Review of Finance (2015) 19: pp. 865–905
doi:10.1093/rof/rfu007
Advance Access publication: March 13, 2014

Stealth Trading and Trade Reporting by Corporate Insiders*

ANDRÉ BETZER[1], JASMIN GIDER[2], DANIEL METZGER[3] and ERIK THEISSEN[4]
[1]*BUW-Schumpeter School of Business and Economics,* [2]*University of Bonn,* [3]*Stockholm School of Economics and* [4]*University of Mannheim and Centre for Financial Research (Cologne)*

Abstract. Regulations in the pre-Sarbanes–Oxley era allowed corporate insiders considerable flexibility in timing their trades and engaging in stealth trading, for example, by executing several trades and reporting them jointly after the last trade. We document that even these lax reporting requirements were frequently violated and stealth trading was common. Event study abnormal returns are larger after reports of stealth trades than after reports of otherwise similar non-stealth trades. Our results imply that delayed reporting impedes the adjustment of prices to the information revealed by insider trades. They lend strong support to the more stringent reporting requirements established by the Sarbanes–Oxley Act.

JEL Classification: G14, G30, G32

1. Introduction

Corporate insiders arguably know more about the prospects of their firms than other market participants. This hypothesis is supported by a host of papers documenting that insider trades, and purchases in particular, convey information to the market (e.g., Seyhun (1986) and Chang and Suk (1998) for the USA; Friederich *et al*. (2002) and Fidrmuc, Goergen, and Renneboog (2006) for the UK). The USA and many other countries have adopted

* We thank two anonymous referees, Wolfgang Bessler, Hans Degryse, Joost Driessen, Peter de Goeij, Frank de Jong, Alexander Kempf, Vincent van Kervel, Torsten Luedecke, Mieszko Mazur, Martin Ruckes, Chester Spatt, Monika Trapp, and seminar participants at ESSEC, Schulich School of Business, at the universities of Bonn, Cologne, Hannover, Karlsruhe, Mannheim, and Tilburg, at the 2008 FMA European Conference in Prague, the 2008 Annual Meeting of the EFMA in Athens, the 5th Conference of the Portuguese Finance Network in Coimbra, the 7th International Conference on Corporate Governance at the University of Birmingham, the 2010 EFA annual meeting in Frankfurt, the 2010 FMA annual meeting in New York, and the 2010 CRSP Forum in Chicago for valuable comments.

regulations that require corporate insiders to report their trades. The model of Huddart, Hughes, and Levine (2001) provides a theoretical justification for these regulations. The authors show that information is reflected in prices more rapidly when insiders have to disclose their trades. Several empirical papers (e.g., Chang and Suk, 1998; Betzer and Theissen, 2009) have shown that share price reactions occur on both the trading and the reporting dates. Thus, without reporting, the market is unable to infer the full information content of the trade, which implies that market prices are distorted in the period between the trading and reporting dates. Delayed reporting, then, may impede the price adjustment to information revealed by the insider trade.

In the era prior to the Sarbanes–Oxley Act (SOX), Section 16 of the Securities Exchange Act required corporate insiders in the USA to report their trades by the 10th of the month following the trade. Thus, the maximum time allowed between the trade and the report was 40 days, allowing corporate insiders considerable flexibility to time their trades and reports. This flexibility could be used strategically. An insider wishing to trade a large quantity could split up the order into several smaller chunks. Splitting up a large order reduces the order's price impact and thus results in reduced execution costs (e.g., Kyle, 1985; Chordia and Subrahmanyam, 2004). Lebedeva, Maug, and Schneider (2013) analyze the trading strategies of corporate insiders in detail. They find strong evidence that insiders split up large trades in order to reduce their price impact. However, if the insider reported each individual trade immediately, the share price reaction on the reporting date would move the price against the insider, and subsequent trades would occur at less favorable prices. Consequently, the insider has an incentive to delay the reporting of a series of trades until after the last transaction. By doing so, insiders can benefit from the reduced price impacts of split-up trades while avoiding the adverse price reaction that immediate reports would trigger. We refer to this trading and reporting pattern as *stealth trading*. In this article we analyze incidences of stealth trades. We identify a stealth trade whenever a trade is either followed by another trade by the same insider before it is reported, or when a trade is executed after another trade by the same insider which has not yet been reported.

Note that the incentive for an insider to engage in stealth trading does not depend on the assumption that the insider trades on private information. The only assumption necessary for our argument is that other market participants believe that insiders possess private information with a positive probability. The stylized fact that prices react to the publication of insider trades supports this assumption.

This article asks three related questions. First, how long are reporting delays during the pre-SOX era? Second, do insiders engage in stealth trading and use their flexibility in choosing the timing and reporting of their trades, and, if so, is this behavior systematically related to the characteristics of the insider or the firm? Third, what are the implications of delayed reporting on the informativeness of prices, and how does the market react to the reporting of stealth trades?

The first question is important because, as argued above, delayed reporting can impede the adjustment of prices to the information revealed by the insider trades. The length of the delays thus matters. The relevance of the second question derives from the observation that stealth trading generates benefits for the insider at the expense of other market participants. If each trade were reported immediately, the second and subsequent trades of a series of insider trades would be executed at prices less favorable to the insider but more favorable to the insider's counterparties. The answer to the third question allows us to assess the relevance of the issues addressed in this article. It is also important because it enables us to draw inferences on the trading motives of insiders engaging in stealth trading. On reporting dates, market participants learn whether an insider has engaged in stealth trading. If market participants believe that insiders possessing private information are more likely to time their trades and reports, one should observe a greater price reaction for stealth trades compared to non-stealth trades with otherwise similar characteristics.

Our results can be summarized as follows. First, reporting delays were substantial in the pre-SOX period. The mean reporting delay was 36.6 days and the median was 24 days, with 18.2% of all trades in our sample reported later than on the 10th of the month following the trade. The very large number of violations of the trade reporting requirement implies that the requirement was apparently not well-enforced. We further find clear evidence of stealth trading. Only 36.0% of the trades in our sample were non-stealth trades (i.e., these trades were reported before the same insider traded again, and they were not preceded by a trade by the same insider that had not yet been reported).

Regression analysis reveals that the occurrence of both late filings and stealth trades is systematically related to firm, trade, and trader characteristics. In particular, the results are consistent with the notion that insiders who are more closely monitored (and who therefore may be facing higher litigation risks) are less likely to file their trades late.

Consistent with previous findings, our event study results show that share prices react to the reporting of insider trades. The cumulative abnormal returns (CARs) over 10-day and 20-day windows are larger after purchases

than after sales. In cross-sectional regressions we find that the magnitude of the price reaction decreases only slowly with the reporting delay (after insider sales), or not at all (after purchases). Thus, our results support the notion that market prices are distorted in the period between a trade and its report. Finally, the market reaction is stronger after reports of stealth trades than after reports of otherwise similar non-stealth trades. Thus, market participants apparently believe that insiders engaging in stealth trading are more likely to possess private information.

As a robustness check we also analyze a post-SOX sample. Reporting delays are significantly shorter post-SOX, but our main conclusions that delayed reporting impedes the adjustment of prices and that stealth trades trigger larger share price reactions than non-stealth trades still hold.

Our results clearly support the more stringent trade reporting requirements established by SOX. They also suggest that countries that currently allow longer reporting delays should consider revising and/or enforcing their regulations. Recent evidence reported by Fidrmuc, Korczak, and Korczak (2013) suggests that some countries do not mandate and enforce timely trade reporting. The authors find median reporting delays of 5 days for Italy, 7 days for Belgium, and 14 days for France.

The paper which comes closest to our research is Brochet (2010). He analyzes the information content of SEC Form 4 Filings before and after the implementation of the SOX. He finds that the information content of insider trades increased after SOX as evidenced by higher abnormal returns and higher trading volume in the post-SOX period. From the observation that "insiders are less likely to sell shares immediately prior to negative stock returns and ahead of earnings news that falls short of analyst forecast" he infers that "there is a decrease in informed insider selling around SOX" (Brochet 2010, p. 420). The main difference between our article and Brochet (2010) is that Brochet does not analyze stealth trading and reporting, which is the main focus of the present article.

Our article is further related to recent papers by Cheng, Nagar, and Rajan (2007), Betzer and Theissen (2010) and Carter, Sattar, and Reeb (2003). Cheng, Nagar, and Rajan (2007) exploit the feature that corporate insiders in the USA could, in certain circumstances, delay the reporting of non-open market trades until the end of the fiscal year of the firm (SEC Form 5 trades). The authors find that insider sales by top executives in Standard & Poor's 500 firms disclosed in such a delayed manner predict negative future returns and lower operating profitability relative to analyst forecasts. Insider purchases, on the other hand, are hardly predictive of future returns. Cheng, Nagar, and Rajan (2007) conclude that "managers in large firms may have used late-disclosure Form 5 sales for information-based

trading" (p. 1861). Betzer and Theissen (2010) use data from Germany to show that substantial reporting delays are common, that the delays are systematically related to firm characteristics, and that abnormal returns after the reporting dates of insider trades are independent of reporting delays. The latter finding implies that prices are distorted in the period between the trading and reporting dates. Carter, Sattar, and Reeb (2003) analyze a sample of insider buy transactions between 1991 and 1994 and find evidence of substantial reporting delays. They further report that CARs in the period between the trading and the reporting date are positively related to the length of the reporting delay.

Our article differs from these papers because it is the first to systematically document stealth trading and to analyze the determinants and implications of this phenomenon. It further differs from Cheng, Nagar, and Rajan (2007) as we do not analyze the relatively small sample of non-open market trades eligible for late reporting but, rather, the much larger sample of all insider trades that were required to be filed on Form 4 of the USA Securities and Exchange Commission (SEC).[1] Betzer and Theissen (2010) analyze reporting delays in Germany but have a much smaller sample (1,977 observations as compared to 317,727 in the present article), and, more importantly, the regulatory regime in Germany is distinctly different from that in the USA.

The remainder of the article is organized as follows. Section 2 describes the data set and presents descriptive statistics. Section 3 presents evidence on delayed trade reporting. Section 4 determines whether incidences of stealth trading and trade reporting took place and also analyzes whether stealth trades are systematically different from non-stealth trades. Section 5 uses event study methodology to compare market responses to stealth and non-stealth trades. In Section 6 we report the results of several robustness checks. Section 7 concludes.

2. Regulatory Background and Data

2.1 REGULATORY BACKGROUND

Trading based on material, non-public information, which is typically referred to as insider trading, is prohibited in the USA according to Rule 10b-5 of the Securities and Exchange Act of 1934. Rule 10b5-1, enacted in 2000, facilitates the prosecution of insider trading. It essentially implies that

[1] The number of Form 5 sales (purchases) for Standard & Poor's 500 stocks during 1998–2001 amounts to 438 (419). The corresponding figures for Form 4 trades are 10,166 and 7,217, respectively (Cheng, Nagar, and Rajan (2007), Table 1 Panel D).

it is unlawful to trade securities while in possession of material non-public information. It is not necessary that the information is causal for the trading decision. Paragraph c of Rule 10b5-1 establishes a safe harbor rule. When a trade was planned before the trader received material information, the trade can still be executed (see Jagolinzer, 2009 for an empirical analysis of pre-planned trades).

These rules apply to all traders, including directors, officers, and beneficial owners of the firm. These persons, often referred to as *corporate insiders*, are more likely to be in possession of material non-public information. Therefore, their trading activities are regulated by Section 16 of the Securities Exchange Act of 1934. According to Section 16 every person who is directly or indirectly the beneficial owner of more than 10% of any class of any equity security or any director or officer of the issuer of such has to disclose her trades. In the era prior to the SOX, Section 16 of the Securities Exchange Act required corporate insiders to report their trades (file a Form 4) by the 10th of the month following the trade. Thus, the maximum time allowed between the trade and the report was 40 days, allowing corporate insiders considerable flexibility to time their trades and reports. Since the implementation of SOX on August 29, 2002, insiders have to report their transactions before the end of the second business day following the day on which the subject transaction has been executed.

Some countries restrict the periods during which insiders are allowed to trade. An example is the LSE model code in the UK which prevents corporate insiders from trading during the 2 months preceding final or interim earnings announcements, and during 1 month prior to quarterly earnings announcements. No comparable rule exists in the USA. However, many firms place internal restrictions on the trading activities of insiders (Roulstone, 2003).

According to Section 16b of the Securities Exchange Act every firm (and any shareholder on behalf of the firm) has the right to claim the insider's trading profit if it is realized within any period of less than 6 months. This is referred to as the short-swing rule.

The Securities Enforcement Remedies and Penny Stock Reform Act of 1990 strengthened the SECs proceeding powers significantly. Cox, Thomas, and Kiku (2003) empirically analyze the SEC's enforcement activities. They identified a total of 24 enforcement proceedings relating to delinquent filings in 2001 and 2002.

2.2 DATA

Our analysis requires data on insider trades, firm characteristics, and stock prices. The data selection process follows that of Lakonishok and Lee (2001)

and Marin and Olivier (2008) and merges data from four different sources, namely, the TFN Insider Filing Data Files, the Center for Research in Security Prices (CRSP) database, the Compustat database, and the I/B/E/S database. The initial sample consists of insider trades reported on SEC Form 4 in companies listed on the New York Stock Exchange, the American Stock Exchange, or the NASDAQ during 1992–2010. This period spans the implementation of SOX, which marked a change in regime because it requires insiders to report a trade within only two working days. Our main analysis focuses on the pre-SOX era. Results for the post-SOX era are presented and discussed in Section 6.

In the following we describe the construction of the sample for the main analysis (i.e., the pre-SOX sample). We begin the sample construction with the TFN database. We include all open market or private purchases (transaction code P) and all open market or private sales (transaction code S) of non-derivative securities whose records were not amended (amendment indicator "blank") between January 1, 1992 and December 31, 2001. Of these transactions, we retain only those filings whose data can be verified by TFN with a high level of confidence (cleanse indicators R and H). The TFN Insider Filing Data Files contain the following information:

- Company name and CUSIP
- Transaction date and reporting date (SEC receipt date)
- Transaction code (purchase or sale), number of shares exchanged in the transaction, and transaction price
- Insider's position within the firm, which we classify into four groups:
  - CEO (also possibly the chairman of the board)
  - Chairman (only if not also the CEO)
  - Executive directors, excluding the CEO
  - Other non-executive officers, affiliates, beneficial owners, or other persons required to report their trades

We exclude all filings that have no entry for transaction price, number of shares, reporting date to the SEC, position of insider, or sector fields, leaving 741,653 records remaining. We also exclude insider transactions whenever the reported transaction price was not within a 20% interval around the CRSP closing price on the insider trading day. We further exclude trades when the number of shares traded exceeded 20% of total shares outstanding. We do not attempt to single out Rule 10b5-1 trades, because very few of these pre-planned trades took place during the pre-SOX era. Brochet (2010), using a sample covering the period 1997–2002, reports that Rule 10b5-1 trades accounted for only 0.55% of the trades in his sample.

We complement the data on insider transactions with supplementary data from various sources. We obtain financial data from the Compustat database. All data items are taken from firm financial statements at the end of the fiscal year preceding the reporting of the insider trades. We measure book leverage (the variable *Leverage*) as the ratio of long-term debt (data item 9) plus debt in current liabilities (item 34) to long-term debt plus debt in current liabilities plus stockholder equity (item 216). Firm size (*Size*) is defined as the natural logarithm of the market value of equity. Tobin's Q (*Q*) is calculated as the ratio of the market value of assets to the book value of total assets (item 6). Following Malmendier and Tate (2007), we define the market value of assets as total assets plus market equity (item 25 times item 199) minus book equity. We calculate book equity as the sum of stockholder equity and balance-sheet deferred taxes and investment tax credits (item 35), where available, minus the preferred stock liquidating value (item 10) and minus post-retirement assets (item 336), where available. When stockholder equity was not available as data item 216, we calculated stockholder equity alternatively as common equity (item 60) plus the preferred stock par value (item 130) or total assets minus total liabilities (item 181). If the preferred stock liquidating value was not available as data item 10, we calculated the preferred stock liquidating value alternatively as redemption value (item 56) or par value (item 130). Return on equity (*RoE*) is net income (item 172) divided by book equity.

Further, we obtain data on analyst forecasts from the I/B/E/S and Compustat databases. We define the variable *Numest* as the total number of analysts covering a company in the last available yearly earnings forecast before the transaction date of the insider trade. We further obtain the dates of all quarterly earnings announcements.

For an observation to be included in our analysis, all the necessary data items in the CRSP, Compustat, and I/B/E/S databases must be available. This requirement reduces the sample to 317,727 observations.

In our empirical analysis, we use the following additional variables. The variable *Delay* is the difference in days between the reporting and transaction dates. We calculate the variable *TradeVolume* as the number of shares exchanged in a transaction times the transaction price, divided by the market value of equity. We define *Number of insiders* as the total number of insiders who traded shares in the same company on the same day. Table I summarizes the definitions of these variables. All variables are winsorized at their 1% and 99% percentiles.

Our analysis uses two different data sets: a "transaction sample" and a "reporting sample." For the transaction sample, we aggregate all transactions by the same insider that are (i) executed on the same day; and

Table I. Description of variables

Variable	Definition
AAR	Daily average abnormal return, calculated using the market model over a 255-day estimation window ending 46 days prior to the announcement date. The market proxy is the CRSP value-weighted index.
CAR	Cumulative abnormal return, calculated using the market model over a 255-day estimation window ending 46 days prior to the announcement date. The market proxy is the CRSP value-weighted index.
CEO (d)	Dummy variable that takes the value 1 if the trader is a CEO, and 0 otherwise. For the cross-sectional regression of CARs, if there are several trades in the same stock on the same day, the highest insider position is selected according to rank, that is, CEO, chairman, executive, director, and other.
Chairman (d)	Dummy variable that takes the value 1 if the trader is the chairman but not the CEO, and 0 otherwise. For the cross-sectional regression of CARs, if there are several trades in the same stock on the same day, the highest insider position is selected according to rank, that is, CEO, chairman, executive, director, and other.
Days to next report	Number of days from the transaction to the next quarterly earnings announcement.
Delay	Lag in days between trading and reporting the transaction. For descriptive statistics and the cross-sectional regression of CARs, Delay is the trading-volume–weighted average delay of all insider trades of a firm reported on the same day.
Director (d)	Dummy variable that takes the value 1 if the trader is a non-executive director, and 0 otherwise. For the cross-sectional regression of CARs, if there are several trades in the same stock on the same day, the highest insider position is selected according to rank, that is, CEO, chairman, executive, director, and other.
Executive (d)	Dummy variable that takes the value 1 if the trader is an executive director but not the CEO, and 0 otherwise. For the cross-sectional regression of CARs, if there are several trades in the same stock on the same day, the highest insider position is selected according to rank, that is, CEO, chairman, executive, director, and other.
First of series	A trade is classified as first of series if the trade is the first trade in a series of trades in which at least one trade is followed by at least one additional trade by the same insider before it is reported.
Late filing (d)	Refers to trades reported after the 10th of the month following the trade. If the 10th of the month falls on a weekend, the trade is classified as late filing if it is reported later than the following Monday.
Book leverage	Ratio of long-term debt plus debt in current liabilities, divided by long-term debt plus debt in current liabilities plus stockholder equity.
Market value of equity ($ millions)	Share price multiplied by the number of shares outstanding.
Number of insiders	Total number of insiders who traded shares in the same company on the same day. For the cross-sectional regression of CARs, this refers to the total number of insiders who reported trades in shares of the same company on the same day.

(continued)

Table I. (Continued)

Variable	Definition
Numest	Total number of analysts covering a company in the month preceding the reporting date of the insider trade.
Other (d)	Dummy variable that takes the value 1 if the trader is a non-executive officer, affiliate, beneficial owner, or other person required to report trades, and 0 otherwise. For the cross-sectional regression of CARs, if there are several trades in the same stock on the same day, the highest insider position is selected according to rank, that is, CEO, chairman, executive, and other.
Pre-announcement (d)	Dummy which is set to 1 if a trade was not executed within a 30-calendar-day window after an earnings announcement, and 0 otherwise. For the cross-sectional regression of CARs, if several trades are reported on the same day, the dummy takes the value 1 if at least one trade was not executed within a 30-calendar-day window after an earnings announcement, and 0 otherwise.
Purchase (d)	Dummy variable that takes the value 1 if the (net) transaction volume of the insider trade is positive, and 0 otherwise.
RoE	Return on equity, defined as net income divided by book equity.
Serial trade	A trade is classified as a serial trade if it follows a trade by the same insider that has not yet been reported.
Size	Firm size (Size) is defined as the natural logarithm of the market value of equity.
Stealth (d)	For the cross-sectional regression of CARs, the dummy variable takes the value 1 if (i) the trade is followed by at least one additional trade by the same insider before it is reported or it follows a trade by the same insider that has not yet been reported; and (ii) the market can infer on the reporting date that the trade was a stealth trade (see Figure 3 for an illustration). All other trades are classified as non-stealth trades and the dummy variable is 0 for these cases. With respect to stealth trades that are reported in an overlapping way, only serial transactions and not just the first transaction can be identified as stealth trades.
Timed (d)	Refers to a trade that is executed within 60 days prior to the next earnings announcement and reported after the announcement (but before the following announcement).
Timed*Stealth	Interaction term of the variables timed and Stealth.
Tobin's Q	Ratio of the market value of assets to the book value of total assets.
TradeVolume	The number of shares exchanged in a transaction times the transaction price, divided by the market equity of the company whose stocks were bought or sold in the insider trade. For the cross-sectional regression of CARs, if several trades were reported on the same day, we sum the total volume of those trades.

(ii) jointly reported on the same day. We present an aggregated transaction as one trade with the net amount traded. The net transaction volume is positive (negative) if the sum of all the individual trades by this particular insider on the same trading day is positive (negative). Arguably, a report that includes both purchases and sales made by the same insider provides a

weaker signal than a report that reports only unidirectional trades. However, only 0.51% of the reports in our sample contain both purchases and sales. This low number is likely to be due to the "short-swing rule," (Section 16b of the Securities Exchange Act; see Section 2 of the article) which requires insiders to return to the firm all profits from roundtrip trades completed within 6 months. Following these calculations, we classify each aggregated transaction as a purchase or a sale. Our final (pre-SOX) transaction sample consists of 111,156 purchases and 206,571 sales (317,727 observations in total). These observations relate to 8,423 different firms and 58,405 distinct firm–years. Note that in the transaction sample, two trades by different insiders are treated as two distinct observations, even if they are executed and/or reported on the same day.

The announcement date in our event study analysis is the day on which an insider trade was filed with the SEC. Therefore, we aggregate all insider trades in the shares of a given firm that were reported on the same day, irrespective of whether the trades were reported by the same insider or by different insiders. This is necessary because otherwise the same event date would be included more than once. We refer to the resulting sample as our (pre-SOX) reporting sample. Again, aggregated transactions are treated as one trade, and the net trade direction and net volume are as defined above. In our regression analysis we control for the aggregate trade volume and the number of insiders that traded on a given day. The final data set for the event study consists of 40,073 purchases and 59,416 sales (99,489 trades in total).

Table II presents descriptive statistics for the firms in our pre-SOX sample. Average firm size, as measured by the market value of equity, is \$3,235.3 million. The firm size distribution is heavily skewed. The average Tobin's Q of the sample firms is 3.17, the average RoE is 0.5%, and the mean book leverage is 41.2%. Mean trade size, expressed as a percentage of the market value of equity, is 0.09%. The average number of insiders trading on a given day is 2.0. The average insider trade was executed 56 calendar days before the firm published its next annual or quarterly earnings report.

Figure 1 shows the distribution of trading dates. Although it appears to follow a weak U-shaped pattern, the general impression from Figure 1 is that trades are more or less evenly distributed over the month. The distribution of reporting dates shown in Figure 2 is dramatically different. The daily frequencies start low (only 0.81% of trades are reported on the first day of the month) and then increases strongly until the 10th of the month. On this day alone, almost 32% of all trades are reported. When we weight the trades by their volume, this number increases further to 42.7%. After the 10th, the frequencies decline sharply. In the second half of the month, there is no single day on which more than 0.75% of trades are reported.

Table II. Descriptive statistics

This table reports summary statistics for the transaction sample. Tobin's Q is calculated as the ratio of the market value of assets to the book value of total assets. The variable RoE is net income divided by book equity. We measure Book leverage as the ratio of long-term debt plus debt in current liabilities to long-term debt plus debt in current liabilities plus stockholder equity. We define the variable Numest as the total number of analysts covering a company in the month preceding the reporting date of an insider trade. We calculate the variable TradeVolume as the ratio of the number of shares exchanged in a transaction times the transaction price to the market equity of the company whose stocks were bought or sold in the insider trade. We define Number of insiders as the total number of insiders who traded their shares in the same company on the same day. Days to next report denotes the number of days from a transaction to the next quarterly earnings announcement. Delay indicates the lag in days between the trading and reporting of a transaction.

Variable	Mean	Standard deviation	Lower quartile	Median	Upper quartile	#
Market value of equity ($ millions)	3,235.317	9,984.814	100.782	372.604	1,476.902	317,727
Tobin's Q	3.173	4.218	1.124	1.716	3.334	317,727
RoE	0.005	0.515	–0.011	0.102	0.172	317,727
Book leverage	0.412	0.252	0.208	0.396	0.574	317,727
TradeVolume	0.09%	0.22%	0.01%	0.02%	0.07%	317,727
NumInsider	1.952	1.824	1	1	2	317,727
Days to next report	56.282	23.695	41	61	76	317,727
Numest	0.776	2.585	0	0	0	317,727
Delay (days)	36.612	103.647	15	24	33	317,727

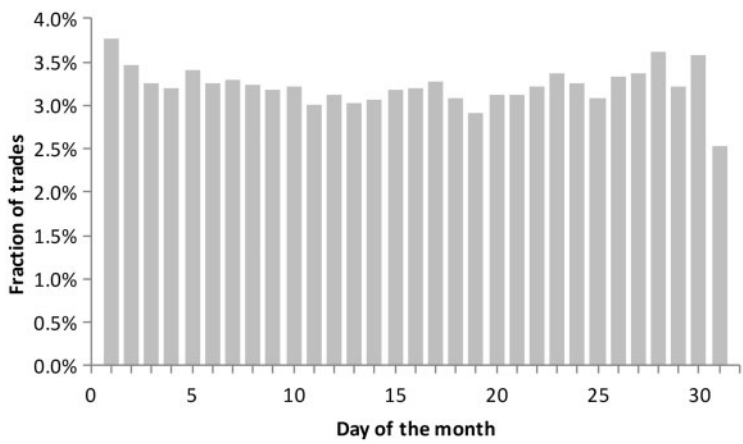

Figure 1. Distribution of trading dates by day of the month.

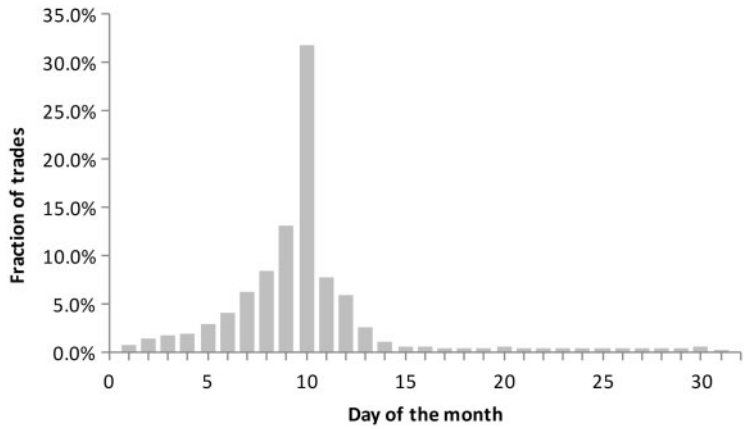

Figure 2. Distribution of reporting dates by day of the month.

There are two not mutually exclusive (and observationally equivalent) explanations for the strong pattern we document. First, many corporate insiders may routinely report all trades made during the previous month on the 10th. This practice may delay the adjustment of prices to the information revealed by the insider trades, and it may be to the disadvantage of other traders (although not intentionally). Whenever share prices react to the reporting of an insider trade, reporting delays imply distorted prices in the period between the trading and filing dates. If an insider executes several trades on different days but reports them jointly, the later trades are executed at prices that are more favorable to the insider than they would have been in the case in which each trade had been reported immediately. This practice is beneficial for the insider but obviously it comes at the disadvantage of the counterparties to the insider's trades. Second, some insiders may *intentionally* delay the reporting of their trades to avoid the price impact triggered by the report. By considering only the trading and filing dates, the two cases—routine reporting on the 10th and intentional delays—cannot be distinguished from each other. However, the share price reaction on the filing date can be expected to reflect the market's beliefs about the insiders' trading motives. Therefore, the analysis of the share price reaction on the reporting date will allow us to draw inferences about the market's assessment of the insiders' trading motives.

3. Reporting Delays

This section presents evidence on the magnitude of reporting delays and the determinants of late filings. The frequency distributions of trading and

Table III. Distribution of delays

This table reports summary statistics for the distribution of reporting delays.

	Pre-SOX			Post-SOX		
	All	Purchases	Sales	All	Purchases	Sales
Mean	36.61	42.29	33.55	7.23	15.34	4.98
Standard deviation	103.64	123.20	91.26	53.78	84.16	41.21
Lower quartile	15	15	16	1	1	1
Median	24	24	24	2	2	2
Upper quartile	33	34	33	3	4	3
Percentage of late filings (%)	18.18	22.33	15.95	9.69	16.67	7.77
#	317,727	111,156	206,571	391,795	84,069	307,566

reporting dates shown in Figure 1 and Figure 2 demonstrate that trades are approximately evenly distributed over the month, whereas reports cluster around the 10th. If insider trades were indeed equally distributed over the days of the month, and if each trade were reported on the 10th of the month after the trade (i.e., on the last permissible day), we would expect an average reporting delay of approximately 25 days. Table III shows the actual reporting delays. The median delay (24 days for purchases and sales) corresponds roughly to the benchmark value derived above. The mean delay is much longer, at 36.6 days.[2] Purchases are reported with longer delays than sales (42.3 days as compared to 33.6 days). This difference may be indicative of deliberate delaying, because previous papers (e.g., Seyhun (1986) and Brochet (2010) for the USA; and Fidrmuc, Goergen, and Renneboog (2006) for the UK) document that insider purchases are more informative, as evidenced by larger abnormal returns. This finding, in turn, implies that insiders who purchase shares are more likely to possess private information and therefore have greater incentives to conceal their trading activity.

The discrepancy between the mean and median reporting delays implies that the distribution of reporting delays is heavily skewed. The magnitude of the average delay further implies that a significant fraction of trades, and of purchases in particular, is reported too late (i.e., later than the 10th of the month following the trade). In fact, Table III reveals that 18.2% of the

[2] This figure is greater than that given in Table 1 of Brochet (2010). The author uses a shorter sample period (starting in 1997) and confines his analysis to trades initiated by the CEO, CFO, COO, board chairs, and presidents. Brochet further measures the reporting delay in *trading* days whereas we measure it in *calendar* days.

trades in our pre-SOX sample were reported too late.[3] We use the term *Late filings* for these cases. Late filings are more common for purchases than for sales (22.3% as compared to 16.0%).

The high percentage of late filings implies that reporting requirements are weakly enforced in the pre-SOX era. This observation is surprising because violations of the reporting requirement are easily detectable: SEC filings include trading and reporting dates, together with a unique person identification number that allows for easy identification of the insider. Cox, Thomas, and Kiku (2003, p. 752), who analyze the SEC's enforcement activities empirically, argue that the SEC's "resources are limited so that priorities must be set".

The percentage of late filings is too large to be explained by accidental omission. Apparently, there is a substantial number of insiders who do not care about the reporting requirements or who deliberately (and possibly strategically) file their reports late. To shed light on this issue, we estimate a linear probability (LPM) model with firm fixed effects in which the dependent variable is 0 if a trade was reported on time (i.e., by the 10th of the month following the trade), and 1 if the trade was reported late. We include firm fixed effects in order to control for differences in corporate governance. Variables measuring the quality of firms' corporate governance (such as the Gompers, Ishii, and Metrick (2003) index) have little time series variation. Therefore, including firm fixed effects in the regression controls for differences in corporate governance. We estimate an LPM rather than a logit model because logit estimators are usually not well behaved when including a large set of dummy variables (e.g., when including firm fixed effects). The slope vector of the LPM provides a linear approximation to the conditional expectation function. In an unreported robustness test, we establish that the LPM and the logit model yield similar results if we do not include firm fixed effects. The independent variables include firm and trade characteristics. We use the number of analysts following as a proxy for investor attention.[4] Trade characteristics include trade volume relative to firm market capitalization and the number of different insiders trading on the same day. We include three further control variables, namely, Tobin's Q as a proxy for the valuation of the firm, RoE as a measure of operating profitability, and book leverage.

[3] These figures take into account the fact that when the 10th of a month is a Saturday or a Sunday, the trade needs only be reported on the 12th or the 11th of that month, respectively.

[4] To avoid multicollinearity, we do not include firm size (the correlation between firm size and number of analysts following is 0.79 in the transaction sample). We obtain very similar results, however, when we replace the number of analysts by firm size.

Many firms restrict insider trading by defining a blackout period during which trading is prohibited. In this context, Bettis, Coles, and Lemmon (2000, p.192) find that "company-level regulation of insider trading is widespread [in the U.S.]. By late 1996, over 92% of [. . .] [their] sample firms have some type of policy regarding insider trading, and 78% of the sample firms have explicit blackout periods during which the company prohibits trading by its insiders. The single most common policy disallows trading by insiders at all times except during a trading window that is open during the period three through 12 trading days after the quarterly earnings announcement." In addition, Roulstone (2003) analyzes a large sample of insider trades in the USA. From the observed trading pattern, he derives whether a firm has a restriction in place. Specifically, he assumes that a firm has a restriction in place when more than 75% of the insider trades occur in the 20 trading days (approximately 1 month) after earnings announcements are made. Since our sample is closer to Roulstone's than to the sample of firms surveyed by Bettis, Coles, and Lemmon (2000), we include the dummy variable *Pre-announcement* in our model, which is set to 1 if a trade was not executed within a 30-calendar-day window after an earnings announcement, and 0 otherwise. Data on earnings announcement dates are missing in some cases. We address this by excluding all observations where the time between the insider trade and the date of the publication of the next quarterly earnings announcement is more than 91 days. We obtain similar results when we include all observations. In the latter case, we misclassify insider trades that were executed within a 30-day window after the publication date of an earnings announcement not included in our data set.

We further define three dummy variables that describe the insider's position in the firm. The first dummy is set to 1 when the CEO is among the traders trading on a given day, and 0 otherwise.[5] The second dummy identifies trades by the chairman of the board (unless the chairman is simultaneously the CEO), and the third dummy identifies trades by other executive directors of the firm. Trades by outside directors, beneficial owners, and others thus constitute the base group.

We estimate a pooled model that includes both purchases and sales, and two separate models including only purchases and only sales, respectively. The pooled model includes a dummy variable that captures differences in the probability of late reporting between purchases and sales. All models include

[5] As a robustness check, we re-estimated the model including *only* trades made by the CEO. The results are similar to those presented below. The main difference is that for the CEO-only sample, we do not find that purchases are more likely to be filed late than sales.

firm fixed effects, sector dummies (where we adopt the classification used in the TFN insider filings) and year dummies. Standard errors are clustered at the firm level. The results are reported in Table IV.

The results reveal that insider trades in more highly leveraged firms are more likely to be reported late. Insider trades in firms with higher RoE are less likely to be reported late. However, this result is only significant in the pooled model. The other firm-specific variables yield inconclusive results.

Considering trade-specific variables, we find (in the pooled model) that there is no significant difference in the probability of late filing between insider purchases and sales. Trades executed during the period prior to earnings announcements are significantly more likely to be reported late. There are two non-mutually exclusive explanations for this finding. First, insiders are more likely to possess relevant private information prior to an earnings announcement and therefore, have an incentive to delay the reporting of their trades. Second, as noted above, many firms have adopted policies that allow insider trades only in a window open for a specified period after the quarterly earnings announcement (Bettis, Coles, and Lemmon 2000). Insiders of these firms are more likely to trade shortly after an earnings announcement and, at the same time, are more likely to be scrutinized and may therefore tend to file their reports on time.

Larger trades and trades executed on days on which several insiders traded are less likely to be reported late. With respect to the position of the insider within the firm, we find that CEOs, chairmen of the board, and executive directors are significantly less likely to file late than other corporate insiders (e.g., non-executive directors and beneficial owners). These findings are again supportive of the notion that insiders who are under closer scrutiny are more reluctant to file their reports late.

In summary, our results are consistent with the notion that the occurrence of late filings is not random. In particular, it appears that insiders who are more closely monitored (and who therefore may be facing higher litigation risk) are less likely to file their trades late.

4. Incidences of Stealth Trading and Trade Reporting

Thus far, we have documented that considerable reporting delays exist and that the reporting requirement is violated in more than 18% of cases. Delayed reporting per se may delay the adjustment of prices, but it does not necessarily benefit the insider. An insider who only wants to execute a single trade has no incentive (beyond convenience) to delay the filing. This incentive is different, however, when the insider intends to trade more

Table IV. Determinants of late filings

This table reports the results of a LPM with firm fixed effects. The dependent variable is a dummy variable which is set to 1 when the insider trade was filed (i.e., later than the 10th of the month following the trade or the next business day when the 10th falls on a weekend) late and is set to 0 else. Purchase (d) is a dummy variable that takes the value 1 if the (net) transaction volume of the respective insider trade is positive, and 0 otherwise. Tobin's Q is calculated as the ratio of the market value of assets to the book value of total assets. RoE is net income divided by book equity. We measure Book leverage as the ratio of long-term debt plus debt in current liabilities to long-term debt plus debt in current liabilities plus stockholders' equity. We define Numest as the total number of analysts covering a company in the month preceding the reporting date of the insider trade. We calculate TradeVolume as $ volume of the trade expressed as a percentage of the market capitalization of the firm. We define Number of insiders as the total number of insiders who traded their shares in the same firm on the same day. Pre-announcement (d) is a dummy variable that is set to 1 if a trade was not executed within a 30-calendar-day window after an earnings announcement, and 0 otherwise. We classified all insiders into five groups (five variables): CEO (d) if the trader was the CEO, Chairman (d) if the trader was the chairman but not the CEO, Executive (d) if the trader was an executive director but not the CEO, Director (d) if the trader was a non-executive director and the reference group Other, which includes all other insiders. Standard errors are clustered at the firm level. Asterisks (* and **) denote statistical significance at the 5% and 1% levels, respectively.

	Late filings		
	(1) Pooled	(2) Purchases	(3) Sales
Purchase (d)	0.008		
	(1.43)		
Tobin's Q	0.001	0.006**	−0.000
	(0.75)	(2.50)	(−0.34)
RoE	−0.011**	−0.012	−0.005
	(−1.97)	(−1.24)	(−0.72)
Book leverage	0.052***	0.052*	0.047**
	(2.92)	(1.72)	(2.00)
Numest	−0.000	−0.001	−0.001
	(−0.24)	(−0.34)	(−0.75)
TradeVolume	−1.559**	−1.426	−1.828**
	(−2.40)	(−1.30)	(−2.53)
Number of insiders	−0.003*	−0.003	−0.003**
	(−1.89)	(−1.15)	(−2.04)
Pre-announcement (d)	0.026***	0.029***	0.023***
	(8.37)	(4.45)	(6.80)
CEO (d)	−0.064***	−0.074***	−0.051***
	(−7.91)	(−4.57)	(−5.78)
Chairman (d)	−0.053***	−0.065***	−0.042***
	(−6.20)	(−3.98)	(−4.25)

(continued)

Table IV. (Continued)

	Late filings		
	(1) Pooled	(2) Purchases	(3) Sales
Executive (d)	−0.055***	−0.065***	−0.046***
	(−8.85)	(−4.51)	(−7.25)
Director (d)	−0.021***	−0.022*	−0.019***
	(−3.27)	(−1.74)	(−2.92)
Year dummies	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Obs	317,727	111,156	206,571
Adjusted R^2	0.2337	0.2836	0.2508

than once. In this case, delaying the reporting of earlier trades avoids the price reaction the report would trigger. Thus, later trades are executed at prices that are more favorable than the prices which would have prevailed if each trade had been reported immediately. Note that this is true irrespective of whether the insider trades on private information. It is sufficient that other market participants believe the insider to be informed with positive probability.

In this section, we look for evidence of stealth trading. We classify a trade as a non-stealth trade if it is (i) not preceded by another trade that has not been reported until the trading date; and (ii) is not followed by another trade before it is reported.[6] All other trades are classified as stealth trades because they are part of a series of trades in which some trades were executed while other trades were not yet reported. Figure 3 demonstrates two cases. Trades 1 and 2 in Panel A of Figure 3 are executed on different days but reported jointly. According to the definition above, both trades are classified as stealth trades. Because they are reported jointly, market participants can infer that the trades are stealth trades. Panel B of Figure 3 shows a different situation, in which trades 1 and 2 are executed on different days, as well as reported on different days. Because trade 1 is reported after trade 2 is executed, both trades are stealth trades according to our definition. However, on the date on which trade 1 is reported, market participants cannot infer that trade 1 is a stealth trade. Upon trade 2 being reported, however, it becomes apparent that both trades are stealth trades. When we

[6] We use an alternative definition as a robustness check. We consider only trades in the same direction (i.e., only purchases or only sales) and consider a series to be terminated when no further trades took place for at least 40 days (the maximum admissible reporting delay). This definition yields the same conclusions.

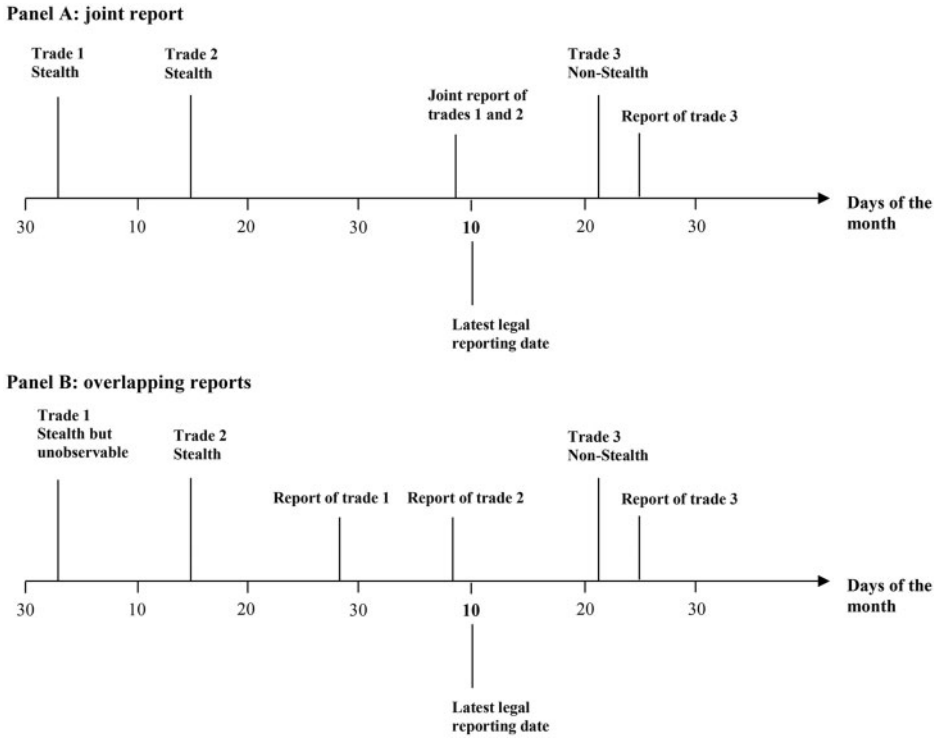

Figure 3. Definition of stealth trading. This figure illustrates our definition of stealth trading and trade reporting. Panel A illustrates the more common case in which two trades (labeled trade 1 and 2) are executed and then reported jointly. Trade 3 is a non-stealth trade because (i) there is no unreported trade by the same insider on the trading day; and (ii) trade 3 is reported before the insider makes another trade. Panel B illustrates the case of overlapping reports. Trades 1 and 2 are stealth trades because trade 1 has not yet been reported on the day on which trade 2 is executed. However, the trades are not reported jointly. Therefore, on the reporting day of trade 1, market participants cannot infer that trade 1 is a stealth trade.

analyze the market response to stealth trades in the next section, we adjust our definition of stealth trades accordingly. A trade is considered as a stealth trade only when market participants can infer that it was a stealth trade. Consequently, trade 1 in Panel B of Figure 3 is classified as a non-stealth trade when we analyze the abnormal returns after the filing of insider trades in Section 6. In the current section, however, we stick with our original definition because here we take the point of view of the insider.

We acknowledge that our classification is conservative. The group of stealth trades does not contain only trades that were deliberately reported late. As previously noted, it is likely that some corporate insiders routinely

report their trades on the 10th of the following month. If an insider adhering to this reporting practice trades several times in a month, our classification scheme will treat these trades as stealth trades.[7] There are two reasons why we stick to our classification. First, we cannot distinguish *why* we observe a specific pattern of trades and reports. Second, even if an insider does not intentionally delay the reporting of the earlier trades of a series, the delayed report still puts the counterparties to the later trades at a disadvantage, since they would have traded at more favorable prices had the insider reported all trades immediately.

The results of a descriptive analysis are reported in Table V. Only 36.0% of the trades in our sample are categorized as non-stealth trades. As one might expect, the percentage of non-stealth trades is lower in the subsample of trades that are filed late. Only 27.4% of these trades are classified as non-stealth trades. This percentage is larger for purchases than for sales (41.3% versus 33.1%, respectively). This finding is surprising at first, since purchases are known to have larger price impacts (which should increase the incentive to delay the reporting of a trade). Further, we documented earlier that average reporting delays are larger for purchases. A potential explanation for the result is the difference in trade size. Table V reveals that insider sales are, on average, much larger than insider purchases. The large sizes of sell orders provide an incentive to split-up trades and report individual trades only after all the trades of a sequence have been executed.

A total of 64.1% of trades in our sample are classified as stealth trades. Each stealth trade is part of a sequence of trades. The end of a sequence is reached when there are no more unreported trades. Table V reveals that 20.8% of the stealth trades are classified as the first trade of a sequence, while 79.2% are classified as second or subsequent trades of a sequence. These numbers imply that a sequence, on average, consists of 4.8 trades. This number is slightly higher for purchases than for sales (5.1 as compared to 4.7).

Table V documents that stealth trading and trade reporting is widely practiced. We therefore now analyze whether stealth trades are systematically different from non-stealth trades. To this end, we estimate LPMs with firm fixed effects in which the dependent variable indicates whether a trade is classified as a stealth or non-stealth trade. The independent variables are the

[7] Our results are also conservative in a second sense. We classify a trade as a stealth trade only when *the same* insider trades several times before reporting the trade. Besides such cases, there are a large number of cases in which insiders trade while the SEC filing of *another* insider is still pending. This sequence also puts the counterparties to the later insider trades at a disadvantage, because they would have traded at more favorable prices had the insider who traded first reported the trade immediately.

Table V. Descriptive statistics of stealth trades

This table presents descriptive statistics for the transactions in our sample sorted by classifying trades into non-stealth and stealth categories. A trade is classified as a stealth trade when it is followed by at least one additional trade by the same insider before it is reported, or if it follows a trade by the same insider that has not yet been reported, and as non-stealth trade otherwise. The stealth category is split into first of series and serial trades. A trade is classified as first of series if the trade is the first trade in a series of trades in which at least one trade is followed by at least one additional trade by the same insider before it is reported. A trade is classified as a serial trade if it follows a trade by the same insider that has not yet been reported. Percentages indicate fractions with respect to all transactions, all purchases, or all sales, respectively. Average volume denotes the average volume of the trade, that is, the number of shares bought or sold multiplied by the transaction price.

	All			Purchases			Sales		
Observations	332,171			118,789			213,382		
	#	Percentage (%)	Average volume $	#	Percentage (%)	Average volume $	#	Percentage (%)	Average volume $
Non-stealth	119,716	36.04	1,234,830	49,102	41.34	209,994	70,614	33.09	1,947,458
Stealth	212,455	63.96	900,657	69,687	58.66	241,345	142,768	66.91	1,222,476
First of series	44,224	20.82	1,242,406	12,564	19.46	229,124	20,660	21.48	1,690,682
Serial trades	168,231	79.18	810,820	56,123	80.54	244,299	112,108	78.52	1,094,428

trade, firm, and trader characteristics introduced in the previous section. We add a dummy variable that identifies trades that are filed late.[8] We estimate a pooled model as well as separate models for purchases and sales. Standard errors are clustered at the firm level.

The results are reported in Table VI. Purchases are less likely to be classified as a stealth trade than sales. This is consistent with the descriptive results presented above, and may be related to the fact that insider purchases on average are much smaller than insider sales. The likelihood of observing stealth trades is lower for firms followed by more analysts. This finding is intuitive because insiders in these firms are more closely monitored. We further find that the likelihood for stealth trades to be higher for firms with higher leverage.

[8] We obtain similar results when we replace the "late reporting" dummy by the reporting delay measured in days. We prefer the specification that includes the dummy because it is more robust in the presence of outliers (i.e., trades reported with extremely long delays). As an additional robustness check, we re-estimated all models after excluding all late filings from the data set. The results are similar to those presented in the text.

Table VI. Determinants of stealth trades

This table reports the results of a LPM with firm fixed effects. The dependent variable is a dummy variable which is set to 1 when the insider trade is classified as a stealth trade (i.e., it is followed by at least one additional trade by the same insider before it is reported, or it follows a trade by the same insider that had not yet been reported) and is set to 0 else. Purchase (d) is a dummy variable that takes the value 1 if the (net) transaction volume of the respective insider trade is positive, and 0 otherwise. Tobin's Q is calculated as the ratio of the market value of assets to the book value of total assets. RoE is net income divided by book equity. We measure Book leverage as the ratio of long-term debt plus debt in current liabilities to long-term debt plus debt in current liabilities plus stockholders' equity. We define Numest as the total number of analysts covering a company in the month preceding the reporting date of the insider trade. We calculate TradeVolume as $ volume of the trade expressed as a percentage of the market capitalization of the firm. We define Number of insiders as the total number of insiders who traded their shares in the same firm on the same day. Pre-announcement (d) is a dummy variable that is set to 1 if a trade was not executed within a 30-calendar-day window after an earnings announcement, and 0 otherwise. The dummy variable Late filing (d) refers to trades reported after the 10th of the month following the trade. If the 10th of the month falls on a weekend, the trade is classified as late filing if it is reported later than the following Monday. We classified all insiders into five groups (five variables): CEO (d) if the trader was the CEO, Chairman (d) if the trader was the chairman but not the CEO, Executive (d) if the trader was an executive director but not the CEO, Director (d) if the trader was a non-executive director and the reference group Other, which includes all other insiders. Standard errors are clustered at the firm level. Asterisks (* and **) denote statistical significance at the 5% and 1% levels, respectively.

	Stealth Trade		
	(1) Pooled	(2) Purchases	(3) Sales
Purchase (d)	−0.0136***		
	(−24.44)		
Tobin's Q	−0.001	−0.006**	−0.000
	(−0.97)	(−2.43)	(−0.02)
RoE	−0.003	−0.006	−0.004
	(−0.72)	(−1.07)	(−0.70)
Book leverage	0.043***	0.030	0.046**
	(2.72)	(1.25)	(2.19)
Numest	−0.002**	−0.003	−0.002**
	(−2.30)	(−0.87)	(−2.43)
TradeVolume	−18.022***	−12.094***	−19.636***
	(−23.08)	(−9.38)	(−21.68)
Number of insiders	−0.022***	−0.030***	−0.021***
	(−11.42)	(−7.93)	(−10.77)
Pre-announcement (d)	−0.004	−0.004	−0.001
	(−1.35)	(−0.87)	(−0.35)

(continued)

Table VI. (Continued)

	Stealth Trade		
	(1) Pooled	(2) Purchases	(3) Sales
Late filing (d)	0.056***	0.069***	0.046***
	(17.58)	(12.37)	(11.81)
CEO (d)	−0.123***	−0.0141***	−0.075***
	(−17.12)	(−11.18)	(−8.70)
Chairman (d)	−0.058***	−0.104***	−0.011
	(−6.30)	(−6.92)	(−1.02)
Executive (d)	−0.246***	−0.252***	−0.203***
	(−41.81)	(−22.25)	(−29.90)
Director (d)	−0.179***	−0.199***	−0.135***
	(−29.08)	(−18.67)	(−18.54)
Year dummies	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Obs	317,727	111,156	206,571
R^2	0.261	0.387	0.245

Turning to the trade-specific variables next, we find that larger trades are less likely to be classified as stealth trades. This finding is consistent with the conjecture that stealth trades are the result of large orders that have been split up into smaller chunks. A trade is also less likely to be classified as stealth when several insiders trade on the same day. We further find that trades that are filed late are more likely to be classified as stealth trades. Interestingly, the chairman of the board, the CEO, other executives, and non-executive directors are all less likely to engage in stealth trading than the members of the reference group. We re-estimated the LPMs including only trades made by the CEO. In this much smaller sample we still find that (i) purchases are less likely to be classified as stealth trades than sales; that (ii) larger trades and trades executed on days on which several insiders traded are less likely to be classified as stealth trades; and that (iii) trades that are filed late are more likely to be classified as stealth trades than trades which are filed on time.

Our results lend support to the hypothesis that insiders time their trades and make strategic use of pre-SOX reporting rules. The next section addresses whether market reactions to the reporting of insider trades take this into account.

5. Market Response to Stealth Trades

This section analyzes share price reactions after the reporting of insider trades using standard event study methodology. This analysis serves a

dual purpose. First, we want to test our conjecture that delayed reporting impedes the adjustment of prices to the information contained in the insider trades. To this end, we analyze whether reporting day CARs decrease with the length of the reporting delay and, if so, how quickly. Finding that CARs decrease quickly with the length of the delay would provide evidence that the market is able to learn the information contained in the insider trade from other sources and thus does not have to rely on the report. On the other hand, a finding that the CAR decreases slowly, or not at all, with the length of the delay would provide evidence that market prices are indeed distorted in the period between the trading and reporting dates.

The second purpose of the analysis is to test whether the CARs are larger after the reporting of stealth trades. The result will allow us to draw conclusions about the market's beliefs about insiders' trading motives. If the market reaction after stealth trades is stronger than after otherwise similar non-stealth trades, this would constitute evidence that the market attributes higher information content to stealth trades.

As noted above, we use standard event study methodology. The event date is defined to be the day on which an insider trade is filed with the SEC. The analysis is based on the reporting sample introduced in Section 2. This sample is obtained by aggregating all insider trades in shares of the same firm that were reported on the same day. We aggregate reports filed by different insiders because otherwise we would double-count observations. We estimate the market model over a 255-day estimation window ending 46 days prior to the announcement date.[9] We use the CRSP value-weighted index as our market proxy, and *t*-statistics are based on the standardized cross-sectional test proposed by Boehmer, Musumeci, and Poulsen (1991).

The event study results are reported in Table VII. We report CARs over four event windows, namely, (0; 1), (0; 2), (0; 10), and (0; 20), and we report separate results for insider purchases and insider sales. Consistent with previous research, we find that CARs over a short event window are small. The CARs over the 2-day window (0; 1) amount to 0.29% for purchases and –0.21% for sales. The CARs increase significantly when the length of the event window are increased. The CARs over the event window (0; 10) are 1.99% for purchases and –0.87% for sales; the corresponding values for the 21-day event window (0; 20) are 2.97% and –2.05%, respectively. These results confirm previous findings that the share price

[9] We choose a longer delay between the end of the estimation window and the event window because we do not want the estimation window to be contaminated by the execution of the insider trade. Note that 46 days is slightly more than the maximum admissible delay for reporting in the pre-SOX era.

Table VII. Event study results

This table shows the CARs over various event windows and various subsamples. Here, * and ** denote statistical significance at the 5% and 1% levels, respectively. The significance levels for the CARs are based on the standardized cross-sectional test of Boehmer, Musumeci, and Poulsen (1991), those for the abnormal returns are based on a Patell (1976) test, and those for the differences are based on a *t*-test for equality of means. Panel A shows CARs and Panel B shows abnormal returns for the pre-SOX era. Panel C shows CARs for the post-SOX era.

Panel A

	Purchases					Sales				
	#	(0; 1)	(0; 2)	(0; 10)	(0; 20)	#	(0; 1)	(0; 2)	(0; 10)	(0; 20)
All	34,648	0.29**	0.59**	1.99**	2.97**	65,319	−0.21**	−0.29**	−0.87**	−2.05**
Stealth	13,782	0.36**	0.76**	2.54**	3.75**	34,735	−0.25**	−0.38**	−1.05**	−2.55**
Non-stealth	20,866	0.25**	0.49**	1.64**	2.45**	30,584	−0.17**	−0.21**	−0.66**	−1.49**
Difference		0.11*	0.27**	0.90**	1.30**		−0.07	−0.17**	−0.39**	−1.06**
Pre-ann.	20,643	0.34**	0.64**	2.16**	3.18**	37,849	−0.19**	−0.24**	−0.91**	−1.99**
Non pre-ann.	14,005	0.22**	0.52**	1.76**	2.66**	27,470	−0.23**	−0.35**	−0.81**	−2.14**
Difference		0.12*	0.12*	0.40**	0.52**		0.04	0.11*	−0.10	0.14
Timed	6,472	0.26**	0.40**	1.28**	1.87**	8,793	0.05	0.03	−0.50**	−1.21**
Non-timed	28,176	0.30**	0.64**	2.16**	3.22**	56,526	−0.24**	−0.33**	−0.92**	−2.19**
Difference		−0.05	−0.23**	−0.88**	−1.34**		0.30**	0.36**	0.43**	0.98**
Delay 0–5	1,160	0.92**	1.32**	3.81**	4.52**	780	0.38**	0.35**	−0.01	−1.07**
Delay 6–10	3,602	0.31**	0.83**	2.31**	3.35**	4,216	−0.26**	−0.36**	−0.65**	−1.71**
Delay 11–5	5,402	0.21**	0.51**	2.07**	2.93**	9,068	−0.13**	−0.19**	−0.86**	−2.01**
Delay 16–20	4,833	0.40**	0.69**	2.02**	3.35**	9,760	−0.11**	−0.20**	−0.91**	−2.11**
Delay 21–25	5,082	0.39**	0.72**	2.23**	3.36**	11,356	−0.22**	−0.30**	−0.81**	−2.04**
Delay 26–30	4,781	0.28**	0.54**	1.57**	2.47**	11,135	−0.31**	−0.46**	−1.05**	−2.32**
Delay 31–35	3,943	0.08	0.38**	1.95**	3.12**	8,448	−0.24**	−0.36**	−0.99**	−2.29**
Delay 36–40	3,788	−0.03	0.22**	1.67**	2.52**	4,570	−0.32**	−0.44**	−0.80**	−2.45**
Delay 41–45	669	0.33**	0.75**	1.15**	1.99**	1,068	−0.23**	−0.15**	−0.69**	−1.52**
Delay > 45	3,202	0.38**	0.55**	1.49**	2.40**	4,918	−0.15**	−0.16**	−0.71**	−1.61**

Panel B

	Purchases			Sales		
Day	Return (%)	Patell Z	#	Return (%)	Patell Z	#
0	0.12	5.377***	40,147	−0.12	−8.377***	59,414
1	0.14	6.313***	40,147	−0.09	−8.146***	59,413
2	0.27	13.646***	40,145	−0.15	−8.984***	59,413
3	0.23	11.565***	40,142	−0.10	−4.913***	59,409
4	0.22	12.243***	40,139	−0.09	−5.025***	59,408
5	0.17	9.711***	40,138	−0.08	−4.407***	59,407
6	0.16	9.662***	40,134	−0.08	−5.799***	59,409
7	0.18	10.259***	40,133	−0.05	−4.045***	59,405
8	0.11	6.491***	40,133	−0.05	−3.355***	59,398
9	0.13	7.005***	40,129	−0.06	−5.450***	59,397

(continued)

Table VII. (Continued)

Panel B

	Purchases			Sales		
Day	Return (%)	Patell Z	#	Return (%)	Patell Z	#
10	0.09	5.086***	40,128	−0.07	−4.210***	59,397
11	0.13	6.004***	40,124	−0.07	−4.372***	59,393
12	0.09	4.858***	40,122	−0.09	−6.629***	59,390
13	0.12	6.293***	40,121	−0.12	−6.473***	59,387
14	0.09	6.317***	40,119	−0.19	−12.161***	59,384
15	0.09	4.028***	40,120	−0.13	−7.746***	59,383
16	0.09	6.561***	40,119	−0.18	−10.564***	59,378
17	0.05	3.956***	40,118	−0.10	−5.904***	59,371
18	0.10	3.431***	40,113	−0.10	−6.905***	59,370
19	0.07	3.308***	40,114	−0.10	−7.435***	59,367
20	0.11	3.699***	40,113	−0.07	−6.351***	59,365

Panel C

	Purchases					Sales				
	#	(0; 1)	(0; 2)	(0; 10)	(0; 20)	#	(0; 1)	(0; 2)	(0; 10)	(0; 20)
All										
Stealth	14,585	1.50	1.90	3.58	4.48	53,768	−0.23	−0.35	−1.03	−1.72
Non-stealth	49,602	1.07	1.32	2.34	3.11	160,070	−0.19	−0.29	−0.87	−1.51
Difference		0.44	0.58	1.24	1.37		−0.04	−0.06	−0.16	−0.21
t-value		7.45	8.26	9.83	19.53		−2.27	−2.79	−3.88	−3.48
Delay 0–2	34,962	1.26	1.54	2.69	3.36	141,417	−0.19	−0.29	−0.91	−1.60
Delay 3–4	11,252	1.00	1.34	2.46	3.42	37,761	−0.23	−0.34	−0.90	−1.52
Delay 5–10	3,047	0.90	1.13	2.10	2.65	6,689	−0.22	−0.35	−1.10	−1.60
Delay > 10	13,484	1.08	1.36	2.62	3.70	22,594	−0.22	−0.35	−0.89	−1.37

reaction is stronger after insider purchases than after insider sales. In Panel B of Table VII, we tabulate the individual daily average abnormal returns (AARs) for purchases and sales over the period (0; 20). The individual AARs are positive (negative) and significant in all cases for purchases (sales).

We test our conjecture that delayed reporting impedes the adjustment of prices to the information contained in the insider trades by analyzing whether reporting day CARs decrease with the length of the reporting delay. To this end we sort the insider trades in our sample into 10 groups with respect to their volume–weighted average reporting delays (delay 0–5 days, 5–10 days, and so on, with trades in the 10th group having a weighted average delay of more than 45 days). We find that the CARs are significantly

different from zero irrespective of the trading delay. They tend to slightly decrease with the length of the delay for purchases, but not for sales. Brochet (2010) reports a similar result. The finding that even reports that are filed with long delays result in significant abnormal returns implies that prices are distorted in the period between the trading and the reporting date of insider trades.

We next compare the CARs after stealth and non-stealth trades. As previously noted, we categorize a trade as a stealth trade only when, market participants can infer that the trade was a stealth trade on the filing date. The results provide clear evidence that market participants attribute higher information content to stealth trades. The share price reaction after these trades is stronger than that after non-stealth trades, irrespective of whether we consider purchases or sales, and irrespective of the length of the event window. Consider the CAR over the 20-day window (0; 20) as an example: It is 3.75% after stealth purchases but only 2.45% after non-stealth purchases. The corresponding figures for stealth and non-stealth sales are −2.55% and −1.49%, respectively. The difference between the price reaction after stealth and non-stealth trades is statistically significant in all cases (based on a *t*-test for equality of means).

Table VII also reports the results of further cross-tabulations. As already noted, many firms restrict insider trading by defining a blackout period during which trading is prohibited. Typically, the blackout period is the period just prior to an earnings announcement (often 2 months; see Bettis, Coles, and Lemmon, 2000; Roulstone, 2003). Such a restriction is based on the assumption that the informational asymmetry between corporate insiders and other market participants is larger prior to earnings announcements. If this assumption is true, we should observe larger CARs after trades that non-restricted insiders execute prior to earnings announcements. To test this hypothesis, we define the dummy variable *Pre-announcement* which is set to 1 if at least one of the trades reported on a given day was executed within a 60-day window prior to an earnings announcement. We find that purchases made during the pre-announcement period result in significantly larger share price reactions. This finding is consistent with the notion that earnings announcements reduce informational asymmetries. For insider sales, there are no significant differences between trades executed during the pre-announcement period and other trades.

We next consider the timing of trades relative to earnings announcement dates. We look at trades that were executed in the period before an earnings announcement but reported after the announcement. To this end, we define the dummy variable *Timed*, which is set to 1 if all trades reported on a given day were executed before and reported after the earnings announcement

date. We find that timed trades convey significantly less information to the market. Considering again the (0; 20) event window as an example, we find a CAR of 3.22% for non-timed purchases and a CAR of only 1.87% for timed purchases. The corresponding figures for sales are –2.19% and –1.21%, respectively. These results are consistent with the notion that earnings announcements reduce the informational asymmetry between insiders and the market.

The results in Table VII suggest that timed trades, that is, trades executed before but reported after an earnings announcement, and trades executed within a 30-day window after an earnings announcement trigger smaller share price reactions. The results also suggest that CAR decreases with the length of the reporting delay for insider purchases but not for sales. However, up to now, we did not control for other firm and trade characteristics. Including such controls is important because we showed previously that trades that are filed late are systematically different from trades that are filed on time. Similarly, we showed that stealth trades are different from non-stealth trades. In addition, reporting stealth trading typically involves reporting several trades on the same day,[10] and therefore, the total reported volume is larger. A potential concern is that the larger CAR may be caused by larger volume.

We therefore estimate cross-sectional regressions that control for the total reported volume and other potentially relevant variables. The dependent variable is the CAR. We report results for CARs measured over the event window (0; 20). Using the shorter event window (0; 10) yields results that are qualitatively similar.

The independent variables include measures of firm characteristics (Tobin's Q, RoE, book leverage, and number of analysts following) and trade characteristics (trading volume relative to the firm's market capitalization and aggregated over all trades that were reported jointly, number of different insiders trading on the same day, and weighted average reporting delay).[11] We further include dummy variables identifying stealth trades,

[10] The typical case is illustrated in Panel A of Figure 3. Several trades are executed on different days but reported jointly. The case illustrated in Panel B of Figure 3, where stealth trades are reported individually, is much less common.

[11] In additional regressions (results are contained in the Internet Appendix) we also include the market-adjusted share price performance in the 5, 10, 15, and 20 trading days (corresponding to 1, 2, 3, and 4 weeks) prior to the trade. In cases in which several trades were reported jointly we use the average of the pre-trade returns of the trades that were reported on the same day. We find that the coefficient on the pre-trade return is negative and significant at the 10% level or better in all cases. Thus, reports of momentum trades [contrarian trades] by insiders trigger weaker [stronger] share price reactions. All other results

trades executed in the period prior to an earnings announcement, and timed trades (i.e., those executed in the period prior to an earnings announcement and reported after the announcement, but prior to the next earnings announcement). We also include the interaction between the timed dummy and the stealth trading dummy. Three additional dummy variables control for the position of the insider in the firm (CEO, chairman of the board, and executive director). If several insiders report their trades on the same day, we choose the highest insider position; that is, we set the dummy to 1 if at least one of the insiders is the CEO, the chairman of the board, or an executive director, and 0 otherwise. We further include year and industry dummies, and firm fixed effects. Standard errors are clustered at the firm level. We estimate separate models for purchases and sales. Note that we expect different signs for the coefficients in the two regressions, because the CARs after purchases are predominantly positive while those after sales are predominantly negative.

The results are shown in Table VIII. We consider the results for insider purchases first. The CARs are smaller for firms with higher values of Tobin's Q and for firms with more analysts following. The other firm characteristics are insignificant. The share price reaction after a purchase does not depend on the transaction volume. It is larger when more than one insider reports trades on the same day. Consistent with our earlier results, we find that purchases executed during the period prior to an earnings announcement trigger significantly larger price reactions. Timed purchases—those that are executed before but reported after an earnings announcement—trigger significantly smaller share price reactions than other purchases. These results are consistent with the notion that earnings announcements convey information to the market and reduce informational asymmetries. Purchases by the CEO, the chairman of the board, executives and directors result in higher CARs than purchases by members of the base group (non-executive directors, affiliates, beneficial owners, and others). This result in general and the relative sizes of the coefficients in particular are consistent with the informational hierarchy hypothesis, which posits that trades by insiders with more privileged access to information convey more information to the market.

The most important results are those with respect to the stealth trading dummy and the reporting delay. Stealth purchases trigger a significantly larger share price reaction, even after controlling for the aggregate transaction volume, the number of insiders reporting trades on the same day, and other relevant variables. The additional abnormal return is 0.9%, which is

reported in Table VIII (in particular those with respect to the delay variable and the stealth trading dummy) are virtually unchanged.

Table VIII. Determinants of CARs (0; 20)

This table reports the results of a regression with firm fixed effects of the reporting day CARs (0; 20) on the explanatory variables listed in the first column. If several transactions in the same stock were reported on the same day, the transactions count as a single observation. A report is classified as a purchase if the net transaction volume reported is positive. Tobin's Q is calculated as the ratio of the market value of assets to the book value of total assets. RoE is net income divided by book equity. Leverage is the ratio of long-term debt plus debt in current liabilities to long-term debt plus debt in current liabilities plus stockholder equity. We define Numest as the total number of analysts covering the company in the month preceding the reporting date of the insider trade. We calculate TradeVolume as the number of shares exchanged in the transaction times the transaction price, divided by the market equity of the company whose stocks were bought or sold in the insider trade. If several trades were reported on the same day, we sum the total volume of these trades. We define Number of insiders as the total number of insiders who reported their trades in the same company on the same day. Pre-announcement (d) is a dummy variable that is set to 1 if the trade (or at least one trade, if several trades are reported on the same day) was not executed within a 30-calendar-day window after an earnings announcement, and 0 otherwise. Timed (d) is a dummy variable that takes the value 1 if the trade is executed within 60 days prior to the next earnings announcement and is reported after the announcement (but before the following announcement). Stealth (d) is set to 1 when (i) the trade is followed by at least one additional trade by the same insider before it is reported or if it follows a not-yet-reported trade by the same insider; and when (ii) the market can infer on the reporting date that the trade was a stealth trade (see Figure 3 for an illustration). All other trades are classified as non-stealth trades. With respect to stealth trades reported in an overlapping way, only serial transactions and not the first transaction can be identified as stealth trades. Timed* Stealth is an interaction term of the variables Timed (d) and Stealth (d). Delay is the trading-volume–weighted average delay of all insider trades of a firm reported on the same day. We classified all insiders into five groups (five variables): CEO (d) if the trader is the CEO, Chairman (d) if the trader is the chairman but not the CEO, Executive (d) if the trader is an executive director but not the CEO, Director (d) if the trader was a non-executive director and the reference group Other, which includes all other insider groups. If there were several trades in the same stock on the same day, the highest insider position is selected according to rank, that is, CEO, chairman, executive, director, and other. Standard errors are clustered at the firm level. Here, * and ** denote statistical significance at the 5% and 1% levels, respectively.

	CARs	
	(1) Purchases	(2) Sales
Tobin's Q	−2.143***	−1.628***
	(−11.82)	(−19.41)
RoE	−1.108***	−1.253***
	(−2.91)	(−3.03)

(continued)

Table VIII. (Continued)

	CARs	
	(1) Purchases	(2) Sales
Book leverage	1.918*	1.226
	(1.80)	(1.32)
Numest	−0.072	−0.001
	(−0.77)	(−0.02)
Volume	45.391	−35.528*
	(1.61)	(−1.87)
Number of insiders	0.375***	−0.482***
	(3.16)	(−6.82)
Pre-announcement (d)	1.405***	0.595***
	(6.37)	(3.75)
Timed (d)	−1.535***	−0.414
	(−4.45)	(−1.28)
Stealth (d)	0.898***	−0.342**
	(3.90)	(−2.11)
Timed*Stealth	−0.128	0.189
	(−0.29)	(0.49)
Delay	−0.003	0.008**
	(−0.72)	(2.30)
CEO (d)	1.243**	−0.645
	(2.45)	(−1.55)
Chairman (d)	1.080*	−0.374
	(1.78)	(−0.87)
Executive (d)	0.963**	−0.594**
	(2.41)	(−2.02)
Director (d)	0.051	−0.459
	(0.13)	(−1.45)
Obs	40,073	59,416
Year dummies	Yes	Yes
Industry dummies	Yes	Yes
Adjusted R^2	0.091	0.073

also economically significant. Note that, on the reporting day (our event day), market participants observe whether a report contains stealth trades. Our results thus imply that market participants believe that stealth purchases are more likely to be motivated by private information than otherwise similar non-stealth trades.

The coefficient on the reporting delay is insignificant, indicating that CARs do not decrease when a trade is reported with a longer delay. Thus, once we control for trade and firm characteristics, the negative relation reported in Table VII disappears. This result supports our conjecture that delayed reporting causes delays in the adjustment of prices.

The results for insider sales differ from those for purchases in several respects. Trades by insiders in more highly valued firms (larger Q) trigger stronger (more negative) price reactions. Sales prior to earnings announcements trigger smaller price reactions than other sales. Price reactions after insider sales filings are stronger when several insiders report trades on the same day. Trading volume, on the other hand, does not have a systematic impact. Trades by CEOs and other executive directors cause stronger price reactions.

The coefficient for the reporting delay is significantly positive, though small in magnitude. Thus, CARs following insider sales tend to decrease when a trade is reported with a longer delay. The decrease is very slow, however. The coefficient of 0.008 implies that increasing the reporting delay by 1 day decreases the reporting day CAR by 0.008%. It would thus take a reporting delay of 256 days until the average reporting day CAR of –2.05% is reduced to 0. Therefore, the conclusion that delayed reporting impedes the adjustment of prices is still valid.

Stealth sales apparently convey more information to the market than non-stealth sales, as indicated by the significantly negative coefficient on the stealth trade dummy. We note, though, that the absolute magnitude of the coefficient is smaller than that of the corresponding coefficient in the regression for insider purchases. This result, combined with the findings that CARs after insider sales are generally smaller and that the timing of the trade and of the report does not affect the magnitude of the price reaction, is consistent with the view that insider sales are generally less likely to be motivated by private information than insider purchases.

6. Robustness

In this section we describe several robustness checks that we implemented in order to assure the reliability of our main results. The results are shown in the Internet Appendix to this article.

6.1 ROUTINE REPORTING

As noted earlier, many insiders appear to routinely file their reports on the 10th of the month following their trades. Consequently, a large fraction of insiders report their trades on the same date (see Figure 2 for evidence). The stronger market response to stealth trading we identified could simply be the result of many insiders reporting trades on the same day. In the baseline regression above we address this issue by controlling for the aggregate

trading volume and for the number of insiders who report their trade on the same day.

It is conceivable that routine reports are less informative than reports filed on other days. Therefore, we re-estimate the regression with two additional dummy variables. The first dummy identifies reports filed on the 9th or the 10th of a month (or the next trading day in case the 10th is a non-trading day), the second interacts this dummy with the dummy identifying stealth trades. For purchases we find that, indeed, reports filed on the 9th or 10th of a month trigger smaller CARs. However, the coefficient on the interaction term is positive and insignificant. This indicates that there is no statistically significant difference between market reactions triggered by stealth trades reported "routinely" and those reported "non-routinely". Thus, the result that stealth trades trigger larger abnormal returns also holds for those reports filed on the 9th or 10th of a month. When we run the same regression for sales, none of the additional coefficient estimates is significant.

6.2 WEAK RULES VERSUS WEAK ENFORCEMENT

In the previous sections we have addressed two distinct phenomena. The first is the lax reporting requirements in the pre-SOX era which allowed insiders to delay their reports without violating the rules. The second is the apparent lack of enforcement which is likely to be responsible for the long reporting delays and the substantial fraction of late filings. The question thus arises whether our results are driven by those cases in which the rules were violated (i.e., the late filings as defined previously).

To address this issue, we re-estimate the regressions for insider purchases and sales including only trades that were reported in time. The coefficient of the "delay" variable in the sales regression is insignificant. This even strengthens our finding that delayed reporting impedes the adjustment of prices to the information contained in the insider trades. All other results are qualitatively unchanged. Additionally, we re-estimate the regressions with all observations but include variables that identify late filings. First, we include the dummy *Late filing* which is set to 1 if at least one of the trades which is jointly reported is filed late and 0 otherwise. Second, we compute the fraction of trades reported too late over the total number of trades reported jointly (*Percent late filing*). For both variables, we find that the market response to sales is more pronounced for trades which are filed too late. The variable Stealth continues to have a significant impact on the market reaction. Thus, our findings are not driven by the lax enforcement in the pre-SOX era.

As a further robustness check, we include a dummy variable that identifies trades made during the first 5 days of a month and reported between the 9th

and the 13th of the following month. The timing of execution and reporting of these trades is such that the reporting delay is maximized within the legal boundaries. The results are again similar to those reported in Section 5.

6.3 MULTIPLE TRADERS VERSUS MULTIPLE TRADES

One may object that the stealth trading effect is merely driven by the fact that several trades are reported jointly on a given event date. Our regression analysis reported in Table VIII controls for both the number of insiders that report trades on the event date and the aggregate trading volume reported. However, these variables do not capture the relative size of the trades of different insiders, that is, whether there were large stakes traded as a block or multiple smaller trades. We therefore repeat the analysis and restrict our sample to cases where only a single insider reports trades on a given event date. We control for the number of trades which the insider reports as well as for trade size. The results are similar to those presented in Section 5 and confirm that the stealth trading effect on the price impact cannot merely be attributed to the number of trades reported.

6.4 CONFOUNDING EVENTS

Certain firm-specific events may have an impact on the trading behavior of insiders, and on the share price reaction to the announcement of insider trades. In our main analysis we have not excluded these cases. In order to make sure that these events do not drive our results we identify all cases in which one of the following events took place:

- Proxy filings and poison pills: We look at all available announcement dates of proxy fights and poison pills from the SDC database.
- Mergers and acquisitions: Based on SDC data, we look at all announcement dates of M&A transactions with a minimum deal value of USD 50 million. We identify all cases in which a sample firm was either the bidder or the target in a transaction.
- Divestiture: We exclude an observation if there was a divestiture in the year prior to the beginning of the estimation period. Divestitures are identified based on Compustat data item sale of property 107, SPPE.
- Major capital expenditures: We define a firm as having major capital expenditure if the year-to-year change in capital expenditure is higher than the 90% (75%) quantile of the capital expenditure change distribution of the previous year.

We exclude all trades that were executed in the period of 14 months prior to until 2 months after such an event. Applying this filter leaves us with 26,373 observations. Our main findings remain qualitatively unchanged.

6.5 FINANCIAL INSTITUTIONS AND FINANCIALLY DISTRESSED FIRMS

Financial institutions are often excluded from empirical analyses because they are deemed different from other firms. We therefore repeat our analysis after exclusion of financial institutions (defined as firms with a SIC code between 6000 and 6999). Our main findings are confirmed for the reduced sample.

By a similar argument, we exclude financially distressed firms from the sample. We identify a firm as financially distressed if it announced bankruptcy (where we use the UCLA-LoPucki database[12] to identify bankruptcies). We exclude observations from our sample if they occur within 1 year prior to financial distress or within 2 years after financial distress. Our main results are robust for this subsample.

6.6 STEALTH TRADING AFTER THE IMPLEMENTATION OF SOX

As discussed in Section 2 SOX resulted in a significant tightening of the regulation. Most importantly, corporate insiders now have to report their trades within two business days. In addition, this change likely strengthened enforcement. In sum, the opportunity for stealth trading seems to be substantially reduced after the implementation of SOX. In this section we use a post-SOX sample (spanning the period from January 2003 to December 2010) to analyze whether the implementation of SOX has changed the insider trade and trade reporting behavior or the effect of insider trading on prices.

Unsurprisingly, the reporting delays are much lower in the post-SOX era. 70.9% of all trades are reported within two calendar days, 91.3% are reported within four calendar days. Given these very short reporting delays, there is obviously much less opportunity for stealth trading than in the pre-SOX era. Consequently, we find that the percentage of non-stealth trades increases from 36.0% pre-SOX to 67.4% post-SOX.

We repeat our event study for the post-SOX sample. The results are shown in the Internet Appendix. Comparing them to the results for the pre-SOX era reveals that the abnormal returns after insider purchases, but not after insider sales, are larger post-SOX. This result is consistent with the findings

[12] See http://lopucki.law.ucla.edu/.

of Brochet (2010). In the post-SOX era, as in the pre-SOX era, stealth trades trigger larger share price reactions than non-stealth trades. This result is more pronounced for insider purchases than for insider sales, but is significant in both cases. Finally, abnormal returns after insider trades appear to be largely independent of the reporting delay. Thus, the conclusion drawn from the pre-SOX sample that prices are distorted in the period between the execution and the reporting of insider trades also holds in the post-SOX era.

We also estimate regressions similar to those documented in Table VIII above. Stealth purchases still trigger a significantly larger price reaction than non-stealth purchases. The magnitude of the effect is smaller than in the pre-SOX era (the coefficient on the stealth trade dummy is 0.51 post-SOX as compared to 0.91 pre-SOX). Stealth sales, on the other hand do not cause significantly larger price reactions than non-stealth sales. The coefficient on the stealth trade dummy is negative as expected, but insignificant. The coefficient for the reporting delay is significantly negative (though small in magnitude relative to the average CAR) for insider purchases and is insignificant for insider sales. Therefore, the conclusion that delayed reporting impedes the adjustment of prices is still valid.

In summary we conclude that our main results still hold in the post-SOX era, but they are less pronounced because of the significantly lower reporting delays in the post-SOX era.

6.7 ANTICIPATION OF SOX

SOX did not come as a surprise. In the wake of the Enron and WorldCom scandals new and more stringent regulation was publicly discussed. This raises the question whether corporate insiders, in anticipation of stricter regulation and enforcement, changed their trading behavior. To address this issue we define the dummy variable *Anticipation* which is set to 1 after the Enron scandal was publicly disclosed on October 22, 2001. We include this dummy variable as an additional regressor in the LPMs presented in Table IV (late filing) and Table VI (stealth trading). We do not find any evidence that the probability of late filings or the probability of stealth trading were lower during the anticipation period. This suggests that corporate insiders did not change their trading behavior in anticipation of SOX.

6.8 OTHER ROBUSTNESS CHECKS

One potential problem with our delay variable lies in the fact that there are obvious outliers in the sample. This is evidenced by a maximum reporting

delay in excess of 10 years. We address this issue by estimating three alternative versions of the model. We use (i) a delay variable that is winsorized at 42 (the maximum delay allowed in the pre-SOX era); and (ii) the log of 1 plus the delay. Both specifications reduce the impact of outliers on the results. They yield results similar to those reported earlier. We thus conclude that outliers are not a major cause for concern.

To avoid multicollinearity, we do not include firm size in our baseline regression (the correlation between firm size and number of analysts following is 0.80 in our reporting sample). When we replace the number of analysts with the natural logarithm of total assets we obtain negative and significant coefficient estimates. Otherwise we obtain very similar results. We also estimate versions of our models that include additional firm characteristics (a measure of asset tangibility as defined in Almeida and Campello (2007) and the standard deviations of returns in the 60 days prior to the event date). Tangibility turned out to be insignificant, return volatility is positive and significant for purchases but not for sales.

In those cases in which several different insiders traded shares of the same firm it may make a difference whether all insiders traded in the same direction or whether some of them traded in the opposite direction. Therefore, we re-estimate our model including a continuous variable measuring aggregate trade direction. It is defined as (number of buys – number of sells) / (number of buys + number of sells). The coefficient of this variable has the expected positive sign and is significant. The other results are similar to those presented in Table VIII.

7. Summary and Conclusion

In the pre-SOX era, corporate insiders in the USA were required to report their trades by the 10th of the month following the trade. Thus, the maximum time allowed between the trade and the report was 40 days, giving corporate insiders considerable flexibility to time their trades and reports. This flexibility may result in stealth trading by corporate insiders. An insider wishing to trade a large quantity may split up an order into several smaller chunks. Splitting up a large order reduces its price impact and thus results in reduced execution costs. By delaying the reporting of the trades of a series until after the last transaction, an insider can avoid the price impact caused by the reports.

This article asks three related questions. First, how long are the reporting delays in the pre-SOX era? Second, do insiders engage in stealth trading and thereby use their flexibility in choosing the timing of their trades and reports?

If so, is stealth trading systematically related to the characteristics of the insider or the firm? Third, what are the implications of delayed reporting on the informativeness of prices and how does the market react to stealth trading and trade reporting?

Our results demonstrate that substantial reporting delays exist. The mean reporting delay was 36.6 days. More than 18% of the trades in our sample were filed late (i.e., later than on the 10th of the month following the trade). The very large number of violations of the trade reporting requirement implies that the requirement was not enforced in the pre-SOX era. Corporate insiders apparently used the available discretion to time their reports. More than two-thirds of the trades in our sample are part of a sequence of trades in which some trades were executed while earlier trades were not yet reported. Stealth trading benefits the insider but is disadvantageous to the counterparties to the insider's trades. If each trade were reported immediately, the second and subsequent trades of a series of insider trades would be executed at prices less favorable to the insider but more favorable to the counterparties.

We find that both the occurrence of late filings and the occurrence of stealth trades are systematically related to the characteristics of the firm, the trade, and the trader. In particular, our results are consistent with the notion that insiders who are more closely monitored (and who therefore may be facing higher litigation risk) are less likely to file their trades late. The probability of observing a stealth trade is larger in firms followed by fewer analysts as well as for larger trades.

Our event study results reveal that share prices react to the reporting of insider trades. In cross-sectional regressions, we find that the magnitude of the price reaction does not decrease with the reporting delay after purchases, and decreases very slowly after sales. Thus, our results support the notion that market prices are distorted in the period between the trade and the report. Consequently, delayed reporting of insider trades impedes the adjustment of prices. Finally, event study CARs are larger after reports of stealth trades compared to the aftermath of otherwise similar non-stealth trades for both purchases and sales. Thus, market participants apparently believe that insiders engaging in stealth trading are more likely to possess private information.

Our results support the more stringent trade reporting requirements established by SOX. They also suggest that strict enforcement of existing regulations is beneficial. Further, our results lead to the conclusion that countries that currently allow for long reporting delays (or do not require corporate insiders to report trades in the shares of their firm) should consider tightening their regulations.

References

Almeida, H. and Campello, M. (2007) Financial constraints, asset tangibility, and corporate investment, *Review of Financial Studies* **30**, 1429–1460.
Bettis, J. C., Coles, J. L., and Lemmon, M. L. (2000) Corporate policies restricting trading by insiders, *Journal of Financial Economics* **57**, 191–220.
Betzer, A. and Theissen, E. (2009) Insider trading and corporate governance – the case of Germany, *European Financial Management* **15**, 402–429.
Betzer, A. and Theissen, E. (2010) Sooner or later – delays in trade reporting by corporate insiders, *Journal of Business Finance and Accounting* **37**, 130–147.
Boehmer, E., Musumeci, J., and Poulsen, A. B. (1991) Event study methodology under conditions of event-induced variance, *Journal of Financial Economics* **30**, 253–272.
Brochet, F. (2010) Information content of insider trades before and after the Sarbanes-Oxley Act, *Accounting Review* **85**, 419–446.
Carter, M. L., Sattar, A. M., and Reeb, D. M. (2003) Evidence on quasi-private information and insider trading, *Financial Analysts Journal* **59**, 60–67.
Chang, S. and Suk, D. Y. (1998) Stock prices and the secondary dissemination of information: the Wall Street Journal's 'Insider Trading Spotlight' column, *Financial Review* **33**, 115–128.
Cheng, S., Nagar, V., and Rajan, M. V. (2007) Insider trades and private information: the case of delayed-disclosure trades, *Review of Financial Studies* **20**, 1833–1864.
Chordia, T. and Subrahmanyam, A. (2004) Order imbalance and individual stock returns: theory and evidence, *Journal of Financial Economics* **72**, 485–518.
Cox, J., Thomas, R., and Kiku, D. (2003) SEC enforcement heuristics: an empirical inquiry, *Duke Law Journal* **53**, 737–779.
Fidrmuc, J. P., Goergen, M., and Renneboog, L. (2006) Insider trading, news releases and ownership concentration, *Journal of Finance* **61**, 2931–2973.
Fidrmuc, J. P., Korczak, A., and Korczak, P. (2013) Why does shareholder protection matter for abnormal returns after reported insider purchases and sales, *Journal of Banking and Finance* **37**, 1915–1935.
Friederich, S., Gregory, S. A., Matatko, J., and Tonks, I. (2002) Short-run returns around the trades of corporate insiders on the London stock exchange, *European Financial Management* **8**, 7–30.
Gompers, P., Ishii, J., and Metrick, A. (2003) Corporate governance and equity prices, *Quarterly Journal of Economics* **118**, 107–155.
Huddart, S., Hughes, J. S., and Levine, C. B. (2001) Public disclosure and dissimulation of insider trades, *Econometrica* **69**, 665–681.
Jagolinzer, A. (2009) Sec Rule 10b5-1 and Insiders' Strategic Trade, *Management Science* **55**, 224–239.
Kyle, A. S. (1985) Continuous auctions and insider trading, *Econometrica* **53**, 1315–1335.
Lakonishok, J. and Lee, I. (2001) Are insider trades informative?, *Review of Financial Studies* **14**, 79–111.
Lebedeva, O., Maug, E., and Schneider, C. (2013) Trading strategies of corporate insiders. Unpublished working paper, University of Mannheim.
Malmendier, U. and Tate, G. (2007) Who makes acquisitions? CEO overconfidence and the market's reaction, *Journal of Financial Economics* **89**, 20–43.
Marin, J. M. and Olivier, J. P. (2008) The dog that did not bark: insider trading and crashes, *Journal of Finance* **63**, 2429–2476.

Patell, J. (1976) Corporate forecasts of earnings per share and stock price behavior: empirical tests, *Journal of Accounting Research* **14**, 246–276.
Roulstone, D. T. (2003) The relation between insider trading restrictions and executive compensation, *Journal of Accounting Research* **41**, 525–551.
Seyhun, H. N. (1986) Insiders' profits, costs of trading, and market efficiency, *Journal of Financial Economics* **16**, 189–212.