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Kemin Wang
Fudan University

Rencheng WANG
Singapore Management University, rcwang@smu.edu.sg

K. C. John Wei
Hong Kong Polytechnic University

Bohui Zhang
Chinese University of Hong Kong

Yi Zhou
Fudan University

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Kemin Wang, Rencheng Wang, K. C. John Wei, Bohui Zhang, and Yi Zhou†

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† Kemin Wang and Yi Zhou are from the School of Management, Fudan University, Shanghai, China, Rencheng Wang is from UQ Business School, the University of Queensland, Brisbane, Australia, K.C. John Wei is from HKUST Business School, Hong Kong, China, and Bohui Zhang is from UNSW Business School, UNSW Australia, Sydney, Australia. Authors' contact information: K. Wang: wangkm@fudan.edu.cn, (86) 21-25011091; R. Wang: r.wang@business.uq.edu.au, (61) 7-33468147; Wei: johnwei@ust.hk, (852)-23587676; Zhang: bohui.zhang@unsw.edu.au, (61) 2-93855834; Zhou: zhouyi@fdsm.fudan.edu.cn, (86) 21-25011078. We are grateful to Russell Investments for providing Russell 3000 index data. We thank Chen Chen, Tai-Yuan Chen, Vivian Fang, Allen Huang, Bin Ke, Clark Liu, Wenlan Qian, Baolian Wang, Haifeng You, Hong Zhang, and workshop participants at University of Queensland, University of Technology at Sydney, Griffith University, and Shanghai University of Finance and Economics for their helpful comments and suggestions. We also thank Alice Cheung for editorial assistance, especially, Peng Lian and Huajie Wang for excellent research assistance. John Wei acknowledges financial support in the form of a Research Infrastructure Grant from Hong Kong's Research Grants Council (RI/93/94.BM02). Bohui Zhang acknowledges the research grants from the ARC discovery grant (DP 120104755) and ARC linkage grant (LP130101050) from the Australian Research Council and the CIFR research grants (E026 and E028) from the Centre for International Finance and Regulation.

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

Abstract

Massa et al. (2015) show that corporate insiders sell more and trade faster in competition with short sellers. By considering the possibility that short sellers may proactively react to insider trading, we propose the new hypothesis that short sellers play a disciplinary role in reducing insiders' opportunistic trading. Using two regulatory experiments (the Regulation SHO program in the U.S. and the deregulation pilot program in China), we document a negative effect of short-selling threats on opportunistic insider selling in both countries. This evidence is supported by insiders' concerns regarding three effects of short selling: increased litigation risk, reputational damage, and loss of incentive compensation.

Keywords: **Regulation SHO; Pilot program; Short selling; Insider trading; U.S.; China**

JEL Classification: **D8; D53; G14; G18**

1. Introduction

Corporate insiders have easy access to non-public information. They can profit from trading, but such activity undermines the interests of outsiders. Short sellers are considered the most informed among outside investors. In the arena of informed traders, it is intriguing to examine how the presence of short sellers affects insiders' trading activity. From an ex ante perspective, Massa et al. (2015) hypothesize that short-selling potential induces insiders to sell more and faster to preempt information competition. This explanation is intuitive under the assumption that short sellers will not respond to insiders' preemptive sales. In this paper, we present a new theoretical prediction by relaxing the assumption of "inactive" short sellers, and we revisit the impact of short-selling threats on insider sales using two quasi-natural experiments.

Our hypothesis differs from Massa et al.'s (2015) *preemptive trading hypothesis* by considering insiders' concerns about the potential reaction of short sellers to opportunistic insider trading. Although insiders apparently sell more and faster in competition with short sellers, they are also aware that their opportunistic behavior can be attacked by short sellers. The rationale is that short sellers have a strong monetary incentive to identify bad news and that their predatory actions increase the probability that and the speed at which the market will uncover bad news, especially the misconduct of insiders (e.g., Karpoff and Lou, 2010; Hirshleifer, Teoh, and Yu, 2011). Therefore, under the threat of short selling, insiders are likely to abstain from opportunistic trading. We call this view the *disciplining hypothesis*.

We posit that short sellers discipline insiders' opportunistic trading through three mechanisms. First, insider trading on material nonpublic information is limited or prohibited by law (e.g., Bhattacharya and Daouk, 2002; Johnson, Nelson, and Pritchard, 2007).[1] Insiders do not want to be sued, and they avoid profitable trades when the associated litigation risk is high (Huddart, Ke, and Shi, 2007).[2] The litigation risk critically depends on the probability of

[1] The modern legal foundation prohibiting opportunistic insider trading in the U.S. on a federal level was established by Rule 10b-5 of the Securities Exchange Act of 1934 and was enforced by the SEC starting from 1961 (Loss and Seligman, 2004; Del Guercio, Odders-White, and Ready, 2015). An insider violates Rule 10b-5 if she trades on material, nonpublic information about a firm to which she owes a fiduciary duty. Although 1983 Supreme Court decision in *Dirks v. SEC* and SEC's Rule 14e-3 broaden the definition of an insider by including employees, "constructive insiders" (e.g., underwriters, accountants, and lawyers who, once hired, have legal duties to keep material information disclosed by the firm as confidential), and the individual owes no fiduciary duty to target firm in mergers and acquisitions, we follow prior studies (e.g., Massa et al. 2015) and focus on officers and directors of a firm. We provide a more detailed discussion of insider trading in China in Section 4.2.1.

[2] For example, the Stanford Law School Securities Class Action Clearinghouse reports that approximately 45% of class lawsuits filings are related to illegal insider trading (Cornerstone Research, 2005).

detection by regulators, but the detection of illegal insider trading is a difficult task for regulators (Cohen, Molloy, and Pomorski, 2012). Previous studies show that short sellers, who benefit from legal actions against insiders, have the incentive and ability to uncover insiders' opportunistic behavior (Dyck, Morse, and Zingales, 2010; Karpoff and Lou, 2010; Khan and Lu 2013; Hirshleifer, Teoh, and Yu, 2011; Massa, Zhang, and Zhang, 2015; Fang, Huang, and Karpoff, forthcoming).[3] For example, Dyck, Morse, and Zingales (2010) show that short sellers detect approximately 15% of corporate frauds. Thus, to ameliorate the fear of litigation, insiders should avoid opportunistic trading under the threat of short selling. We refer to this effect of short selling as the *litigation risk channel*.[4]

Second, insiders view reputation as a valuable asset over the life of their career. Their reputation can be significantly impaired if opportunistic insider trading is identified and exposed to the public by whistleblowers such as short sellers (Klein and Leffler, 1981; Dyck, Volchkova, and Zingales, 2008; Dai, Parwada, and Zhang, 2015). As the broadest information intermediaries, the media can help short sellers disseminate news of insiders' misconduct to the public (Fombrun and Shanley, 1990; Carroll and McCombs, 2003; Ljungqvist and Qian, 2016).[5] Thus, we expect insiders to reduce opportunistic sales when the media is readily accessible to short sellers. We refer to this mechanism as the *reputation channel*.

Third, insiders' personal wealth is tied to firm value via incentive compensation. An incentive package includes not only current stock and option holdings but also future increases in cash compensation. Both components are positively associated with the stock price. In the presence of bad news, insiders are expected to fight against short sellers and to hold stock holdings to protect the stock price (Khanna and Mathews, 2012; Li and Zhang, 2015). Opportunistic sales, in contrast, not only accelerate the decline of the stock price but also entice short sellers to further attack the firm value. Therefore, insiders with larger incentive

[3] Note that the litigation risk is also related to the expected loss once detected (e.g., the outcome of a lawsuit). The literature suggests that the presence of short sellers increases the probability that a lawsuit will be filed and that other legal costs against parties engaged in misconduct will be incurred (Blau and Tew, 2014). This is consistent with our view that insiders fear litigation against illegal trading once detected by short sellers.

[4] Not all types of opportunistic insider trading trigger enforcement actions by regulators or are ruled as illegal. Securities class actions or investigations on potential managerial misconducts, however, can be augmented even if the targeted insider trading is legal. Therefore, if the probability of enforcement and the resulting punishment, as *perceived* by insiders, can be influenced by the threat of short selling, we expect the threat of short selling more likely to deter insiders' opportunistic sales in firms with higher litigation risk.

[5] It becomes popular that short sellers publish their reports to expose incidence of alleged managerial misconduct on the Internet (e.g., Muddy Waters). The rise of those internet-based short sellers attracts considerable attention of mainstream media such as the *Wall Street Journal*, *Reuters*, and *Bloomberg BusinessWeek* (Reuters 2011).

compensation restrict their opportunistic sales in a battle with short sellers. We refer to this constraining effect of short selling as the *incentive compensation channel*.

The above conjectures should be tested empirically with regard to the way the presence of short sellers affects insider selling. The greatest challenge lies in identifying a causal effect. We fill this gap and examine our hypothesis by using two regulatory experiments: the Regulation SHO program in the U.S. and the deregulation pilot program in China. This empirical design has two unique features. First, the U.S. and China represent the two poles of the world for insider trading. The U.S. is governed by the most stringent securities regulations and is characterized by a low level of opportunistic trading (Bhattacharya and Daouk, 2002; La Porta, Lopez-de-Silanes, and Schleifer, 2006; Hail and Leuz, 2006). In contrast, as the largest emerging market, China is poorly regulated by its legal systems and is flooded with illegal trading (Piotroski and Wong, 2012). Given the large institutional differences between the two countries, any consistent findings would support the generalizability of our analysis.[6]

Second, the two regulatory experiments on the threat of short selling are relatively exogenous. The U.S. Regulation SHO program began in 2005 and lasted until 2007. The Securities and Exchange Commission (SEC) *randomly* selected one-third of the stocks from the Russell 3000 index as pilot stocks and exempted these stocks from price restrictions that were related to short selling. Comparably, the China Securities Regulatory Commission (CSRC) introduced the deregulation pilot program of short selling in 2010. One-third of the listed stocks in China were *gradually* included in the pilot program from 2010 to the present, creating both time-series and cross-sectional variation in short-selling restrictions. Both regulatory changes were adopted to reduce short-selling constraints and to make short selling more feasible. Using the two quasi-natural experiments within the context of a single study strengthens the credibility of our investigation.[7]

We begin by documenting a strong negative relation between short-selling threats and

[6] Bertomeu, Beyer and Taylor (2015) state, "…[I]t is often the case that a single QNE (quasi-natural experiment) is presented as a stand-alone study. One approach to address concerns about generalizability is to offer evidence on multiple QNEs within a context of a single study, or to combine evidence from one or more QNEs with large-sample descriptive evidence that is consistent with a causal mechanism. The more evidence that is brought to bear across different samples, the stronger the case for generalizability…" (p.33).

[7] One of the central concerns with the difference-in-differences design is the risk of confounding effects that would cause the treatment group to change its behavior absent a change in short-selling restrictions. This concern is exacerbated when the treatment occurs at only one point in time in contrast to staggered changes in short-selling restrictions. Because the Chinese experiment contains multiple exogenous changes during the sample period, this experiment helps to mitigate the risk of confounding effects.

insiders' opportunistic sales in U.S. and Chinese experiments. The effect is not only statistically significant but also economically relevant. For example, pilot firms following the U.S. Regulation SHO program (the Chinese deregulation pilot program) experience an 11% (53%) reduction in insiders' opportunistic sales relative to control firms. In contrast, we find a weak effect of lifting short-selling restrictions on insiders' routine sales in the U.S. experiment and no effect in the Chinese experiment. These results are robust to a set of placebo tests, such as post-reversal and trend analyses. Consistent with the disciplinary role of short sellers, these findings suggest that the increased threat of short selling discourages insiders from making opportunistic sales.

Next, we provide supporting evidence of three underlying economic channels through which short sellers discipline opportunistic insider selling, namely, litigation risk, reputation, and incentive compensation. First, the disciplining effect of short selling on insiders' opportunistic sales is more pronounced in firms that face higher litigation risk. Second, the negative relation between short-selling potential and insider's opportunistic sales is magnified by media coverage. Third, short selling attenuates more opportunistic sales in firms where insiders have a greater amount of stock and option holdings that are tied to firm value.

In the last step, we provide additional evidence in support of our *disciplining hypothesis*. First, it is plausible that the negative effect of short-selling threats on insider sales is through the real effect of short selling on a firm's financing activities (Grullon, Michenaud, and Weston, 2015). Given that short selling may depress the stock price and that the lowered stock price increases the cost of raising capital in the equity market, insiders may avoid sales that attract the attention of short sellers if the firm's external financing is needed in the future. We rule out this possibility by showing that the negative effect of short selling on insider sales is unaffected by financial constraints.

One may arguably still apply Massa et al.'s (2015) *preemptive trading hypothesis* to explain our finding that insiders liquidate a large fraction of their shares before the implementation of short-selling deregulations; hence, they subsequently have fewer shares to sell. To address this possibility, we implement two approaches. First, we follow Bertrand and Mullainathan (2003) and examine insider sales in pre- (post-) announcement and pre- (post-) implementation periods. We find a persistent trend of a decrease in insider opportunistic sales for pilot firms over the post-implementation period but no significant trend over the pre-implementation period. Our

baseline finding also holds if we exclude the testing period (pre- and post-announcement) in Massa et al. (2015). Second, we focus on insider purchases. Massa et al.'s (2015) theory has no prediction of the relation between the threat of short selling and insiders' opportunistic purchases. In contrast to their theory, our *disciplining hypothesis* implies that short sellers could also restrain insiders from buying stocks opportunistically. Indeed, when we use insiders' opportunistic purchases as the dependent variable, we observe a similar negative effect of short-selling threats on opportunistic insider trading.

Hypothetically, the decrease in insider sales can also be explained by the increase in insiders' short selling of their own firm shares. Even though insiders are strictly prohibited from short selling by the Securities Exchange Act of 1934 and corporate restrictions in the U.S., the deregulation program in China does not perfectly prohibit insiders from shorting their own firm shares in our early sample period.[8] We challenge this alternative view with two empirical findings. First, as discussed above, our results continue to hold when we focus on insider purchase transactions, which are expected to be positively associated with the short position within a reasonable time. Second, we exclude the period between March 31, 2010, and March 18, 2013, from the Chinese sample because the new rules released on the latter date do not allow insiders to short sell their own firm shares. We find consistent evidence based on the redefined period for the Chinese sample.

Finally, we perform robustness tests on the baseline analysis. Specifically, we examine whether the threat of short selling reduces the profitability of insider sales, and we conduct sensitivity tests using alternative measures of opportunistic selling. We obtain consistent evidence that short sellers not only attenuate insiders' opportunistic sales across different measures but also reduce the profitability of their sales. Again, all the findings are consistent across the U.S. and Chinese samples.

We contribute to two strands of the literature. The primary contribution is to the literature on insider trading and short selling. From an ex ante perspective, although the pioneering study of Massa et al. (2015) shows that short-selling potential induces insiders to sell more and faster to preempt information competition, we provide new evidence that short sellers play a disciplinary

[8] Specifically, Section 16(c) prohibits short sales by insiders, an early example of the overall intent of the Exchange Act's regulation prohibiting perceived unfair trading practices. An example of corporate restrictions can be found at http://www.accuridecorp.com/investors/insider-traiding-guidelines/.

role in reducing insiders' opportunistic sales. Based on short sellers' interactions with insiders, our evidence is also consistent with previous findings on the ex post reaction of short selling to insider trading. For example, Khan and Lu (2013) find a significant increase in short sales before the public is informed of large insider sales, which demonstrates short sellers' ability to profit from insiders' opportunistic trading. From the perspective of insiders, Gao, Ma, and Ng (2015) show that insiders learn information from short selling and subsequently trade in the same direction as short sellers.

More broadly, our study contributes to the studies that explore mechanisms to discipline opportunistic insider trading. Previous studies find that insiders exploit their private information and earn significant abnormal profits from their trading (e.g., Seyhun, 1986; Piotroski and Roulstone, 2005; Jagolinzer, 2009; Ravina and Sapienza, 2010). Opportunistic insider trading can be significantly limited when firms receive intensive media coverage (Dai, Parwada, and Zhang, 2015), when they adopt corporate policies to prevent insider trading, such as a voluntary blackout period (Bettis, Coles, and Lemmon, 2000) or general counsel pre-approval requirements (Jagolinzer, Larcker, and Taylor, 2011), or when they have stringent internal governance (Dai et al., 2014). In addition to firm-level disciplining environments, legal rules play an important role in regulating insiders' opportunistic behavior (e.g., Bhattacharya and Daouk, 2002; Bris, 2005). In contrast to the previous literature, we provide the first evidence that short sellers, a group of well-informed and sophisticated investors, can effectively reduce opportunistic insider trading.

The remainder of the study is organized as follows. In Section 2, we review the literature and discuss our empirical predictions. Section 3 outlines our empirical methodology and describes the data. Section 4 presents the empirical results. In Section 5, we show the results of additional tests to mitigate several alternative hypotheses. Finally, we provide concluding remarks in Section 6.

2. Hypothesis Development

Insiders have direct access to private information. Without disclosing private information to the public, they can profit from trading opportunistically (e.g., Jaffe, 1974; Seyhun, 1986, 1998; Lin and Howe, 1990; Rozeff and Zaman, 1988; Lakonishok and Lee, 2001; Ke, Huddart, and Petroni, 2003; Piotroski and Roulstone, 2005; Marin and Olivier, 2008; Jagolinzer, 2009; Cohen, Molloy, and Pomorski, 2012). 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 the most informed and sophisticated. The informativeness of short selling has been demonstrated in three collections of evidence. First, increases in short interest predict lower future stock returns (e.g., 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, Lee, and Werner, 2009; Rapach, Riggenbach, and Zhou, 2015). Second, as a type of whistleblower, short sellers can effectively detect and reveal the misconduct of managers (Dyck, Morse, and Zingales, 2010; Karpoff and Lou, 2010; Hirshleifer, Teoh, and Yu, 2011; Blau and Tew, 2014). Third, short sellers have sophisticated skills in analyzing both private and public available information (Engelberg, Reed, and Ringgenberg, 2012; Khan and Lu, 2013). These characteristics of short sellers pose a threat to insiders who trade on private information at the cost of outsiders. A natural question is how insiders should react to the threat of short selling.

Motivated by a stylized model, Massa et al. (2015) propose a view that the presence of short sellers acts as a stimulus for insiders to trade more and faster. This hypothesis is founded on two intuitions. First, if insiders wait, competition from short sellers would jeopardize the profitability of insider trading. Second, faster and more insider sales can preempt short sellers. The explanation is completely plausible in a pure competition game among informed traders for trading profits. However, insiders' informed trading differs from that of short sellers because insider trading on material nonpublic information is limited or prohibited by law (e.g., Bhattacharya and Daouk, 2002; Johnson, Nelson, and Pritchard, 2007). Even though not all types of opportunistic insider trading would be ruled as illegal by regulators, investors may still lose confidence in firms where insiders conduct the opportunistic behavior because opportunistic insider trading also signals a weak corporate governance environment of firms and can impair firms' future prospects (Bhattacharya and Daouk, 2002; Francis et al., 2008). If insiders earn trading profits based on private information at the cost of outsiders (including short sellers), short sellers have an incentive to act as whistleblowers and attack these insiders' opportunistic behavior. The attack by short sellers increases insiders' litigation risk and diminishes insiders' reputation and incentive compensation. Therefore, we propose the *disciplining hypothesis*, as follows:

H1: *The threat of short selling deters insiders' opportunistic sales.*

In addition, we formulate three testable mechanisms through which the threat of short selling

reduces opportunistic insider selling:

M1: *The decrease in opportunistic sales is stronger in firms with higher litigation risk.*

M2: *The decrease in opportunistic sales is stronger in firms with higher media coverage.*

M3: *The decrease in opportunistic sales is stronger in firms in which executives have a larger amount of incentive compensation.*

3. Research Design

3.1. Data

3.1.1. The U.S. Experiment

In July 2004, the SEC issued its 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.[9] Pilot stocks were exempted from short-sale price tests during the SHO program. According to the SEC, pilot stocks were selected by i) sorting the Russell 3000 index stocks into three groups—stocks listed on the NYSE, Amex, and NASDAQ; ii) ranking these stocks in each group in May 2004 by their average daily dollar volume over the year prior to the issuance of the pilot order from highest to lowest (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. 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 usually involved in mergers and acquisitions or that have other significant corporate events are dropped from the index. Following the SEC's 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 stocks 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 the insider trading data from Thomson Reuters. The initial insider trading sample includes open-market purchases and sales made by insiders reported in Form 4.[10] We then exclude insider trading records with a cleansing code of "A" or "S" assigned by Thomson Reuters. Following prior studies (e.g., Ke, Huddart, and Petroni, 2003; Cheng and Lo, 2006;

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

[10] Our results remain similar if we consider option exercises in addition to open-market purchases and sales.

Cohen, Molloy, and Pomorski, 2012; Massa et al., 2015), we only include insider trading transactions made by officers and directors and aggregate sales (or purchases) made by insiders belonging to the same firm on the same trading day.[11]

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 that of the SHO program.[12] We further exclude observations for which financial data or stock prices are missing from Compustat and CRSP in the test period. Our final sample comprises 55,139 firm-quarters from 2,932 unique firms, of which 978 are pilot firms and 1,954 are control firms.[13]

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. Because the SHO program started on May 1, 2005, we define the SHO program period as 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, *During*, to indicate the two time periods above: *During* equals one if a fiscal quarter begins after May 1, 2005, and ends before July 6, 2007, and zero otherwise.

3.1.2. The China Experiment

We now discuss the short-selling deregulation in China. On March 31, 2010, the China Securities Regulatory Commission (CSRC) introduced the deregulation pilot program of short selling ("CSRC" program, hereafter) in the Shanghai and Shenzhen stock exchanges. The most interesting feature of this experiment is that the list of firms eligible for short selling changes over time, which creates both time-series and cross-sectional variation in short-selling restrictions. That is, this program gradually allowed short selling for a selected number of stocks

[11] Our main tests focus on insider sales alone. An alternative way to calculate insider trading activities is to combine transactions with different trading directions and to define insider trading as either net purchase or total trading amount (e.g., Lakonishok and Lee, 2001; Dai, Parwada, and Zhang, 2015). Our results are not affected if we use the combined measures as above. More discussion on insider purchase is provided in Section 5.2.2.

[12] After July 6, 2007, the SEC abolished the uptick rule for all U.S. exchange-traded securities. We examine this effect in the post-pilot program period as a placebo test to reinforce our main findings in Section 4.2.

[13] We emphasize the results from this unbalanced sample. However, the results of a balanced sample are consistently similar.

starting from 2010. Initially, only 90 blue-chip stocks were selected into the program according to the requirements of large firm size and high past operating performance. Although the above selection criteria make the experiment less clean than the SHO experiment in the U.S., the experiment remains unlikely to create spurious correlation because we explicitly control for selection criteria by using relevant firm characteristics in our analysis.

In each of the following revisions of qualification standards from 2010, a certain number of stocks were added to the program list. After several rounds of loosening standards, a total of 942 unique stocks were included in the deregulation pilot program list from March 2010 to December 2014. This number accounts for approximately one-third of the listed stocks in China. In addition to the stock inclusion, 83 firms were removed from the program list during the period because they failed to satisfy certain qualification requirements. Some of them were excluded but re-included in the list by the CSRC. To simplify our interpretation, we exclude from our sample stocks that have ever been removed from the program list, although our results remain the same when including these stocks in our sample. For robustness, we specifically test the removed stocks in the placebo analysis (see Section 4.1.2).[14]

Accordingly, we define stocks that remain in and have never been removed from the deregulation pilot program list at the end of December 2014 as pilot firms and firms that have never been included in the program list as control firms. This selection results in a sample of 859 pilot firms and 1,606 control firms. Accordingly, in the Chinese experiment, we redefine the indicator variable, *Pilot*, as equal to one if a firm's stock is designated as a short-selling target by the CSRC and zero otherwise. Next, we redefine *During*, an indicator variable that equals one if a pilot firm's fiscal quarters start after the firm is designated available for short selling by the CSRC or if a control firm's fiscal quarters start after March 2010 and zero otherwise.

We obtain the insider trading data of Chinese firms from the Wind database. The initial insider trading sample includes open-market purchases and sales made by insiders reported in the Shanghai and Shenzhen stock exchanges. Following the same rule as the U.S. insider trading sample, we include only insider trading transactions made by directors and officers as well as aggregate sales (or purchases) made by insiders belonging to the same firm on the same trading day. To mitigate the difference between U.S. and Chinese insider trading samples, we focus on

[14] Appendix B shows the timeline of pilot program on short selling of the CSRC. There are six major revisions on the pilot list between 2010 and 2014, with several minor revisions between major revisions.

directors and officers who do not have shares more than 5% over the total shares outstanding of their firms. We find consistent evidence even when removing this ownership restriction.

By merging the short-selling data with the insider trading data, we construct a Chinese sample over the period from January 2007 to December 2014. We further exclude observations for which financial data or stock prices are missing from the China Stock Market Accounting Research (CSMAR) database in the test period. Our final sample comprises 56,975 firm-quarters from 2,465 unique firms, of which 859 are pilot firms and 1,606 are control firms. Among the 859 pilot stocks, 469 are listed on the Shanghai stock exchange and 390 on the Shenzhen stock exchange.

3.2. Empirical Design

To examine the effect of short-selling threats on insider sales (H1), we first partition our insider selling activities into opportunistic sales and routine sales based on the Cohen, Molloy, and Pomorski (2012) method for both the U.S. and Chinese samples. Specifically, we designate an insider's trades on a stock in a particular month as either opportunistic or routine according to the insider's past history of trades.[15] To obtain enough data to track the trading history, we require that every insider have at least one transaction in each of the three preceding years. If the insider has traded consecutively in a particular month over the past three years, we classify her trades in that month in subsequent years as routine trades and her trades in other months as opportunistic trades.[16] By definition, routine and opportunistic trades from different insiders may co-exist in a given firm-quarter. We specify the following model for our main analysis in the U.S. and Chinese samples and examine the effect of short-selling threats on insiders' opportunistic and routine sales, respectively:

[15] In a given firm-quarter, there are three types of trades if we use the Cohen, Molloy, and Pomorski (2012) method: routine, opportunistic, and unclassified. Unclassified trades are made by some insiders who fail to trade at least once in each of the three preceding years. We exclude these unclassified trades in our tests. Our results are not affected if we include them as opportunistic trades.

[16] To increase the testing power, we use two years instead of three as the measurement window for insider trades for Chinese firms because more than 91% of insiders in China do not have a trading history of more than three years and because the insider trading information is not available before 2006. Our results are not affected if we use three years as the measurement window. To validate that such classification method is applicable in the Chinese sample, we replicate the results of Table 2 in Cohen, Molloy, and Pomorski (2012). We find that opportunistic sales (purchases) are negatively (positively) and significantly associated with future abnormal return, but routine trades are not (untabuled). Arguably, our empirical definition of opportunistic trades may still have potential measurement errors when we include trades that are unrelated to the misconduct of insiders or when we exclude trades that are related to such misconduct. Such errors, however, would likely weaken our findings. We also mitigate this concern by using the Ke, Huddart, and Petroni (2003) method and report the results in Section 5.4.2.

$$Opprtn\ (Routine) = \alpha_0 + \alpha_1 Pilot \times During + \alpha_2 Pilot + \alpha_3 During + Controls + \varepsilon, \quad (1)$$

where *Opprtn* (*Routine*) is the aggregate number of opportunistic (routine) sales in terms of shares sold by insiders within each firm-quarter as a percentage of the firm's shares outstanding. *Pilot* and *During* are defined in Section 3.1. Our main interest is the interaction term between *Pilot* and *During*. We expect the coefficient on *Pilot*×*During* to be significantly negative for opportunistic sales, whereas we have no prediction for routine sales. Following prior studies (e.g., Cheng and Lo, 2006), we control for firm size (*logMV*), market-to-book equity ratio (*MB*), firm performance (*Ret* and *ROE*),[17] and options granted to managers (*Grants*). These variables are measured at the end of the last quarter and are defined in Appendix A. We also control for opportunistic (or routine) sales in the previous quarter and include industry- and quarter-fixed effects in regressions.

Furthermore, we examine the economic channels through which short sellers impose the disciplining effect on insiders (M1-M3) using the following model:

$$\begin{aligned} Opprtn\ (Routine) = {} & \alpha_0 + \alpha_1 Pilot \times During \times Channel + \alpha_2 Pilot \times During \\ & + \alpha_3 Pilot + \alpha_4 Pilot \times Channel + \alpha_5 During \\ & + \alpha_6 During \times Channel + \alpha_7 Channel \\ & + Controls + \varepsilon, \end{aligned} \quad (2)$$

where *Channel* is a list of channel variables that are related to litigation risk (litigious industries and litigation risk index) (Francis, Philbrick, and Schipper, 1994; Kim and Skinner, 2012), reputation (media coverage of all firm activities or media coverage of the firm's insider trading activities) (Dai, Parwada, and Zhang, 2015), and incentive compensation (shares and option holdings of a firm's CEO or those of the firm's top five most highly paid executives). We expect the coefficients on the three-way interaction terms among *Pilot*, *During*, and each *Channel* to be significantly negative, implying that short sellers' disciplining role operates through the three channels (M1-M3).

[17] Our untabulated result remains unchanged if we include the interaction terms of *ROE* (and *Ret*) with *Pilot*, *During*, and *Pilot*×*During* to control for the changes in firm performance that may affect insider selling and short-selling activities.

3.3. Summary Statistics

Table 1 presents the descriptive statistics for pilot and control firms before the announcement and implementation of SHO and CSRC programs, respectively. Following previous studies (e.g., Fang, Huang, and Karpoff, forthcoming; Grullon, Michenaud, and Weston, 2015), we compare pilot and control firms' insider trading information and firm characteristics in the fourth quarter of 2003 for the U.S. sample and in the fourth quarter of 2009 for the Chinese sample. The two periods are the fiscal year ends chosen immediately before the announcements of the two pilot programs.

First, we report statistics on the measures of insider trading. We aggregate insider trading for each firm-quarter, separating insider sales from insider purchases.[18] For the U.S. sample, the average volumes of shares sold and purchased by insiders are 0.263% and 0.006% of shares outstanding (*Shares*) for pilot firms, respectively. The corresponding numbers for control firms are 0.252% and 0.004%. The average dollar amounts of quarterly insider sales and purchases (*Value*) are $3.566 million and $0.033 million for pilot firms, respectively. The corresponding numbers for control firms are $3.074 million and $0.027 million. As reported in the table, there are no significant differences between pilot and control firms for the U.S. sample in any of the insider trading measures prior to the announcement of the pilot program. When considering the Chinese sample, we find qualitatively similar results for the difference between pilot and control stocks. The statistics of insider trading variables are comparable to those in previous studies (e.g., Lian, Wang, and Zhang, 2013; Wu and Li, 2015). The results show that insiders have similar trading behavior between pilot and non-pilot firms for both the U.S. and Chinese samples.

We also report the descriptive statistics for our control variables: firm size (*logMV*), market-to-book equity ratio (*MB*), stock return (*Ret*), returns on equity (*ROE*), and option grants (*Grants*). The univariate tests show no significant differences in any of these firm characteristics for U.S. firms, supporting the SHO program as a natural experiment. Regarding the Chinese sample, we find that pilot stocks are significantly larger and perform better than control stocks (*logMV* and *ROE*), which is consistent with the fact that the CSRC selected pilot stocks according to firm size and past operating performance. We explicitly control for these relevant

[18] If no insider trades are reported for a given firm-quarter, the firm-quarter is assigned a value of zero for both the U.S. and Chinese samples. Approximately 80% (93%) of the firm-quarters have no insider purchases, and 48% (87%) have no insider sales during the sample period of the U.S. sample (the Chinese sample). Our results remain similar if we exclude firms with no insider trading during the sample period, which reduces our sample size to 53,078 and 9,632 firm-quarters for the U.S. and Chinese samples, respectively.

variables in our tests for Chinese stocks to mitigate the concern that such a non-random selection procedure could potentially create a spurious correlation between short selling and insider trading activities.

[Insert Table 1 Here]

4. Empirical Results

4.1. Main Results

4.1.1. Baseline Findings

As a preliminary analysis, we report the results of univariate difference-in-differences (hereafter, DiD) tests examining H1 in Table 2. Specifically, we compute the change in insider selling from the pre-program period to the during-program period separately for both pilot and control firms. We then take a second difference between the two groups to obtain the DiD estimates.

First, we find that insiders in pilot firms, on average, show a significant reduction in opportunistic selling during both the SHO program (Δ*Opptrn* = -0.361, *t*-stat. = -1.88) and the CRSC program (Δ*Opptrn* = -0.461, *t*-stat. = -1.88). In contrast, insiders in control firms do not experience a significant change in their selling activities (Δ*Opptrn* = -0.142, *t*-stat. = 1.05) for U.S. firms, but insiders in Chinese control firms significantly increase their opportunistic selling (Δ*Opptrn*=2.368, *t*-stat. = 7.98).[19] This potential time trend in the Chinese experiment would be well controlled in the DiD analysis. The DiD results indicate that compared with the control firms, pilot firms in the U.S. sample (the Chinese sample) experience a significant decrease in insider sales due to exogenous shocks that increase the exposure of pilot firms to short selling. Regarding routine trading, our results either show a significantly smaller magnitude of the DiD change in insider sales for the U.S. sample (Δ*Routine* = -0.111, *t*-stat. = -2.67) or fail to find any significant value for the Chinese sample (Δ*Routine* = 0.006, *t*-stat. = 0.22). In addition, routine selling for insiders in U.S. pilot firms does not decrease (Δ*Routine* = 0.019, *t*-stat. = 0.59), whereas routine selling for insiders in pilot firms from China significantly increases (Δ*Routine* = 0.043, *t*-stat=3.26). Overall, these findings from the univariate DiD tests provide initial evidence

[19] The CSRC program period coincides with the end of the blackout period of insiders in the Growth Enterprise Market, which leads to an abnormal increase in insider selling. Any impact from this could weaken our results if insiders in pilot firms have a great incentive to cash out their shares. This issue is properly considered in our multivariate analysis.

consistent with the disciplining role of short sellers on insider trading.

We then formally perform the multivariate analysis to estimate equation (1) for opportunistic insider sales and routine insider sales using the U.S. and Chinese samples, respectively. We first report the results with industry and quarter fixed effects but without controlling for other variables in the first two columns. We then report the results with control variables in the last two columns for each country. Consistent with the results of the univariate tests, the coefficients on *Pilot*×*During* for opportunistic insider sales are significantly negative without control variables (coeff. = -0.481, *t*-stat. = -1.73) or with control variables (coeff. = -0.421, *t*-stat. = -2.29) for the U.S. sample. In terms of the economic magnitude, pilot firms following the SHO program experience an 11% reduction of insiders' opportunistic sales relative to control firms.[20] With regard to routine sales, the corresponding coefficients on *Pilot*×*During* are still significantly negative (t-stat. = -2.23 or -1.65), but Chow tests display a significantly smaller decrease in routine sales than in opportunistic sales, suggesting that the disciplining effect of short sellers is concentrated on opportunistic insider trading.

We find consistent evidence, with an even larger economic magnitude in the Chinese sample than in the U.S. sample. Specifically, after controlling for firm characteristics, we find that the CSRC program reduces insiders' opportunistic sales for pilot firms by 53% relative to those for control firms, but it does not have a significant impact on routine sales. The Chow tests again confirm that the impact of short-selling threats is concentrated on opportunistic insider sales.

4.1.2. Placebo Tests

In this section, we perform placebo tests to ensure the validity of our DiD analysis, and we discuss the assumptions behind our identification strategy for both the U.S. and Chinese samples. Specifically, we evaluate the extent to which the SHO or CSRC programs are exogenous by using the post reversal test for each country.

In the U.S. sample, we first include the period subsequent to the end of the SHO program (July 2007) until December 2009 and create a dummy variable (*Post*) to indicate firm quarters following the SHO program period. If the difference in opportunistic insider sales between the

[20] We divide the coefficient on *Pilot*×*During* (-0.421) by the product of 100 and the mean of opportunistic sales (*Opptrn* in %) over our sample period (0.039, untabulated). The results remain similar if we use opportunistic sales of pilot firms prior to the SHO program as the benchmark (0.041, untabulated). We continue to use this method to calculate the economic magnitude in the remainder of the paper.

pilot and control firms is indeed driven by the variation in short-selling restrictions (and thus short-selling threats) across the two groups of firms during the SHO program (from May 2005 to June 2007), we expect the difference in opportunistic insider sales to vanish when the regulatory difference disappears in the latter period.

We report the results of the U.S. sample in Table 4. The testing period in Columns 1 and 2 is from January 2002 to December 2009. In Columns 3 and 4, we exclude the quarters during the SHO program and directly compare the difference in insider sales between the periods prior to the SHO program (from January 2002 to April 2005) and those following the end of the program (from July 2007 to December 2009). The results clearly show that lowering short-selling restrictions is associated with a lower level of opportunistic insider sales in pilot firms relative to control firms and that this effect disappears after June 2007.

Next, we conduct the post reversal test for the Chinese sample similar to the U.S. sample. Note that the introduction of deregulated short selling in China per se changes the list of firms eligible for short-selling over time, which helps to mitigate the issue from other omitted effects over the time trend. In other words, the results of Table 3 for the Chinese sample provide similar insight as the results in Columns 1 and 2 of Table 4 when we focus on the U.S. sample. To make our results more comparable between the U.S. and Chinese samples, we construct a new pilot sample (*Pilot*=1) composed of 79 firms that were designated as short-selling targets by the CSRC but were excluded from the qualification list during the period from March 2010 to December 2014.[21] Naturally, the period when these stocks were included in the list is the "during" program period (*During*=1), and the period when these stocks were removed is the "post"-program period (*Post*). We find 79 matching firms (*Pilot*=0) within the same industry for each of our new pilot firms from the control sample used in our main tests (Table 3) based on the closest firm size and stock return volatility. The results in Columns 5 – 8 show evidence consistent with the U.S. sample that the difference in opportunistic insider sales vanishes when the regulatory difference disappears in the "post" period.

The above results confirm that the aforementioned opportunistic insider selling difference between pilot and control firms is specifically caused by the SHO program or the CSRC

[21] There are 83 firms that were designated as short-selling targets by the CSRC but excluded from the program list during March 2010 and December 2014. We exclude one firm that was removed by the CSRC on December 4, 2014, because it was delisted from the Shenzhen Stock Exchange. We also drop the other three firms that were excluded from and re-included in the list by the CSRC more than once because we have insufficient data to find a matched firm for their "pre", "during" and "post" periods.

program.

4.2. Economic Channels

4.2.1. The Litigation Risk Channel

The main channel through which short sellers discipline opportunistic insider selling is the fear they instill of litigation against illegal trading. Therefore, we expect that insiders will become less incentivized to profit from nonpublic material information when they face a higher litigation risk (M1).

Empirically, we first examine the U.S. sample by following Francis, Philbrick, and Schipper's (1994) approach and defining *HighLR* equal to one if a firm is in a litigious industry and zero otherwise. Litigious industries include Biotechnology (SIC 2833 to 2836 and 8731-8734), Computers (SIC 3570 to 3577 and 7370 to 7379), Electronics (SIC 3600 to 3674), and Retailing (SIC 5200 to 5961). Column 1 of Table 5 reports the estimation results. Consistent with M1, in equation (2), we show that *Pilot*×*During*×*HighLR* is negatively significant at the 5% level (coeff. = -0.973, *t*-stat. = -2.47), whereas *Pilot*×*During* is not significant (*t*-stat. = 0.20) (in Column 1). This result suggests that relative to insiders in control firms, insiders in pilot firms reduce significantly more opportunistic sales only for firms from highly litigious industries. Switching to the Chinese sample, we define litigious industries as industries in which firms or affiliated people were punished for capital market-related misconduct by the CSRC from 2000 to 2010. The full definition is given in Appendix A. The Chinese evidence in Column 5 is similar to that of the U.S. sample.

As an alternative measure, we use a firm-level litigation risk proxy, which measures the predictability of securities class actions in the U.S. (or the CSRC punishment cases in China) against firms based on their corporate attributes. We calculate this measure by using Kim and Skinner's (2012) litigation probit model (2) coefficient estimates. The results remain similar (in Columns 3 and 7).

Routine sales, by definition, are not motivated by private information and, hence, are less likely to be considered misconduct or attacked by short sellers. Therefore, conditional on litigation risk, we do not have the same expectation regarding routine sales as we do for opportunistic sales. Columns 2, 4, 6, and 8 report the results. We do not find that routine sales have any significant changes over the sample period using either measure of litigation risk in the

two countries. This result suggests that the occurrence of routine sales is unaffected by short-selling threat through litigation risk.

An alternative view is that Chinese insiders' litigation risk is trivial, and hence the decrease in opportunistic sales should not be explained by the litigation risk channel for the Chinese sample. We disagree with this view for two reasons. First, China first made insider trading illegal in 1993 by releasing the Provisional Regulations on the Administration of Stock Issuance and Trading as well as the Provisional Measures for the Prohibition of Securities Fraud. Since then China has developed the insider trading enforcement system and used the U.S. as the benchmark. The number of rules that regulate insider trading, for example, doubles over the past two decades. Second, in 1994, only one year after making insider trading illegal by law, China took the first enforcement action on insider trading. Economist (1994) concludes that "Chinese securities law has made a quantum leap in the era of insider trading". Recently, the number of enforcement actions has increased by four times over our sample period. All the evidence suggests that the litigation risk of illegal insider trading in China is not negligible although smaller than that in the U.S.

In summary, the results suggest that the disciplining effect of short selling is stronger when managers are more concerned with the potential litigation risk for both the U.S. and Chinese markets.

4.2.2. The Reputation Channel

With respect to the reputation channel, we hypothesize that the decrease in insider opportunistic selling during the SHO program is stronger for firms when the media is readily accessible to short sellers (M2). To test this conjecture, we use media coverage to represent insiders' concerns regarding their reputational capital. The rationale is consistent with previous evidence that insiders' potential reputational loss from opportunistic behaviors is positively associated with media coverage (Fombrun and Shanley 1990; Carroll and McCombs 2003; Dyck, Volchkova, and Zingales, 2008; Dai, Parwada, and Zhang, 2015).

Specifically, we define *HighMedia* as equal to one if the number of news articles of a firm in the prior year is greater than the sample 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 prior insider trading activities. We report the estimation results in Table 6. Consistent

with the reputation channel in M2, the results indicate that media coverage increases the impact of the SHO program or the CSRC program on insider opportunistic selling. Specifically, we find that *Pilot×During×HighMedia* is negatively significant at the 5% level across the two measures of media coverage (with t-stat. ranging from -2.07 to -2.88) (in Columns 1, 3, 5, and 7). The magnitude of the reduction in opportunistic sales is significantly greater than that of routine sales (in Columns 2, 4, 6, and 8), although insiders also significantly reduce their routine sales for firms with higher insider trading-related media coverage (in Column 4).

Overall, the results suggest that the disciplining effect of the short sellers is stronger when managers are more concerned with their reputational capital.

4.2.3. The Incentive Compensation Channel

With respect to the incentive compensation channel, we argue that insiders with larger incentive compensation would restrict their opportunistic sales in a battle with short sellers (M3). To test this conjecture, we use variables for equity-related holdings (stocks and options) for CEOs and the top five highest-paid executives, respectively, as proxies for insiders' concerns regarding their personal monetary wealth (Roulstone, 2003; Ofek and Yermack, 2000). Our results are robust to using restricted stock holdings or the current equity-related compensation alone.

Specifically, we define *HighEqWealth* as equal to one if a firm's CEO (or top five highest-paid executives) has equity-related holdings greater than the sample median and zero otherwise. We report the estimation results in Table 7. Consistent with the incentive compensation channel in M3, we find that the coefficient on *Pilot×During×HighEqWealth* is negatively significant at the 5% level when we focus on a CEO's or top five executives' incentive compensation in both countries (with *t*-stat. ranging from -2.07 to -2.90) (in Columns 1, 3, 5, and 7). Moreover, the magnitude of the reduction in routine sales is significant only when we focus on the top five executives in the U.S. sample; we do not have a clear prediction regarding how they should be affected by the SHO program.

Overall, our results indicate that the disciplining effect of short sellers is stronger when managers are more concerned with their personal wealth for both the U.S. and Chinese markets.

5. Additional Tests

Our interpretation of the above results is that the SHO and CSRC programs sufficiently

reduce the cost of short selling to increase the disciplining role of short sellers. In this section, we provide additional evidence to examine whether our finding can be explained by several alternative channels.

5.1. Real Effects on Financing Activities

Our proposed channels on the effect of short sellers are based on the cost of insider selling in terms of insiders' concerns about litigation risk, reputation, and incentive compensation. However, the participation of short sellers, a group of pessimistic investors in the market, can create down-side pressure on a firm's stock price and decrease the firm's financing and investment opportunities (e.g., Grullon, Michenaud, and Weston, 2015). Thus, it is possible that insiders' tendency to decrease opportunistic selling during the pilot program reflects changes in the cost of external financing.

By decreasing opportunistic insider selling, insiders can prevent the negative consequence of short selling on corporate real activities. In other words, the financing cost channel through Grullon, Michenaud, and Weston (2015) expects that insiders in pilot firms have a greater incentive to keep the stock price high in case external financing is needed in the future. As a result, during the pilot program, insiders in firms that are more financially constrained would be less likely to offload their holdings than those in less financially constrained firms. If we fail to find this evidence, it may suggest that the negative effect of short selling on insider sales cannot be explained by the external financing concern.[22]

To test this conjecture, we use a set of variables to measure whether a firm is financially constrained, namely, the size-age (SA) index and free cash flow (Dechow, Sloan, and Sweeney, 1996; Hadlock and Pierce, 2010). We define *FConstraint* as equal to one if a firm is financially constrained and zero otherwise. A firm is classified as financially constrained if the firm is in the upper half of the SA index or the lower half of free cash flow based on the sample medians. We construct all the above measures at the beginning of the quarter. See Appendix A for details. If the alternative explanation is true, we should expect a larger decrease in opportunistic insider-selling activities for financially constrained pilot firms.

[22] Note that the direct negative consequence of being targeted by short sellers may be the potential decline of the stock price, which could be related to both insiders' personal benefits and firms' interests. It is extremely difficult to distinguish one from the other because both interests are usually tied to the stock market performance. The limitation of our tests on Grullon, Michenaud, and Weston (2015) is that we could only speak to one of the firm's interests, financing activities, but not to the others.

Table 8 reports the regression results. We find that the coefficients on *Pilot*×*During* are consistently and significantly negative using both proxies for financial constraints across Columns 1, 3, 5, and 7 (with *t*-statistics ranging from -1.75 to -2.61). However, none of the coefficients on *Pilot*×*During*×*FConstraint* is significantly negative at conventional levels. Our untabulated results continue to hold if we use bond ratings, net leverage, dividend payout ratio, or equity issuance in the next quarter as alternative measures of financial constraints (Kaplan and Zingales, 1997; Almeida, Campello, and Weisbach, 2004; Hadlock and Pierce, 2010; Campello and Graham, 2013).

Overall, the results mitigate the concern that our findings are due to the negative impact of short selling on firms' financing activities.

5.2. Preemptive Trading

Arguably, one may still apply the Massa et al.'s (2015) *preemptive trading hypothesis* to explain our finding that insiders liquidate their shares over the period after the announcement but before the implementation of short-selling deregulations and, hence, that they have fewer shares to sell thereafter. To address this possibility, we implement two approaches: 1) pre-announcement and pre-implementation tests and 2) insider purchases.

5.2.1. Pre-announcement and Pre-implementation Tests

Using a trend test, we can examine whether the difference in opportunistic insider sales between pilot and control firms is due to any pre-trends of anticipating the potential competition with short sellers, as implied in Massa et al. (2015). Specifically, we create four dummy variables to indicate five periods over the quarters from January 2002 to June 2007, the same sample period used in Table 3.

We follow Massa et al. (2015) and define Pre_1 equal to one for quarters between April 2004 and June 2004 and zero otherwise. Pre_1 accounts for the potential reaction from insiders who may anticipate the announcement of the SHO program in July 2004. Pre_2 equals one for quarters between July 2004 and April 2005 and zero otherwise. Pre_2 is used to account for any potential changes in insider selling after insiders in pilot firms are informed that short sellers will be less restricted in the near future (starting from May 2005).

Next, we divide the SHO program period into two periods. $During_0$ equals one for quarters

between May 2005 and June 2006, which is the first year of the SHO program, and zero otherwise. $During_1$ accounts for the remaining quarters in the SHO program, equal to one for quarters between July 2006 and June 2007 and zero otherwise. Overall, if the difference in opportunistic insider sales between pilot firms and control firms does indeed come from the difference in short-selling restrictions (or threat) between the two groups of firms during the SHO program (instead of others), we expect opportunistic insider sales to show a significant decrease in the periods during the SHO program ($During_0$=1 or $During_1$=1) but not in the periods before May 2005 (Pre_1=1 and Pre_2=1).

We report the results of the U.S. sample in Columns 1 and 2 of Table 9. We find that neither of the coefficients on $Pilot \times Pre_1$ and $Pilot \times Pre_2$ is statistically different from zero at conventional levels. This result indicates that insiders in pilot firms do not change opportunistic insider sales relative to control firms either in a short window immediately before the announcement of the SHO program (from April 2004 to June 2004) or during the period between the announcement and implementation of the SHO program (from July 2004 to April 2005). This finding further indicates that our pilot and control groups are similar in years before the *actual* change in short-selling restriction.[23] In addition, we find that the coefficient on $Pilot \times During_0$ is negative and statistically significant at the 10% level, suggesting that the decrease in opportunistic insider sales started immediately after the implementation of the SHO program. Furthermore, the effect is persistent because the coefficient on $Pilot \times During_1$ is also negative and statistically significant.

Note that the results of insignificant changes in opportunistic insider sales over the periods Pre_1 and Pre_2 in Column 1 help mitigate the concern that the decrease in insider sales that we document after the implementation of the SHO program is potentially mechanical. That is, insiders in pilot firms do not liquidate their shares before the implementation of the SHO program. To further rule out the possibility that our baseline finding could be contaminated by the period between the announcement and implementation of the SHO program, we exclude the entire period from April 2004 to November 2004. Columns 3 and 4 show that our results in Table 3 are robust to this alternative sample.

Next, we conduct a trend analysis using the same sample in Table 3 to examine whether the

[23] Our results do not contradict those of Table 4 in Massa et al. (2015) because we show that the difference between $Pilot \times Pre_1$ and $Pilot \times Pre_2$ is significant, although neither coefficient is significant on its own.

change in opportunistic insider sales in China is anticipated. Specifically, we create three dummy variables to indicate three periods over the quarters from January 2007 to December 2014, the same sample period used in Table 3. First, the pre-CSRC program period is defined as the period between January 2007 and the date when pilot stocks are designated as available for short selling by the CSRC on March 31, 2010, for control firms. We split the pre-CSRC program period equally into three stages: the early stage, middle stage, and last stage. Then, we define Pre_1 (Pre_2) as equal to one for quarters during the middle stage (last stage) of the pre-CSRC program period and zero otherwise.

We next divide the period of the CSRC program into two periods. We define $During_0$ as an indicator variable that equals one for quarters during the first two years of the CSRC program period and zero otherwise. $During_1$ equals one when accounting for the remaining quarters until the end of December 2014 for each firm and zero otherwise.

Again, we find that the difference exists only in the periods during the CSRC program ($During_0$=1 and $During_1$=1), not during the periods before the CSRC program (Pre_1=1 or Pre_2=1). Finally, the results of Columns 7 and 8 show that our findings of the Chinese sample are not driven by the potential and mechanical reverse of insider selling during the announcement window when we exclude the observations from August 2009 to March 2010.[24]

5.2.2. Insider Purchases

In addition, we investigate whether short sellers affect opportunistic insider purchases, an effect that is consistent with our disciplining effect of short sellers but that cannot be explained by the *preemptive trading hypothesis* as in Massa et al. (2015). Massa et al. (2015) propose that short sellers compete for profits based on negative information on future firm performance with insiders, but they do not speak to the effect on insider purchases, which are more closely related to positive information.[25]

Our proposed hypothesis on the disciplinary role of short sellers, however, also applies to

[24] On January 8 2010, the State Council of China, or cabinet, authorized the CSRC to introduce a pilot period for short selling in the domestic A-share market. The question that had been left unanswered was which companies could participate in such trading. Most analysts expected a small number of big blue-chip companies to be given access to the new tools before the program gradually expands to second-tier companies. (http://www.ftchinese.com/story/001030749/en?page=rest; http://www.chinadaily.com.cn/cndy/2010-01/11/content_9295776.htm).

opportunistic insider purchase transactions. For example, an insider may purchase shares prior to a public disclosure on a favorable corporate investment decision. Although the disclosure itself is good news, investors may still lose confidence in firms with insiders who conduct this opportunistic behavior because opportunistic insider purchases signal a weak corporate governance environment of firms (Bhattacharya and Daouk, 2002; Francis et al., 2008). The rationale is that short sellers pursue monetary benefits not only from disclosing poor firm fundamentals but also from revealing insiders' misconduct, which can impair firms' future prospects. We thus expect the threat of short selling to discourage opportunistic insider purchases as well.[26]

To test this conjecture, we re-estimate equation (1) with the dependent variables of opportunistic (routine) insider purchases over a quarter. The results in Table 10 show that insiders in pilot firms decrease their opportunistic purchasing during the pilot program period for both the U.S. and Chinese samples. Although the economic effects are significantly smaller than those of opportunistic insider sales, the results further support our proposed hypothesis that short sellers discipline opportunistic insider trading.

5.3. The Short Selling of Insiders

An alternative explanation for the negative association between short selling pressure and insider selling is the increase in insiders' short selling of their own firm shares during the SHO and CSRC programs.

However, our results can hardly be explained by this alternative view for the following reasons. First, insiders are strictly prohibited from short selling their own firm shares by the Securities Exchange Act of 1934 (Act 34) and corporate restrictions in the U.S. Specifically, Section 16(c) of the Act 34 explicitly makes it unlawful for an insider to make short sales. In addition, the Dodd-Frank Wall Street Reform and Consumer Protection Act amended Section 14 of the Act 34 to authorize the SEC adopt rules requiring firms disclose all purchase transactions designed to hedge or offset short transactions. Consequently, rather than grant the right to enter into such hedging transactions to particular insiders, U.S. firms have established the policy

[26] There are also counter-arguments suggesting that purchase transactions per se not only could contain a signal to indicate potential positive firm performance in the future but also could be an effective way for insiders to communicate private information to the public. If either effect is strong enough, it is likely to yield results contrary to our findings.

forbidding any insiders from engaging hedging transactions including short sales.[27]

Second, although the CSRC program does not completely prohibit Chinese insiders from shorting their own firm shares over the entire period, Table 11 shows that our baseline findings of the Chinese sample remain similar if we exclude the sample period between March 31, 2010, and March 18, 2013. Excluding the above sample period could help to mitigate concern over the short selling of insiders because the Shanghai and Shenzhen stock exchanges released new rules that explicitly disallow insiders to short their own firm shares starting from March 18, 2013. As a result, the short selling of insiders in China is less of a concern for the remaining sample period.

Finally, in Section 5.2, we find that our results continue to hold when we focus on insider purchase transactions, which should be positively associated with the short position in a reasonable amount of time. This helps to mitigate the concern that some firms do not have corporate policies to prohibit the short selling of insiders.

5.4. Robustness Tests

To corroborate our main results, we perform several robustness tests on the disciplining effect of short selling. Specifically, we examine whether insiders in pilot firms make lower profits from offloading their own shares, and we perform sensitivity tests using alternative measures on insider selling.

5.4.1. Insider Selling Profits

To complement our inferences based on trading intensity, we start by investigating whether short sellers affect the profitability of insider selling. Specifically, we define the profitability of insider sales (*Profitability*) as either the market-adjusted abnormal return or the abnormal return adjusted by 25 size and market-to-book portfolios. The two abnormal returns are estimated in a window of 60 calendar days after a transaction. We multiply the return values by negative one.

We divide our sample into opportunistic and routine trades and then re-estimate equation (1) using *Profitability* as the dependent variable for both the U.S. and Chinese samples. Table 12 presents the results. Consistent with the disciplining hypothesis, the coefficient on *Pilot*×*During* is negatively significant for opportunistic insider sales (with *t*-statistics ranging from -1.81 to

[27] An example of corporate restrictions can be found at http://www.accuridecorp.com/investors/insider-traiding-guidelines/.

-2.51). The difference in point estimates between opportunistic and routine trades is significant at the 5% level (one-tailed).

Together with our main results, this finding suggests that short sellers not only attenuate the execution of transactions but also reduce the profitability of trades.

5.4.2. Alternative Measures of Insider Selling

Next, we conduct three additional tests using alternative measures of insider selling. First, we redefine whether insider sales are opportunistic or routine based on Ke, Huddart, and Petroni (2003), who show that corporate insiders trade on non-public information about a forthcoming earnings break as early as nine quarters prior to the break in the U.S. For China, we replicate Ke, Huddart, and Petroni (2003) based on insider trading during the pre-program period. We find that insiders do not tend to sell their stocks until five quarters before an earnings break, perhaps partially because the risk of litigation in China is lower than that in the U.S.

Using the Ke, Huddart, and Petroni (2003) method, we can mitigate the potential measurement errors of the Cohen, Molloy, and Pomorski (2012) method to define opportunistic selling, especially for the Chinese sample.[28] It could also help to address whether our results are robust to the inclusion of unclassified trades in the Cohen, Molloy, and Pomorski (2012) method.

Accordingly, we classify all nine (five) quarters before an earnings break of a given firm as opportunistic-trade quarters (*Break*) and all others as routine-trade quarters (*NonBrk*) for the U.S. (Chinese) sample. This approach gives 38,920 (25,144) opportunistic-trade firm-quarters and 7,901 (17,290) routine-trade firm-quarters in the U.S. (China). We then re-estimate equation (1) for the two types of insider selling groups. We also include the number of quarters for which quarterly earnings increase consecutively before the break (*Length*) as a parsimonious control for the effect of the prior string length of earnings increase and firm performance on the stock trading decisions of insiders (Ke, Huddart, and Petroni, 2003). Our results are not affected if we consistently use five quarters for both U.S. and Chinese samples.

We report the results in Columns 1, 2, 7, and 8 of Table 13. The coefficient on *Pilot*×*During* is significant for insider sales in firm-quarters followed by earnings breaks (*Break*) (coeff. = -2.897,

[28] The Cohen, Molloy, and Pomorski (2012) method does require that an insider have a reasonable length of trading records in past years, and this requirement affects how accurately we classify a certain future trade as an opportunistic one. Ke, Huddart, and Petroni (2003) take a different view and could increase our testing power for the Chinese sample, in which more than 91% of insiders in China do not have a trading history of more than three years.

t-stat. = -2.98 for the U.S. sample and coeff. = -2.202, *t*-stat. = -3.15 for the Chinese sample). The Chow test also confirms that the coefficient on *Pilot*×*During* is more negative for opportunistic insider sales (*Break*) than for routine insider sales (*NonBrk*), and the difference is significant at the 10% level (one-tailed) in both countries. The results present the same picture as the main results in Table 3.

Second, instead of using aggregate insider sales as the dependent variable, we calculate the average value per sale (*TradeSize*) for robustness. *TradeSize* is measured as the average value of shares traded per sale by insiders as a percentage of shares outstanding in a given quarter.[29] In addition, we add the last quarter's trading size (*LagTradeSize*) as the control. Columns 3, 4, 9, and 10 present the regression results. The trading size of opportunistic sales decreases significantly (with t-statistics of -3.51 and -3.45 for the U.S. and Chinese samples, respectively). Specifically, in the U.S. sample, the coefficient on *Pilot*×*During* for *TradeSize* is -0.083 for opportunistic sales, which is greater than the posited estimate of that for routine sales (coeff. = -0.014) and is significant at the 5% level (one-tailed). The Chow test shows that this difference becomes even more significant when we focus on the Chinese sample (the 1% level of the one-tailed test). The result again supports our H1 that insiders in pilot firms become more cautious when opportunistically selling shares and that they reduce the trading amount per transaction.

Finally, we measure insider selling activities by using the change in insiders' portfolios. Specifically, we use the proportion of insider sales scaled by insiders' holdings (*SharesFract*) as the dependent variable in equation (1). *SharesFract* is defined as shares sold by insiders as a percentage of the initial holdings of insiders in a quarter. Columns 5, 6, 11, and 12 report the results. We continue to find that our results remain similar, suggesting that our findings are robust to this alternative scale.

6. Conclusion

Opportunistic insider trading is considered the most unfair expropriation of outsiders. In an arena of informed traders, it is intriguing to examine how the presence of short sellers affects insiders' trading activities. In particular, from an ex ante perspective, Massa et al. (2015) hypothesize that short-selling potential induces insiders to sell more and faster to preempt

[29] Our untabulated results remain similar if we use the logarithm of 1 plus *TradeSize* or the sum of the dollar amount of all insider sales over the quarter.

information competition. We relax the assumption of “inactive” short sellers and formulate the new theoretical prediction that the threat of short selling would deter opportunistic insider trading.

We revisit the impact of short-selling threat on insider sales using two quasi-natural experiments. Specifically, consistent with our hypothesis, we show that insiders’ opportunistic sales become smaller not only in the SHO program in the U.S. but also in the Chinese deregulation program. Importantly, further evidence supports three channels through which the threat of short selling reduces opportunistic insider selling. Specifically, we find that the impact of pilot programs on opportunistic sales is most pronounced among firms with higher litigation risk and greater reputation concerns and whose insiders have more equity-based incentive compensation. Finally, we show that our findings are not explained by alternative explanations such as financial constraints or preemptive trading.

Broadly speaking, our evidence advocates the beneficial role of informed investors—they not only contribute to the informational efficiency of the stock market but also monitor insiders in the corporate market.

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Appendix A – Variable definitions

Variables	The U.S. Sample	The Chinese Sample
Dependent variables		
Shares	Shares sold or purchased by insiders as a percentage of the firm's shares outstanding, aggregated for each firm-quarter.	
Value	The U.S. dollar (or RMB Yuan) value of shares sold or purchased by insiders, aggregated for each firm-quarter.	
Opptrn	Shares that are opportunistically sold or purchased by insiders as a percentage of the firm's shares outstanding, aggregated for each firm-quarter (in %). Opportunistic trades are defined as in Cohen, Malloy, and Pomorski (2012).	
Routine	Shares that are routinely sold or purchased by insiders as a percentage of the firm's shares outstanding, aggregated for each firm-quarter (in %). Routine trades are defined as in Cohen, Malloy, and Pomorski (2012).	
TradeSize	The average value of shares traded per sale or purchase by insiders as a percentage of the firm's shares outstanding in a given quarter.	
Profitability	(1) market-adjusted abnormal return, which is the sum of the difference between daily raw return and daily market return over 60 days after the transaction, multiplied by -1. Market return is the value weighted average return of U.S. CRSP stocks (or all A shares in China); (2) annualized abnormal return of the 25 size and market-to-book portfolio adjusted return estimated for 60 days after the transaction, multiplied by -1.	
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.	An indicator variable that equals one if a stock is designated as the short-selling target in the CSRC program, and zero otherwise.
During	An indicator variable that equals one for fiscal quarters starting after May 1, 2005 and ending before July 6, 2007, and zero otherwise.	An indicator variable that equals one for fiscal quarters starting from being designated as a short selling target by the CSRC, or after March 2010 for control firms, and zero otherwise.
Post	An indicator variable that equals one if fiscal quarters starting after July 6, 2007 and ending before Dec. 31, 2009, and zero otherwise.	An indicator variable that equals one for fiscal quarters starting from when a stock is excluded from the pilot list by the CSRC, and zero otherwise.
Pre_1	An indicator variable that equals one for fiscal quarters between April 2004 and June 2004, and zero otherwise.	An indicator variable that equals one for quarters during the middle 1/3 length of pre-CSRC program period, and zero otherwise. Pre-CSRC program period, the period between January 2007 and the date when a firm is designated as a short selling target by the CSRC, or March 31, 2010 for control firms.

Pre_2	An indicator variable that equals one for quarters between July 2004 and April 2005 and zero otherwise.	An indicator variable that equals one for quarters during the last 1/3 length of pre-CSRC program period, and zero otherwise. Pre-CSRC program period, the period between January 2007 and the date when a firm is designated as a short selling target by the CSRC, or March 31, 2010 for control firms.
$During_0$	An indicator variable that equals one for quarters between May 2005 and June 2006 and zero otherwise.	An indicator variable that equals one for quarters during the first two years after the CSRC program period for pilot firms, or after March 2010 for control firms, and zero otherwise.
$During_1$	An indicator variable that equals one for quarters between July 2005 and June 2007 and zero otherwise.	An indicator variable that equals one for quarters during the period starting from two years after the CSRC program for pilot firms, or after March 2010 for control firms, and zero otherwise.
Channel-related variables		
HighLR	High litigation risk, which equals one if 1) a firm is in a litigious industry (Francis, Philbrick, and Schipper 1994); 2) a firm's litigation risk (Kim and Skinner 2012) is above the sample median, and zero otherwise. Litigious industries include Biotechnology (SIC 2833 to 2836 and 8731-8734), Computers (SIC 3570 to 3577 and 7370 to 7379), Electronics (SIC 3600 to 3674), and Retailing (SIC 5200 to 5961). A firm's litigation risk is calculated using the Kim and Skinner (2012) litigation probit model (2) coefficient estimates.	High litigation risk, which equals one if 1) a firms is in a litigious industry. To define the litigious industry, we use the CSRC punishment data between 2000 and 2010 from Wind database. We aggregate the number of cases for each industry and define an industry as the litigious industry if the number of cases is in the top one-third of the distribution; 2) a firm's litigation risk (Kim and Skinner 2012) is above the sample median, and zero otherwise. A firm's litigation risk is calculated using the Kim and Skinner (2012) litigation probit model (2) coefficient estimates.
HighMedia	High media coverage, which equals one if the number of news articles of a firm in the prior year is greater than the sample 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 (or China National Knowledge Infrastructure, CNKI database).	
HighEqWealth	High equity personal wealth which equals one if 1) CEO's equity wealth, 2) top five most paid executives' total equity wealth of a firm 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 shares outstanding of the firm.	

FConstraint	Financial constrained, which equals one if the firm is financially constrained, and zero otherwise. A firm is classified as financially constrained if 1) in the upper half of the SA index based on its sample median. The SA index is a combination of asset size and firm age and is calculated as $-0.737\times Assets+0.043\times Assets^2-0.040\times Age$, where *Assets* is the natural log of book assets (in millions) and *Age* is the number of years from the first year that a firm has a non-missing stock price in Compustat; 2) in the bottom half of free cash flow based on its sample median, which is calculated as cash from operations minus average capital expenditure in the past four quarters, scaled by the sum of long-term and short-term debt.
Other firm characteristics	
logMV	The natural logarithm of market value (in millions of the U.S. dollars or RMB Yuan) of the firm at the end of quarter *t*-1.
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.

Appendix B. CSRC Program

	(1)	(2)	(3)	(4)	(5)
Effective Date (Included/Excluded)	Initial Sample (# of firms in the list) (Incl.)	Initial Sample (# of unique firms in the list) (Incl.)	Firms in the Final Sample (Incl.)	Firms removed by the CSRC (Incl.)	Firms removed by the CSRC (Excl.)
31/03/2010	90	90	76	14 (10)	0
between 2010/04 and 2011/11	6	6	6	0	6 (3)
5/12/2011	189	185	132	53	1
31/01/2013	276	275	264	11	52
between 2013/02 and 2013/09	0	0	0	0	6
16/09/2013	206	186	181	5	0
between 2013/09 and 2014/09	0	0	0	0	4
22/09/2014	218	200	200	0	13
4/12/2014	0	0	0	0	1(0)
Total	985	942	859	83 (79)	83 (79)

This appendix illustrate the list of firms are eligible for short selling by the CSRC over time. Column 1 shows the initial sample that contains all firms if they are once included by the CSRC. Column 2 shows the number of unique firms reported in column 1. Column 3 shows the number of unique firms that are included by the CSRC and never excluded from the list till December 2014. Columns 4 and 5 shows that firms that are removed from the list during our sample period. Column 4 shows the time when they are included. Column 5 shows that time when they are excluded.

Table 1. Comparison of insider-trading behavior and key firm characteristics in pilot and control firms before the SHO or CSRC program

	Panel A: The U.S. Sample								Panel B: The Chinese Sample							
	Pilot group (*Pilot* = 1)			Control group (*Pilot* = 0)			Tests for diff.		Pilot group (*Pilot* = 1)			Control group (*Pilot* = 0)			Tests for diff.	
Variable	Mean	Median	Std.	Mean	Median	Std.	Mean *t*-stat.	Median *z*-stat.	Mean	Median	Std.	Mean	Median	Std.	Mean *t*-stat.	Median *z*-stat.
Insider trading variables																
Sales																
Shares (%)	0.263	0.010	0.683	0.252	0.016	0.607	**0.42**	**-1.17**	2.094	0.000	12.080	3.661	0.000	28.951	**-1.29**	**0.79**
Value (mil.)	3.566	0.120	9.178	3.074	0.195	7.474	**1.41**	**-1.57**	1.234	0.000	7.335	1.079	0.000	8.352	**0.37**	**0.91**
Purchases																
Shares (%)	0.006	0.000	0.029	0.004	0.000	0.023	**1.43**	**1.13**	0.019	0.000	0.176	0.041	0.000	0.402	**-1.33**	**0.13**
Value (mil.)	0.033	0.000	0.153	0.027	0.000	0.143	**0.92**	**1.27**	0.012	0.000	0.094	0.012	0.000	0.108	**-0.03**	**0.20**
Firm characteristics																
logMV	6.842	6.631	1.419	6.807	6.566	1.434	**0.61**	**1.01**	8.417	8.253	1.115	7.334	7.323	0.697	**22.60*** **	**19.14*** **
MB	3.076	2.113	2.936	3.063	2.107	3.049	**0.11**	**0.20**	2.820	2.223	2.426	2.796	2.053	2.656	**0.17**	**1.04**
Ret	0.120	0.087	0.216	0.127	0.088	0.227	**-0.77**	**-0.20**	0.004	-0.038	0.180	-0.009	-0.042	0.175	**1.33**	**1.32**
ROE	0.019	0.028	0.089	0.017	0.029	0.088	**0.61**	**-0.85**	0.028	0.027	0.037	0.014	0.015	0.043	**6.71*** **	**8.19*** **
Grants	0.001	0.000	0.002	0.000	0.000	0.002	**0.30**	**1.25**	0.000	0.000	0.001	0.000	0.000	0.000	**1.26**	**1.58**

This table 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 for the US sample (Panel A) and at the end of 2009, the fiscal year before the launch of short selling program by the CSRC (Panel B). Full definitions are given in the Appendix A. For the Chinese sample, we multiple the results of *Shares* (%) by 100 for readability. The *t* test (Wilcoxon test) is used to test for the difference between the means (medians) of the pilot and control firms. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively (two-tailed test).

Table 2. Short-Sale Restrictions and Insider Sales: Univariate Analysis

	Panel A: The U.S. Sample			Panel B: The Chinese Sample		
Variable	Pilot Diff (During-Pre)	Control Diff (During-Pre)	Diff-in-Diff (Average Treatment Effect)	Pilot Diff (During-Pre)	Control Diff (During-Pre)	Diff-in-Diff (Average Treatment Effect)
Δ*Opptrn*	**-0.361***	0.142	**-0.503****	**-0.461***	2.368***	**-2.829*****
(*t*-stat)	**(-1.88)**	(1.05)	**(-2.08)**	**(-1.88)**	(7.98)	**(-6.71)**
Δ*Routine*	0.019	0.130***	-0.111***	0.043***	0.037*	0.006
(*t*-stat)	(0.59)	(4.97)	(-2.67)	(3.26)	(1.85)	(0.22)
Diff.	**-0.380††**	0.012	**-0.392†**	**-0.504†**	2.331†††	**-2.835†††**
(*f*-stat)	**(3.35)**	(0.02)	**(2.64)**	**(3.68)**	(17.29)	**(22.41)**

This table compares shares sold by insiders (*Opptrn* and *Routine*) in the pre-SHO or pre-CSRC program period and during-SHO or during-CSRC program period. Insider transactions are classified as either opportunistic (*Opprtn*) or routine (*Routine*) transactions, which is based on the Cohen, Malloy, and Pomorski (2012) method. Δ*Opptrn* (Δ*Routine*) represents the difference in *Opptrn* (or *Routine*) from one period to another as indicated in each column. We aggregate insider sales made by all insiders over each firm-quarter. We multiple all the results by 100 for readability. Full definitions are given in the Appendix A. The *t*-statistics are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively (two-tailed test). †††, ††, and † indicate significance at the 1%, 5%, and 10% levels, respectively (one-tailed test).

Table 3. Short-Sale Restrictions and Insider Sales: Regression Analysis

	Panel A: The U.S. Sample				Panel B: The Chinese Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var. =	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*
Pilot×During	**-0.481***	**-0.112****	**-0.421****	**-0.080***	**-2.556*****	**0.016**	**-1.117****	**0.015**
	(-1.73)	**(-2.23)**	**(-2.29)**	**(-1.65)**	**(-5.75)**	**(0.83)**	**(-2.45)**	**(0.69)**
Pilot	0.291	0.005	0.259	-0.005	-0.559**	-0.001	0.230	-0.005
	(0.96)	(0.09)	(1.25)	(-0.13)	(-2.31)	(-0.11)	(1.02)	(-0.91)
During	-1.272*	-0.226***	-0.689	-0.111***	0.203	0.044**	0.194	0.043**
	(-1.77)	(-2.95)	(-1.34)	(-3.90)	(0.55)	(2.25)	(0.54)	(2.23)
logMV			-0.248***	0.015			-0.930***	-0.000
			(-4.45)	(1.36)			(-6.99)	(-0.06)
MB			0.143***	0.018***			-0.143***	0.005**
			(5.53)	(3.77)			(-4.11)	(2.70)
Ret			2.835***	0.099**			2.146**	-0.021
			(7.29)	(2.36)			(2.50)	(-0.51)
ROE			8.411***	0.682***			8.647***	0.338***
			(10.15)	(6.50)			(6.22)	(2.82)
Grants			95.565***	13.429***			-24.553	-1.363*
			(3.51)	(3.20)			(-0.75)	(-1.81)
LagSales			0.320***	0.368***			0.173***	0.008
			(17.88)	(11.21)			(7.57)	(1.34)
Adj. R^2	2.3%	1.6%	13.5%	15.5%	1.4%	0.1%	4.4%	0.1%
No. of obs.	55,139	55,139	55,139	55,139	56,975	56,975	56,975	56,975
Chow test on *Pilot×During*	F=1.76†		F = 3.13††		F=32.96†††		F=6.06††	

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

$$Opprtn\ (Routine) = \alpha_0 + \alpha_1 Pilot \times During + \alpha_2 Pilot + \alpha_3 During + Controls + \varepsilon. \quad (1)$$

LagSales represents opportunistic (or routine) insider sales in the previous quarter when the dependent variable is *Opptrn* (or *Routine*), respectively. Full definitions are given in the Appendix A. Intercepts and industry- and year-quarter-fixed effects are included in all regressions, but are not reported. We multiple 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. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively (two-tailed test). †††, ††, and † indicate significance at the 1%, 5%, and 10%, respectively levels (one-tailed Chow test, i.e., F test).

Table 4. Placebo Tests

	Panel A: The U.S. Sample				Panel B: The Chinese Sample			
	Pre, During, vs. Post		**Pre vs. Post**		**Pre, During, vs. Post**		**Pre vs. Post**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var. =	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*
Pilot×During	**-0.398****	**-0.066**			**-0.935****	**-0.006**		
	(-2.31)	**(-1.38)**			**(-2.46)**	**(-1.81)**		
Pilot×Post	**-0.353**	**-0.051**	**-0.241**	**-0.058**	**0.030**	**-0.017**	**0.055**	**-0.017**
	(-1.49)	**(-1.45)**	**(-1.06)**	**(-1.57)**	**(0.17)**	**(-0.66)**	**(0.11)**	**(-0.54)**
During	-0.608	-0.074**			0.386	-0.007		
	(-1.11)	(-2.26)			(1.22)	(-0.58)		
Post	-0.898	-0.169	-1.049***	0.033***	-0.618**	0.004	-1.006	0.006
	(-1.17)	(-1.36)	(-4.91)	(5.55)	(-2.26)	(0.19)	(-1.41)	(0.32)
Pilot	0.237	-0.02	0.078	0.004	0.095	0.006	0.104	0.007
	(1.25)	(-0.64)	(0.44)	(0.11)	(0.33)	(0.69)	(0.26)	(1.08)
logMV	-0.170***	0.018**	-0.128***	0.020**	-0.392**	-0.005	-0.378*	-0.006
	(-3.77)	(1.97)	(-2.74)	(2.32)	(-2.84)	(-0.63)	(-1.78)	(-1.02)
MB	0.130***	0.017***	0.127***	0.017***	-0.059*	0.001*	-0.093**	0.001
	(5.71)	(3.93)	(5.23)	(3.52)	(-2.22)	(2.22)	(-2.07)	(0.74)
Ret	2.144***	0.100***	1.800***	0.093***	-0.288	0.003	-0.249	0.002
	(6.75)	(3.31)	(5.96)	(2.59)	(-1.11)	(0.35)	(-0.32)	(0.20)
ROE	6.241***	0.528***	5.755***	0.462***	2.347	0.123	4.220*	0.136
	(9.24)	(6.23)	(8.21)	(5.28)	(0.76)	(0.98)	(1.98)	(0.61)
Grants	84.837***	12.084***	98.449***	10.411***	-12.513***	-0.058	-4.928	0.034
	(3.65)	(3.01)	(3.93)	(2.81)	(-3.60)	(-0.99)	(-0.42)	(0.28)
LagShares	0.320***	0.389***	0.321***	0.381***	0.183***	-0.004	0.287*	-0.004**
	(19.92)	(12.05)	(16.22)	(12.74)	(32.36)	(-1.71)	(1.86)	(-2.29)
Adj. R^2	13.5%	16.7%	13.8%	16.1%	5.6%	1.0%	10.2%	1.1%
No. of obs.	73,583	73,583	53,580	53,580	4,370	4,370	3,661	3,661
Chow test on *Pilot×During*	F = 3.27††				F=6.00††			
Chow test on *Pilot×Post*	F = 1.88†		F=0.77		F=0.05		F=0.02	

This table reports the estimation results from the placebo tests on the SHO (or CSRC) program. For the U.S. (or Chinese) sample, columns 1 and 2 (columns 5 and 6) include both *During* and *Post* periods. Columns 3 and 4 (columns 7 and 8) include only *Pre* and *Post* periods. *LagSales* represents opportunistic (or routine) insider sales in the previous quarter when the dependent variable is *Opptrn* (or *Routine*), respectively. Full definitions are given in the Appendix A. We multiple all the coefficients by 100 for readability. Intercepts and industry- and year-quarter-fixed effects are included in all regressions, but are not reported. *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). †††, ††, and † indicate significance at the 1%, 5%, and 10%, respectively levels (one-tailed Chow test, i.e., F test).

Table 5. Insider Sales and Litigation Risk Channel

	Panel A: The U.S. Sample				Panel B: The Chinese Sample			
	Litigious Industries		KS LitigRisk		Litigious Industries		KS LitigRisk	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var. =	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*
Pilot×During×HighLR	**-0.973****	**-0.066**	**-0.674***	**-0.054**	**-1.025***	**-0.002**	**-1.652***	**0.023**
	(-2.47)	**(-0.64)**	**(-1.67)**	**(-0.74)**	**(-1.89)**	**(-0.04)**	**(-1.71)**	**(0.30)**
Pilot×During	0.036	-0.063	0.085	-0.053	-0.392	0.018	-0.747*	0.024
	(0.20)	(-1.15)	(0.31)	(-0.72)	(-0.70)	(0.54)	(-1.76)	(0.65)
Pilot	0.130	0.011	-0.136	-0.041	0.831***	-0.024	0.385	-0.015
	(0.68)	(0.28)	(-0.43)	(-0.91)	(3.13)	(-0.95)	(1.63)	(-0.97)
Pilot×HighLR	-0.233	-0.053	0.586	0.075	-0.299	0.027	-0.024	0.030
	(-0.53)	(-0.60)	(1.41)	(1.42)	(-0.84)	(1.00)	(-0.07)	(1.50)
During	-0.807**	-0.133***	-1.491***	-0.119**	0.068	0.009	-0.294	0.044*
	(-2.37)	(-6.14)	(-3.28)	(-2.44)	(0.15)	(0.32)	(-0.72)	(1.78)
During×HighLR	-0.323	0.027	0.562	-0.011	0.375	0.046	1.699***	0.033
	(-1.13)	(0.39)	(1.64)	(-0.25)	(0.85)	(1.32)	(3.59)	(1.09)
HighLR	1.727***	0.206***	-1.231***	-0.154***	0.756***	-0.020	0.307*	-0.028
	(4.94)	(3.42)	(-4.35)	(-3.73)	(2.81)	(-0.74)	(1.81)	(-1.35)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	14.4%	15.5%	13.8%	15.5%	4.10%	0.10%	4.60%	0.20%
No. of obs.	53,532	53,532	53,532	53,532	56,975	56,975	46,778	46,778
Chow test on *Pilot×During×HighLR*	F=3.42††		F=2.42†		F=3.51†		F=2.86†	

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

$$Opprtn\ (Routine) = \alpha_0 + \alpha_1 Pilot\times During\times Channel + \alpha_2 Pilot\times During + \alpha_3 Pilot + \alpha_4 Pilot\times Channel + \alpha_5 During + \alpha_6 During\times Channel + \alpha_7 Channel + Controls + \varepsilon. \quad (2)$$

where *Channel* presents *HighLR*, indicating high litigation risk. Full definitions of the variables are given in the Appendix A. We multiple all the coefficients by 100 for readability. Intercepts and industry- and year-quarter-fixed effects are included in all regressions, but are not reported. *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). †††, ††, and † indicate significance at the 1%, 5%, and 10%, respectively levels (one-tailed Chow test, i.e., F test).

Table 6. Insider Sales and Reputation Channel

	Panel A: The U.S. Sample				Panel B: The Chinese Sample			
	All News		Insider Trading Related News		All News		Insider Trading Related News	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var. =	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*
Pilot×During×HighMedia	**-0.972****	**-0.137**	**-1.098****	**-0.173***	**-1.365****	**-0.006**	**-2.014*****	**0.056**
	(-2.07)	**(-1.55)**	**(-2.21)**	**(-1.88)**	**(-2.51)**	**(-0.12)**	**(-2.88)**	**(1.50)**
Pilot×During	0.113	0.002	-0.094	0.003	-0.661	0.002	0.182	-0.034**
	(0.36)	(0.04)	(-0.36)	(0.07)	(-1.16)	(0.18)	(0.27)	(-2.22)
Pilot	-0.054	-0.024	0.010	-0.023	-0.367	-0.000	-0.840	0.015
	(-0.26)	(-0.59)	(0.04)	(-0.70)	(-1.24)	(-0.05)	(-1.63)	(1.31)
Pilot× HighMedia	0.468	0.044	0.719*	0.049	1.085***	-0.005	1.359**	-0.025
	(1.30)	(0.68)	(1.91)	(0.80)	(3.18)	(-0.34)	(2.33)	(-1.40)
During	-1.601**	-0.299**	-1.390**	-0.269**	-0.588	0.018	-1.085*	0.045*
	(-2.44)	(-2.22)	(-2.05)	(-2.04)	(-1.40)	(1.24)	(-1.92)	(1.92)
During× HighMedia	1.018***	0.238***	1.087***	0.239***	1.662***	0.055	1.839***	0.005
	(3.99)	(4.99)	(3.63)	(4.33)	(3.68)	(1.51)	(2.94)	(0.17)
HighMedia	0.436**	-0.001	1.134***	0.160***	0.443	0.008	0.139	0.017
	(2.28)	(-0.03)	(5.72)	(4.74)	(1.54)	(0.46)	(0.27)	(0.76)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	14.1%	16.0%	14.1%	16.2%	4.60%	0.20%	4.50%	0.20%
No. of obs.	50,961	50,961	50,961	50,961	56,975	56,975	56,975	56,975
Chow test on *Pilot×During×HighMedia*	F=3.39††		F=3.71††		F=6.17††		F=8.84†††	

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

$$Opprtn\ (Routine) = \alpha_0 + \alpha_1 Pilot{\times}During{\times}Channel + \alpha_2 Pilot{\times}During + \alpha_3 Pilot + \alpha_4 Pilot{\times}Channel + \alpha_5 During + \alpha_6 During{\times}Channel + \alpha_7 Channel + Controls + \varepsilon. \quad (2)$$

where *Channel* presents *HighMedia*, indicating high media coverage. Full definitions of the variables are given in the Appendix A. We multiple all the coefficients by 100 for readability. Industry- and year-quarter-fixed effects are included in all regressions, but are not reported. *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). †††,†† and † indicate significance at the 1%, 5%, and 10% levels, respectively (one-tailed test, i.e., F test).

Table 7. Insider Sales and Personal Wealth Channel

	Panel A: The U.S. Sample				Panel B: The Chinese Sample			
	CEO's equity wealth		Top 5 Executives' equity wealth		CEO's equity wealth		Top 5 Executives' equity wealth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var. =	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*
Pilot×During×HighEqWealth	**-1.284****	**-0.131**	**-1.271****	**-0.160**	**-1.828****	**-0.028**	**-2.699*****	**0.043**
	(-2.07)	**(-1.13)**	**(-2.09)**	**(-1.51)**	**(-2.09)**	**(-0.48)**	**(-2.90)**	**(0.77)**
Pilot×During	0.301	0.010	0.437	0.050	-0.176	0.030	0.484	0.008
	(1.06)	(0.20)	(1.42)	(0.82)	(-0.32)	(0.81)	(1.35)	(0.75)
Pilot	-0.131	-0.010	-0.059	-0.046	0.258	-0.003	-0.002	-0.004
	(-0.60)	(-0.19)	(-0.22)	(-0.79)	(0.66)	(-0.11)	(-0.01)	(-0.51)
Pilot×HighEqWealth	0.411	-0.006	0.204	0.043	-0.499	-0.015	-0.018	-0.010
	(0.83)	(-0.06)	(0.40)	(0.50)	(-0.84)	(-0.39)	(-0.03)	(-0.70)
During	-1.303*	-0.082	-1.250	-0.100	-0.441	0.021	-1.325***	0.004
	(-1.74)	(-0.46)	(-1.59)	(-0.55)	(-0.95)	(0.67)	(-3.47)	(0.42)
During×HighEqWealth	-0.084	0.027	0.261	0.048	1.064*	0.050	2.638***	0.072*
	(-0.23)	(0.33)	(0.78)	(0.70)	(1.93)	(1.36)	(3.06)	(1.93)
HighEqWealth	1.710***	0.191***	1.762***	0.102	3.494***	0.038	2.606***	0.026
	(5.64)	(2.93)	(6.38)	(1.60)	(7.34)	(1.20)	(5.03)	(1.49)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	16.9%	17.6%	17.2%	18.7%	5.1%	0.2%	5.2%	0.2%
No. of obs.	25,600	25,600	26,925	26,925	56,975	56,975	56,975	56,975
Chow test on *Pilot×During×HighEqWealth*	F=3.84††		F=3.62††		F=4.53††		F=8.58†††	

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

$$Opprtn\ (Routine) = \alpha_0 + \alpha_1 Pilot\times During\times Channel + \alpha_2 Pilot\times During + \alpha_3 Pilot + \alpha_4 Pilot\times Channel + \alpha_5 During + \alpha_6 During\times Channel + \alpha_7 Channel + Controls + \varepsilon. \quad (2)$$

where Channel presents *HighEqWealth*, indicating high personal wealth related to their own firms' equities. Full definitions of the variables are given in the Appendix A. We multiple all the coefficients by 100 for readability. Industry- and year-quarter-fixed effects are included in all regressions, but are not reported. *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). †††,†† and † indicate significance at the 1%, 5%, and 10% levels, respectively (one-tailed test, i.e., F test).

Table 8. Insider Sales and financial constraints

	Panel A: The U.S. Sample				Panel B: The Chinese Sample			
	SA Index		Free Cash Flow		SA Index		Free Cash Flow	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var. =	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*
Pilot×During×FConstraint	**-0.024**	**-0.007**	**0.094**	**-0.087**	**0.021**	**-0.008**	**-0.223**	**-0.001**
	(-0.06)	**(-0.10)**	**(0.25)**	**(-1.05)**	**(0.04)**	**(-0.15)**	**(-0.36)**	**(-0.02)**
Pilot×During	-0.399**	-0.074**	-0.687**	-0.023	-1.235**	0.013	-1.012*	0.005
	(-2.05)	(-1.96)	(-2.01)	(-0.38)	(-2.61)	(0.50)	(-1.75)	(0.11)
Pilot	0.172	0.053	0.425	0.002	0.098	0.002	0.270	0.010
	(0.80)	(1.17)	(1.37)	(0.03)	(0.61)	(0.38)	(1.02)	(1.20)
Pilot× FConstraint	0.225	-0.109*	-0.136	0.055	-0.344	-0.014	0.080	-0.008
	(0.60)	(-1.67)	(-0.42)	(0.93)	(-0.95)	(-1.10)	(0.17)	(-1.12)
During	-0.668	-0.134	-0.910**	-0.047	-0.252	0.048***	0.527	0.062*
	(-1.34)	(-1.05)	(-2.16)	(-1.32)	(-0.82)	(3.44)	(1.07)	(1.70)
During×FConstraint	-0.059	0.039	-0.055	0.003	1.000*	-0.006	0.025	-0.025
	(-0.15)	(0.90)	(-0.19)	(0.10)	(1.72)	(-0.19)	(0.04)	(-0.78)
FConstraint	1.308***	0.192***	-0.554**	-0.105***	1.107***	0.014	0.445	-0.000
	(5.46)	(4.88)	(-2.53)	(-4.13)	(3.21)	(1.10)	(1.25)	(-0.11)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	13.7%	15.6%	12.7%	13.7%	4.8%	0.1%	5.4%	0.2%
No. of obs.	55,139	55,139	40,924	40,924	56,599	56,599	43,004	43,004
Chow test on *Pilot×During×Fconstraint*	F=0.00		F=0.16		F=0.26		F=0.13	

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

$$Opprtn\ (Routine) = \alpha_0 + \alpha_1 Pilot\times During\times FConstraint + \alpha_2 Pilot\times During + \alpha_3 Pilot + \alpha_4 Pilot\times FConstraint + \alpha_5 During + \alpha_6 During\times FConstraint + \alpha_7 FConstraint + Controls + \varepsilon.$$

where *FConstraint* represents whether a firm is financially constrained. A firm is classified as financially constrained if it is in the upper half of SA Index , or bottom half of free cash flow. We construct the constraint measures at the beginning of the quarter. Full definitions of the variables are given in the Appendix A. We multiple all the coefficients by 100 for readability. Intercepts and industry- and year-quarter-fixed effects are included in all regressions, but are not reported. *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). †††, ††, and † indicate significance at the 1%, 5%, and 10%, respectively levels (one-tailed Chow test, i.e., F test).

Table 9. Pre-announcement and Pre-implementation Tests

	Panel A: The U.S. Sample				Panel B: The Chinese Sample			
	Trend Test		Exclude Apr. 2004 - Nov. 2004		Trend Test		Exclude Aug. 2009 - Mar. 2010	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var. =	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*
Pilot×*Pre*$_1$	**-0.589**	**-0.070**			**-1.040**	**0.006**		
	(-1.21)	**(-1.00)**			**(-0.86)**	**(0.61)**		
Pilot×*Pre*$_2$	**0.245**	**-0.068*****			**-0.784**	**-0.017**		
	(1.26)	**(-2.66)**			**(-1.07)**	**(-1.25)**		
Pilot×*During*$_0$	**-0.502***	**-0.096***			**-1.178***	**-0.049**		
	(-1.90)	**(-1.68)**			**(-1.79)**	**(-1.09)**		
Pilot×*During*$_1$	**-0.454****	**-0.108**			**-1.857*****	**-0.026**		
	(-2.12)	**(-1.53)**			**(-2.69)**	**(-1.31)**		
Pilot×*During*			**-0.503****	**-0.086***			**-1.226****	**0.009**
			(-2.54)	**(-1.74)**			**(-2.57)**	**(0.43)**
Pre$_1$	-1.734**	-0.036			0.210	0.001		
	(-2.12)	(-0.43)			(0.21)	(0.66)		
Pre$_2$	-2.223***	-0.102***			-0.616	0.007		
	(-2.99)	(-3.48)			(-0.96)	(0.63)		
During$_0$	-2.927***	-0.233*			-0.823	0.086**		
	(-2.77)	(-1.70)			(-1.23)	(2.20)		
During$_1$	-4.072***	-0.353*			-0.847	-0.002		
	(-3.04)	(-1.72)			(-0.90)	(-0.28)		
During			-0.642	-0.105***			0.259	0.045**
			(-1.24)	(-3.99)			(0.67)	(2.42)
Pilot	0.249	0.018	0.344	0.001	0.240	-0.008	0.298	-0.005
	(1.11)	(0.55)	(1.58)	(0.02)	(0.58)	(-1.43)	(1.08)	(-1.25)
logMV	-0.245***	0.015	-0.228***	0.014	-0.837***	0.003	-0.921***	0.003
	(-4.43)	(1.37)	(-4.25)	(1.27)	(-4.81)	(0.54)	(-6.48)	(0.74)
MB	0.145***	0.018***	0.136***	0.019***	-0.143***	0.005**	-0.156***	0.005**
	(5.50)	(3.78)	(5.39)	(3.81)	(-3.55)	(2.03)	(-4.26)	(2.40)
Ret	2.698***	0.090**	2.817***	0.099**	2.106**	-0.023	2.050**	-0.021

	(6.97)	(2.09)	(6.63)	(2.19)	(2.40)	(-0.62)	(2.17)	(-0.48)
ROE	8.403***	0.681***	8.322***	0.697***	8.541***	0.337**	8.409***	0.329**
	(10.16)	(6.50)	(9.71)	(6.43)	(5.49)	(2.56)	(5.76)	(2.49)
Grants	95.338***	13.453***	90.714***	14.122***	-25.036	-1.316	-24.314	-1.241
	(3.55)	(3.20)	(3.57)	(3.06)	(-0.79)	(-1.63)	(-0.72)	(-1.64)
LagShares	0.319***	0.367***	0.332***	0.371***	0.173***	0.008	0.168***	0.007
	(17.73)	(11.20)	(15.94)	(11.14)	(7.80)	(1.26)	(6.92)	(1.26)
Adj. R^2	13.5%	15.5%	13.8%	15.5%	4.4%	0.2%	4.2%	0.1%
No. of obs.	55,139	55,139	49,146	49,146	56,975	56,975	52,680	52,680
Chow test on *Pilot*×*Pre1*	F = 1.42				F=0.59			
Chow test on *Pilot*×*Pre2*	F = 2.80††				F=1.23			
Chow test on *Pilot*×*During0*	F = 1.94†				F=3.08†			
Chow test on *Pilot*×*During1*	F = 2.47†				F=8.19†††			
Chow test on *Pilot*×*During*			F = 4.10††				F=6.64††	
Pilot×*Pre1*<*Pilot*×*Pre2*	F = 2.28†	F = 0.00			F=0.85	F=3.09†		

This table reports the estimation results from pre-announcement and pre-implementation tests on the SHO (or CSRC) program. Columns 1 and 2 (Columns 5 and 6) report the results of trend analyses along *Pre* and *During* periods. Columns 3 and 4 (columns 7 and 8) include *Pre* and *During* periods, but exclude the period between April 2004 and November 2004 (August 2009 and March 2010) for the U.S. (or Chinese) sample, respectively. Full definitions are given in the Appendix A. We multiple all the coefficients by 100 for readability. Intercepts and industry- and year-quarter-fixed effects are included in all regressions, but are not reported. *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). †††, ††, and † indicate significance at the 1%, 5%, and 10%, respectively levels (one-tailed Chow test, i.e., F test).

Table 10. Short-Sale Restrictions and Insider Purchases

	Panel A: The U.S. Sample		Panel B: The Chinese Sample	
	(1)	(2)	(3)	(4)
Dep. Var. =	*Opprtn*	*Routine*	*Opprtn*	*Routine*
Pilot×During	**-0.013***	**0.000**	**-0.164****	**-0.006****
	(-1.67)	**(-0.53)**	**(-2.39)**	**(-2.35)**
Pilot	0.012	0.000	-0.024	-0.001**
	(1.45)	(-0.47)	(-1.19)	(-2.04)
During	-0.053***	0.000	0.101*	0.005**
	(-9.65)	(-3.90)	(1.82)	(2.33)
logMV	-0.017***	-0.000**	0.019	0.001
	(-10.12)	(-2.17)	(0.83)	(0.77)
MB	0.002***	0.000	0.005	0.000
	(2.81)	(0.25)	(0.58)	(1.00)
Ret	-0.040***	0.000	-0.026	-0.000
	(-3.77)	(-0.67)	(-0.42)	(-0.07)
ROE	-0.017	0.000	0.360	0.050*
	(-0.53)	(0.31)	(1.37)	(1.78)
Grants	2.075***	0.000	43.864	-0.107**
	(2.62)	(0.09)	(0.82)	(-2.25)
LagSharesPct	0.238***	0.463***	0.244**	0.249**
	(10.31)	(13.98)	(2.61)	(2.21)
Adj. R^2	7.9%	30.5%	5.8%	4.6%
No. of obs.	55,139	55,139	56,975	56,975
Chow test on *Pilot×During*	F=3.08††		F = 5.41††	

This table reports the results of regressions estimating the difference-in-differences in insider purchases between pilot and non-pilot firms and between the pre-SHO (or pre-CSRC program) and during program periods. We estimate the following regression model:

$$Opprtn\ (Routine) = \alpha_0 + \alpha_1 Pilot{\times}During + \alpha_2 Pilot + \alpha_3 During + Controls + \varepsilon. \quad (1)$$

where *Opprtn* (or *Routine*) is shares purchased by insiders as a percentage of the firm's shares outstanding (in %). Full definitions of the variables are given in the Appendix A. We multiple all the coefficients by 100 for readability. Intercepts and industry- and year-quarter-fixed effects are included in all regressions, but are not reported. *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). †††, ††, and † indicate significance at the 1%, 5%, and 10%, respectively levels (one-tailed Chow test, i.e., F test).

Table 11. What If Insiders Engage in Short Sales?

	The Chinese Sample			
	(1)	(2)	(3)	(4)
Dep. Var. =	*Opprtn*	*Routine*	*Opprtn*	*Routine*
Pilot×During	**-2.810*** **	**0.045***	**-1.442** **	**0.044***
	(-4.98)	**(1.94)**	**(-2.54)**	**(1.75)**
Pilot	-0.550**	-0.001	0.245	-0.004
	(-2.21)	(-0.15)	(1.10)	(-0.64)
During	0.919	0.022**	0.848	0.020*
	(0.78)	(2.11)	(0.80)	(1.84)
logMV			-0.953***	-0.001
			(-6.17)	(-0.31)
MB			-0.150**	0.004
			(-2.47)	(1.70)
Ret			2.426**	-0.008
			(2.20)	(-0.22)
ROE			8.742***	0.286*
			(4.68)	(2.05)
Grants			-19.650	-1.351**
			(-0.46)	(-2.42)
LagSharesPct			0.180***	0.018
			(7.62)	(1.31)
Adj. R^2	1.70%	0.20%	5.40%	0.20%
No. of obs.	33,576	33,576	33,576	33,576
Chow test on *Pilot×During*	F=25.29		F = 6.84††	

This table reports the results of regressions estimating the difference-in-differences in insider sales between pilot and non-pilot firms and between the pre-CSRC program and during program periods. We exclude the sample period between March 31, 2010 and March 18, 2013 and estimate the following regression model:

$$Opprtn\ (Routine) = \alpha_0 + \alpha_1 Pilot \times During + \alpha_2 Pilot + \alpha_3 During + Controls + \varepsilon. \quad (1)$$

where *Opprtn* (or *Routine*) is shares sold by insiders as a percentage of the firm's shares outstanding (in %). Full definitions of the variables are given in the Appendix A. We multiple all the coefficients by 100 for readability. Intercepts and industry- and year-quarter-fixed effects are included in all regressions, but are not reported. *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). †††, ††, and † indicate significance at the 1%, 5%, and 10%, respectively levels (one-tailed Chow test, i.e., F test).

Table 12. Insider Trading Profitability

Dep. Var. = *Profitability* (in %)	Panel A: The U.S. Sample				Panel B: The Chinese Sample			
	Market-adjusted return		Size and MB 25 portfolio adjusted return		Market-adjusted return		Size and MB 25 portfolio adjusted return	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variable	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Opprtn*	*Routine*
Pilot×During	**-0.077***	**-0.004***	**-0.087****	**-0.000****	**-0.186***	**-0.009**	**-0.381****	**-0.006**
	(-1.93)	**(-1.83)**	**(-2.42)**	**(-2.26)**	**(-1.81)**	**(-0.53)**	**(-2.51)**	**(-0.31)**
Pilot	0.049**	0.001	0.043*	0.002	-0.560***	-0.013**	-0.375***	-0.019
	(1.99)	(0.92)	(1.91)	(1.12)	(-4.21)	(-2.12)	(-4.50)	(-1.53)
During	0.119*	0.010	0.030	0.004	0.087	-0.003	0.376*	-0.002
	(1.72)	(1.26)	(0.53)	(0.65)	(0.58)	(-0.19)	(1.94)	(-0.11)
logMV	-0.018**	0.000	-0.009*	0.000	0.333***	0.015***	0.111***	0.005*
	(-2.13)	(0.21)	(-1.67)	(0.35)	(16.65)	(3.41)	(3.35)	(1.83)
MB	0.006	0.000*	0.008**	0.000***	0.037***	0.002	-0.006	0.001
	(1.31)	(1.81)	(2.28)	(2.78)	(3.33)	(0.98)	(-0.54)	(0.58)
Ret	0.050	0.000	0.028	-0.000	4.681***	0.066***	0.590**	-0.020
	(0.80)	(0.14)	(0.49)	(-0.23)	(10.52)	(4.50)	(2.25)	(-0.92)
ROE	0.464***	0.020*	0.330***	0.019**	0.372	-0.012	-0.893	0.010
	(3.64)	(1.94)	(2.97)	(2.05)	(1.18)	(-0.14)	(-1.26)	(0.12)
Grants	0.074*	0.101	0.066*	0.117	26.216	3.873	-8.319	0.944
	(1.95)	(0.37)	(1.73)	(0.68)	(0.57)	(1.71)	(-0.47)	(0.67)
LagSharesPct	0.153**	0.003	0.087	0.003	0.009***	0.004	0.008***	0.006
	(2.03)	(1.26)	(1.57)	(1.39)	(3.85)	(0.43)	(3.43)	(1.16)
Adj. R^2	0.4%	0.1%	0.2%	0.1%	3.9%	0.2%	1.2%	0.1%
No. of obs.	55,139	55,139	55,139	55,139	56,975	56,975	56,975	56,975
Chow test on *Pilot×During*	F=3.84††		F=5.63†††		F=3.08†		F=7.14†††	

This table reports presents the future return after insider sales. We estimate the following regression model:

$$Profitability = \alpha_0 + \alpha_1 Pilot \times During + \alpha_2 Pilot + \alpha_3 During + Controls + \varepsilon.$$

Full definitions of the variables are given in the Appendix A. We multiple all the coefficients by 100 for readability. Intercepts and industry- and year-quarter-fixed effects are included in all regressions, but are not reported. *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). †††, ††, and † indicate significance at the 1%, 5%, and 10%, respectively levels (one-tailed Chow test, i.e., F test).

Table 13. Alternative Measures of Insider Selling

	Panel A: The U.S. Sample						Panel B: The Chinese Sample					
Dep. Var. =	*Shares* (in %)		*TradeSize* (in %)		*SharesFract* (in %)		*Shares* (in %)		*TradeSize* (in %)		*SharesFract* (in %)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Break	*NonBrk*	*Opprtn*	*Routine*	*Opprtn*	*Routine*	*Break*	*NonBrk*	*Opprtn*	*Routine*	*Opprtn*	*Routine*
Pilot×During	**-2.897*** **	**0.012**	**-0.083*** **	**-0.014** **	**-1.100** **	**0.068**	**-2.202*** **	**-0.984**	**-0.568*** **	**-0.004**	**-25.178** **	**1.073**
	(-2.98)	**(0.01)**	**(-3.51)**	**(-2.13)**	**(-2.11)**	**(0.43)**	**(-3.15)**	**(-1.02)**	**(-3.45)**	**(-0.35)**	**(-2.16)**	**(1.46)**
Pilot	1.186	-0.630	0.062**	-0.042	0.181	-0.255**	0.510	0.132	0.153*	0.000	4.191	-0.353
	(1.32)	(-0.44)	(2.08)	(-0.59)	(0.34)	(-2.48)	(1.59)	(0.39)	(1.92)	(0.04)	(0.45)	(-1.09)
During	-2.902**	7.165***	-0.187**	-0.006	-3.068***	-0.265	1.226*	1.203*	0.354***	0.020**	7.127	0.107
	(-2.08)	(2.65)	(-2.01)	(-0.61)	(-3.41)	(-1.01)	(1.96)	(1.82)	(3.25)	(2.52)	(0.72)	(0.14)
logMV	-2.275***	-2.775***	-0.061***	0.001	2.572***	0.406***	-1.543***	-1.346***	-0.321***	0.001	-8.800**	0.011
	(-7.30)	(-5.95)	(-6.60)	(0.37)	(10.64)	(6.38)	(-5.36)	(-5.15)	(-6.93)	(0.49)	(-2.64)	(0.07)
MB	0.538***	0.956***	0.019***	0.003***	0.271***	0.089***	-0.272***	-0.155***	-0.047***	0.001**	-1.556*	0.138**
	(4.49)	(2.83)	(4.58)	(3.72)	(3.43)	(3.84)	(-4.30)	(-3.26)	(-4.87)	(2.29)	(-1.78)	(2.58)
Ret	15.477***	18.033***	0.355***	0.021**	5.327***	0.327**	3.547***	0.215	0.904***	-0.011	77.142***	-0.553
	(8.43)	(5.99)	(6.09)	(2.28)	(7.58)	(2.24)	(2.83)	(0.20)	(3.24)	(-0.63)	(4.14)	(-0.54)
ROE	35.041***	51.439***	1.310***	0.165***	16.486***	2.205***	8.339***	9.862***	1.915***	0.109**	118.600	8.931*
	(8.67)	(6.86)	(10.09)	(6.97)	(8.54)	(5.55)	(3.84)	(4.30)	(4.72)	(2.47)	(1.27)	(1.95)
Grants×100	4.175***	3.638	0.126**	0.029***	2.673***	0.586***	0.024	-0.748*	0.004	-0.003	0.462	-0.613***
	(3.19)	(1.14)	(2.51)	(3.05)	(4.03)	(3.26)	(0.03)	(-1.80)	(0.02)	(-0.41)	(0.04)	(-2.94)
LagShares	0.272***	0.267***					0.159***	0.181***				
	(13.41)	(11.18)					(5.84)	(3.78)				
Length	0.263***	0.370***					0.155*	0.141*				
	(4.32)	(3.49)					(1.81)	(1.79)				
LagTradeSize			0.266***	0.285***					0.101***	0.020		
			(17.41)	(10.01)					(5.10)	(1.36)		
LagSharesFract					0.000	0.504***					0.117***	0.007
					(0.20)	(2.57)					(6.19)	(1.43)
Adj. R^2	13.8%	14.7%	9.8%	10.1%	4.1%	2.5%	4.8%	5.3%	2.4%	0.2%	2.1%	0.2%
No. of obs.	38,920	7,901	55,139	55,139	55,139	55,139	25,144	17,290	56,975	56,975	56,975	56,975
Chow test on *Pilot×During*	F = 2.07†		F=3.80††		F =6.01†††		F=3.32†		F=12.44†††		F=5.01††	

This table presents the results of alternative level or partition analysis. We estimate the following regression model:

$$DepVar = \alpha_0 + \alpha_1 Pilot \times During + \alpha_2 Pilot + \alpha_3 During + Controls + \varepsilon,$$

where *DepVar* represents *Shares*, *TradeSize*, or *SharesFract. Shares* is shares sold by insiders as a percentage of the firm's shares outstanding of a quarter in columns 1, 2, (or 7 and 8). *TradeSize* is the average value of shares sold per sale transaction by insiders as a percentage of the firm's shares outstanding of a quarter in columns 3, 4, (or 9 and 10). *SharesFract* is shares sold by insiders as a percentage of the initial holdings of insiders of a quarter in columns 5, 6, (or 11 and 12). Insider transactions are classified as either opportunistic (*Opprtn*) or routine (*Routine*) transactions, which is based on the Cohen, Malloy, and Pomorski (2012) method in columns 3, 4, 5 and 6 (or 9, 10, 11, and 12). Alternatively, insider transaction is classified as either opportunistic (*Break*) or routine (*NonBrk*) transactions based on the Ke, Huddart, and Petroni (2003) method in columns 1, 2, (or 7 and 8). *LagShares* (*LagTrade* or *LagSharesFract*) represents opportunistic (or routine) insider sales in the previous quarter when the dependent variable is *Shares* (*TradeSize* or *SharesFract*), respectively. Full definitions are given in the Appendix A. We multiple all the coefficients by 100 for readability. Intercepts and industry- and year-quarter-fixed effects are included in all regressions, but are not reported. *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). †††, ††, and † indicate significance at the 1%, 5%, and 10%, respectively levels (one-tailed Chow test, i.e., F test).