0.01	***	42	-0.39%	0.02	**
(0,0)	84	0.03%	0.60		42	0.01%	0.88		42	0.05%	0.53	
(+1,+5)	85	0.35%	<0.01	***	43	0.34%	0.04	**	42	0.36%	0.03	**
(+1,+10)	85	0.79%	<0.01	***	43	0.95%	<0.01	***	42	0.63%	<0.01	***
(+1,+20)	85	0.78%	<0.01	***	43	1.08%	<0.01	***	42	0.47%	0.06	

Table 19. Attributes of Corporate Hedging Announcements

This table describes the different attributes of hedging announcements. An event/announcement is categorized as *Contaminated* (*uncontaminated*) when there are other (no other) firm-specific news items between event days -1 and +1. An event is categorized as *Market View* when the firm making the hedging-related announcement explicitly claims that the change in hedging policy is a result of its expectations about future gold prices. Events are categorized as *Bank Loans* when the change in hedging policy is a consequence of a loan-related transaction. Events are categorized as *Ex ante* (*Ex post*) when the change in hedging policy is announced in advance of (subsequent to) its implementation.

	Hedging Increases	Hedging Decreases	Total
Panel A: Contaminated vs. Uncontaminated			
Contaminated	24	66	90
Uncontaminated	14	49	63
Total	38	115	153
Panel B: Reason for Change in Hedging Policy			
Market View	24	88	112
Bank Loans	6	10	16
Others	8	17	25
Panel C: Timing of the Change in Hedging Policy relative to Announcement			
Ex ante	14	52	66
Ex post	24	63	87

Table 20. Descriptive Statistics of Firm and Event Characteristics

This table presents summary statistics of firm and event characteristics. Firm Size is the log of the total assets in millions of US$. Leverage is the ratio of long-term debt and total assets. Quick Ratio is the ratio of current assets and current liabilities. Tax Savings is the ratio of tax loss carry forwards and total assets. Information Asymmetry is the percentage error in analyst forecasts. Institutional Ownership is the ratio of total number of shares owned by institutions and total number of shares outstanding. Hedge Quantity Change is the change in the log of ounces of gold being hedged. Hedge Proportion Change is the ratio of Hedge Quantity Change and the following year's annual production.

	Mean	Median	Std	N
Panel A: Firm Characteristics				
Size	7.692	8.101	1.273	133
Leverage	0.154	0.125	0.120	133
Quick Ratio	3.073	1.805	5.554	150
Tax Savings	0.052	0.037	0.066	133
Information Asymmetry	0.123	0.125	0.070	115
Institutional Ownership	0.317	0.237	0.269	142
Panel B: Event Characteristics				
Hedge Quantity Change	13.541	13.542	1.183	116
Hedge Proportion Change	0.667	0.164	1.614	116

Table 21. Effect of Corporate Hedging Announcements on Gold Returns

Panel A reports the mean cumulative abnormal gold (spot) returns for all corporate announcements regarding a reduction in hedging. Day 0 represents the day a reduction in hedging was announced. Abnormal returns are calculated using the mean-adjusted model. Panel B reports the mean cumulative abnormal gold (spot) returns for all corporate announcements regarding an increase in hedging. Day 0 represents the day an increase in hedging was announced. CDA is the Brown and Warner test statistic that accounts for cross-sectional dependence of abnormal returns and Generalized Sign Z is the non-parametric test statistic. The symbols *,**, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively, using a 2-tailed test.

Panel A: Gold Market Reactions to Corporate Announcements of Decreases in Hedging				
Days	N	Mean CAR	CDA	Generalized Sign Z
(0,0)	**115**	**0.48%**	**5.780*** **	**3.564*** **
(-10,-1)	115	0.45%	1.700*	0.42
(-5,-1)	115	0.28%	1.485	0.05
(-1,+1)	115	0.63%	4.354***	3.009**
(+1,+5)	115	0.58%	3.123**	0.605
(+1,+10)	115	0.59%	2.250**	0.975

Panel B: Gold Market Reactions to Corporate Announcements of Increases in Hedging				
Days	N	Mean CAR	CDA	Generalized Sign Z
(0,0)	**38**	**-0.10%**	**-0.682**	**-0.527**
(-10,-1)	38	2.16%	4.724***	0.149
(-5,-1)	38	0.80%	2.490**	0.149
(-1,+1)	38	0.27%	1.098	0.825
(+1,+5)	38	-0.32%	-0.991	-2.556**
(+1,+10)	38	-1.41%	-3.084***	-1.542

Table 22. Gold Abnormal Returns and Event Characteristics

This table reports the results of a cross-sectional regression conducted to analyze the relation between gold (spot) abnormal returns and different attributes of the hedging related announcement (event). The dependent variable is the event-day gold (spot) abnormal return. Big Five is a binary variable equal to 1 for the 5 largest companies in the sample. Hedge Quantity Change is the change in the log of ounces of gold being hedged. Hedge Proportion Change is the ratio of Hedge Quantity Change and the following year's annual production. Market View is a binary variable equal to 1 when the firm making the hedging related announcement explicitly claims that the change in hedging policy is a result of its expectations about future gold prices. Ex post is a binary variable equal to 1 when an announcement is made after the gold is sold. Inc_Dec is a binary variable equal to 1 when the announcement is related to an increase in hedging. CAR_Pre_Gold is the 20-day historical CAR (between event days -20 and -1) in the gold market. The p-values are adjusted for heteroskedasticity and are presented in parentheses below the respective coefficients. The symbols *, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively, using a 2-tailed test.

Variable	Model 1	Model 2	Model 3
C	0.0021	0.0026	0.0025
	(0.6131)	(0.4512)	(0.3176)
Big Five	-0.0021	-0.0032	-0.0026
	(0.3749)	(0.1991)	(0.2218)
Market View	-0.0049	-0.0050	-0.0052
	(0.0116)**	(0.0747)*	(0.0055)***
Hedge Quantity Change	-0.0001		
	(0.9310)		
Hedge Proportion Change		-0.0005	
		(0.4266)	
CAR_Pre_Gold	-0.0187	-0.0189	-0.0323
	(0.3823)	(0.3097)	(0.0501)*
Inc_Dec	0.0014	0.0007	0.0019
	(0.4388)	(0.8141)	(0.2327)
Ex post	0.0007	0.0012	0.0005
	(0.7677)	(0.6077)	(0.8094)
Uncontaminated	-0.0035	-0.0023	-0.0038
	(0.1723)	(0.6077)	(0.1064)
Adjusted R-Squared	0.0511	0.0295	0.0806
N	118	111	153

Table 23. Central Bank Announcements

Panel A presents summary statistics of central bank announcements. Quantity is the log of the total ounces of gold sold. Panel B reports the mean abnormal gold (spot) returns for all central bank announcements regarding gold selling. Day 0 represents the day the gold sale was announced. Abnormal returns are calculated using the mean-adjusted model. Day 0 represents the day a reduction in hedging was announced. CDA is the Brown and Warner test statistic that accounts for cross-sectional dependence of abnormal returns and Generalized Sign Z is the non-parametric test statistic. The symbols *, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively, using a 2-tailed test.

Panel A: Descriptive Statistics of Central Bank Announcements

Country/Central Bank	Mean Quantity	Median Quantity	N
Argentina	15.194	15.194	1
Australia	15.491	15.491	1
Belgium	15.304	15.684	4
Canada	11.430	11.456	56
European Central Bank	13.264	13.264	1
France	16.770	16.770	2
Germany	16.770	16.770	1
IMF	16.453	16.453	1
Russia	14.325	13.996	8
Spain	13.712	13.706	3
Sweden	12.676	12.676	2
Swiss	17.019	17.544	3
UK	13.781	13.592	20

Panel B: Effect of Central Bank Announcements on Gold Returns

Days	N	Mean CAR	CDA	Generalized Sign Z
(0,0)	**103**	**-0.12%**	**-1.381**	**-1.253**
(-10,-1)	103	-0.08%	-0.295	-1.45
(-5,-1)	103	0.07%	0.346	-1.056
(-1,+1)	103	-0.13%	-0.853	-0.268
(+1,+5)	103	0.10%	0.524	0.717
(+1,+10)	103	0.25%	0.942	-0.662

Table 24. Effect of Hedging Announcements on Equity Returns

Panels A and B report mean cumulative abnormal stock returns for all announcements of a decrease in hedging. Panel C and D report mean cumulative abnormal stock returns for all announcements of an increase in hedging. In Panels A and C, the abnormal returns are calculated using a two-factor model which includes commodity returns (gold, spot) along with the standard equity market returns. In Panels B and D, the abnormal returns are calculated using a five-factor model which adds the Fama French factors and the momentum factor to the aforementioned two-factor model. CDA is the Brown and Warner test statistic that accounts for cross-sectional dependence of abnormal returns and Generalized Sign Z is the non-parametric test statistic value. The symbols *, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively, using a 2-tailed test.

Panel A: Equity Market Reactions to Corporate Announcements of Decreases in Hedging (Two-Factor Model)				
Days	N	Mean CAR	CDA	Generalized Sign Z
(0, 0)	**115**	**1.46%**	**3.592*** **	**4.143*** **
(0,+1)	115	1.19%	2.067**	4.143***
(-10,-1)	114	0.12%	0.091	0.303
(-5,-1)	115	0.84%	0.917	1.422
(-1,+1)	115	0.72%	1.017	2.644***
(+1,+5)	115	-0.76%	-0.833	0.303
(+1,+10)	115	0.32%	0.251	1.795*
Panel B: Equity Market Reactions to Corporate Announcements of Decreases in Hedging (Five-Factor Model)				
Days	N	Mean CAR	CDA	Generalized Sign Z
(0, 0)	**115**	**1.45%**	**3.622*** **	**4.116*** **
(0,+1)	115	1.12%	1.966**	3.741***
(-10,-1)	114	-0.13%	-0.100	0.462
(-5,-1)	115	0.70%	0.776	1.582
(-1,+1)	115	0.65%	0.942	2.992**
(+1,+5)	115	-0.63%	-0.699	0.649
(+1,+10)	115	0.60%	0.470	2.141**
Panel C:Equity Market Reactions to Corporate Announcements of Increases in Hedging (Two-Factor Model)				
Days	N	Mean CAR	CDA	Generalized Sign Z
(0, 0)	**38**	**-2.05%**	**-3.045** **	**-2.293** **
(0,+1)	38	-1.51%	-1.583	-1.968**
(-10,-1)	38	-0.37%	-0.173	-0.344
(-5,-1)	38	-0.69%	-0.460	-0.019
(-1,+1)	38	-2.17%	-1.860*	-1.643
(+1,+5)	38	2.27%	1.510	0.955
(+1,+10)	38	1.83%	0.861	-0.019
Panel D:Equity Market Reactions to Corporate Announcements of Increases in Hedging (Five-Factor Model)				
Days	N	Mean CAR	CDA	Generalized Sign Z
(0, 0)	**38**	**-2.08%**	**-3.121*** **	**-2.310** **
(0,+1)	38	-1.66%	-1.759*	-2.310**
(-10,-1)	38	-0.21%	-0.100	-0.361
(-5,-1)	38	-0.78%	-0.527	-0.361
(-1,+1)	38	-2.41%	-2.087**	-1.661*
(+1,+5)	38	1.89%	1.267	0.613
(+1,+10)	38	1.52%	0.720	0.288

Table 25. Equity Abnormal Returns, and Event and Firm Characteristics (Two-Factor Model)

This table reports the results of a cross-sectional regression conducted to analyze the relation between equity abnormal returns and different attributes of the hedging related announcement (event) and hedging related firm. The dependent variable is the event-day equity abnormal return. *CAR_Pre_Gold* is the 20-day historical CAR (between event days -20 and -1) in the gold market. *CAR_Pre_Firm* is the 20-day historical CAR for the firm making the announcement. *Hedge Quantity Change* is the change in the log of ounces of gold being hedged. *Hedge Proportion Change* is the ratio of Hedge Quantity Change and the following year's annual production. *Market View* is a dummy equal to 1 when the firm making the hedging related announcement explicitly claims that the change in hedging policy is a result of its expectations about future gold prices. *Big Five* is a binary variable equal to 1 for the 5 largest companies in the sample. *Size* is the log of total assets of the firm (in millions) making the announcement. *Info_Asy* is the percentage error in analysts' forecasts. *Distress* is the ratio of long-term debt to firm size. *Tax Savings* is the ratio of deferred taxes and firm size. *Uncontaminated* is a dummy variable equal to 1 for uncontaminated events. *Inc_Dec* is a dummy variable equal to 1 when the announcement is related to an increase in hedging. The p-values are adjusted for heteroskedasticity and are presented in parentheses below the respective coefficients. The symbols *, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively, using a 2-tailed test.

Variable	Model 1	Model 2	Model 3	Model 4
C	0.008	-0.047	0.041	0.016
	(0.5906)	(0.1007)	(0.0445)**	(0.7802)
CAR_Pre_Gold	0.276	0.265	0.330	0.297
	(<0.01)***	(<0.01)***	(<0.01)***	(<0.01)***
CAR_Pre_Firm	0.095	0.092	0.051	0.010
	(0.0114)**	(0.0143)**	(0.2775)	(0.8366)
Hedge Quantity Change			-0.003	0.000
			(0.5580)	(0.9752)
Hedge Proportion Change			0.000	0.000
			(0.9471)	(0.9839)
Market View	-0.021	-0.024	-0.036	-0.020
	(0.0299)**	(0.0151)**	(<0.01)***	(0.1202)
Big Five	0.018		0.020	
	(0.0499)**		(0.1214)	
Size		0.009		0.002
		(0.0156)**		(0.6722)
Info_Asy				0.004
				(0.8062)
Distress	-0.096	-0.090	-0.145	-0.206
	(<0.01)***	(0.0113)**	(<0.01)***	(<0.01)***
Tax Savings	0.008	-0.002	-0.016	0.144
	(0.9089)	(0.9770)	(0.7161)	(0.1490)
Uncontaminated	-0.014	-0.013	-0.017	0.013
	(0.1080)	(0.1258)	(0.1455)	(0.2680)
Inc_Dec	-0.001	0.001	-0.008	0.030
	(0.9206)	(0.9479)	(0.5183)	(0.0406)
Adjusted R-Squared	0.244	0.256	0.392	0.452
N	132	132	92	67

Table 26. Equity Abnormal Returns, and Event and Firm Characteristics (Five-Factor Model)

This table reports the results of a cross-sectional regression conducted to analyze the relation between equity abnormal returns and different attributes of the hedging related announcement (event) and hedging related firm. The dependent variable is the event-day equity abnormal return. *CAR_Pre_Gold* is the 20-day historical CAR (between event days -20 and -1) in the gold market. *CAR_Pre_Firm* is the 20-day historical CAR for the firm making the announcement. *Hedge Quantity Change* is the change in the log of ounces of gold being hedged. *Hedge Proportion Change* is the ratio of Hedge Quantity Change and the following year's annual production. *Market View* is a dummy equal to 1 when the firm making the hedging related announcement explicitly claims that the change in hedging policy is a result of its expectations about future gold prices. *Big Five* is a binary variable equal to 1 for the 5 largest companies in the sample. *Size* is the log of total assets of the firm (in millions) making the announcement. *Info_Asy* is the percentage error in analysts' forecasts. *Distress* is the ratio of long-term debt to firm size. *Tax Savings* is the ratio of deferred taxes and firm size. *Uncontaminated* is a dummy variable equal to 1 for uncontaminated events. *Inc_Dec* is a dummy variable equal to 1 when the announcement is related to an increase in hedging. The p-values are adjusted for heteroskedasticity and are presented in parentheses below the respective coefficients. The symbols *, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively, using a 2-tailed test.

Variable	Model 1	Model 2	Model 3	Model 4
C	0.009	-0.050	0.031	0.000
	(0.466)	(0.085)	(0.599)	(0.9972)
CAR_Pre_Gold	0.255	0.243	0.304	0.281
	(<0.01)***	(<0.01)***	(<0.01)***	(<0.01)***
CAR_Pre_Firm	0.091	0.086	0.021	-0.017
	(0.017)**	(0.023)**	(0.646)	(0.7358)
Hedge Quantity Change			0.000	0.001
			(0.939)	(0.8207)
Hedge Proportion Change			0.000	0.000
			(0.999)	(0.9676)
Market View	-0.024	-0.0264	-0.033	-0.020
	(0.013)**	(<0.01)***	(<0.01)***	(0.1215)
Big Five	0.020		0.017	
	(0.034)**		(0.169)	
Size		0.009		0.002
		(0.01)***		(0.6698)
Info_Asy				0.002
				(0.8782)
Distress	-0.092	-0.035	-0.115	-0.192
	(0.011)**	(0.015)**	(<0.01)***	(<0.01)***
Tax Savings	-0.001	-0.066	0.002	0.152
	(0.989)	(0.884)	(0.974)	(0.1305)
Uncontaminated	-0.013	-0.009	-0.015	0.012
	(0.138)	(0.165)	(0.154)	(0.2873)
Inc_Dec	-0.004	-0.002	-0.012	0.033
	(0.723)	(0.873)	(0.286)	(0.0276)**
Adjusted R-Squared	0.227	0.240	0.280	0.423
N	132	132	92	67

Table 27. Industry Effects of Corporate Hedging Announcements

Panels A and B report the mean cumulative abnormal stock returns of all companies in the gold mining industry (SIC=1041) (excluding announcing firms) for all corporate announcements regarding a decrease in hedging and Panels C and D report the same for increases in hedging. Day 0 represents the day of the hedging announcement. In Panels A and C, the abnormal returns are calculated using a two-factor model which includes commodity returns (gold, spot) along with the standard equity market returns. In Panels B and D, the abnormal returns are calculated using a five-factor model which adds the Fama French factors and the momentum factor to the aforementioned two-factor model. CDA is the Brown and Warner test statistic that accounts for cross-sectional dependence of abnormal returns and Generalized Sign Z is the non-parametric test statistic value. The symbols *, **, and *** denote significance at the 0.10, 0.05 and 0.01 levels, respectively, using a 2-tailed test.

Panel A: Industry Reactions to Corporate Announcements of Decreases in Hedging (Two-Factor Model)				
Days	N	Mean CAR	CDA	Generalized Sign Z
(0, 0)	**3949**	**0.53%**	**3.654*** **	**4.541*** **
(0,+1)	3949	0.55%	2.694**	4.797***
(-10,-1)	3947	0.52%	1.129	2.088**
(-5,-1)	3949	1.01%	3.110**	4.908***
(-1,+1)	3949	0.96%	3.816***	7.028***
(+1,+5)	3949	-0.04%	-0.116	0.464
(+1,+10)	3949	0.33%	0.712	4.447***
Panel B: Industry Reactions to Corporate Announcements of Decreases in Hedging (Five-Factor Model)				
Days	N	Mean CAR	CDA	Generalized Sign Z
(0, 0)	**3949**	**0.56%**	**3.958*** **	**4.536*** **
(0,+1)	3949	0.59%	2.958***	3.994***
(-10,-1)	3947	0.65%	1.464	3.102***
(-5,-1)	3949	1.09%	3.471***	6.273***
(-1,+1)	3949	0.97%	3.983***	7.149***
(+1,+5)	3949	0.20%	0.630	1.956**
(+1,+10)	3949	0.62%	1.385	5.460***
Panel C: Industry Reactions to Corporate Announcements of Increases in Hedging (Two-Factor Model)				
Days	N	Mean CAR	CDA	Generalized Sign Z
(0, 0)	**1616**	**-0.63%**	**-2.670** **	**-3.678*** **
(0,+1)	1616	-0.53%	-1.581	-2.006**
(-10,-1)	1616	-2.08%	-2.804**	-3.773***
(-5,-1)	1616	-0.98%	-1.868*	-1.009
(-1,+1)	1616	-0.65%	-1.610	-2.405*
(+1,+5)	1616	0.38%	0.717	1.832*
(+1,+10)	1616	0.83%	1.123	2.779***
Panel D: Industry Reactions to Corporate Announcements of Increases in Hedging (Five-Factor Model)				
Days	N	Mean CAR	CDA	Generalized Sign Z
(0, 0)	**1616**	**-0.62%**	**-2.734*** **	**-4.593*** **
(0,+1)	1616	-0.62%	-1.914**	-2.921***
(-10,-1)	1616	-1.67%	-2.329**	-3.044***
(-5,-1)	1616	-0.91%	-1.796*	-1.775*
(-1,+1)	1616	-0.73%	-1.861*	-3.021***
(+1,+5)	1616	0.09%	0.173	0.218
(+1,+10)	1616	0.28%	0.393	0.866

Table 28. Industry Abnormal Returns, and Event and Firm Characteristics

This table reports the results of a cross-sectional regression conducted to analyze the relation between abnormal stock returns and characteristics of all companies in the gold mining industry (SIC=1041) (excluding announcing firms), and different attributes of hedging announcements (event). *CAR_Pre_Gold* is the 20-day historical CAR (between event days -20 and -1) in the gold market. *CAR_Pre_Industry* is the 20-day historical CAR for the gold industry sample excluding the firm making the announcement. *Hedge Quantity Change* is the change in the log of ounces of gold being hedged. *Hedge Proportion Change* is the ratio of Hedge Quantity Change and the following year's annual production. *Market View* is a dummy equal to 1 when the firm making the hedging related announcement explicitly claims that the change in hedging policy is a result of its expectations about future gold prices. *Big Five* is a binary variable equal to 1 for the 5 largest companies in the sample. *Size* is the log of total assets of the firm (in millions) making the announcement. *Info_Asy* is the percentage error in analysts' forecasts. *Distress* is the ratio of long-term debt to firm size. *Uncontaminated* is a dummy variable equal to 1 for uncontaminated events. *Inc_Dec* is a dummy variable equal to 1 when the announcement is related to an increase in hedging. The p-values are adjusted for heteroskedasticity and are presented in parentheses below the respective coefficients. The symbols *, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively, using a 2-tailed test.

Variable	Model 1	Model 2	Model 3	Model 4
C	0.001	-0.002	-0.010	0.012
	(0.5580)	(0.4997)	(0.3365)	(0.3281)
CAR_Pre_Gold	0.079	0.080	0.063	0.092
	(<0.01)***	(<0.01)***	(<0.01)***	(<0.01)***
CAR_Pre_Industry	0.021	0.021	0.001	-0.009
	(<0.01)***	(<0.01)***	(0.9832)	(0.1566)
Hedge Quantity Change			-0.001	-0.001
			(0.4971)	(0.3231)
Hedge Proportion Change			-0.001	-0.001
			(0.3588)	(0.0687)*
Market_View	-0.005	-0.005	0.000	-0.001
	(0.0165)**	(0.0116)**	(0.9542)	(0.5131)
Big Five	-0.000		-0.004	
	(0.8007)		(0.0372)**	
Size		0.001		0.001
		(0.2099)		(0.8418)
Info_Asy				0.000
				(0.8028)
Distress	-0.016	-0.018	-0.005	-0.016
	(0.0164)**	(< 0.01)***	(0.4739)	(0.0760)*
Uncontaminated	-0.004	-0.004	-0.002	-0.001
	(0.0399)**	(0.047)**	(0.1793)	(0.7399)
Inc_Dec	0.000	0.000	-0.004	-0.001
	(0.9603)	(0.8485)	(0.0505)*	(0.5904)
Adjusted R-Squared	0.012	0.012	0.009	0.020
N	5098	5098	4011	2484

Table 29. Implementation of Hedging Announcements

This table presents summary statistics of changes in Total Committed Hedge (the theoretical maximum a producer might have to sell due to derivative positions) after announcements relating to hedging decreases. Quarterly Change is the change in *Total Committed Hedge* in one quarter after the announcement, expressed as a percentage of yearly production. *Yearly Change* is the change in Total Committed Hedge in one year after the announcement, expressed as a percentage of yearly production.

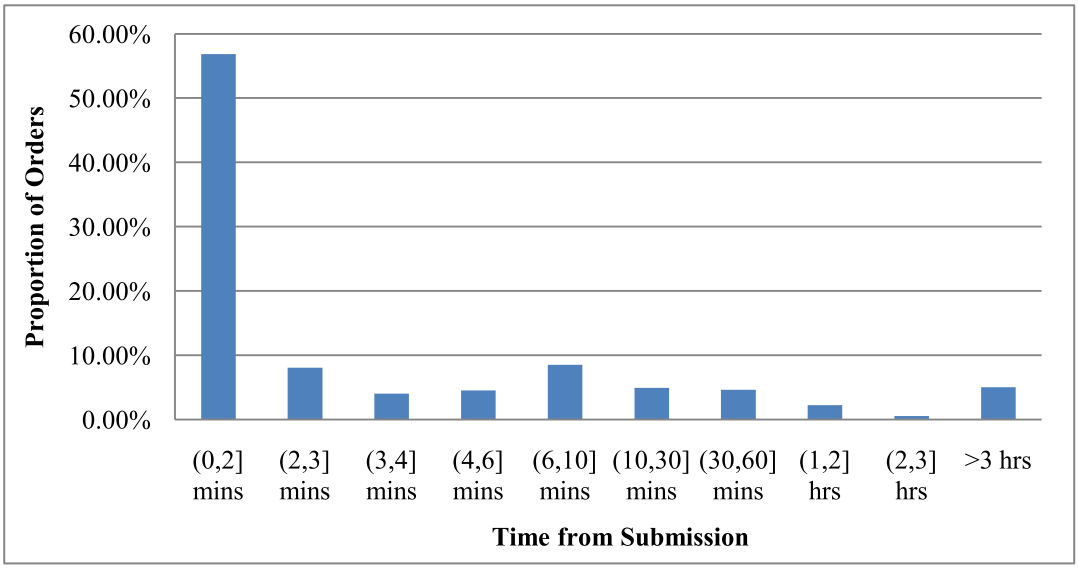
Changes in Total Committed Hedge	Mean	Median	Std	N
Quarterly Change	-10.42%	-7.46%	13.90%	34
Yearly Change	-36.52%	-29.73%	41.49%	34

Appendix B: Figures

Figure 1. Order Revisions and Time from Submission

The following graphs show the distribution of order cancellations (Panel A) and order modifications (Panel B) over time from order submission.

Panel A: Order Cancellations



Panel B: Order Modifications

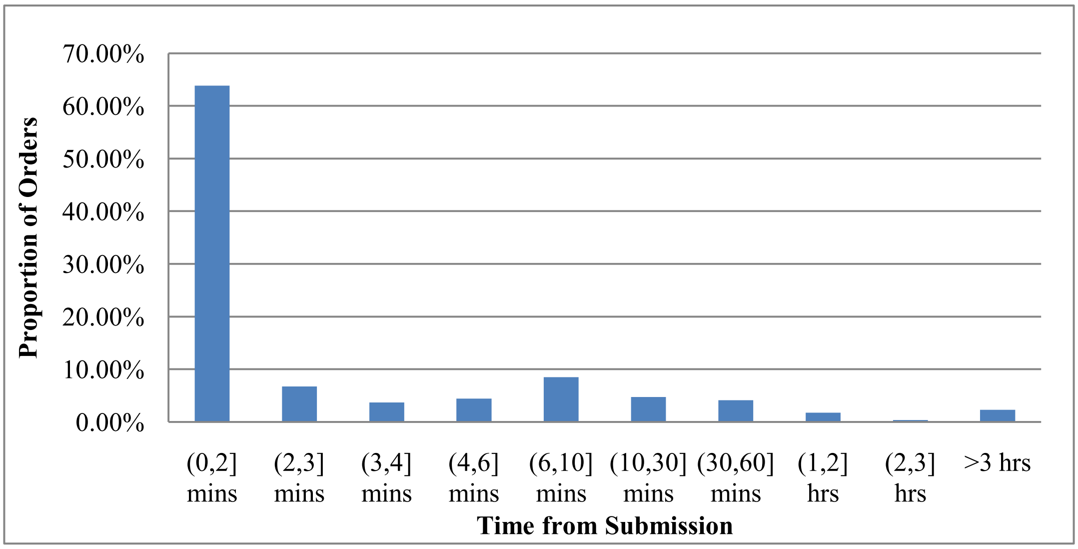
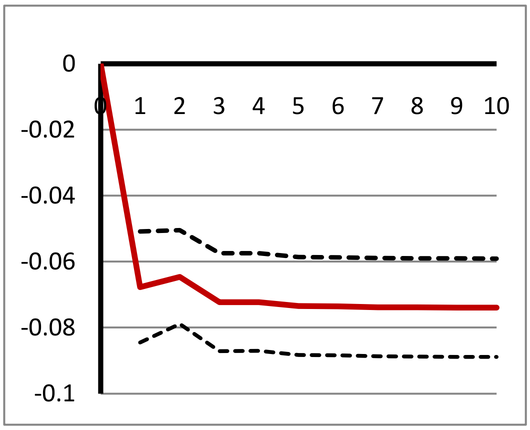


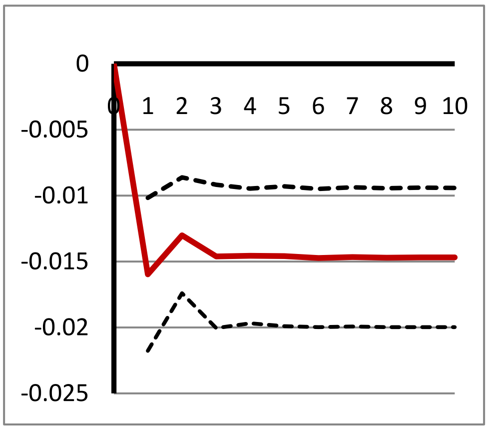
Figure 2. Impulse Response Functions, Naked Short Selling

Plots of accumulated impulse response functions depicting the impact of a one-standard deviation shock in *ONSR*. The VAR model used for estimation is Model 2 and the sample is the 'overall market', as described in Table VI. The response variables in the four panels are, respectively, *PE Volatility*, *Volatility*, *Spread* and *Order Imbalance*, as defined in Table 9. The horizontal axis are time periods (days), following the initial shock (day 0). All variables are standardized and winsorized by security. Impulse response coefficients are estimated by security; mean values are depicted. 5% confidence intervals are computed using cross-sectional standard error estimates.

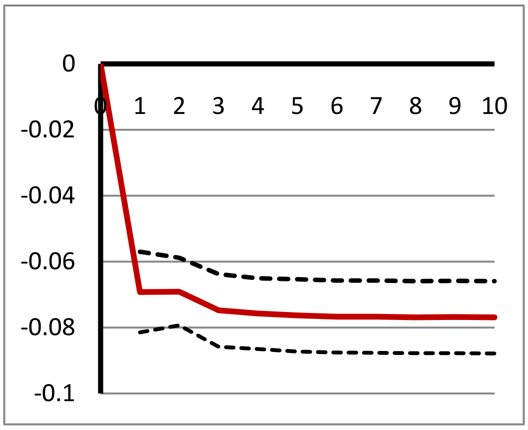
Panel A: PE Volatility



Panel C: Spread



Panel B: Volatility



Panel D: Order Imbalance

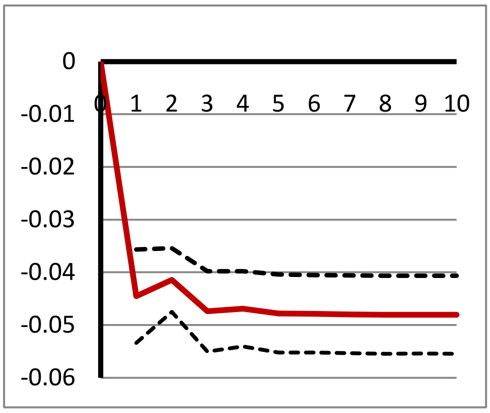
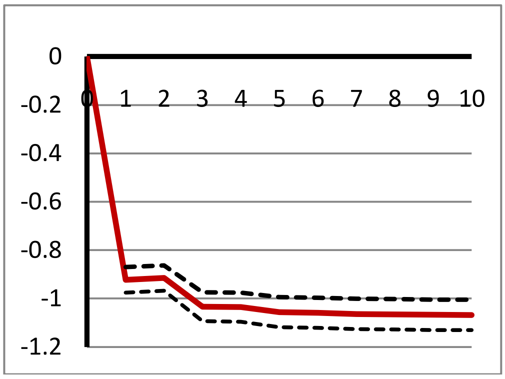


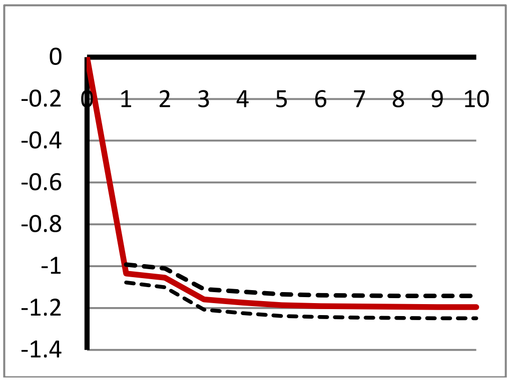
Figure 3. Impulse Response Functions, Covered Short Selling

Plots of accumulated impulse response functions depicting the impact of a one-standard deviation shock in *OCSR*. The VAR model used for estimation is Model 2 and the sample is the 'overall market', as described in Table VI. The response variables in the four panels are, respectively, *PE Volatility*, *Volatility*, *Spread* and *Order Imbalance*, as defined in Table 9. The horizontal axis are time periods (days), following the initial shock (day 0). All variables are standardized and winsorized by security. Impulse response coefficients are estimated by security; mean values are depicted. 5% confidence intervals are computed using cross-sectional standard error estimates.

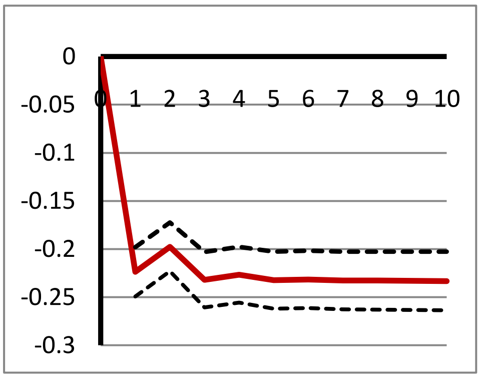
Panel A: PE Volatility



Panel B: Volatility



Panel C: Spread



Panel D: Order Imbalance

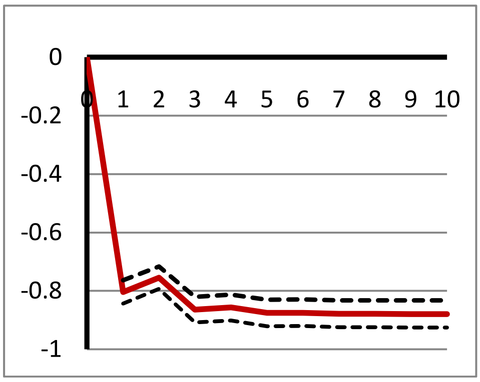


Figure 4. Naked Short Selling and Returns in 2008, Bear Sterns

Plot of *Outstanding Naked Short Ratio* and *Return Index* related to Bear Sterns Companies Inc. common stock (ticker: BSC) against calendar date. The *Return Index* is set to 1 on the 1st of January, 2008; $\textit{Return Index}_i = \sum_{j=1}^{i}(1 + R_{BSC,j})$. $R_{BSC,j}$ is the observed total return for BSC on day *j*, from the CRSP database.

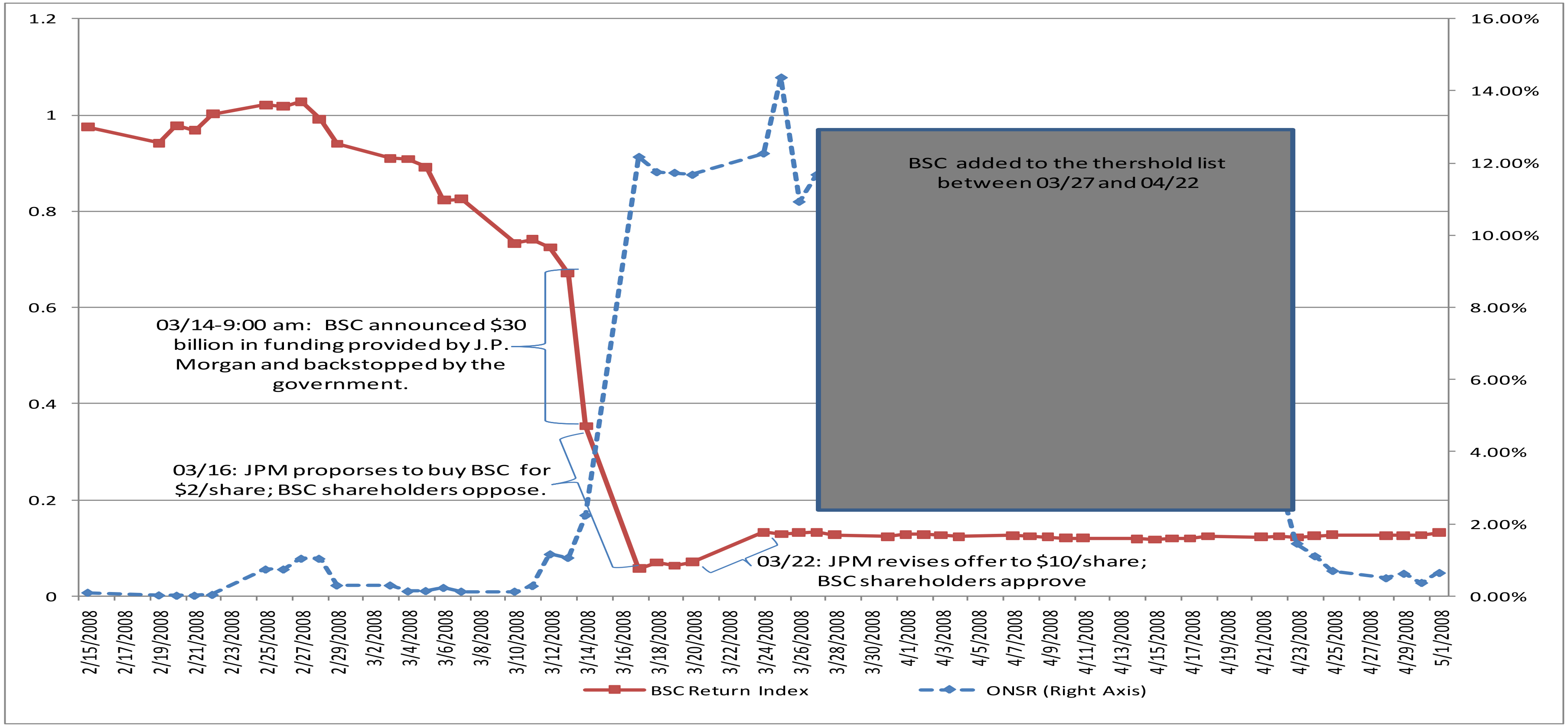


Figure 5. Naked Short Selling and Returns in 2008, Lehman

Plot of *Outstanding Naked Short Ratio* and *Return Index* related to Lehman Brothers Holdings Inc. (ticker: LEH) *over calendar time*. The *Return Index* is set to 1 on the 1st of January, 2008; $Return\ Index_i = \sum_{j=1}^{i}(1+R_{LEH,j})$. $R_{LEH,j}$ is the observed total return for LEH on day *j*, from the CRSP database.

Figure 5A

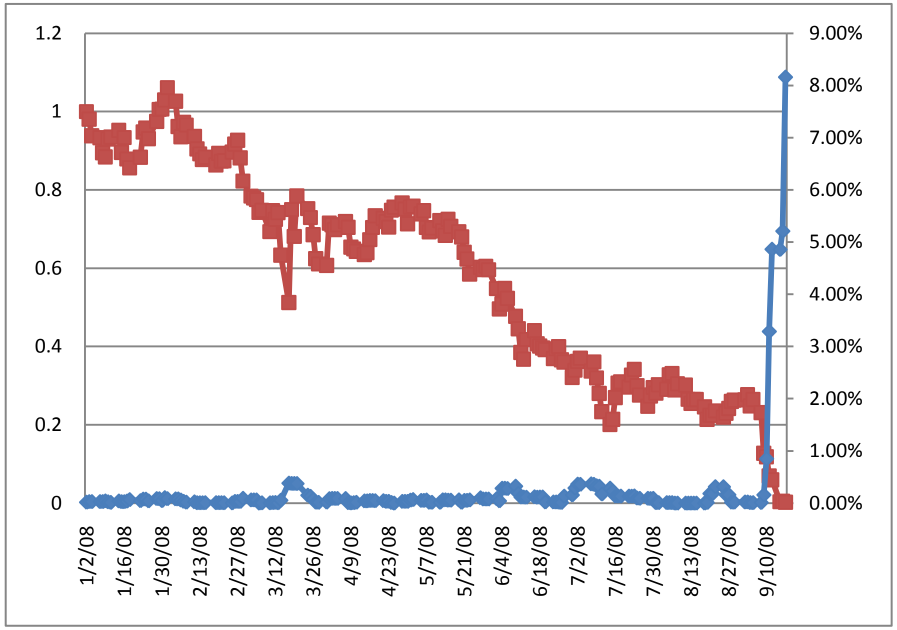


Figure 5B

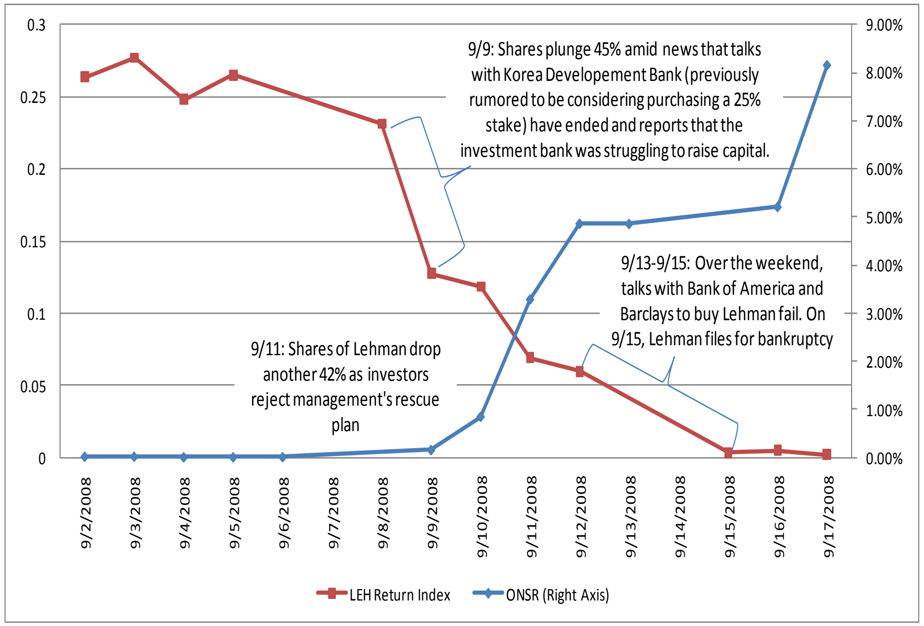


Figure 6. Naked Short Selling and Returns in 2008, Merrill Lynch

Plot of *Outstanding Naked Short Ratio* and *Return Index* related to Merrill Lynch & Co., Inc. (ticker: MER) *over* calendar time. The *Return Index* is set to 1 on the 1st of January, 2008; $Return\ Index_i = \sum_{j=1}^{i}(1 + R_{MER,j})$. $R_{MER,j}$ is the observed total return for MER on day *j*, from the CRSP database.

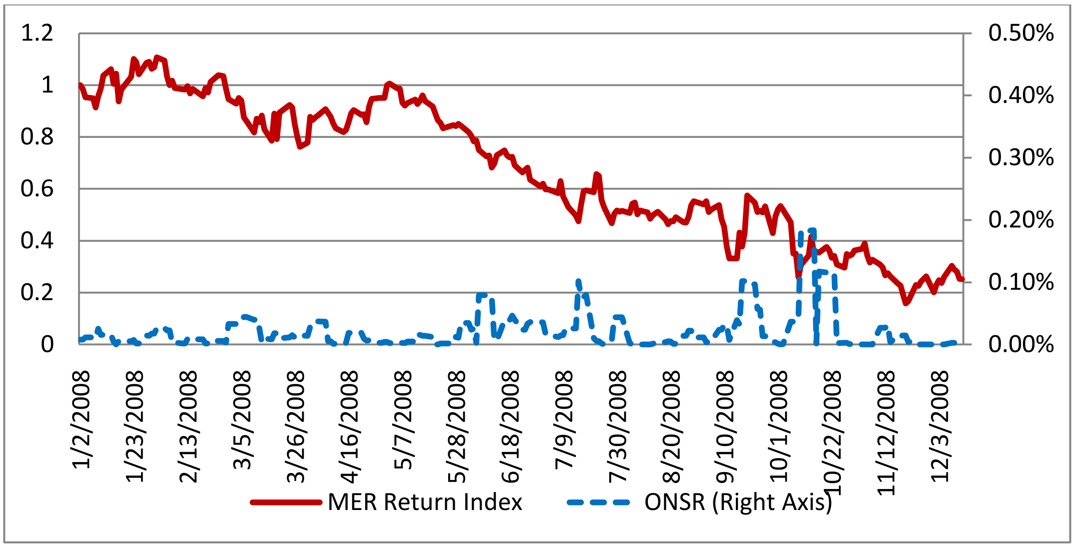


Figure 7. Naked Short Selling and Returns in 2008, AIG

Plot of *Outstanding Naked Short Ratio* and *Return Index* related to American International Group (ticker: AIG) *over* calendartime. The *Return Index* is set to 1 on the 1st of January, 2008;. $R_{AIG,j}$ is the observed total return for AIG on day *j*, from the CRSP database.