Insider Sales under the Threat of Short Sellers: New Hypothesis and New Tests

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Abstract

Using the Regulation SHO program as a quasi-experiment, we document that the threat of short selling has a negative effect on the volume of opportunistic insider selling and a positive effect on its profitability for each transaction. These effects are stronger among firms with higher litigation risk, greater media coverage, and executives who have more of their firms' stock-related holdings. We further find robust evidence when we extend the analyses to short selling deregulations in the Chinese and Hong Kong stock exchanges. Overall, our findings suggest that short sellers play a disciplinary role in opportunistic insider selling.

Keywords: Regulation SHO; short selling; insider trading; disciplining hypothesis; crowding-out hypothesis

JEL Classification: D8; D53; G14; G18

Data Availability: Data are available from the public sources cited in the text.

I. INTRODUCTION

We investigate how the threat of short selling affects insider sales. Taking an *ex ante* perspective, Massa, Qian, Xu, and Zhang (2015) "(MQXZ 2015" hereafter) hypothesize and find that the presence of short sellers induces insiders to sell more and faster to preempt information competition. This explanation is intuitive under the assumption that short sellers do not respond to insiders' preemptive sales in a pure profit competition game (e.g., Kyle 1984). Such an assumption overlooks the insiders' concerns about the potential reaction of short sellers to opportunistic insider selling. When considering the potential reaction from short sellers, in contrast, we find that the threat of short selling has a negative effect on the volume of opportunistic insider selling and a positive effect on trading profits for each transaction. This finding suggests that short sellers play a disciplinary role in opportunistic insider selling.

We argue that the threat of short sellers lowers the insiders' incentives to pursue opportunistic behaviors at the expense of outsiders because short sellers have a strong monetary incentive to identify and exploit the negative impact of such behaviors on stock prices (e.g., Karpoff and Lou 2010; Hirshleifer, Teoh, and Yu 2011). Among such opportunistic behaviors, opportunistic insider selling has long been considered an unfair expropriation of outsiders (e.g., Will 1987). Although an insider can gain private benefits by conducting opportunistic selling, the insider might also suffer from pecuniary and reputational costs if short sellers expedite the exposure of her trading to the public. Because of this trade-off, insiders reduce or even abstain from opportunistic insider selling under the threat of short selling. Nevertheless, should insiders continue to take the risk of selling shares, we expect their sales to be more profitable to outweigh the higher potential costs of trading. We call this the disciplining hypothesis.

It is also possible that the participation of short sellers affects the distribution of information between insiders and short sellers. Short sellers have the ability to obtain private information and

compete with insiders for trading profits (MQXZ 2015; Gao, Ma, and Ng 2018). Prior studies (e.g., Grullon, Michenaud, and Weston 2015; Fang, Huang, and Karpoff 2016; Ke et al. 2018) show that short selling reduces information asymmetry and improves stock price efficiency, making opportunistic insider sales less profitable. As a result, insiders sell less and make less profit because the negative firm-specific information has already been incorporated into the stock price by the short sellers in a timely manner. We call this reduction in the potential benefits of insider sales the crowding-out hypothesis.

To address the possibility that short selling is an endogenous variable, we exploit an exogenous variation in shorting requirements caused by the Rule 202T pilot program of Regulation SHO (hereafter "Reg SHO") conducted by the US Securities and Exchange Commission (SEC) from 2005 to 2007.[1] The SEC randomly selected one third of the stocks from the Russell 3000 index as pilot firms and exempted them from short selling price restrictions, which sets the stage for a difference-in-differences analysis. Prior studies show that Reg SHO effectively reduces short selling constraints for pilot firms than for other firms in the Russell 3000 index (i.e., the control firms) during the program (e.g., SEC 2007; Alexander and Peterson 2008; Diether, Lee, and Werner 2009; Grullon et al. 2015; Boehmer, Jones, and Zhang 2019), or increases the potential of short selling from the insiders' perspective (e.g., Massa, Zhang, and Zhang 2015; Fang et al. 2016).

We test our predictions using 55,002 firm-quarters, including 974 pilot firms and 1,935 control firms, over the period from the pre-program period (i.e., January 2002 to April 2005) to the post-program period (i.e., May 2005 to July 2007). We find that compared with the control firms, pilot firms experience an 11% reduction in insiders' opportunistic sales, measured by non-routine insider sales as defined by Cohen, Malloy, and Pomorski (2012), from the pre- to

[1] We use "Reg SHO," "SHO program," or "the program" interchangeably in this paper.

post-program period. In addition, each sale transaction becomes more profitable and such an impact is also economically significant. For example, the increase in insiders' trading profits from pilot firms following Reg SHO is 9% of the standard deviation of opportunistic selling profits relative to control firms after the program takes effect. The results are consistent with the disciplining hypothesis but fail to support the crowding-out hypothesis.

Moreover, consistent with the findings in previous studies that the effect of Reg SHO decreases with firm size, we also find that the reduction in insider selling volume and the increase in trading profits per transaction are concentrated among small firms. Further, using routine insider sales as a placebo test, we find no significant impact of Reg SHO on routine insiders' selling volume and their associated trading profits. We also conduct a dynamic test and show that pilot and control firms exhibit similar trends in opportunistic sales before the program, which validates the parallel trends assumption.

We perform cross-sectional analyses to better understand the economic mechanisms underlying our findings. First, we expect short sellers to target firms with a higher litigation risk because these firms are easier to attack and the potential benefits to short sellers are higher (Walker and Forbes 2013). Second, when media exposure is intensive, we expect insiders' reputations to be significantly impaired if opportunistic insider trading is identified and exposed to the public by short sellers (Dyck, Volchkova, and Zingales 2008; Dai, Parwada, and Zhang 2015). Third, when the insiders' personal wealth is tied to the firm's value via stock and option holdings, insiders with larger stock and option holdings should be more likely to restrict their opportunistic sales to lower the potential threat posed by short sellers.

Consistent with the above predictions, we find that our baseline effects of Reg SHO on insider selling volume and profits are stronger for (i) firms with a higher litigation risk based on

whether a firm has a higher tendency to be sued by shareholders, (ii) those with a greater number of news articles on their past insider trading activities, and (iii) those in which the insiders' personal wealth, measured by executive stock and option holdings, is high.

Next, we reconcile our finding of a negative impact of the threat of short selling on the volume of opportunistic insider sales with MQXZ's (2015) finding of a positive impact. First, different from our focus on the *effectiveness* date of Reg SHO, MQXZ (2015) conduct a difference-in-differences analysis around the *announcement* of Reg SHO. Consistent with MQXZ (2015), we also find an increase in opportunistic insider sales for pilot firms shortly after the *announcement* of Reg SHO (i.e., from the pre-treatment benchmark period between April and June 2004 to the period between September and November 2004). However, this result does not hold if we use the quarter during January and March 2004 (i.e., one quarter before what used by MQXZ 2015) as the pre-treatment benchmark period. Our results suggest that MQXZ's (2015) findings be driven by some unobservable effects, which are associated with changes in opportunistic insider sales immediately prior to the *announcement* of the SHO program.

Second, we demonstrate that the use of lendable shares as a proxy for short-selling pressure as in MQXZ (2015) might result in potential endogeneity issues such as the omitted confounding effect in the insider trading setting (e.g., D'Avolio 2002; Appel, Gormley, and Keim 2016; Dai et al. 2016; Campello, Matta, and Saffi 2018). Specifically, if we include firm fixed effects or conduct a change-on-change analysis, the positive and significant association between lendable shares and insider selling documented by MQXZ (2015) becomes insignificant or even negative.

To further mitigate the risk of confounding effects that might cause the treatment group to change its behavior even if there were no change to short selling restrictions,[2] we examine the

[2] This concern is exacerbated when the treatment occurs at only one point in time in contrast to staggered changes in short selling restrictions.

alternative settings of China and Hong Kong. In China, short selling was prohibited until 2010, when Shanghai and Shenzhen Stock Exchanges implement a deregulation pilot program of short selling. Since then, the pilot program has undergone several revisions. The designed list of stocks eligible for short selling has increased from 90 in 2000 to around 950 in 2006 and since. Similar to China, the Hong Kong Stock Exchange introduced its own pilot program of short selling deregulation in 1994 and the pilot program has also undergone serval revisions. Both staggered deregulations generate both time-series and cross-sectional variations in short-selling constraints for stocks listed on the exchanges. Using the variations in short-selling constraints for firms listed in China and Hong Kong, we continue to find that the threat of short selling reduces opportunistic insider sales, but increases their selling profits.

The U.S. and China represent contrasting institutional environments (e.g., culture, market development, and legal environment) (Bertomeu, Beyer, and Taylor 2016). Obtaining the same inferences under different settings not only highlights the generalizability of our results, but also suggests that our results are less likely to be driven by unobservable omitted variables. It is unlikely that these variables, if any, would simultaneously exist in two totally independent samples of insiders from institutional environments that differ substantially on many important dimensions.[3]

We contribute to the literature in two ways. The primary contribution to the accounting and finance literature is on the interaction between insider trading and short selling. The commonly held view is that insiders and short sellers act merely in a profit competition over common private information about future firm performance. MQXZ (2015) suggest that the threat of short sellers

[3] We acknowledge that it is still difficult to fully rule out some confounding effects (e.g., the government's tightening of monitoring during the same period, especially for China), or other omitted variable issues (e.g., the degree of impact differs across firms with stocks that can be sold short and those not). Therefore, the results from Hong Kong and China settings should be interpreted with caution.

compels insiders to sell more and faster to preempt trading profit competition. However, MQXZ (2015) implicitly assume that short sellers do not react to preemptive insider trading. By considering an alternative conceptual framework that takes into account the costs of these (preemptive) insider sales, we provide new evidence that the threat of short sellers plays a disciplinary role in the insiders' opportunistic sales. Therefore, our study provides new insights to regulators weighing up the benefits and costs of short selling restrictions.

Previous studies focus on whether insiders or short sellers have better information about the future performance of firms with bad news (e.g., Khan and Lu 2013; Gao et al. 2018). In this study, we do not assume that either insiders or short sellers are more informed than the other. Based on the premises that opportunistic insider selling is a type of potentially unethical managerial behavior that might destroy firm value, and that short sellers have a strong monetary incentive to detect and reveal value-destroying news of a company, we examine whether the threat of short sellers deters opportunistic insider selling. Specifically, short sellers do not merely compete with insiders for the information underlying specific opportunistic sales; they also profit from the longer-term effects of opportunistic insider sales (e.g., litigation risk and reputational damage of firms). Our empirical findings thus provide a foundation for future theoretical modeling of the impact of short selling on financial markets and regulatory attitudes.

Our study also has important policy implications. For instance, our study might be of interest to regulators who seek to understand the mechanisms that can discipline opportunistic insider trading. Previous studies find that insiders exploit private information and consequently earn abnormal profits from opportunistic trading (e.g., Seyhun 1986; Piotroski and Roulstone 2005; Jagolinzer 2009; Ravina and Sapienza 2010). Previous studies also point to the value of intensive media coverage (Dai et al. 2015), the adoption of corporate policies such as voluntary blackout

periods (Bettis, Coles, and Lemmon 2000) or general counsel pre-approval requirements (Jagolinzer, Larcker, and Taylor 2011), and stringent internal governance (Dai et al. 2016) in limiting opportunistic insider behavior. We provide seminal evidence that short sellers, a group of well-informed and sophisticated investors, can effectively reduce opportunistic insider selling by complementing other forces such as legal enforcement, reputation, and internal governance. Finally, our findings might also help short sellers and insiders understand each other in terms of the interactive effects of their potential and actual trading behaviors.

II. BACKGROUND AND HYPOTHESIS DEVELOPMENT

Reg SHO Program

In July 2004, the SEC issued a pilot order (Securities Exchange Act Release No. 50104) and published a list of 986 U.S. stocks (referred to as pilot stocks) listed on the NYSE, Amex, and NASDAQ.[4] The pilot stocks were exempted from short-sale price tests during the program. According to the SEC, pilot stocks were selected in a three-step process: (i) sorting the Russell 3000 index stocks into three groups—stocks listed on the NYSE, Amex, and NASDAQ; (ii) ranking stocks from the highest to the lowest in each group in May 2004 by their average daily dollar volume over the year prior to the issuance of the pilot order (i.e., from June 2003 through May 2004); and (iii) choosing every third stock in each group. The remaining stocks in the Russell 3000 index function as the control group in this study.

Prior studies have shown that this program effectively reduces short selling constraints and makes short selling more feasible (e.g., SEC 2007; Alexander and Peterson 2008; Diether et al. 2009; Grullon et al. 2015; Boehmer et al. 2019). For example, Diether et al. (2009) show that short sellers become more active (shown by an increase in both short sale frequency and the number of shares sold short divided by daily trading volume) in pilot stocks than control stocks

[4] The pilot order notice is available at http://www.sec.gov/rules/other/34-50104.htm.

after the effective date of Reg SHO. This finding is supported by a number of studies (e.g., SEC 2007; Alexander and Peterson 2008). More recently, Boehmer et al. (2019) also find that short sellers trade pilot stocks more aggressively and control stocks less aggressively. Taken together, these results suggest that Reg SHO is effective in terms of the intensity or aggressiveness of short selling activities.[5]

It is important to note that our key theoretical motivation is not fully based on the assumption of Reg SHO's impact on *observed* short selling (e.g., Black et al. 2019). Instead, we follow prior studies (e.g., Massa, Zhang, and Zhang 2015; Fang et al. 2016) and expect insiders to adjust their behavior to reflect the increased potential for short selling and the consequent increase in the expected costs of opportunistic insider selling.[6] Because insiders adjust their behavior endogenously, short seller activities associated with opportunistic insider selling may decrease and the total observed amount of short selling could increase, decrease, or remain the same. An observed change in actual short selling is therefore neither necessary nor sufficient to declare that an increased *prospect* of short selling could affect insiders' opportunistic selling (Fang, Huang, and Karpoff 2020).[7]

Hypothesis Development

Insiders have direct access to private information. If they do not disclose such private information to the public, they can profit from opportunistic trading (e.g., Ke, Huddart, Petroni

[5] Note that Grullon et al. (2015) also find significant lower stock returns for pilot small stocks in the long-term (i.e., six months, one year, or two years) after the *effective* date of Reg SHO, suggesting that the SHO program is effective in terms of its impact on pilot stocks, but its impact is concentrated on small firms. We examine whether the effect of Reg SHO on opportunistic insider sales varies with firm size in Section IV.

[6] This view of "perceived risk" is consistent with our theoretical motivation and has been widely adopted in the literature (e.g., Huddart et al. 2007; Donelson and Yust 2014).

[7] As supplementary evidence, we investigate the market's awareness of Reg SHO in the *announcement* and *effective* periods. Our analysis (untabulated) of the Google Search Index finds that the search index for Reg SHO reaches 34.2 one quarter before the effective date (May 1, 2005), which is around three times more than before the announcement date, even including the quarter when Reg SHO was announced (July 28, 2004). The rise in search volume implies that public concerns about potential short selling increases when Reg SHO is implemented.

2003; Piotroski and Roulstone 2005; Cohen et al. 2012).[8] The majority of opportunistic insider trading is prohibited by law and is often considered an unfair expropriation of outsiders (e.g., Will 1987).

Among outside investors, short sellers are well established as the most informed and sophisticated. First, short sellers are sophisticated analysts of both private and public information (e.g., Khan and Lu 2013; MQXZ 2015; Gao et al. 2018; Rapach, Riggenbach, and Zhou 2016).[9] Second, as a type of whistleblower, short sellers can effectively detect and reveal value-destroying events that are related to firm misconduct—in particular, opportunistic insider selling *itself* (Dyck et al. 2010; Karpoff and Lou 2010; Hirshleifer et al. 2011; Blau and Tew 2014). Thus, short sellers pose a threat to insiders who trade on private information at the cost of outsiders. A natural follow-on question is: How do insiders react to the threat of short selling?

Motivated by a stylized model, MQXZ (2015) propose that the presence of short sellers acts as a stimulus for insiders, causing them to trade at higher volumes and faster. This hypothesis is founded on two intuitions. First, if insiders wait, competition from short sellers might jeopardize the profitability of insider selling. Second, faster and more insider sales can preempt short sellers. These two explanations are completely plausible in the competition among informed traders who trade purely for profits.

However, it is more natural to expect that insiders consider both trading profits and the *costs* of using material nonpublic information. First, the law limits or prohibits insider trading based on material nonpublic information, and violations can attract significant litigation costs (e.g., Bhattacharya and Daouk 2002; Johnson, Nelson, and Pritchard 2007). Second, although not all

[8] See other examples including Jaffe (1974), Seyhun (1986, 1998), Lin and Howe (1990), Rozeff and Zaman (1988), Lakonishok and Lee (2001), Marin and Olivier (2008), and Jagolinzer (2009).
[9] See other examples such as Dechow et al. (2001), Christophe, Ferri, and Angel (2004), Asquith, Pathak, and Ritter (2005), Cohen, Diether, and Malloy (2007), Boehmer, Jones, and Zhang (2008), Diether et al. (2009), and Engelberg, Reed, and Ringgenberg (2012).

types of opportunistic insider trading are considered illegal by regulators, such trading might cause a loss of confidence in the insiders and their firms as the opportunistic behavior is publicly revealed. Thus, such insider trading activities lead not only to lengthy and costly investigations (Walker and Forbes 2013), but also signal a weak corporate governance environment and impair the firm's future prospects, which hurts the insiders' public image and the portion of their personal wealth related to the firm's value. More importantly, the potential to recover this value is not likely to be high (Fombrun and Shanley 1990; Bhattacharya and Daouk 2002; Carroll and McCombs 2003; Francis et al. 2008).

If insiders use private information to profit at the expense of outsiders, short sellers have a monetary incentive to increase their short selling profits by acting as whistleblowers and attacking opportunistic insider behavior. Potential or actual attacks by short sellers increase the insiders' trading costs. Therefore, we propose the following *disciplining hypothesis*:

H1: The threat of short selling reduces the volume of opportunistic insider sales.

We note that there might be two different explanations for the negative relation between the threat of short selling and opportunistic insider sales. First, as discussed, the "cost" argument of our disciplining hypothesis suggests that the threat of short selling increases the expected costs of opportunistic insider sales. We therefore expect the *realized* insider sales to be more profitable in the presence of short sellers because insiders would not make such trades unless the perceived benefits were high enough to compensate for the higher costs associated with the disciplinary role of short sellers. The above discussions lead to the following hypothesis:

H2a: The threat of short selling increases opportunistic inside selling profits per trade.

Second, it is possible that the participation of short sellers affects the distribution of information between insiders and short sellers. Short sellers have the ability to obtain private

information and compete with insiders for trading profits (MQXZ 2015; Gao et al. 2018). Prior studies (e.g., Grullon et al. 2015; Fang et al. 2016; Ke et al. 2018) show that short selling reduces information asymmetry and improves stock price efficiency. As a result, insiders might sell less because short sellers ensure that the negative firm-specific information is incorporated into the stock price in a timely manner, which makes opportunistic insider sales less profitable. We call this reduction in the potential benefits of insider sales the *crowding-out hypothesis*:

H2b: The threat of short selling reduces opportunistic inside selling profits.

We further propose the cross-sectional predictions on three channels through which the expected costs of opportunistic insider sales are increased by the presence of short sellers, namely, litigation risk, reputation concern, and personal wealth.

First, we argue that short sellers play a role in the detection and enforcement of illegal insider trading, and their effect is stronger when insiders have a greater concern about litigation. Litigation and enforcement actions against insider trading benefit the trade of short sellers who have a motive and ability to uncover the insiders' opportunistic behavior (Dyck et al.2010; Karpoff and Lou 2010; Khan and Lu 2013; Hirshleifer et al. 2011; Fang et al. 2016). The literature also suggests that the presence of short sellers yields increased legal costs for parties engaged in misconduct (Walker and Forbes 2013; Blau and Tew 2014). The higher litigation risk of a firm provides a signal to short sellers that targeting such firms will be more successful (Francis et al. 1994; Gande and Lewis 2009; Kim and Skinner 2012) and more profitable than others. We expect that insiders who are more exposed to litigation risk will be more likely to avoid opportunistic selling because the litigation costs are higher and short sellers are more likely to target their firms due to higher potential benefits. Should insiders continue to take the risk of selling shares, we expect their sales are more profitable to thus outweigh the higher litigation

costs.[10]

Second, insiders' and their firms' reputations can be significantly impaired if opportunistic insider trading is identified and exposed by whistleblowers such as short sellers (Klein and Leffler 1981; Dyck et al. 2008; Dai et al. 2015). We argue that the media increase the impact of short sellers' attacks on insider reputation. The media have an incentive to report firms' negative news to gain readership (Core, Guay, and Larcker 2008; Carroll 2010; Ahern and Sosyura 2014). Negative news reports provided by short sellers meet this demand. As the broadest information intermediaries, the media help short sellers disseminate the news of insider misconduct to the public (Fombrun and Shanley 1990; Carroll and McCombs 2003; Ljungqvist and Qian 2016), and such media coverage stresses the insiders' misconduct. Previous studies show that the media have an incentive to report unfavorable news about firms that are more well-known (Dyck and Zingales 2002; Miller 2006; Dyck et al. 2010; Cohen et al. 2015). As such, the negative impact of short selling on the insiders' reputational capital is greater for firms with more media coverage (Dyck et al. 2008; Dai et al. 2015). Thus, we expect insiders to reduce opportunistic sales when the media are readily accessible to short sellers. For insiders who continue to take the risk of selling shares, we expect their sales to be more profitable to outweigh the concerns about attracting more media coverage.

Lastly, previous studies show that insiders have a strong incentive to protect the value of their stakes under the threat of short sellers (Khanna and Mathews 2012; Li and Zhang 2015). Although insiders might earn personal benefits via opportunistic selling in the short run, they

[10] It is noteworthy that although not all types of opportunistic insider trading trigger enforcement actions by regulators or are ruled as illegal, securities class actions or investigations of potential managerial misconducts can be initiated even if the targeted insider trading is legal. If a firm believes it is facing a short selling attack or is dealing with the spread of misleading or outright false information about legitimate insider trading, it is still costly for the firm to initiate defensive actions such as litigation and disclosure to government regulators because the malicious actions of short sellers are difficult to prove.

might lose personal wealth if they still hold a large stake of their firms. Khanna and Mathews (2012) suggest that the negative impact of short selling on stock price can last over the long run. Consistent with this, Li and Zhang (2015) show that managers, in response to the threat of short selling, reduce the precision of bad news forecasts to minimize their influence on share prices, especially when the managers' wealth is more strongly tied to the stock price. Insiders with greater stock-related holdings of their firms might not be able to off-load all of their holdings within a short timeframe. We expect that under the thread of short selling, these insiders have greater expected costs of opportunistic selling as their personal wealth is more tied to firm value. Therefore, the disciplining effect of short sellers is stronger among insiders with greater stock-related holdings of their firms.

We formulate three testable hypotheses based on the above discussions as follows:

H3a: The disciplining effect of short selling on opportunistic insider selling (H1 and H2a) is stronger among firms with higher litigation risk.

H3b: The disciplining effect of short selling on opportunistic insider selling (H1 and H2a) is more pronounced among firms with greater media coverage.

H3c: The disciplining effect of short selling on opportunistic insider selling (H1 and H2a) is stronger for firms that have insiders whose personal wealth is more tied to their firms' stock-related holdings.

III. RESEARCH DESIGN

Data

To construct our sample, we begin with stocks that were included in the Russell 3000 index in 2004. According to the Russell index manual, firms that are involved in mergers and acquisitions or that have other significant corporate events are dropped from the index. Following the SEC's Reg SHO selection criteria, we exclude stocks that are not listed on the NYSE, Amex, or NASDAQ, and stocks that went public after April 30, 2004. We define firms

listed in the SEC's pilot order as pilot firms and the remaining firms in our sample as control firms. This selection procedure results in a sample of 985 pilot firms and 1,967 control firms.

Next, we obtain insider trading data from Thomson Reuters. The initial insider trading sample includes open-market sales and purchases made by insiders reported in Form 4. Following prior studies (e.g., Ke et al. 2003; Cheng and Lo 2006), we only include insider trading transactions made by officers and directors and aggregate transactions made by insiders belonging to the same firm on the same trading day. By merging the short selling data with the insider trading data, we construct a U.S. sample over the period from January 1, 2002, to July 6, 2007. The ending date coincides with Reg SHO's end date.[11] We further exclude observations for financial data and stock prices that are required for variable construction but missing from Compustat or the Center for Research in Security Prices (CRSP). Our final sample consists of 55,002 firm-quarters from 2,909 unique firms, of which 974 are pilot firms and 1,935 are control firms.

To facilitate the empirical analysis, we use an indicator variable, *Pilot*, to denote pilot firms. *Pilot* equals one if a firm's stock was designated as a pilot stock in the SHO program and zero otherwise. Pilot firms constitute the treatment sample and non-pilot firms serve as the control sample. As the SHO program started on May 1, 2005, we define the SHO program period as those fiscal quarters starting after May 1, 2005 and ending before July 6, 2007. Similarly, we define the pre-SHO program period as those quarters beginning after January 1, 2002 and ending before May 1, 2005. Following the definition of the SHO program period, we create a variable,

[11] On June 13, 2007, the SEC announced plans to eliminate all short sale price tests, effective July 6, 2007 (i.e., the 2007 repeal of uptick rule). We do not consider whether the effects on opportunistic insider sales are reversed after the program end is an appropriate check on the internal validity of the difference-in-differences tests in our setting. Boehmer et al. (2019) suggest that the removal of the uptick rule for all stocks in 2007 makes synchronous portfolio trading on pilot and control stocks easier and less costly to execute. Therefore, it is possible that insiders of pilot firms continue to be extremely concerned about the high costs of opportunistic selling even after the end of Reg SHO.

During, to indicate the above two periods: *During* equals one if a fiscal quarter begins after May 1, 2005 and ends before July 6, 2007, and zero otherwise.

Empirical Design

To examine the effect of the threat of short selling on insider sales (H1), we first partition our insider selling activities into opportunistic sales and routine sales based on the method developed by Cohen et al. (2012). Specifically, we designate an insider's trades on a stock in a particular month as either opportunistic or routine based on the insider's past history of trades.[12] To obtain enough data to track an insider's trading history, we require every insider to have at least one transaction in each of the three preceding years to classify a transaction to be routine or opportunistic. If the insider has traded in a particular month over the past three years, we classify all the trades she makes in that month as routine trades and her trades in other months as opportunistic trades. By definition, routine and opportunistic trades of multiple insiders may co-exist in a given firm-quarter.

We specify the following model for our main analysis and examine the effect of the threat of short selling on opportunistic insider sales:

$$\begin{aligned} OpptunSales \text{ or } OpptunProfits \; &= \alpha_0 + \alpha_1 Pilot \times During + \alpha_2 Pilot \\ &+ \alpha_3 During + \gamma X + FixedEffects + \varepsilon, \end{aligned} \tag{1}$$

where $OpptunSales$ is the aggregate number of opportunistic sales in terms of shares sold by insiders within each firm-quarter as a percentage of the firm's outstanding shares. $OpptunProfits$ is profits per trade from opportunistic insider sales over a given quarter, which is defined as the quarterly average of the product of total opportunistic insider sales as a percentage of shares outstanding of a trading day and cumulative abnormal return, times -1. This

[12] In the Cohen et al. (2012) method, three types of trades could exist in a given firm-quarter: routine, opportunistic, and unclassified. Unclassified trades are trades made by an insider who failed to trade in three consecutive years at least once during the sample period. We exclude these unclassified trades in our tests.

measure captures the profits earned by insiders per trading day in dollar amount as a percentage of a firm's market value. Our main interest is the interaction term between $Pilot$ and $During$. The disciplining hypothesis (H1) predicts the coefficient on $Pilot \times During$ (α_1) to be negative when the dependent variable is $OpptunSales$. When the dependent variable is $OpptunProfits$, the disciplining hypothesis (H2a) predicts a positive coefficient on $Pilot \times During$ (α_1), while the crowding-out hypothesis (H2b) predicts a negative coefficient.

X represents a vector of variables that controls for the confounding effects related to firm characteristics. First, following previous studies (e.g., Cheng and Lo 2006; Grullon et al. 2015), we include a set of control variables ($Controls$). Specifically, we control for firm size ($logAT$), market-to-book equity ratio (MB), firm performance (Ret and ROE), options granted to managers ($Grants$), opportunistic sales in the previous quarter ($Lag1OpptunSales$), and opportunistic sales in the same quarter of the last year ($Lag4OpptunSales$) to control for seasonality. Second, we also include the interaction terms of $Controls$ with $Pilot$ and $During$ to control for the changes in firm characteristics across pilot and control firms that may affect insider selling and short-selling activities. Lastly, we control for industry and year-quarter fixed effects in regressions.[13] We winsorize all continuous variables sorted by *Pilot* and *During* at the 1st and 99th percentiles. These control variables are measured at the end of the last quarter and are defined in Appendix A.

Summary Statistics

Panel A of Table 1 presents the descriptive statistics for pilot and control firms before the announcement and implementation of the SHO program. Following previous studies, we

[13] While $During$ captures the general difference in opportunistic selling between pre- and post-Reg SHO, our year-quarter fixed effects capture the common trend of opportunistic selling for each fiscal quarter given that not all firms share the same calendar quarters with their fiscal quarters.

compare pilot and control firms' insider trading information and firm characteristics in the fourth quarter of 2003, which is the fiscal year end immediately before the announcement of Reg SHO.

First, we report the statistics on the measures of insider sales, aggregating insider sales for each firm-quarter. If no insider trade is reported for a given firm-quarter, the firm-quarter is assigned a value of zero.[14] The average volume of shares sold by insiders is 0.257% of shares outstanding ($Sales$) for pilot firms and 0.251% for control firms. The average dollar amounts of quarterly insider sales ($Value$) are $3.563 million for pilot firms and $3.068 million for control firms. We find that insiders' selling profits in a quarter (*Profits*) are -0.273% and -0.134% for pilot and control firms, respectively. They represent that the average selling profits made by insiders per trade (day) over a quarter is very small and approximately zero for both pilot and control firms relative to their firm's market value. As reported in the table, there are no significant differences between pilot and control firms in any of the insider trading measures, suggesting that insiders in pilot and control firms have similar trading behaviors prior to the announcement of the program.

Panel B of Table 1 reports the descriptive statistics of the key variables in the whole sample. The average volume of shares opportunistically sold by insiders is 0.038% of shares outstanding and trading profits are about -0.025% relative to their firm's market value.[15] The sample firms have the mean total assets of around $1,204 million (log values = 7.093 reported in the table) and the mean market-to-book ratio of 3.04. Quarterly stock returns are around 6% and return on equity is about 2%. Most firm-quarters show zero option grants, consistent with firms granting

[14] Our results remain similar if we exclude (1) firms without insider trading during our entire sample period; and (2) firm-quarters that do not have any opportunistic insider sales ($OpptunSales$).

[15] Consistent with Cohen et al. (2012), we find that the average trading profits of opportunistic sales based on abnormal returns are slightly negative. Untabulated result shows that the negative profits are driven by the observations prior to the SHO program. Specifically, the average trading profits are -0.09% prior to the program and 0.07% during the program. It suggests the importance of applying the DID approach since the overall trading profits are larger after the program takes effect.

options on an infrequent basis, often only once each year.

Panels C1 and C2 of Table 1 report the correlations of our variables. The correlations are all small in magnitude. The highest variance inflation factor (VIF) is only 2.08 (untabulated), indicating low multicollinearity among the predictors of insider sales and their profits.

IV. EMPIRICAL RESULTS

Main Results of Opportunistic Selling Volume

As a preliminary analysis, we start with a univariate analysis by examining the differences in the changes of opportunistic insider sales ($OpptunSales$) between pilot and control firms across the pre-program period and the program period itself. Specifically, we define the second quarter of 2005 or 2005Q2 (the Reg SHO effective date) as event time 0 and take the changes in $OpptunSales$ from two years before the program (i.e., event year -2) to one year before the program (i.e., event year -1), and from two years before the program to t years during the program (t = 1 or 2).

Figure 1A plots the changes in $OpptunSales$ for the event windows (-2, -1), (-2, 1), and (-2, 2). The difference in the change in $OpptunSales$ from event year -2 to -1 between pilot and control firms is not significant from zero (t-stat. = -0.19).[16] However, when we expand the event window to (-2, 1), our pilot sample exhibits a decrease but the control sample does not. Further, the difference in the change between pilot and control firms becomes larger over the (-2, 1) window than the (-2, -1) window (from -0.0006 to -0.0055). Further extending the event window to (-2, 2), we find an even larger decrease for pilot firms, with such a decrease being significantly bigger than that of control firms (t-stat. = -1.84). Our results suggest that $OpptunSales$ for pilot firms decreases and the decrease becomes larger than that for control

[16] In this univariate analysis, we follow Villa (2016) and report t-statistics based on robust standard errors clustered by firm and year-quarter as we do in the multivariate analysis. We adopt the same empirical method for the remaining univariate analysis.

firms after the program becomes effective.

We next conduct a multivariate analysis to examine whether the univariate association illustrated above is maintained after controlling for other factors. Table 2 reports the baseline results of Eq. 1 for opportunistic insider sales and routine insider sales. The first two columns show that the coefficient on $Pilot \times During$ for opportunistic insider sales is significantly negative, and this negative effect holds with or without the interaction terms between $Pilot$ ($During$) and $Controls$. In terms of economic magnitude, pilot firms following the SHO program experience an 11% reduction in insiders' opportunistic sales relative to control firms.[17] Column (3) of Table 2 shows that insiders in pilot firms do not significantly reduce their routine sales during the program and are not different from control firms.

We further examine whether the observed reduction in opportunistic insider sales is stronger for firms that are most affected by Reg SHO. Grullon et al. (2015) show that Reg SHO has a greater impact on smaller firms. In line with their findings, Boehmer et al. (2019) also show that the increase in short selling aggressiveness is concentrated among smaller firms. Columns (4) and (5) Table 2 report the results conditional on firm size ($logAT$). Specifically, we partition our sample into two subsamples for big and small firms according to the sample median of total assets at the quarter end before the effective date of the SHO program. Consistent with the expected firm-size effect, we find that the reduction in opportunistic sales is concentrated on insiders in smaller pilot firms.

Main Results of Opportunistic Selling Profits

In the previous section, consistent with the *disciplining hypothesis*, we provide robust evidence that opportunistic insider sales decline more in pilot firms than control firms during the

[17] We divide the coefficient on $Pilot \times During$ (-0.430) by the product of 100 and the mean of opportunistic sales ($OpptunSales$) over our sample period (0.038, Table 1 Panel B). The results remain similar if we use the opportunistic sales of the pilot firms prior to the SHO program as the benchmark (0.040).

SHO program period. We next examine the trading profits of observed opportunistic insider sales.

We first conduct a univariate analysis. Figure 1B plots changes in $OpptunProfits$ as we did for $OpptunSales$ in Figure 1A. Similar to the pattern of $OpptunSales$, we do not find significant difference in the change in $OpptunProfits$ from event year -2 to -1 between pilot and control firms (*t*-stat. = 0.11). Importantly, our sample pilot firms have a significant increase in $OpptunProfits$ relative to control firms after the program takes effect, with *t*-stat. = 2.20 and 2.07 for event windows (-2, 1) and (-2, 2), respectively.

In the multivariate analysis, we re-estimate Eq. 1 with the dependent variable of opportunistic insider sales' profits ($OpptunProfits$). As we only focus on the firm-quarter observations with opportunistic insider sales, the sample size reduces from 55,002 to 14,664. Table 3 shows that the coefficient on the interaction term between $Pilot$ and $During$ is significantly positive and this effect is also stronger for smaller firms. Moreover, column (3) of Table 3 shows that trading profits for routine sales among pilot firms do not exhibit a significant change during the program relative to control firms. In terms of economic magnitude, we find an increase in $OpptunProfits$ of insiders from pilot firms following the SHO program approximately 9% of the standard deviation of opportunistic selling profits relative to control firms after the program takes effect.[18]

Taken together, our results from $OpptunProfits$ are consistent with the prediction of our *disciplining hypothesis* (H2a) rather than the *crowding-out hypothesis* (H2b). Specifically, our results suggest that the realized insider sales per trade are more profitable for pilot firms during

[18] Given the mean and median value of $OpptunProfits$ is too close to zero, we calculate the economic magnitude based on the standard deviation of $OpptunProfits$. We divide the coefficient on $Pilot \times During$ (0.106) by and the standard deviation of opportunistic selling profits ($OpptunProfits$) over our sample period (1.191, Table 1 Panel B).

the SHO program than before, because insiders in pilot firms will not make trades unless the perceived benefits are high enough to compensate for the higher costs associated with the disciplining role of short sellers.

Test of the Parallel Trend Assumption

An important assumption for the DID analysis, the parallel trend assumption in our setting, is that in the absence of Reg SHO, the differences in opportunistic sales and their associated profits between pilot firms and control firms are similar prior to the program and the documented disciplining effect does not exist until the program becomes effective. Therefore, we assess the internal validity of our results by re-estimating our main analysis using a set of trend analyses. Specifically, we create an additional indicator variable, $Pre1$, which equals to one for observations one year before the program (i.e., fiscal quarters ending between May 1, 2004 and April 30, 2005). We also include its interaction term with $Pilot$ in Eq. 1. Columns (1) and (2) of Table 4 show that the coefficient on $Pilot \times Pre1$ is not statistically significant. It suggests that the changes in opportunistic insider sales and their associated profits of our pilot firms do not occur before the program.

Cross-Sectional Analysis

To examine cross-sectional predictions, we estimate the following model:

$$\begin{aligned} OpptunSales\ \ (OpptunProfits\ ) = {} & c_0 + c_1 Pilot \times During \\ & + c_2 Pilot \times During \times Channel\ \ + \gamma Z + FixedEffects + \varepsilon, \end{aligned} \tag{2}$$

where $Channel$ is a list of channel variables that are related to litigation concerns (industry litigiousness or litigation risk index) (Francis et al. 1994; Kim and Skinner 2012), reputation concerns (media coverage of all firm activities or media coverage of the firm's insider trading activities) (Carroll and McCombs 2003; Carroll 2010; Dai et al. 2015), and personal wealth (shares and option holdings of a firm's CEO or those of the firm's top five most highly paid

executives) (Ofek and Yermack 2000; Roulstone 2003). For each $Channel$ variable, each firm-year is classified into either a High group (i.e., $HighLitig$, $HighRep$, or $HighWealth$ = 1) or a Low group (i.e., $HighLitig$, $HighRep$, or $HighWealth$ = 0). The detailed classifications of groups are described in Appendix A.

H3a, H3b, and H3c predict that the coefficients on the three-way interaction term between $Pilot$, $During$, and $Channel$ to be negative for the tests on the volume of opportunistic insider selling and positive for the tests on the profits of opportunistic insider selling, implying that short sellers' disciplinary role operates through the three channels associated with expected costs of trading.[19] Our discussion below focuses on the volume of opportunistic insider selling for brevity (Table 5 Panel A), and the results of the profits of opportunistic insider selling are generally consistent although weaker (Table 5 Panel B).

To examine the litigation risk channel, we first define $HighLitig$ as equal to one if a firm is in a litigious industry and zero otherwise (Francis et al. 1994). As an alternative measure, we further incorporate a proxy for firm-level litigiousness, which captures a set of firm characteristics related to securities class action litigations. We calculate this proxy using Kim and Skinner's (2012) model. Columns (1) and (2) of Table 5 Panel A report the estimation results of the volume of opportunistic insider selling. We show that $Pilot \times During \times HighLitig$ is negatively significant (coeff. = -0.764; *t*-stat. = -1.67) in column (2) when we consider firm-level Kim and Skinner's (2012) litigation index, whereas $Pilot \times During$ is not significant (coeff. = 0.170; *t*-stat. = 0.38). In addition, while $Pilot \times During \times HighLitig$ is only marginally significant in column (1), the sum of $Pilot \times During \times HighLitig$ and $Pilot \times During$ is

[19] Similarly, Z represents a vector of variables that controls for the confounding effects of firm characteristics. In addition to the interaction terms of $Controls$ with $Pilot$ ($During$), we further include the interaction terms of $Channel$ with $Pilot$, $During$, and $Controls$ to control for the changes in firm characteristics across pilot and control firms that may affect insider selling and short selling activities.

significantly different from zero at the 1% level for both columns. This result suggests that the negative impact of the threat of short selling on opportunistic insider sales is only pronounced for firms with high litigation risk.[20]

To examine the reputation concern channel, we define $HighRep$ as one if the number of news articles about a firm in the prior year is greater than the annual industry median and zero otherwise. In addition, we construct a more specific variable of media coverage on insider trading by focusing on articles that are specific to past insider trading activities. We report the estimation results in columns (3) and (4) of Table 5 Panel A. Consistent with the reputation channel, we find that the reputation concern captured by media coverage increases the impact of the SHO program on opportunistic insider selling when we consider media coverage related to insider trading. Specifically, we find that $Pilot \times During \times HighRep$ is negatively significant, and the sum of the coefficients on $Pilot \times During$ and $Pilot \times During \times HighRep$ is also significant at the 5% level in column (4).[21] It suggests that the disciplining effect of short sellers is stronger when managers are more concerned about their reputational capital specifically related to insider trading.

To test the personal wealth channel, we define $HighWealth$ as one if a firm's CEO (or top five highest-paid executives) has stock-related holdings greater than the sample median and zero otherwise. We report the estimation results in columns (5) and (6) of Table 4 Panel A. Consistent

[20] As supplementary analyses, we conduct two additional tests to validate our assumption that litigation against insider trading induces short selling activities. Our untabulated results show that investors react negatively to enforcement actions related to insider trading. This evidence supports our conjecture that short sellers have an incentive to expose opportunistic insider trading. Consistent with this view, we also find that short sellers increase their shorting position significantly around the time when the announcements of enforcement actions are made (untabulated). In summary, the results suggest that the disciplining effect of short selling is stronger when managers are more concerned about potential litigation risk.

[21] Note that an alternative view is that greater media coverage may mitigate opportunistic insider sales in the first place unconditional on any short selling activities (e.g., Dai et al. 2015). Another view is that the effect of short sellers on opportunistic insider sales is greater for the firms with *ex ante* lower visibility (media coverage). Neither of these views is supported by our results.

with the personal wealth channel, we find that the coefficient on $Pilot \times During \times HighWealth$ is negatively significant for both measures of personal equity wealth. It suggests that the effect is significantly greater in the high equity wealth group than in the low equity wealth group, and consistent with the prediction that the disciplining effect of short sellers is stronger when managers are more concerned about their personal wealth.

Table 5 Panel B shows the results where $OpptunProfits$ is the dependent variable. Columns (1) and (2) show that compared with those in Panel A, the results become weaker, and insignificant when we examine the effect of litigation risk. Consistent with those in Panel A, we find that the increase in trading profits is greater for higher media coverage related to insider trading (column (4)) and greater executives' personal wealth tied to the firm value (column (6)). In sum, our results are consistent with the notion that insiders are not willing to trade unless the benefits outweigh the higher costs associated with the disciplinary role of short sellers. A potential explanation for the weaker effect of firm litigation risk on insider trading profits could be that opportunistic profits cannot fully mitigate the costs of litigation risk, because firm litigation risk may also increase with the trading profits.

We note that none of these tests are able to rule out all of the possible alternative explanations individually due to their inherent limitations, but together they provide evidence on the channels through which short sellers discipline opportunistic insider selling.[22]

Reconciliation with the Findings in MQXZ (2015)

In the previous sub-section, we document strong evidence that short sellers reduce insiders' opportunistic selling. This is inconsistent with MQXZ (2015), who find that corporate insiders in

[22] Our untabulated results show that the partition dummy variables based on litigation, reputation, and personal wealth are just moderately correlated across the three channels (the Spearman correlations ranging from -0.194 to 0.224, with the absolute value of correlation coefficients ranging from 0.016 to 0.224). Intuitively, we find higher correlation coefficients for the variables within the same channel (ranging from 0.339 to 0.719). It suggests that our partition samples across the different channel analysis are not highly overlapped.

firms with a larger proportion of lendable shares tend to sell more and trade faster when they are in competition with short sellers for trading profits.

One potential explanation for these different results is the endogeneity issue that arises from using lendable shares as a proxy for short-selling pressure (e.g., Cohen, Diether, and Malloy 2007; Beneish, Lee, and Nichols 2015). For example, lendable shares are related to several confounding effects such as shorting demand or even slow-moving, time-invariant firm-specific characteristics (e.g., D'Avolio 2002; Campello et al. 2018).

To examine this possibility, we first replicate the tests in Table 3 of MQXZ (2015). In results not tabulated for brevity, consistent with their finding, we indeed find that the association between lendable shares and insider selling is significantly positive (with a *t*-stat. of 5.42).[23] The above positive and significant association between lendable shares and insider selling, however, vanishes or moves in the opposite direction (and is consistent with our disciplining hypothesis) if we include firm fixed effects or conduct a change-on-change analysis. Collectively, our results suggest that using lendable shares indeed results in some endogeneity issues (e.g., unobservable time-invariant factors that are related to lendable shares and insider selling or due to the potential reverse causality issue). Hence the positive effect of short selling competition on insider sales as identified in MQXZ (2015) should be interpreted with caution.[24]

To address the endogeneity concerns about lendable shares, MQXZ (2015) also use Reg

[23] We do not include those results for brevity, but all untabulated results are available upon request.

[24] To mitigate the above concern, MQXZ (2015) use an instrumental variable (IV) approach (ETF ownership as the IV) and argue that ETFs are among the main participants in the share lending market and are passive investors who do not typically engage in active trading. However, neither the entering nor the exiting of indexes is random. For example, indexers usually select a group of stocks with certain common characteristics. For example, MQXZ (2015) mention that ETF ownership may be related to uncontrolled firm-specific characteristics such as investor attention or liquidity, which may directly increase the price efficiency of firms and effectively discipline managers (e.g., Chung, Elder, and Kim 2010). Therefore, using ETF ownership as an IV is likely biased and the endogeneity issue may not be resolved. We follow Crane, Michenaud, and Weston (2016) and conduct a regression discontinuity design (RDD) analysis based on the Russell 1000/2000 indexes. We find that the instrumented lendable shares are no longer positively associated with insider selling (untabulated). The results are available upon request.

SHO as an exogenous event. Unlike our analysis, they examine the changes in insider sales surrounding the *announcement* of the SHO program. They show that opportunistic insider sales increase in the period after the announcement but before the short-selling deregulation takes effect. It is possible that insiders significantly liquidate their shares over the period right after the announcement, and hence they have fewer shares to sell thereafter in the SHO program period. However, our following additional analysis does not support this explanation.

First, we plot the average opportunistic sales ($OpptunSales$) in each quarter from 2003Q4 to 2007Q2. Figure 2 shows that the trends of pilot and control firms in the pre-effective period (i.e., before 2005Q3) are largely similar and pilot firms do not reduce their opportunistic sales until after the SHO program is effective. In addition, consistent with MQXZ (2015), we find that there is indeed an increase in opportunistic sales for pilot firms (relative to control firms) after the *announcement* of the SHO program (i.e., after 2004Q3), especially if we compare it with the quarter immediately before the announcement, 2004Q2 (i.e., the benchmark pre-announcement period used by MQXZ 2015). However, we find such an increase becomes less visible if we compare it with other periods prior to 2004Q2, for example, 2004Q1 or 2003Q4. We conjecture that the finding of an increase in opportunistic sales of pilot firms may be sensitive to the choice of a benchmark pre-announcement period.

To formally test the above conjecture, we next focus on the testing period similar to that of MQXZ (2015) and examine the impact of the SHO program *announcement* on insider opportunistic sales. Specifically, we define $PostAnnouncement$ as equal to one for the quarter between October 2004 and December 2004 or 2004Q4, and zero otherwise (the list of stocks in the SHO program was announced in July 2004). We then re-estimate Eq. 1 to compare the opportunistic insider sales in the quarter between April 2004 and June 2004 or 2004Q2 (hereafter

"Massa et al. Benchmark") with the opportunistic insider sales during the post-announcement period. We report the regression results in column (1) of Table 6. We find that the coefficient on $Pilot \times PostAnnouncement$ is positively significant, which is consistent with the findings of MQXZ (2015), and suggests that pilot insiders trade more after the announcement of the SHO program than non-pilot insiders.

To test the robustness of this result, we implement a different specification. Specifically, we create an alternative benchmark period for the three months prior to the "Massa et al. Benchmark" period (i.e., the quarter between January and March 2004 or 2004Q1). Next, we compare opportunistic insider sales during the post-announcement period (2004Q4) and the alternative benchmark period (2004Q1). Column (2) of Table 6 reports the estimation results. Interestingly, relative to the alternative benchmark period (2004Q1), the previously observed increase in opportunistic insider sales becomes insignificant ($Pilot \times PostAnnouncement$). These results suggest that the findings in MQXZ (2015) could be particularly affected by a change in opportunistic insider sales prior to the announcement of the SHO experiment due to some unobservable effect. The results in columns (3) and (4) show that our findings are not affected if we exclude the entire period used by MQXZ (2015) from April 2004 to November 2004, or the entire year of 2004.

Robustness Tests

Alternative Measures of Opportunistic Insider Trading

First, we redefine whether insider sales are opportunistic or routine by following Ke et al. (2003), who show that corporate insiders make trades on non-public information about a forthcoming earnings "break" (i.e., a break in a string of consecutive increases in quarterly earnings) as early as nine quarters prior to the break news being publicized. Using the Ke et al. (2003) method (herein "KHP method"), we can mitigate the potential measurement errors of the

Cohen et al. (2012) method for defining opportunistic selling.[25] The KHP method can also answer whether our results are robust to the inclusion of unclassified trades in the Cohen et al. (2012) method. Accordingly, we classify all nine quarters before an earnings break of a given firm as opportunistic-trade quarters. This approach gives 38,831 opportunistic-trade firm-quarters. We consider all of the insider sales made during opportunistic-trade quarters as opportunistic sales and zero opportunistic sales in other quarters ($KHPSales$). The results (untabulated) show that our inferences are not affected.

Ex Ante Trading Profits

In addition to the test of insider trading profits based on an *ex post* perspective, we conduct an analysis from an *ex ante* perspective. Prior studies (e.g., Fang et al. 2016; Ke et al. 2018) have suggested that stock prices are more efficient and information asymmetry is reduced in the post-SHO program period. A reduction in information asymmetry between insiders and outsiders may decrease the over-valuation of pilot stocks and thus decrease the potential "benefits" that insiders can obtain from insider trading. If our baseline results on the reduction in opportunistic insider selling are driven by a decrease in trading profits, we expect the reduction to be stronger for firms with greater decreases in information asymmetry during the SHO program.

Using analyst forecast dispersion and analyst forecast errors to measure information asymmetry (e.g., Boehme, Danielsen, and Sorescu 2006; Ke et al. 2018), we identify the subsamples of firms for which insider selling profits are most likely to be lower during the SHO program. Specifically, we partition our sample into two subsamples based on the change in information asymmetry: firms with a decrease in information asymmetry ($DecInfoAsy = 1$) and firms with no decrease in information asymmetry ($DecInfoAsy = 0$). We next re-estimate

[25] The Cohen et al. (2012) method requires an insider to have a reasonable length of trading records, and this requirement affects how accurately we can classify opportunistic trades.

Eq. 1 for each subsample separately. Our results (untabulated), suggest that the reduction in opportunistic sales is not driven by a greater reduction in *potential* profits because the coefficient on $Pilot \times During$ is not significantly negative for the subsample of firms with a decrease in information asymmetry ($DecInfoAsy = 1$). This finding suggests that our results cannot be purely driven by the "benefit" effect of the SHO program on opportunistic insider sales.[26]

Additional Control Variables

Previous studies show that Reg SHO has an impact on corporate policies and market characteristics. There is no panacea for the issue of confounding factors, but a common approach is to explicitly consider a vector of control variables that captures the factors documented in the literature. Thus, we control for additional variables (AC) related to information environments, disclosure properties, or managerial behaviors that previous studies have identified as being affected by Reg SHO. Specifically, we consider accrual earnings management (Fang et al. 2016), earnings forecast precision (Li and Zhang 2015), and readability of 10-K filings (Li and Zhang 2015), audit effort (Hope, Hu, and Zhao 2017), and analyst forecast characteristics (Ke et al. 2018). In addition, we include their interaction terms with $During$ and $Pilot$ (i.e., AC, $During \times AC$, and $Pilot \times AC$) in Eq. 1. Our untabulated results show that our main findings remain strong after controlling for these variables.

Alternative Settings

We examine our hypothesis by using a regulatory experiment: Reg SHO in the U.S. We then consider two alternative settings, the staggered deregulation pilot programs of short selling in China and Hong Kong, to assess the generalizability of our findings.

[26] Note that the cross-sectional variation in the change in information asymmetry reflects the potential profits that insiders can earn *ex ante,* and not the actual profits *ex post*. Using the decrease in information asymmetry to proxy for the overall potential profits of opportunistic sales, our results are independent of insiders' trading decisions and the potential self-selection bias is smaller than that based on observed insider sales.

First, on March 31, 2010, the China Securities Regulatory Commission (CSRC) introduced a pilot program of short selling deregulation in Shanghai and Shenzhen Stock Exchanges that allowed 90 stocks to be sold short. The designated list of stocks eligible for short selling has since experienced five major revisions between 2011 and 2016, and a subset of stocks was added to or removed from the list. At the end of 2016, 950 stocks are included in the list, and this number remains stable with quarterly adjustment since then. This staggered deregulation pilot program generate both time-series and cross-sectional heterogeneity in short-sale constraints. We use the Chinese data from March 2010 to December 2014 to examine the effect of allowance for short selling on insider sales. Columns (1) and (2) of Table 7 report the results that align with our main analysis. Specifically, relaxation of short-sale restrictions reduces insider opportunity sales. The large economic impact observed in the Chinese sample is consistent with the Chinese market being poorly regulated and characterized by a large volume of illegal trading prior to the program (Piotroski and Wong 2012).[27]

As an additional robustness check, following the spirit of Massa et al. (2015), we use a Hong Kong sample during 2012-2015 as a supplementary setting. The Hong Kong setting is similar to the Chinese setting. Specifically, we identify a treatment group of stocks that are eligible for short selling during the period of 2014 to 2015 but not eligible during the period of 2012 to 2013, and a control group of stocks that are not eligible for short selling during the entire period of 2012-2015.[28] As a result, the designated list of stocks eligible for short selling changes

[27] We note that there may be two concerns about the Chinese setting. First, the firms included in the CRSC program must satisfy certain criteria. Although we explicitly control for the selection criteria by using the relevant firm characteristics in our analysis, the CRSC program is inherently less clean than the SHO experiment in the U.S. Second, although the litigation risk of insider trading is non-trivial in the Chinese stock market over our testing period, the overall institutional environment could still be weak, which leads to the question of whether the economic channels we document for the U.S. can be applied to China.

[28] Short selling was prohibited until January 1994, when Hong Kong introduced a pilot scheme allowing short selling for a list of 17 stocks. Since then, the list of firms that are eligible for short selling has changed over time similar to the Chinese setting. Due to data limitation of historical insider trading, we start our testing sample period

over time, which generates both time-series and cross-sectional variations in short-sale restrictions. Using the Hong Kong sample can help to mitigate the concerns about the unique characteristics of the China market.[29] In addition, Hong Kong has a more mature capital market and has better investor protections than mainland China.[30] Columns (3) and (4) of Table 7 report the results. Using the Hong Kong sample, we also find less opportunistic insider selling for firms eligible for short selling. However, the caveat is that it is still difficult to fully rule out some confounding effect (e.g., the government's tightening of monitoring during the same period in China), or other omitted variable issues (e.g., the degree of impact differs across firms with stocks that can be sold and those not). Therefore, the results of Chinese and Hong Kong settings should be interpreted with caution.

V. Conclusion

We investigate how the threat of short selling affects insider sales. From an ex ante perspective, MQXZ (2015) hypothesize that short sellers induce insiders to sell more and faster to preempt information and profit competition. We relax the assumption of "inactive" short sellers and formulate a new hypothesis that the threat of short sellers deters opportunistic insider trading.

We examine this new hypothesis using a quasi-natural experiment in the U.S. Consistent with our prior, we show that the volume of opportunistic insider sales is reduced for pilot firms in the SHO program, especially for smaller firms. Furthermore, we find that although insiders in

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from 2012 and focus on a four-year period ending 2015.

[29] Our untabulated Hong Kong results show that the number of non-state-owned firms eligible for short selling is about nine times larger than that of Chinese state-owned firms. The average level of short interests (total number of shares shorted in a quarter scaled by shares outstanding) in a Chinese state-owned firm is twice as high as the level in non-state-owned firms.

[30] According to La Portal et al. (1997), Hong Kong's score on the Rule of Law is 8.22, far above the world average of 6.85, suggesting that it has a strong tradition of law and order. In La Porta et al. (2002), Hong Kong's Anti-Director Rights score, an index aggregating shareholder rights, is among the highest in the world, indicating that Hong Kong has some of the best shareholder protections in the world.

pilot firms reduce their opportunistic sales during the SHO program, the realized insider trades are more profitable than in the pre-program period. Taken together, our results suggest that the presence of short sellers increases the expected costs of opportunistic insider selling.

Further analysis supports our identification of three channels through which the threat of short selling increases insiders' concerns about the costs of opportunistic selling. Specifically, we find that the impact of the pilot program on insider opportunistic sales and their profits per trade is most pronounced among firms with higher litigation risk, with greater reputation concerns, and whose insiders have more stock-related holdings. The findings are more consistent with our disciplining hypothesis. Finally, we show that our results continue to hold in two alternative settings of China and Hong Kong. Taken together, our evidence supports the beneficial role of informed outside investors—they not only contribute to the informational efficiency of the stock market but also help to monitor and discipline insiders in the corporate market.

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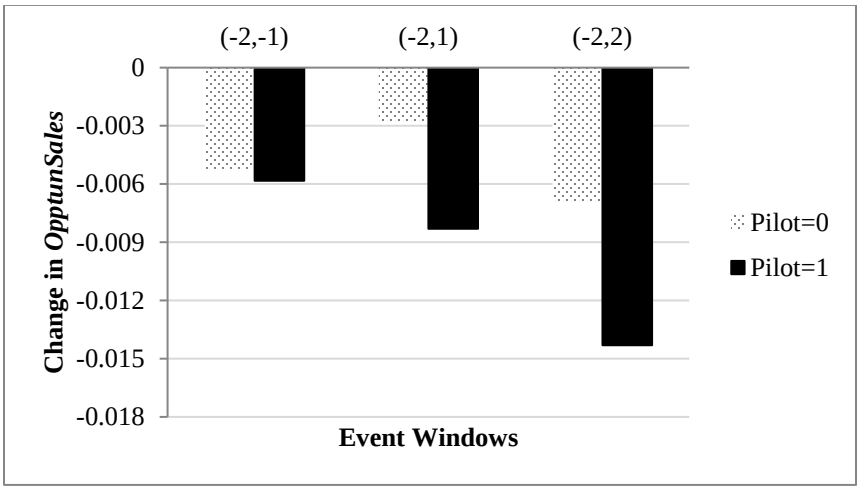
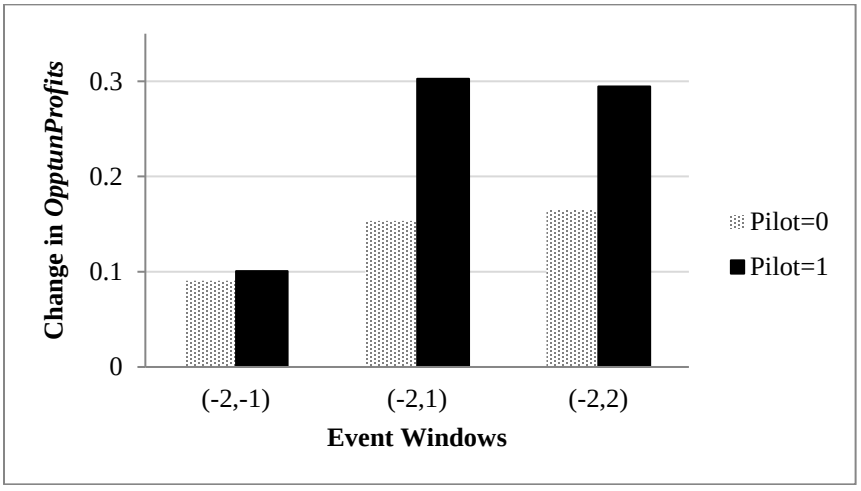
Figure 1A. Average Changes in Opportunistic Insider Sales ($OpptunSales$) along the SHO program



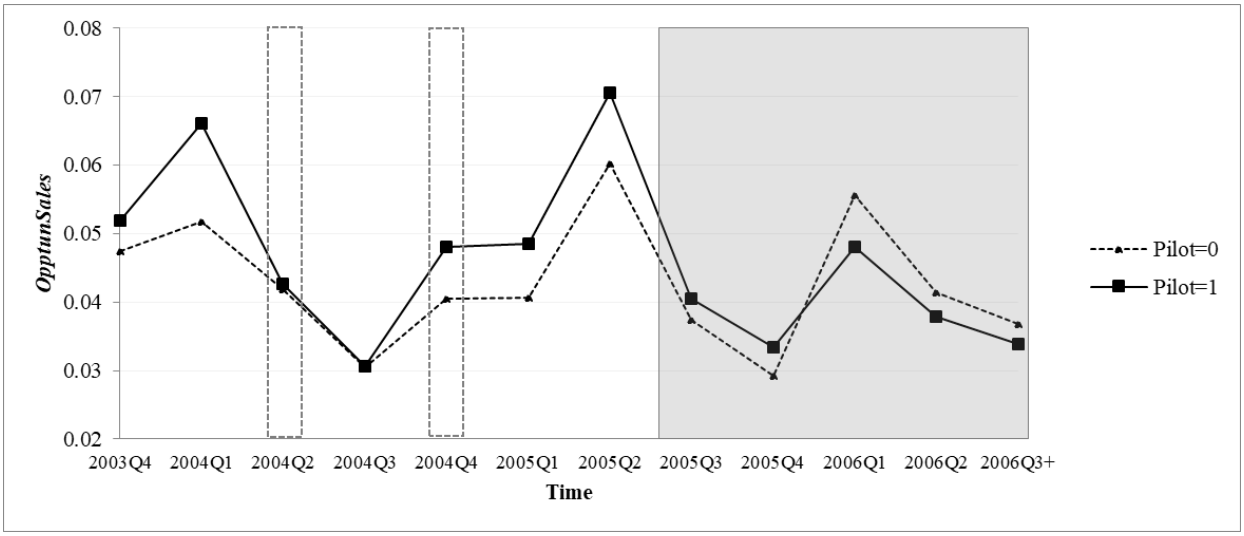
This figure plots the changes in the level of opportunistic insider sales ($OpptunSales$) for pilot firms ($Pilot = 1$) and control firms ($Pilot = 0$) around the SHO program takes effect. We define the second quarter of 2005 (the Reg SHO effective date) as event time 0. The change in $OpptunSales$ is computed from two years before the quarter when the SHO program becomes effective (i.e., year -2) to one year before the program (i.e., year -1) and to *t* years (*t* = 1 and 2) after the program commences. All variables are defined in Appendix A.

Figure 1B. Average Changes in Opportunistic Insider Selling Profits (*OpptunProfits*) along the SHO program



This figure plots the average changes in the profits of opportunistic insider sales per trade ($OpptunProfits$) for pilot firms (*Pilot* = 1) and control firms (*Pilot* = 0) around the SHO program takes effect. We define the second quarter of 2005 (the Reg SHO effective date) as event time 0. The change in *OpptunProfits* is computed from two years before the quarter when the SHO program becomes effective (i.e., year -2) to one year before the program (i.e., year -1) and to *t* years (*t* = 1 and 2) after the program commences. All variables are defined in Appendix A.

Figure 2. Opportunistic Insider Sales Trends



This figure shows the quarterly average level of opportunistic insider sales ($OpptunSales$) from 2003Q4 to 2007Q2 for pilot (*Pilot* = 1) and control (*Pilot* = 0) firms, respectively. 2006Q3+ represents all quarters from 2006Q3 to 2007Q2. The grey area represents the testing period after SHO program commencement. The two dotted boxes represent the testing period of MQXZ (2015). All variables are defined in Appendix A.

TABLE 1: Descriptive Statistics

Panel A: Insider-trading and Key Firm Characteristics of Pilot and Control Firms before the SHO Program

	Pilot group (*Pilot* = 1)			Control group (*Pilot* = 0)			Tests for diff.	
Variable	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	Mean *t*-stat.	Median *z*-stat.
Insider Sales								
Sales	0.257	0.010	0.664	0.251	0.016	0.606	0.21	-1.20
Value	3.563	0.112	9.215	3.068	0.194	7.443	1.42	-1.52
Profits	-0.273	0.010	2.925	-0.134	0.009	2.279	-0.95	0.07
Firm Characteristics								
logAT	6.959	6.802	1.673	6.987	6.909	1.775	-0.40	-1.12
MB	3.045	2.113	2.746	3.034	2.107	2.898	0.10	0.26
Ret	0.122	0.087	0.22	0.129	0.089	0.235	-0.87	-0.31
ROE	0.019	0.028	0.082	0.017	0.029	0.085	0.67	-0.79
Grants	0.001	0.000	0.002	0.001	0.000	0.002	-0.04	1.34

Panel B: Summary Statistics

	N	Mean	Median	Std. Dev.
OpptunSales	55,002	0.038	0.000	0.128
OpptunProfits	14,664	-0.025	0.004	1.191
logAT	55,002	7.093	6.987	1.728
MB	55,002	3.041	2.207	2.775
Ret	55,002	0.058	0.042	0.223
ROE	55,002	0.016	0.028	0.086
Grants	55,002	0.001	0.000	0.003

Panel C1: Correlation Matrix of *OpptunSales* tests

	OpptunSales	*logAT*	*MB*	*Ret*	*ROE*	*Grants*
OpptunSales		-0.091***	0.073***	0.0876***	0.087***	0.034***
logAT	0.057***		-0.179***	-0.065***	0.234***	-0.137***
MB	0.197***	-0.152***		0.143***	0.012***	0.030***
Ret	0.094***	-0.015***	0.173***		0.031***	0.028***
ROE	0.194***	0.250***	0.324***	0.086***		-0.098***
Grants	0.048***	-0.057***	0.071***	-0.005	-0.072***	

Panel C2: Correlation Matrix of *OpptunProfits* tests

	OpptunProfits	*logAT*	*MB*	*Ret*	*ROE*	*Grants*
OpptunProfits		0.026***	0.007	-0.034***	-0.039***	-0.015*
logAT	-0.030***		-0.164***	-0.128***	0.127***	-0.149***
MB	0.028***	-0.162***		0.135***	0.198***	0.036***
Ret	-0.022***	-0.087***	0.147***		-0.000	0.046***
ROE	-0.057***	0.167***	0.390***	0.042***		-0.072***
Grants	-0.011	-0.058***	0.080***	0.015*	-0.045***	

This table reports sample descriptive statistics. Panel A reports summary statistics for insider trading measures and firm characteristics for pilot firms (*Pilot* = 1) and control firms (*Pilot* = 0), measured at the end of 2003, the fiscal year before the SEC's selection of pilot firms. The *t*-statistic (Wilcoxon test) is used to test for the difference between the means (medians) of the pilot and control firms. Panel B reports the summary statistics of variables used in our main analysis. Panels C1 (C2) reports the correlation matrix between variables in main tests of *OpptunSales* (*OpptunProfits*). The upper triangle reports the Pearson correlation and the lower triangle reports the Spearman correlation. Full definitions are given in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

TABLE 2
Baseline Results of Opportunistic Sales

	(1)	(2)	(3)	(4)	(5)
	Full Sample	Full Sample	Full Sample	Big Firms	Small Firms
Dep. Var. =	*OpptunSales*	*OpptunSales*	*RoutineSales*	*OpptunSales*	*OpptunSales*
Pilot×During	-0.385**	-0.430**	-0.010	-0.113	-0.754**
	(-2.15)	(-2.34)	(-0.24)	(-0.53)	(-2.57)
Pilot	0.163	-0.711	-0.078	-0.419	-0.928
	(1.02)	(-1.15)	(-0.84)	(-0.41)	(-0.54)
During	0.337	0.467	-0.200***	0.269	-0.883
	(0.73)	(0.74)	(-3.38)	(0.44)	(-0.59)
logAT	-0.379***	-0.420***	-0.025***	-0.493***	-0.274
	(-8.43)	(-6.37)	(-2.94)	(-7.48)	(-1.40)
MB	0.084***	0.095***	0.006	0.084	0.150***
	(4.00)	(2.90)	(1.19)	(1.47)	(3.06)
Ret	2.989***	2.483***	0.179***	1.777***	2.632***
	(6.98)	(5.82)	(3.51)	(3.09)	(5.53)
ROE	8.169***	8.642***	0.597***	5.292***	10.579***
	(11.25)	(8.06)	(3.71)	(4.78)	(6.71)
Grants	68.255***	85.998***	4.634	143.946**	63.294**
	(2.86)	(3.18)	(1.30)	(2.44)	(2.32)
Lag1OpptunSales	26.412***	26.316***		21.595***	27.645***
	(17.47)	(13.10)		(13.21)	(9.93)
Lag4OpptunSales	19.947***	19.019***		23.751***	16.536***
	(15.94)	(11.83)		(7.76)	(10.39)
Lag1RoutineSales			20.596***		
			(9.34)		
Lag4RoutineSales			60.909***		
			(30.83)		
Pilot×Controls	No	Yes	Yes	Yes	Yes
During×Controls	No	Yes	Yes	Yes	Yes
Adj. R^2	0.171	0.172	0.475	0.171	0.170
No. of obs.	55,002	55,002	55,002	28,103	26,899
F-test for Diff in Coeff on *Pilot×During* between columns (4) and (5)				F = 3.18*	

This table reports the results of regressions estimating the difference-in-differences in opportunistic insider sales between pilot and non-pilot firms and comparing the pre-SHO and during SHO program periods. We estimate the following regression model:

$$OpptunSales\ \ (RoutineSales) = \alpha_0 + \alpha_1 Pilot \times During + \alpha_2 Pilot + \alpha_3 During + \gamma X + Industry\ \ FE + Year\text{-}quarter\ \ FE + \varepsilon.$$

X represents a vector of variables, including control variables (*Controls*), and the interaction terms between *Pilot* (*During*) and *Controls*. Full definitions of the variables are given in Appendix A. Intercepts and industry- and year-quarter-fixed effects are included in all regressions, but are not reported. Interaction terms between *Pilot* (*During*) and *Controls* are included in columns 2-5, but are not reported. In columns 4 and 5, we partition our sample into two subsamples of big firms and small firms according to the sample median of total assets at the quarter end before the effective date of the SHO program. All the coefficients are multiplied by 100 for readability. The *t*-statistics based on robust standard errors clustered by firm and year-quarter are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

TABLE 3
Baseline Results of Opportunistic Selling Profits

	(1)	(2)	(3)	(4)	(5)
	Full Sample	Full Sample	Full Sample	Big Firms	Small Firms
Dep. Var. =	*OpptunProfits*	*OpptunProfits*	*RoutineProfits*	*OpptunProfits*	*OpptunProfits*
Pilot×During	0.122***	0.106**	0.045	0.032	0.171*
	(2.77)	(2.47)	(0.69)	(0.79)	(1.85)
Pilot	-0.080**	-0.396**	-0.504	-0.153	-1.215**
	(-2.10)	(-2.57)	(-1.42)	(-1.12)	(-2.42)
During	-0.060	0.330**	0.164	0.416***	1.021*
	(-0.58)	(2.06)	(0.60)	(2.67)	(1.84)
logAT	0.020	0.027**	0.033	0.031**	0.148**
	(1.43)	(2.02)	(1.62)	(2.45)	(2.09)
MB	0.007	0.017**	0.017**	0.024**	0.014
	(1.41)	(2.07)	(2.01)	(2.54)	(1.13)
Ret	-0.114	-0.125	-0.207	-0.192	-0.023
	(-0.86)	(-0.74)	(-1.21)	(-1.35)	(-0.11)
ROE	-0.848***	-1.289***	-1.075*	-1.354***	-1.199**
	(-3.45)	(-3.28)	(-1.85)	(-3.59)	(-2.05)
Grants	-6.710	-5.140	1.788	-6.700	-4.285
	(-1.60)	(-1.02)	(0.23)	(-0.65)	(-0.48)
Lag1OpptunSales	-0.065	-0.154		-0.212	-0.118
	(-0.89)	(-1.12)		(-1.15)	(-0.71)
Lag4OpptunSales	-0.059	-0.154		-0.298**	-0.108
	(-1.07)	(-1.54)		(-2.04)	(-0.66)
Lag1RoutineSales			0.138		
			(1.09)		
Lag4RoutineSales			-0.113		
			(-0.54)		
Pilot×Controls	No	Yes	Yes	Yes	Yes
During×Controls	No	Yes	Yes	Yes	Yes
Adj. R^2	0.016	0.020	0.040	0.028	0.025
No. of obs.	14,664	14,664	3,838	8,401	6,263
F-test for Diff in Coeff on *Pilot×During* between columns (4) and (5)				F = 1.82	

This table reports the estimation results of the following regression model:

$$OpptunProfits\ \ (RoutineProfits) = \alpha_0 + \alpha_1 Pilot \times During + \alpha_2 Pilot + \alpha_3 During + \gamma X + Industry\ \ FE + Year\text{-}quarter\ \ FE + \varepsilon.$$

X represents a vector of variables, including control variables (*Controls*), and the interaction terms between *Pilot* (*During*) and *Controls*. Full definitions of the variables are given in Appendix A. Intercepts and industry- and year-quarter-fixed effects are included in all regressions, but are not reported. Interaction terms between *Pilot* (*During*) and *Controls* are included in columns 2-5, but are not reported. In columns 4 and 5, we partition our sample into two subsamples of big firms and small firms according to the sample median of total assets at the quarter end before the effective date of the SHO program. The *t*-statistics based on robust standard errors clustered by firm and year-quarter are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

TABLE 4
Parallel Trend Analysis

	(1)	(2)
Dep. Var. =	*OpptunSales*	*OpptunProfits*
Pilot×Pre1	0.040	-0.032
	(0.14)	(-0.42)
Pilot×During	-0.416*	0.096**
	(-1.91)	(2.05)
Pilot	-0.767	-0.377**
	(-1.24)	(-2.55)
Pre1	-2.571**	0.586***
	(-2.30)	(2.78)
During	-2.188**	0.569***
	(-2.56)	(3.17)
Controls	Yes	Yes
Pilot×Controls	Yes	Yes
Pre1×Controls	Yes	Yes
During×Controls	Yes	Yes
Adj. R^2	0.173	0.022
No. of obs.	55,002	14,664

This table reports the estimation results of the following regression model:

$$DepVar = b_0 + b_1 Pilot \times Pre1 + b_2 Pilot \times During + b_3 Pilot + b_4 Pre1 + b_5 During + \gamma X + Industry\ \ FE + Year\text{-}quarter\ \ FE + \varepsilon.$$

where *DepVar* represents *OpptunSales* or *OpptunProfits*. *X* represents a vector of variables, including control variables (*Controls*), and the interaction terms between *Pilot* (*During*) [*Pre1*] and *Controls*. Full definitions of the variables are given in Appendix A. Intercepts and industry- and year-quarter-fixed effects are included in all regressions, but are not reported. All the coefficients in column 1 are multiplied by 100 for readability. The *t*-statistics based on robust standard errors clustered by firm and year-quarter are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

TABLE 5
Economic Channels

Panel A. Level of Opportunistic Sales

	Litigation Channel		Reputation Channel		Personal Wealth Channel	
Channel =	*HighLitig*		*HighRep*		*HighWealth*	
	Industry Litigiousness	Firm Litigiousness	All News	Insider Trading Related News	CEO's Equity Wealth	Top 5 Executives' Equity Wealth
Dep. Var. = *OpptunSales*	(1)	(2)	(3)	(4)	(5)	(6)
Pilot×*During*	-0.320	0.170	0.013	-0.177	0.418*	0.592**
	(-1.47)	(0.38)	(0.06)	(-0.85)	(1.67)	(2.16)
Channel×*Pilot*×*During*	-0.528	-0.764*	-0.305	-0.735*	-0.955**	-1.359***
	(-1.52)	(-1.67)	(-0.85)	(-1.71)	(-1.96)	(-3.65)
Pilot, During, Channel, Pilot×*Channel, During*×*Channel*	Yes	Yes	Yes	Yes	Yes	Yes
Controls, Pilot×*Controls, During*×*Controls*	Yes	Yes	Yes	Yes	Yes	Yes
Channel×*Controls*, *Pilot*×*Channel*×*Controls, During*×*Channel*×*Controls*	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.174	0.176	0.174	0.177	0.214	0.212
No. of obs.	53,473	53,473	50,850	50,850	25,564	26,887
Test on *Pilot*×*During* + *Channel*×*Pilot*×*During*	-0.849***	-0.595***	-0.291	-0.912**	-0.537	-0.767**
	(-2.90)	(-3.40)	(-1.29)	(-2.48)	(-1.23)	(-2.12)

This table reports the estimation results of the following regression model:

$$OpptunSales = c_0 + c_1 Pilot \times During + c_2 Channel \times Pilot \times During + \gamma Z + Industry\ \ FE + Year\text{-}quarter\ \ FE + \varepsilon,$$

where *Channel* refers to high litigition risk (*HighLitig*) in columns 1 and 2, high reputation concern (*HighRep*) in columns 3 and 4, and high equity wealth (*HighWealth*) in columns 5 and 6. The sample size varies because some cross-sectional variables are not available for all observations. *Z* represents a vector of variables, including *Pilot*, *During*, *Controls*, *Pilot*×*Controls*, *During*×*Controls*, *Channel*, *Pilot*×*Channel*, *During*×*Channel*, *Channel*×*Controls*, *Pilot*×*Channel*×*Controls*, *During*×*Channel*×*Controls*, *Channel*×*Industry Effects*, and *Channel*×*Year Effects*. Full definitions of the variables are given in Appendix A. We multiply all the coefficients by 100 for readability. The *t*-statistics based on robust standard errors clustered by firm and year-quarter are reported in parentheses. Intercept, industry- and year-quarter-fixed effects, and *Z* are included in all regressions but not reported. We do not include industry fixed effects and the interaction of *HighLitig* and industry fixed effects in column (1). ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

TABLE 5 (Cont'd)

Panel B. Profits of Opportunistic Sales

	Litigation Channel		Reputation Channel		Personal Wealth Channel	
Channel =	*HighLitig*		*HighRep*		*HighWealth*	
	Industry Litigiousness	Firm Litigiousness	All News	Insider Trading Related News	CEO's equity wealth	Top 5 Executives' equity wealth
Dep. Var. = *OpptunProfits*	(1)	(2)	(3)	(4)	(5)	(6)
Pilot×*During*	0.098**	0.107	0.132	0.031	-0.030	-0.064**
	(2.01)	(0.96)	(1.14)	(0.42)	(-0.75)	(-2.33)
Channel×*Pilot*×*During*	0.037	-0.002	-0.071	0.148*	0.131	0.170*
	(0.46)	(-0.02)	(-0.55)	(1.74)	(1.35)	(1.82)
Pilot, *During*, *Channel, Pilot*×*Channel, During*×*Channel*	Yes	Yes	Yes	Yes	Yes	Yes
Controls, Pilot×*Controls, During*×*Controls*	Yes	Yes	Yes	Yes	Yes	Yes
Channel×*Controls*, *Pilot*×*Channel*×*Controls, During*×*Channel*×*Controls*	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.027	0.032	0.042	0.025	0.021	0.026
No. of obs.	14,481	14,481	14,025	14,025	8,047	8,497
Tests on *Pilot*×*During* + *Channel*×*Pilot*×*During*	0.135**	0.105**	0.062	0.179***	0.101	0.106
	(2.08)	(2.20)	(1.42)	(3.45)	(1.29)	(1.25)

This table reports the estimation results of the following regression model:

$$OpptunProfits = c_0 + c_1 Pilot \times During + c_2 Channel \times Pilot \times During + \gamma Z + Industry\ \ FE + Year\text{-}quarter\ \ FE + \varepsilon,$$

where *Channel* refers to high litigition risk (*HighLitig*) in columns 1 and 2, high reputation concern (*HighRep*) in columns 3 and 4, and high equity wealth (*HighWealth*) in columns 5 and 6. The sample size varies because some cross-sectional variables are not available for all observations. *Z* represents a vector of variables, including *Pilot*, *During*, *Controls*, *Pilot*×*Controls*, *During*×*Controls*, *Channel*, *Pilot*×*Channel*, *During*×*Channel*, *Channel*×*Controls*, *Pilot*×*Channel*×*Controls*, *During*×*Channel*×*Controls*, *Channel*×*Industry Effects*, and *Channel*×*Year Effects*. Full definitions of the variables are given in Appendix A. The *t*-statistics based on robust standard errors clustered by firm and year-quarter are reported in parentheses. Intercept, industry- and year-quarter-fixed effects, and Z are included in all regressions but not reported. We do not include industry fixed effects and the interaction of *HighLitig* and industry fixed effects in column (1). ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

TABLE 6
Reconciliations with MQXZ (2015)

	(1)	(2)	(3)	(4)
	Announcement Effect		Implementation Effect	
Dep. Var. = *OpptunSales*	MQXZ's (2015) benchmark period: Apr. to Jun. 2004	An alternative benchmark period: Jan. to Mar. 2004	Excluding Apr. – Nov. 2004	Excluding 2004
Pilot×*PostAnnouncement*	1.209**	0.160		
	(2.43)	(0.21)		
Pilot	-2.503*	-1.825	-0.517	-0.273
	(-1.84)	(-0.75)	(-0.81)	(-0.42)
PostAnnouncement	1.597*	-2.206		
	(1.83)	(-1.12)		
Pilot×*During*			-0.496**	-0.387*
			(-2.48)	(-1.78)
During			0.655	0.554
			(0.98)	(0.84)
Controls	Yes	Yes	Yes	Yes
Pilot×*Controls*	Yes	Yes	Yes	Yes
PostAnnouncement ×*Controls*	Yes	Yes	No	No
During×*Controls*	No	No	Yes	Yes
Adj. R^2	0.186	0.200	0.175	0.173
No. of obs.	4,876	4,853	49,016	43,850

This table reports the estimation results from the reconcilation tests with MQXZ (2015). Columns 1 and 2 report the results of the announcement effect as in MQXZ (2015). We follow MQXZ (2015) and exclude the observations between July 2004 and September 2004. Column 1 compares the post-announcement period from October 2004 to December 2004 with the benchmark period from April 2004 to June 2004. Column 2 compares the post-announcement period from October 2004 to December 2004 with the benchmark period from January 2004 to March 2004. Column 3 (4) reports the robustness check of the results in column 2 of Table 2 after excluding the period between April 2004 and November 2004 (of year 2004), respectively. Full definitions of the variables are given in Appendix A. All the coefficients are multiplied by 100 for readability. Intercept, industry and year-quarter fixed effects are included in all regressions but not reported. The t-statistics based on robust standard errors clustered by firm and year-quarter are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

TABLE 7
Short Sale Restrictions and Insider Sales: Chinese and Hong Kong Samples

	(1)	(2)	(3)	(4)
Sample =	Chinese Sample		Hong Kong Sample	
Dep. Var. =	*OpptunSales*	*OpptunProfits*	*OpptunSales*	*OpptunProfits*
Pilot×During	-0.011***	0.365**	-0.102*	0.487***
	(-3.26)	(2.25)	(-1.79)	(2.99)
Pilot	-0.084***	1.322*	-0.329*	1.313
	(-3.96)	(1.66)	(-1.82)	(0.74)
Controls	Yes	Yes	Yes	Yes
Pilot×Controls	Yes	Yes	Yes	Yes
During×Controls	Yes	Yes	Yes	Yes
Adj. R^2	0.040	0.010	0.007	0.112
No. of obs.	54,637	6,969	12,824	584

This table reports the results of regressions estimating the difference-in-differences in opportunistic insider sales and their profits between pilot and non-pilot firms and between the pre-CSRC and CSRC program periods of the Chinese and Hong Kong samples.We estimate the following regression model:

$$DepVar = \alpha_0 + \alpha_1 Pilot \times During + \alpha_2 Pilot + \alpha_3 During + \gamma X + Industry\ \ FE + Year\text{-}quarter\ \ FE + \varepsilon.$$

where *DepVar* represents *OpptunSales* or *OpptunProfits.* We follow the same definitions of *OpptunSales* and *OpptunProfits* as in Appendix A except that we use two years instead of three years to define routine sales because more than 90% of insiders in China do not have a trading history of more than three years and also because the insider trading information is not available before 2006. We define all other sales that are not routine sales as opportunistic sales for the Chinese and Hong Kong samples. In columns 1 and 2, *Pilot* is an indicator variable that equals one if a stock is designated as the short selling target in the CSRC program, and zero otherwise. *During* is an indicator variable that equals one for fiscal quarters from the time a stock has been designated as a short selling target by the CSRC during March 2010 and December 2014 for pilot firms, or all fiscal quarters during March 2010 and December 2014 for control firms, and zero otherwise. In columns 3 and 4, *Pilot* is an indicator variable equal to one for stocks that can be shorted during 2014 to 2015 and cannot be shorted during 2012 to 2013, and zero for other stocks that cannot be shorted over 2012-2015. *During* is an indicator variable that equals one for fiscal quarters during 2014 and 2015. *X* represents a vector of variables, including control variables (*Controls*), and the interaction terms between *Pilot* (*During*) and *Controls* as in Eq. 1. We do not include *Grant* in the test of the Hong Kong sample due to lack of available data. Other definitions of the variables are given in Appendix A. Intercepts and industry- and year-quarter-fixed effects, and *X* are included in all regressions, but are not reported. The *t*-statistics based on robust standard errors clustered by firm and year-quarter are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively (two-tailed test).

APPENDIX A: Variable Definitions

Dependent Variables	
OpptunSales	Shares that are opportunistically sold by insiders (officers or directors) as a percentage of the firm's shares outstanding, aggregated for each firm-quarter (in %). Opportunistic trades are defined as in Cohen et al. (2012). An insider is considered to be an officer or a director if his/her highest role (ROLECODE1) belongs to 'CB', 'D', 'DO', 'H', 'OD', 'VC', 'AV', 'CEO', 'CFO', 'CI', 'CO', 'CT', 'EVP', 'O', 'OB', 'OP', 'OS', 'OT', 'OX', 'P', 'S', 'SVP', 'VP' in Thomson Reuters database. We add up net opportunistic sales (i.e. opportunistic sales - opportunistic purchases) in days with positive net opportunistic sales (i.e., opportunistic sales > opportunistic purchases) over the quarter.
RoutineSales	Shares that are routinely sold by insiders (officers or directors) as a percentage of the firm's shares outstanding, aggregated for each firm-quarter (in %). Routine trades are defined as in Cohen et al. (2012). We add up net routine sales (i.e. routine sales - routine purchases) in days with positive net routine sales (i.e., routine sales > routine purchases) over the quarter.
OpptunProfits	The profits from opportunistic insider sales for a given quarter, which is defined as the quarterly average of the product of total opportunistic insider sales as a percentage of shares outstanding of a trading day as defined in *OpptunSales* and cumulative abnormal return (in %), times -1. Cumulative abnormal return is defined as the cumulative market adjusted return for the 60 trading days after the trading day.
RoutineProfits	The profits from routine insider sales for a given quarter, which is defined as the quarterly average of the product of total routine insider sales as a percentage of shares outstanding of a trading day as defined in *RoutineSales* and cumulative abnormal return (in %), times -1. Cumulative abnormal return is defined as the cumulative market adjusted return for the 60 trading days after the trading day.
Experiment-Related Variables	
Pilot	An indicator variable that equals one if a stock is designated as the pilot stock in the SHO program, and zero otherwise.
During	An indicator variable that equals one for fiscal quarters starting after May 1, 2005 and ending by July 6, 2007, and zero otherwise.
Pre1	An indicator variable that equals one for fiscal quarters ending between May 1, 2004 and April 30, 2005, and zero otherwise.
PostAnnouncement	An indicator variable that equals one for the fiscal quarters starting after October 1, 2004 and ending by December 31, 2004, and zero otherwise.
Channel-Related Variables	
HighLitig	High litigation risk, which equals one if (1) a firm is in a litigious industry (Francis et al. 1994) at the end of prior year; (2) a firm is in a litigious industry as in (1) or a firm's litigation index (Kim and Skinner 2012) at the end of last year is above the sample median of each year, and zero otherwise. Litigious industries include Biotechnology (SIC 2833-2836 and 8731-8734), Computers (SIC 3570-3577 and 7370-7379), Electronics (SIC 3600-3674), and Retailing (SIC 5200-5961). A firm's litigation index is calculated using the Kim and Skinner (2012) litigation probit model (2) coefficient estimates.
HighRep	High reputation concern, which equals one if the number of news articles of a firm in the prior year is greater than the annual industry median, and zero otherwise. The number of news articles is counted by (1) all news articles; (2) news articles that are specific to regulatory releases of prior insider trading activities. The news articles are collected from RavenPack.
HighWealth	High equity personal wealth which equals one if (1) the CEO's equity wealth of the prior year, (2) the top five most highly paid executives' total equity wealth of a firm of the prior year is greater than the sample median, and zero otherwise. Equity wealth is calculated as a manager's stock-related holdings (stocks and options), scaled by total common shares outstanding of the firm.
Other Variables	
Sales	Shares sold by insiders as a percentage of the firm's shares outstanding, aggregated for each firm-quarter (in %). We add up net sales (i.e. sales - purchases) in days with positive net sales (i.e.,

	sales > purchases) over the quarter.
Value	The US dollar value of shares sold by insiders, aggregated for each firm-quarter (in millions). We add up net sales value (i.e. sales value - purchases value) in days with positive net sales (i.e., sales > purchases) over the quarter.
Profits	The profits from insider sales for a given quarter, which is defined as the quarterly average of the product of total insider sales as a percentage of shares outstanding of a trading day as defined in *Sales* and cumulative abnormal return (in %), times -1. Cumulative abnormal return is defined as the cumulative market adjusted return for the 60 trading days after the trading day.
logAT	The natural logarithm of total assets of the firm at the end of quarter *t*-1 (in millions USD except for in million RMB Yuan for columns 1 and 2 of Table 7 and in million Hong Kong dollars in columns 3 and 4 of Table 7).
MB	Market-to-book equity at the end of quarter *t*-1.
Ret	The buy-and-hold raw stock return in quarter *t*-1.
ROE	Net income for quarter *t*-1, scaled by beginning book value of equity.
Grants	The number of options granted in quarter *t*-1, scaled by the number of outstanding shares.
Length	The number of quarters of the string of consecutive quarterly earnings increases before a break.
Lag1OpptunSales	*OpptunSales* in quarter *t*-1.
Lag4OpptunSales	*OpptunSales* in quarter *t*-4.
Lag1RoutineSales	*RoutineSales* in quarter *t*-1.
Lag4RoutineSales	*RoutineSales* in quarter *t*-4.