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How much does an Illegal Insider Trade?*

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ABSTRACT

This paper examines the choice of trade size by an illegal insider. Previous literature (i.e. Meulbroek 1992) tends to focus on the price impact of such a trader. Using a unique data set hand-collected from the litigation reports of the Securities and Exchange Commission and court cases, we provide evidence, which suggests that the size of an illegal insider's trade is a function of the value of his private information, the probability of detection and the expected penalty if detected. Our results have important implication for security market regulators.

I. INTRODUCTION

Insider trading remains an important concern of regulators despite almost two decades of innovation in enforcement since the prominent scandals of the 1980s.[1] For example, in the 2008 fiscal year, the Securities and Exchange Commission (SEC) initiated the largest number of insider trading cases in the Commission's history.[2] Indeed, a recent action brought against employees of several well-known Wall Street firms and hedge funds[3] reveals that the scale of insider trading schemes remains as large as they were in the 1980s.

While there is considerable academic interest in insider trading, only a handful of studies provide an empirical examination of the issue.[4] Meulbroek

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1 Seyhun (1992) documents the changes in enforcement policy over time. See *re Ivan F. Boesky Sec. Litig.*, 957 F.2d 65, 69 (2d Cir. 1992), which is one of the most famous insider trading cases prosecuted.
2 See Security and Exchange Commission Annual Report 2008.
3 See Security and Exchange Commission Litigation Release No. 20367, November 20, 2007.
4 Academic interest toward insider trading primarily addresses the debate as to whether insider trading should be made illegal or not. See for example, Manne (1966), Carlton and Fischel (1983), Cox (1986), Demsetz (1986), Manove (1989), Ausubel (1990), Leland (1992), Georgakopoulos (1993) and Manne (2005).

(1992) uses data sourced from SEC case files and finds that trading by illegal insiders is correlated with abnormal price movement and volume. Cornell and Sirri (1992) examine illegal insider trading before the 1982 acquisition of Campbell Taggart by Anheuser-Busch and conclude that insider trading leads to more informative prices, but does not impair liquidity. Chakravarty and McConnell (1997) reach similar conclusions but use data from Ivan Boesky's trades in Carnation prior to the company's takeover by Nestle. However, Chakravarty and McConnell (1999), using the same data set, find that Ivan Boesky's trades did not move prices any more than normal buyer-initiated trades, casting doubt over the proposition that insider trading, *per se*, moves prices. Finally, Fishe and Robe (2004) examine a sample of 30 stocks featured in an influential *Business Week* column and find that illegal insider trading is associated with wider spreads and lower depth in specialist markets. The empirical literature to date has focused on the effect of illegal insider trading on market behavior.[5] In this paper, we extend this literature by examining the behavior of insiders. Specifically, we identify and test empirically the factors that affect the 'intensity' of an illegal insider's crime (i.e. the volume that they trade) using data collected from publicly available SEC litigation reports.

The behavior of illegal insiders is important to regulators. An understanding of the factors that influence the volume traded by insiders can assist in building better detection mechanisms. To this end, one contribution of our paper is to show that insiders attempt to 'hide in the crowd'. This questions the practice of examining abnormal price and volume movements to detect illegal trading behavior. Furthermore, our research question is important from a policy perspective. DeMarzo et al. (1998) develop a model to determine the optimum regulation of illegal insider trading. They show that it is optimum to investigate information events for which preannouncement volume exceeds some threshold, which is dependent on the value of the non-public information. Upon detection, an insider is then levied the maximum penalty. In developing their model, the authors *assume* that insiders set volume to maximize their expected profit, where profit (and hence volume) is a function of the expected return from the illegal behavior, the probability of detection and the expected penalty (DeMarzo et al. 1998, p. 611). Our paper explicitly tests the validity of such models with empirical data.

The results indicate that the volume traded by insiders is directly proportional to the value of their information. The position taken by an insider is also negatively related to the magnitude of the penalty. Both of these findings suggest that individuals trade off the costs and benefits of crime. Our empirical findings also indicate that the volume traded by insiders is influenced by several variables related to the probability of detection. Specifically, the volume traded by an insider is smaller if the trade is made in a specialist market. Trading closer

5 Rather than using actual insider trading data, a related strand of empirical literature uses the date of initiation and first enforcement of insider trading laws to make inferences regarding the efficacy of insider trading laws (Battacharya and Daouk 2002; Beny 2005, 2008; Bris 2005; Fernandes and Ferreira 2009). Our research is also distinct from these studies.

to the announcement day is associated with a reduction in volume traded. Finally, the results imply that insiders behave as investors would by taking smaller positions in riskier stocks. Previous literature suggests that price volatility is a measure of liquidity, and hence this result suggests that insiders will trade less aggressively in a market that is less liquid. As shown in the robustness tests, these findings are robust to sample selection biases.

The remainder of this paper is organized as follows. Section 2 develops a number of testable hypotheses, which predict the determinants of trade size chosen by illegal insiders. Section 3 describes the data and sample selection procedure, and Section 4 reports the empirical results. The results of a number of additional tests are reported in Section 5, and Section 6 concludes.

II. HYPOTHESIS DEVELOPMENT

A rational insider must weigh the benefits of trading illegally with his private information against the possibility that he gets caught and receives a penalty (Seyhun 1992). Harris (2003, p. 588) explains that to detect insider trading cases, exchange 'surveillance officers look for suspicious events – typically, large price changes on large volumes'. Therefore, on one hand an insider has the incentive to trade maximally in order to profit the most from his information, on the other hand he needs to reduce the trade size and 'hide in the crowd' to avoid detection. Based on this trading dilemma faced by insiders, the following hypotheses are proposed.

Hypothesis 1: The volume transacted by an insider is positively related to the value of his insider information.
Hypothesis 2: The volume transacted by an insider is negatively related to the expected penalty he will receive if caught.
Hypothesis 3: The volume transacted by an insider is negatively related to the variability of asset returns.
Hypothesis 4: The volume transacted by an insider is negatively related to the probability of detection.

Hypothesis 1 and 2 state that the greater the value of the information obtained by the insiders the more they will trade (greed), and the greater the penalty the less they will trade (fear). Previous literature demonstrates that the insiders tend to trade less aggressively in less liquid markets (Kyle 1985), and return variability is also negatively related to market liquidity (Stoll 1978; O'Hara and Oldfield 1986; Grossman and Miller 1988; Amihud 2002). Therefore, a positive relation between insider trading volume and return volatility is expected in Hypothesis 3.

Hypothesis 1–3 can be directly tested using variables observable from the market and insider trading data. However, Hypothesis 4 corresponds to the probability of insider's trade being detected, which is not directly observable. In our empirical analysis in Section 3, we consider the following variables that can act as proxies for the probability of detection.

Several empirical studies examine whether specialist market makers are better at detecting informed trading than dealers in a multiple market maker environment. In an analysis of repeated illegal insider trading in 116 stocks, Fishe and Robe (2004) document that specialists in exchange-listed securities reduce depth and increase bid–ask spreads after illegal insiders begin trading. In contrast, for NASDAQ-listed securities quoted depth decreases, but less so than for New York Stock Exchange (NYSE)-listed stocks, with no significant reduction in bid–ask spreads. Analogously, Garfinkel and Nimalendran (2003) document that effective bid–ask spreads for NYSE-listed stocks, are larger *vis-à-vis* NASDAQ-listed stocks on days when registered insiders (i.e. legal) trade. Rather than analyze particular incidences of informed trading, Heidel and Huang (2002) examine how the probability of informed trading in general differs as stocks transfer from dealer to specialist markets, and *vice versa*. Their results imply that a move to a multiple-dealer environment coincides with a higher likelihood of informed trading overall, suggesting that this market structure increases 'the odds of losing to an informed trade'. Theoretical work also predicts that specialists might be better able to avoid informed trading.[6] The results of these studies imply that illegal insiders wishing to conceal their presence will trade less aggressively in a specialist market such as the NYSE.

The probability of being detected may be a function of the time between the trade by the insider and the date of the announcement. If insiders believe that trading immediately before an information announcement is more likely to be detected, then a rational insider will trade as far away from the information event as possible.[7] Park et al. (1995) provide a simple model of how insiders time their trades with respect to forthcoming earnings announcements. Their model predicts that insider trading decreases substantially as the earnings day approaches. Their analysis of reported (i.e. legal) insider trades supports the predictions of their hypothesis. Ke et al. (2003) provide corroborating evidence for this proposition. They examine reported insider trading activity before earnings announcements in which reported earnings decrease compared to the corresponding quarter of the previous year. Such events, which the authors term a 'break', are shown to involve significant abnormal negative price reaction on announcement days. The authors demonstrate that there is a significant reduction in insider selling activity in the two quarters prior to a 'break'. This is consistent with detection minimization behavior on the part of insiders and similarly to Park et al. (1995) suggests that the probability of detection is lower the further away an insider trades from an information event. Both papers

6 For example, Benveniste et al. (1992) model the repeated interactions between brokers and specialists on the floor of the exchange. Their model predicts that floor brokers have an incentive to signal to specialists when they suspect their client is informed, in order to avoid subsequent sanctions from the specialist and to reduce overall costs for their clients.

7 An alternative explanation, unrelated to the probability of sanction but with the same expected outcome, is that insiders will trade larger amounts as early as possible to maximize expected profit before the information is impounded into the price by other informed traders.

examine SEC lodged trades by insiders prior to earnings announcements. To the extent that this behavior extends to covert trades committed before earnings and merger announcements, we conjecture that trading further away from the announcement decreases the probability of detection.

The degrees of separation between the trader and any individual who has access to non-public information might determine the volume they trade. To successfully prosecute an individual for illegal insider trading, the regulator must establish possession of material non-public information, a process that is often difficult or even 'impossible' (Harris 2003, p. 588). If it is easier to establish possession of information for employees of the company in question than those that are separated from that information, then direct insiders will trade less, if at all, because their probability of sanction is higher.

Finally, if the volume traded by the insider is significant relative to the usual volume traded in the security then the regulator may be more likely to investigate those particular trades (Harris 2003, p. 588). If this is the case, then the insider will trade fewer shares in less liquid securities.

The testable implications of the hypotheses developed in this section are that the volume traded by an insider is a function of the following variables: (i) the value of the information possessed by the insider; (ii) the expected penalty if the insider is detected and sanctioned; (iii) the price volatility of the underlying asset traded by the insider; (iv) the market structure of the exchange on which the insider trades (specialist versus competing dealer market); (v) the amount of time before the information release date that the insider trades; (vi) whether the insider has a direct relationship with the company (e.g. employee or contractor); and (vii) the volume traded by the insider relative to normal volume. In the next section, we describe how each of these variables is measured with the sample data.

III. DATA AND SAMPLE SELECTION

Insider trading data are hand-collected from litigation reports made available on the SEC website (http://www.sec.gov/litigation/litreleases.shtml), which report the investigation and action taken against a defendant. The litigation reports are ordered according to their release date rather than the actual date of the defendants' crime. There can be considerable difference between these two dates. The delay between the commission of the crime and subsequent regulatory action reflects the time needed to detect the transgression, establish the case and finally initiate civil proceedings. It is at this stage that the website would first make mention of the defendant and alleged transgression. More time elapses as the matter is heard in a relevant court and penalty is imposed if the defendant is deemed liable. Each of these additional determinations may result in a separate litigation report. All cases are carefully examined to determine the eventual verdict and penalty, and cases for which the defendant is deemed not liable are excluded from the sample. The final sample includes litigation reports on all illegal insider transactions that were made between

January 1, 1996 and December 31, 2004.[8] Since common stock trades have by far the greatest representation in the litigation reports available on the SEC website, this security type is chosen for analysis.[9] We focus exclusively on insider trades before merger and earnings announcements since these are one of the most common yet significant types of information events in financial markets.[10]

The litigation reports are non-standardized documents and provide varying levels of detail about the insiders' trades. The name of the defendant, the volume traded, the price of the transaction, the date of trade, the date the insider's information became public, the security being traded and the penalty leveled against the insider are collected from the litigation reports if available. Additional information is sourced from *Lexis Nexis* data base, which provides case judgments.

Using the information collected from litigation reports and *Lexis Nexis* data base, we identify the aggregate volume traded by an individual before a given news announcement in a given security. Insider volume is aggregated because the focus of this study is the choice of trading volume by an insider before a given announcement and not how the insider chooses to break up orders across time before the announcement.

While it is possible for individuals to trade in more than one listed company or for several different individuals to trade shares in the same company, each observation in our sample represents a unique insider–company combination, i.e. what Meulbroek (1992) refers to as an insider trading 'episode'.[11] For each insider trade, the closing and opening prices surrounding the first trading day and the date of the news announcement are identified. Total daily traded volume in the security in the 30 days preceding the first trade is calculated. All price and volume data are sourced from Bloomberg. Finally, we consult the annual report corresponding to the financial year of the insider's trade to

8 The start of our sample period corresponds to the first full year in which the SEC began publishing litigation reports on its website, and the sample period ends in 2004 because beyond this date a significant amount of cases are not resolved before December 31, 2007, when we started collecting our sample. The litigation reports presented on the SEC website concern all types of security fraud, but in this study we only focus on the insider trading cases.

9 Cases involving solely options or derivatives trading were not examined. Cases where the insider(s) traded both options and common stock are included but only the common stock trades are analyzed.

10 A total of 55% of insider trade cases are associated with merger and earnings announcements, which are the two most common information types in our sample. Furthermore, as recommended by Chakravarty and McConnell (1999), part of our methodology involves matching insider trades to non-insider trades to determine the extent of the difference in behavior between insiders and non-insiders. One aspect of the matching criteria involves matching on information type and often it is not possible to match trades associated with (relatively) rare or idiosyncratic information events.

11 In one case an individual traded the same security before two different news announcements pertaining to the same company. This was defined as two separate trades and so technically each observation in our sample represents a unique insider–company–news announcement combination.

Table 1 Total population of illegal transactions from January 1996 to December 2004

Year	All illegal transactions in population	Illegal transactions included in sample	Percentage (%)
1996	33	23	69.70
1997	53	32	60.38
1998	57	31	54.39
1999	65	47	72.31
2000	98	67	68.37
2001	38	25	65.79
2002	28	19	67.86
2003	47	40	85.11
2004	22	12	54.55
Total	441	296	67.12

This table reports the total population of illegal trades in the litigation reports filed between January 1997 and December 2007. It consists of all prosecuted illegal trades in common stock before merger and earnings announcements. The year corresponds to the year in which the insider trade occurred and not the year of prosecution. This table also reports the number of trades remaining in the sample once filters have been applied to exclude observations that contain missing information on trading volume, penalty imposed or transaction date in the litigation reports.

determine which exchange the stock was listed on at the time of the illegal trading activity.

The litigation reports are not standardized and therefore, many observations are lost due to incomplete case data (e.g. the volume traded by an insider or the penalty imposed is not reported). Table 1 describes the proportion of observations that are included in our sample compared to the population of all trades over the sample period. The sample consists of 296 insider trades out of a possible 441 (earnings and merger announcements for publicly listed companies only), representing 67% of the population. As a point of comparison, Meulbroek (1992) who uses publicly available litigation reports and confidential case files to build her sample, is only able to analyze data that capture 69% of defendants charged with insider trading between the years 1980 and 1989.[12] While the unit of observation in her study – 'defendants' – is not the same as the unit analyzed in this study, it nevertheless highlights the significant data loss that occurs when attempting to extract information from non-standardized litigation reports.

Table 2 depicts the sample selection criteria. There are a total of 441 illegal insider trades executed over the sample period. As described earlier, a number of trades are excluded from the final sample because trade volume, penalty or dates of trades are not reported in the data sources, which are necessary for the construction of the dependent and independent variables in our empirical tests.

12 Our sample selection criteria are similar to Meulbroek's (1992) except that her analysis does not require data on the penalty leveled against the insider.

Table 2 Sample selection criteria

Filter	Observations lost	Observations available
All insider trades	0	441
Not reported:		
Volume traded by insider	69	372
No data on penalty	67	305
No data on date of trade	9	296
Total available trades		296
Percentage of total		67.12%

This table shows how the final sample of 296 illegal insider transactions are determined from an initial pool of 441 available observations. All data is sourced from SEC litigation reports available from the SEC website: http://www.sec.gov/litigation/litreleases.shtml.

Table 3 Distribution of sample across years

Year	Trades	Defendants	Stocks	Cases		
				Merger	Earnings	Total
1996	23	23	13	15	2	17
1997	32	20	22	14	1	15
1998	31	29	17	13	6	19
1999	47	42	26	23	3	26
2000	67	59	31	24	5	29
2001	25	24	13	10	4	14
2002	19	19	12	9	1	10
2003	40	37	16	14	4	18
2004	12	12	10	7	3	10
All years	296	265	160	129	29	158

This table reports the distribution of the sample across years, by insider trades, defendants, stocks and cases. The value in the 'All years' field does not necessarily equal the sum of the individual years because several defendants/stocks/cases are present in the sample in more than 1 year.

Specifically, 69 observations are excluded because the data sources do not report the amount of shares traded by the insider. A further 67 observations are excluded because the data sources do not report the penalty imposed upon the illegal insider. Finally, nine observations are excluded because the date on which the insider performed the trade is not reported. The final sample is comprised of 296 trades, which represent the trading of 265 defendants across 160 stocks, prosecuted in 158 cases (see Table 3). The majority of our sample involves prior knowledge of mergers or tender offers (i.e. 249 trades in 129 cases), while a smaller number relates to earnings announcements (i.e. 48 trades in 29 cases).

Descriptive statistics of the sample trades are reported in Table 4. Table 4 documents the median number of shares traded by insiders at 4625 shares per trade with a value of approximately \$75,000. This trade size is within the range designated by Barclay and Warner (1993) as 'medium-sized' trades (500–9999

Table 4 Descriptive statistics

	Mean	Median	Standard deviation
Volume	24,454	4,625	97,555
Volume/liquidity	34.49%	4.54%	105%
Dollar value	$531,597	$74,969	2,441,500
Absolute price change (dollar value)	$10.18	$6.78	11.39
Absolute price change (%)	48.11%	40.27%	38.75%
Days	18.33	7.00	28.26
Number of trades	1.52	1.00	1.28
Imputed profit (loss avoided)	$215,696	$26,860	1,010,638
Penalty	$341,068	$67,511	804,868
Penalty (scaled by imputed profit)	11.96	2.09	63.14

This table provides mean, median, maximum and minimum values for the sample of 296 illegal insider trades across several variables. *Volume* represents the amount of stock traded by the insider. *Volume/liquidity* represents the total volume traded by the insider divided by the stock's average daily trading volume on the 30 days prior to the day on which the insider firstly traded. *Dollar value* is equal to the volume of the trade multiplied by the closing price on the day before the insider's first trade. *Price change* is the absolute percentage price change between the closing price on the day of the announcement and the day before the insider's first trade. *Days* represents the volume-weighted average number of calendar days between the day of information announcement and that the insider performed the first trade. *Number of trades* is the number of transactions per insider trade case. *Imputed profit (loss avoided)* is the dollar value of profit made or losses avoided by the insider. This value is calculated by first determining the absolute price change between the closing price the day before the insider's first trade and the closing price on the day of the news announcement. This is then multiplied by the number of shares involved in the insider's trade to calculate the imputed profit or loss avoided. *Penalty* is equal to the dollar value of the penalty imposed by the Securities and Exchange Commission. The penalty is the total sum of disgorgement, civil penalties, criminal sanctions and interest and is on a per insider trade case basis. The penalty is also reported when scaled by the imputed profit of the insider.

shares), the amount most likely to be used by informed traders to 'stealth trade'.[13] The volume traded by insiders represents 4.54% (median) of the average daily trading volume in the stock over the previous 30 days. On average, insiders trade around 7 days before the announcement and use between one to two transactions to implement their strategy.[14]

Table 4 also reports the imputed profit or loss avoided by the insiders. This value is calculated by first determining the absolute price change between the closing price of the day before the insider trade and the day of the news announcement. This is then multiplied by the number of shares in the insider trade to calculate the imputed profit or loss avoided. The median profit earned or loss avoided reaped by insiders per security they traded is $26,860.

13 Barclay and Warner (1993) analyze individual trades rather than the total volume an insider trades as is the case in this study. However, the median individual trade size of our sample is 2000, which also fits within the 'medium-sized' category of Barclay and Warner.

14 The relatively short time distance reported in Table 4 between information announcement and trades by prosecuted insiders in our sample lends support to the conclusion reached by Ke et al. (2003), who state that to avoid legal jeopardy insiders purposely avoid trading closer to the information announcement day.

The median penalty is \$67,511 and the median penalty per dollar of (imputed) profit or loss avoided is 2.00. In civil cases the penalty assessment is the sum of all monies that the defendant is forced to pay. This usually involves full disgorgement of profits, some civil penalty and interest assessments. If a defendant is given a criminal sanction beyond a civil penalty, this is added to the total penalty leveled against the individual. In litigation reports, sanctions are often reported *per individual*. When determining the penalty *per trade*, penalty assessments for individuals are scaled by the profit made per trade for the individual in question. For example, if a defendant makes a profit of \$10,000 in one trade and \$40,000 in another trade, the penalty for that individual is split across those trades using a ratio of 1:4. It is also apparent that most of the variables in Table 4 are highly skewed to the right. Consequently in the following regression analysis, variables are log-transformed to control for this skewness in the underlying distributions.

IV. EMPIRICAL RESULTS

To test the hypotheses proposed in Section 2, the following regression is estimated:

$$\begin{aligned} Volume = \alpha + \beta_1 \times Price\ change + \beta_2 \times Penalty + \beta_3 \times Std.\ dev. \\ + \beta_4 \times Specialist + \beta_5 \times Days + \beta_6 \times Direct\ insider + \varepsilon \end{aligned} \tag{1}$$

In particular, we regress the volume traded by the insider on a number of variables that proxy for the information possessed by the insider (Hypothesis 1), the expected penalty (Hypothesis 2), return volatility (Hypothesis 3) and the probability of detection (Hypothesis 4). Following previous studies (i.e. Cornell and Sirri 1992; Seyhun 1992; Chakravarty and McConnell 1997; Fishe and Robe 2004), the dependent variable, *volume* is the natural log of total volume transacted by the insider scaled by the average daily trading volume of the same security over the previous 30 days.[15] This produces a more appropriate measure of the dependent variable in a cross-sectional regression where traded volumes can differ substantially simply due to variations in trading activity.

To estimate the expected return of the stock associated with the news announcement as in Hypothesis 1, variable *Price Change* is measured as the natural log of the absolute dollar return between the closing price on the day before the first trade executed by the insider and the day of the information announcement.[16,17] The variable, *Penalty* is equal to the natural log of the

15 We also use the time-weighted volume for insiders engaging in multiple transactions, where the weight for each trade is the number of days between the day of trade and the announcement day. The results are similar to those presented in Table 5.

16 When the information announcement occurs after the market close, the opening price on the next day is used to calculate the price change.

17 With the possession of private and price-sensitive information, the insider can estimate the size of the price movement likely to be precipitated as a consequence of the release of the

penalty per trade scaled by the trading volume (described earlier).[18] The variable *Standard Deviation* is the natural log of the standard deviation of daily closing price returns in the 30 days prior to the first trade, which captures the variability of the asset returns.

As described in Section 2, several variables are used to proxy for the probability of detection. *Specialist* is a dummy variable that equals 1 if the insider traded on the NYSE or American Stock Exchange and zero otherwise (NASDAQ). The variable *Days* is equal to the natural log of 1 plus the volume-weighted average number of calendar days before the information announcement that the insider performed the trades.[19] *Direct Insider* captures the proximity of the illegal trader to the inside information. It equals 1 if the individual is an employee, legal advisor or consultant to the company about to make the information announcement or in the case of a merger an employee, consultant or legal advisor of the target or acquiring firm. It is equal to zero otherwise.

Regression results are reported in Table 5. The *F*-statistic is 20.98 and significant at the 0.01 level. The model describes between 29% of the variation in the dependent variable.

Consistent with Hypothesis 1, the coefficient on the price change variable is positive and statistically significant at the 0.01 level. The value of the coefficient indicates that for every 1% increase in dollar return, insiders will trade 0.45% more shares relative to the average daily volume in the security. Therefore, in total dollar terms and holding all other things equal, a 1% increase in the dollar return increases the payoff to the insider by 1.45%, because the insider also enlarges their trading position. This is consistent with models of informed and insider trading, where individuals trade larger volumes when there are greater rents to be made (e.g. Easley and O'Hara 1987; Seyhun 1992). The coefficient on the variable of imposed penalty is negative and significant at the 0.01 level. The negative parameter estimate confirms that insiders respond significantly to greater expected penalties by lowering their trading volume.

The coefficient value of 0.37 indicates that for every 1% increase in penalty per share an insider will trade 0.37% less volume. The finding of penalty effect

information to others. On this basis, the ex-post return is used as a proxy for the expected return at the time the insider trades.

18 The Securities Exchange Act of 1934 as amended on October 13, 2009, sets in section 21A subsection (3) the maximum penalty for an insider to be 'the greater of $1 million or three times the amount of the profit gained or loss avoided'. Hence, at the time that an insider trades, they can estimate the size of the penalty they will incur if they are prosecuted. On this basis, it is reasonable to use the ex-post penalty charged as a proxy for the expected penalty at the time the insider trades. For robustness check, we also construct an ex-ante proxy for the expected penalty using the average ratio of penalty to insider profits over the past 3 years before each insider case in our sample, and the results are similar to those reported here.

19 $Days = \sum_{i=1}^{n} \frac{d_i * v_i}{V_n}$ where n is the number of trades that the current insider trades, d_i is the number of calendar days before the information announcement that the insider performed trade i, v_i is the volume for trade i, and V_n is the total volume traded. This measurement yields the same results compared with if the number of trading days is used.

Table 5 Regression results

Variables	Coefficient	Standard error	*t*-statistic	Variance inflation
Intercept	–3.74	0.42	–8.90	0
Price change	0.45	0.12	3.75	1.65
Penalty	–0.37	0.07	–5.29	1.44
Standard deviation	–0.96	0.15	–6.40	1.58
Specialist	–0.79	0.24	–3.29	1.07
Days	0.28	0.10	2.80	1.07
Direct insider	–0.45	0.24	–1.88*	1.07

$n = 296$ *F-statistic* = 20.98 *Adj.* R^2 = 28.89%
Note: * denotes significance at the 0.05 level, all other statistics are significant at the 0.01 level. This table reports coefficient estimates and corresponding test statistics for equation (1). The regression contains the following variables: the dependent variable, *Volume* is equal to the natural log of the total volume traded by an individual before a given news announcement and standardized by the average daily traded volume in the preceding 30 days; *Price change* is the natural log of the return between the last price on the day preceding the insider's first trade and the last price on the day of the news announcement; *Penalty* is equal to the natural log of the penalty leveled against the insider for that particular trade, divided by the size of insider trade; *Standard deviation* is the natural log of the standard deviation of closing price returns in the 30 days preceding the insider's first trade; *Specialist* is a dummy variable equal to 1 if the stock is traded on the NYSE or AMEX and zero otherwise; *Days* is the natural log of the volume-weighted number of days before the information announcement that the insider traded the shares; and *Direct insider* is a variable equal to 1 if the individual is an employee, legal advisor or consultant to the company and zero otherwise. Standard errors are corrected for heteroskedasticity (White 1980).

is consistent with Garfinkel (1997) who examines the effect of increased sanctions under the *Insider Trading and Securities Fraud Enforcement Act (1988)* (ITSFEA) on insider trading before earnings announcements. In the post-ITSFEA environment, the paper documents significantly less (legal) insider trading before announcements as well as larger price movements after the date of the earnings announcement. Both of these findings are consistent with the notion that insiders respond to perceived harsher sanctions by reducing their traded volume.

Consistent with Hypothesis 3, the results in Table 5 confirm that insiders respond to variability in asset returns by reducing their traded volume. The coefficient on the natural log of return standard deviation is –0.96 and significant at the 0.01 level. The variables used to proxy for the probability of detection confirm that insiders respond to an increase in this probability by reducing their traded volume. The coefficient on *Specialist* is negative and significant at the 0.01 level. This confirms that insiders trade less volume in a specialist market on the NYSE or AMEX. Previous studies show that NYSE specialists are more able to detect informed trading (e.g. Fishe and Robe 2004; Garfinkel and Nimalendran 2003). This finding suggests that insiders are aware of this and consequently trade less in order to avoid detection.

The coefficient on *Days* is positive and significant at the 0.01 level, confirming that insiders trade less as the announcement day approaches. This is consistent with detection minimization behavior and the model proposed by Park et al. (1995). Finally, the coefficient on *Direct Insider* is negative and significant at the 0.05 level. This is consistent with Hypothesis 4 and the notion that as it gets closer to the information announcement day insiders will trade less because it is easier for the regulator to establish a connection between the insider and the information.

Since variables in equation (1) may be correlated, we estimate the variance inflation factor for each independent variable.[20] The results show that multicollinearity does not significantly affect our regression model.

V. ADDITIONAL TESTS

A. Sample selection bias

The sample used in this study consists of illegal insiders who were caught and successfully prosecuted by the SEC. However, the conclusions we seek to draw concern all individuals who trade on non-public information, not just those who are caught. Since some proportion of illegal insider trading goes undetected, it is possible that the results provided in Section 4 are biased because the behavior of detected insider traders may be systematically different to the behavior of non-detected insider traders (see Meulbroek 1992).

It is possible that non-detected insider traders, because they remain undetected, might exhibit behavior that is more furtive than their detected counterparts. For example, for a given level of expected return, the non-detected insiders might be less aggressive in their trading – i.e. they trade less relative to the detected group. Since our sample consists only of detected transgressors, this may create a bias toward finding a significant relationship between expected returns and the position taken by the insider.

In terms of the expected penalty components – the magnitude of the penalty and the probability of detection – any bias will work against finding significance. This is because, for a given level of expected penalty, less aggressive trading implies greater sensitivity to this determinant in the direction predicted by the hypothesis. Therefore, the coefficients on levied penalty and the variables that proxy for the detection functions, presented in Section 4, would likely be greater in magnitude with a non-detected sample. A similar argument can be made for the other determinant – asset return variability. Therefore, if sample selection bias exists, it may change the magnitude but not the overall conclusions in relation to penalty, detection and asset variability.

To empirically test for the effects of any bias, we broadly draw upon the rationale that Meulbroek (1992) uses to account for the same sample selection bias in her study. Meulbroek (1992) study concludes that insider trading moves stock prices. This conclusion is reached because the author documents a correlation between insider trading days and abnormal returns. She recognizes a

20 In a separate test (not reported), we find that the variables of *Price change*, *Penalty*, *Standard Deviation* and *Specialist* are significantly correlated. We thank an anonymous referee for pointing this out.

selection bias exists in her sample because a part of the detection methodology used by the SEC is to examine days of abnormal price movements. Meulbroek overcomes this bias by dividing her sample into a subsample based on various methods used to detect illegal trades, noting that some forms of detection do not necessarily involve abnormal price movements as part of the detection criteria. Essentially her methodology involves identifying those observations in her sample that most closely resemble the non-detected sample of traders. Thus, she is able to divide her sample of insider trades into two groups – those likely to exhibit a bias in the same direction as her findings and those that are least likely to exhibit a bias.[21] A comparison between the two groups provides an indication of the extent of sample selection bias, if any.

Meulbroek (1992) recognizes that there are numerous ways for an individual to be detected and prosecuted by the SEC. However, we do not have access to referral data that she uses to ascertain how the individual is detected. Fortunately, an insight into the primary detection methodology can be found in Harris (2003), which states that large price and volume movements before an information announcement trigger suspicion (Harris 2003, p. 588).[22] After this the investigators compare lists of traders in that security with those that could know the information (Harris 2003, p. 589). His description of the typical investigation procedure implies three potential criteria for detection – trading with concurrent abnormal price movements, trading with concurrent abnormal volume movements and trading with *apparent* access to the sensitive information.

Following Meulbroek (1992), the sample of insider trading cases are divided into two groups based on the potential for bias. For each insider trade in the sample, we determine if this trade corresponds to a day with abnormal price movements or abnormal volume. Consistent with Meulbroek (1992), 'abnormal' refers to any value that is more than three standard deviations away from the mean, where mean and standard deviation benchmarks are calculated during the 150 days before the first trade by the insider in that security. Furthermore, all individuals classified as direct insiders in Section 4 are considered to satisfy the criteria of apparent access to non-public information. We choose to be conservative when assessing a trade as having a low potential for sample selection bias. Therefore, any trade that satisfies at least one of the three criteria is considered 'detected under normal SEC and exchange procedures' and has a high potential for sample selection bias.

Of the 296 trades, 185 are considered to be detected via the method described in Harris (2003). The remaining 111 observations are considered to have a low potential for sample selection bias. The log-linear regression is reestimated for each subsample. However, since the dummy variable, *Direct Insider*, is used to partition the sample, it can not appear again in the regression. The results of the regression are presented in Table 6.

21 For more detail see Meulbroek (1992, p. 1679).
22 Larry Harris served as Chief Economist at the SEC from July 2002 to June 2004.

Table 6 Tests on sample selection bias

Variables	High potential for sample selection bias (n = 185)		Low potential for sample selection bias (n = 111)	
	Coefficient	Standard error	Coefficient	Standard error
Intercept	–3.84	0.51*	–3.93	0.70*
Price change	0.35	0.14*	0.81	0.23*
Penalty	–0.31	0.10*	–0.46	0.13*
Standard deviation	–0.91	0.19*	–1.11	0.27*
Specialist	–0.70	0.29*	–1.31	0.44*
Days	0.27	0.12*	0.08	0.20
F-Stat	11.09*		13.58*	
Adjusted R^2	21.51%		36.38%	

Note: * denotes significance at the 0.01 level.
This table reports the coefficient estimates and *t*-statistics (in brackets) for the following regression:

$$Volume = \alpha + \beta_1 Price\ change + \beta_2 Penalty + \beta_3 Std.\ dev. + \beta_4 Specialist + \beta_5 Days + \beta_7 Specialist + \varepsilon$$

Of the 296 trades in our sample, 185 are considered to be detected via the method described in Harris (2003), which are deemed to have a high potential for sample selection bias. These observations are concurrent with abnormal volume or abnormal price movements of the security on the day, or are traded by an individual who is an employee, legal or financial consultant to the firm (i.e. *direct insider* = 1). The remaining 111 observations are considered to have a low potential for sample selection bias. The first column presents the results from the regression using the sample of high potential for sample selection bias. The second column presents the results from the regression for the remaining observations. Standard errors are corrected for heteroskedasticity (White 1980).

The results indicate that there are only modest differences between the two subsample. In terms of sign, all coefficients are the same and in the direction consistent with the hypotheses in Section 2. Sample selection bias, however, might affect the magnitude of the response to the determinants. Our previous discussion of potential sample selection bias issues highlights *Price Change* as the variable that could be significantly affected by sample selection bias. As in Table 6, the coefficient value on *Price Change* is 0.81, which is considerably larger than the coefficient for the high sample selection bias sample.

To formally test the overall differences in the coefficient values between the two models we perform a Chow test. The resultant test statistic is 0.87 with degrees of freedom (6, 284), which does not allow rejection of the null hypothesis that the coefficient values between the models are equal (p-value is 0.52). This confirms that our conclusions, with respect to how illegal insiders respond to key drivers of the model, are not materially affected by sample selection bias.

B. Comparison to 'normal' trades

The results in Section 4 are not conclusive insofar as they may reflect normal trading behavior of which insider trading is a subset. For example, we document that insiders trade less when engaging with specialists. It is unclear whether this is a general phenomenon of all trading or unique to illegal insiders. Essentially, our initial analysis is not able to distinguish between insider and normal trading behavior.[23]

To overcome this problem a sample of trades that are not likely to involve illegal insiders need to be identified. In particular, the following sampling procedure is implemented to generate a sample of control transactions that represent normal trades. A list of merger and earnings announcements, sourced from Thomson DataStream, forms the pool of potential control announcements. For each announcement underlying an illegal insider trade in our sample (i.e. the treatment sample), a sample of stocks with a suitable announcement (i.e. the control sample) is found that most closely resembles the features of the treatment announcement. We impose criteria that: (i) announcements of the same information type are matched to each other (i.e. treatment merger announcements are matched against a pool of other merger announcements); and (ii) the control announcement is made within 1 year of the treatment announcement. Following Huang and Stoll (1996), Cao et al. (1997) and Cao et al. (2005), matching is then based on three primary criteria – price, volume and market capitalization of stocks (i.e. size).[24] For each unique announcement in the treatment sample, *i*, the following score is constructed for each potential control announcement of stock *j*:

$$score_{i,j} = \left(\frac{price_i - price_j}{\frac{price_i + price_j}{2}}\right)^2 + \left(\frac{volume_i - volume_j}{\frac{volume_i + volume_j}{2}}\right)^2 + \left(\frac{size_i - size_j}{\frac{size_i + size_j}{2}}\right)^2 \tag{2}$$

where price, volume and size represent the daily averages of price, traded volume and market capitalization over the period t = –200 to t = –100 (t = 0 is the announcement date) for stock *j*. This benchmark period is consistent with Cao et al. (2005). For each treatment announcement, the announcement, which has the minimum score, is chosen as the matching control announcement.

When a suitable announcement is found, the entire stream of trades *x* days prior to the announcement are sourced from the Thomson Reuters DataScope data base, where *x* corresponds to the closest integer value for the *days* variable of the particular trade.[25] For each matched sample of trades, the dependent

23 One of the first papers to incorporate non-insider trades into their analysis of insider trading behavior is Park et al. (1995). This issue is also raised by Chakravarty and McConnell (1999).

24 It is widely accepted that trading volume and market capitalization are important stock characteristics. However, previous literature also demonstrates that the level of stock price contains new information to investors (Grinblatt et al. 1984; McNichols and Dravid 1990; Desai and Jain 1997), and affects market liquidity (Dennis and Strickland 2003; Dhar et al. 2004; Goyenko et al. 2006; Pavabutr and Sirodom, 2010), cost of capital (Lin et al. 2009) and transaction costs through the relative bid–ask spread (Ho and Stoll 1981; Huang and Stoll 1996; Venkataraman 2001; Werner 2003). Therefore, we include price as one of the criteria to find a matched sample.

25 Consistent with our matching criteria, there is little difference in either the market capitalization or trading activity between the treatment and control samples. The average market

variable and the covariates *price change*, *std. dev.*, *days* and *specialist* are constructed in the same manner as for the treatment sample.[26] After pooling both insider trades (the treatment sample) and non-insider trades (the control sample), we estimate a regression that allows us to identify the effect of the determinants on insider volume beyond what would otherwise occur with all trades.

In particular, the following cross-sectional regression is estimated using the combined sample of treatment and control observations:

$$\begin{aligned} Volume = \alpha + \beta_1 Price\ change + \beta_2 Std.\ dev. + \beta_3 Specialist + \beta_4 Days \\ + \beta_5 D + \beta_6 Price\ changeD + \beta_7 SpecialistD + \beta_8 DaysD + \varepsilon \end{aligned} \tag{3}$$

where D is a dummy variable, which equals 1 if the trade is part of the treatment sample and zero otherwise. These coefficients on the interactive variables identify the incremental effect that an independent variable has on volume if the trade is executed by an illegal insider. The results are presented in Table 7 in the column entitled 'Full Sample'.

Several important findings emerge from these results. The coefficients on the independent variables indicate that for normal traders, changes in stock prices have a positive and significant correlation with traded volume, while the standard deviation of returns has a negative and significant correlation with traded volume. This is consistent with the results documented in Table 5 for illegal insiders. In contrast, the coefficient on *Specialist* is positive and significant at the 0.01 level, indicating that investors tend to make large transactions in markets with a specialist in place. The coefficient on *Days* also exhibits a sign opposite to that in Table 5, indicating that the closer it is to the information announcement day, the larger the volume traded by a non-insider.

The coefficients on the interactive variables demonstrate the incremental effect of price change, volatility, specialist and days prior to announcement on the volume traded by illegal insiders. Specifically, the coefficient on *Price change D* is positive, 0.22, and significant at the 0.01 level, which indicates that insiders trade more volume than a non-insider for a given expected return. The coefficients on *Std. dev. D* and *Specialist D* are negative and significant, which is consistent with the results in Table 5. This indicates that compared with a non-insider, insiders are more sensitive to the underlying stock price volatility and the existence of a specialist in the market, and will adjust their trading volume accordingly. The positive and significant coefficient on *Days D* suggests that insiders will trade less when it gets closer to the public release of their information. These results confirm that after controlling for the normal relationship between trade size and the various independent variables, the incre-

capitalization of the firms in the treatment sample is \$3.44 billion while for the control sample it is \$3.51 billion. The average traded volume for the treatment sample is 426,300 shares while for the control sample it is 432,000 shares. The average score is 0.07 indicating that the cumulative difference over the three matching criteria is small. Overall, the control sample is a reasonable match to the treatment sample.

26 The penalty and direct insider variables can not be constructed for the control trades.

Table 7 Tests on comparison between sample transactions and normal trading

Variables	Full Sample (n = 296)		Treatment sample involving only one trade by insider (n = 219)	
	Coefficient	Standard error	Coefficient	Standard error
Intercept	5.85	0.0072**	5.82	0.01**
Price change	0.05	0.0023**	0.04	0.003**
Standard deviation	–0.07	0.0048**	–0.06	0.01**
Specialist	0.23	0.0071**	0.27	0.01**
Days	–0.05	0.0031**	–0.03	0.004**
D	–10.48	0.24**	–10.49	–0.26**
Price change D	0.22	0.07**	0.16	0.08*
Standard deviation D	–1.09	0.09**	–1.14	0.11**
Specialist D	–0.85	0.15**	–1.07	0.17**
Days D	0.25	0.06**	0.20	0.07**
F-Stat	2407**		1621**	
Adjusted R^2	12.22%		12.37%	

Note: * denotes significance at the 0.05 level; and ** denotes significance at the 0.01 level. This table reports the results of regression:

$$Volume = \alpha + \beta_1 Price\ change + \beta_2 Std.\ dev. + \beta_3 Specialist + \beta_4 Days + \beta_5 D + \beta_6 Price\ changeD + \beta_7 SpecialistD + \beta_8 DaysD + \beta_9 PenaltyD + \beta_{10} Direct\ insiderD + \varepsilon$$

which is estimated on a sample representing 296 insider trades and a control sample of trades that are based on a set of matched information announcements. For each announcement underlying an illegal insider trade in our sample (i.e. the treatment sample), a sample of stocks with a suitable announcement is identified when the announcement has the same information type as (i.e. merger or earnings announcement) and was made within 1 year of the announcement that underlies the insider's trade. For each of these stocks, a score is calculated according to equation (2), and the stock with the lowest value, and its associated announcement, are identified as the matched stock and announcement. Once a matched announcement is found, the entire stream of trades of the matched stock x days prior to the matched announcement are taken as the control sample, where x corresponds to the closest integer value for the *days* variable of the insider trade. D is a dummy variable that takes a value of 1 if the current observation is from the treatment sample. The results are also reported based on a subset of treatment sample, which is obtained by selecting those insider trades that only involve one transaction. Standard errors are corrected for heteroskedasticity (White 1980).

mental effect of these variables on the volume traded for the sample of illegal insiders is in the direction predicted by Hypothesis 1 to Hypothesis 4 and statistically significant.

It appears that illegal insiders trade less than non-insiders. The coefficient on the dummy variable D is –10.48 and significant at conventional levels. This result is consistent with our findings in Table 4 and the stealth trading hypothesis that informed traders tend to conceal their private information by engaging in medium-sized trades (Barclay and Warner 1993; Chakravarty 2001), as well as the notion that in order to avoid being detected, illegal insiders will trade less volume to 'hide in the crowd'.

These results, however, may be biased as the dependent variable for the treatment sample (i.e. the illegal insider trades) may represent multiple trades in a security, whereas the control sample (i.e. the sample of normal trades) contains only single trades. To ensure the internal validity of findings, we restrict the sample to include only those insider trades involving single trades, which results in 219 such observations in the treatment sample. The results are presented in Table 7 in the column 'Treatment sample involving only one trade by insider', and are not materially different from those discussed earlier.

C. Additional robustness tests

i. Alternative proxy for market liquidity

Our results in Table 5 show that insiders trade less aggressively in more volatile markets. Previous literature demonstrates that price volatility is a measure of market liquidity (Stoll 1978; O'Hara and Oldfield 1986; Grossman and Miller 1988; Amihud 2002). Hence this result indicates that insiders trade less in less liquid markets, which is consistent with our conclusion that insiders try to 'hide in the crowd'.

To further examine this liquidity effect, we construct a new variable using the bid–ask spread, another well-established proxy for liquidity. More specifically, we calculate daily time-weighted bid–ask spread (TWBAS) for 30 days before each insider case in our sample, and construct the following ratio:

$$Liquidity\_change = \frac{TWBAS_0}{\frac{1}{30}\sum_{i=-30}^{-1} TWBAS_i}$$

where $TWBAS_0$ is the TWBAS on the day of insider trading. This ratio measures the change of bid–ask spread on the day of the illegal insider trade relative to the average of bid–ask spreads over the previous 30 days. This ratio is then included in equation (1) and the results are reported in Table 8.

The results show that all of our previous findings are robustness. More importantly, the *Liquidity change* variable is significantly negatively related to the insider trading volume, which suggests that insiders trade less (more) when the spreads are larger (smaller). Since the bid–ask spread is a measure of market liquidity, these results are consistent with the finding on return volatility and support our conclusion that insiders 'hide in the crowd'.

ii. Scheduled and unscheduled announcements

Chae (2005) shows that discretionary liquidity traders trade less prior to scheduled earnings announcements, while the pricing behavior of market makers who are better informed do not differ between scheduled and unscheduled announcements. In order to test whether insiders behave asymmetrically to scheduled and unscheduled announcements, we include into equation (1) a new dummy variable, *D_scheduled*, which equals 1 if the current announcement is scheduled. The results (not reported) show that the coefficient is negative and insignificant, which is consistent with Chae (2005) that the trading behavior of

Table 8 Robustness tests on changes of liquidity

Variables	Coefficient	Standard error	*t*-statistic
Intercept	–3.29	0.59	–5.6
Price change	0.46	0.13	3.62
Penalty	–0.30	0.09	–3.26
Standard deviation	–1.10	0.18	–6.15
Specialist	–0.26	0.17	–1.52*
Days	0.27	0.11	2.47
Direct insider	–0.55	0.27	–2.05**
Liquidity change	–0.91	0.30	–3.07

n = 296 *F-statistic* = 13.47 *Adj.* R^2 = 27.42%
Note: * and ** denotes significance at the 0.1 and 0.05 level, respectively. All other statistics are significant at the 0.01 level
This table reports the results of robustness tests on changes of liquidity. All variables are specified as in equation (1), except for an additional variable *Liquidity change*. Daily time-weighted bid–ask spread is calculated for a 30-day period before each insider trading case as well as for the day of insider trade. The *Liquidity change* variable is calculated as the ratio of the time-weighted bid–ask spread on the day of insider trade and the average spread over the 30 days before the day of insider trade. Standard errors are corrected for heteroskedasticity (White 1980).

informed traders does not vary between scheduled and unscheduled announcements.[27] All our major findings remain in these tests.

VI. CONCLUSION

This paper is the first study to provide a comprehensive empirical examination of the determinants of the volume traded by illegal insiders. Hypotheses are proposed and subsequently tested on a sample of illegal insider trades drawn from cases prosecuted by the SEC. Our results demonstrate that there is a positive relation between subsequent price change in a security and the volume traded by insiders. The results also indicate that there is a negative relationship between imposed sanction and volume. This suggests that insiders trade off the costs and benefits associated with utilizing their illegal information. Illegal insider traders are sensitive to variability in asset returns, and trade less if the underlying market is less liquid and if the probability of being detected is high.

The results of this paper have two important implications. First, the findings provide support for the use of profit-based objective functions to model insider trading behavior (e.g. DeMarzo et al. 1998), in which insider traders balance the expected revenue from trading against the costs and probability of getting caught. Second and more importantly, our findings support the notion that insiders attempt to 'hide in the crowd', which is consistent with the stealth trading hypothesis (Barclay and Warner 1993; Chakravarty 2001). This casts doubt over the use of algorithms by regulators (Harris 2003, p. 588) designed to detect potential illegal insider trading by examining abnormal trading activities.

27 The results are available from the authors upon request.

For example, Meulbroek (1992) documents that 60% of cases are originated by referrals from parties other than the SEC and security exchanges (Meulbroek 1992, p. 1682, Table 7). Hence more rigorous and sophisticated detection methods are needed to assist security regulators in their ongoing battle with insider traders.

There are a number of possible directions for future research. First, while our study focuses on the trading of equities, previous literature suggests that informed traders may trade derivatives to realize their profits (Easley et al. 1998; Chakravarty et al. 2004). Therefore, one possible direction for future research is to examine the use of derivatives in illegal insider's trades. Second, following previous research (e.g. Cornell and Sirri 1992; Meulbroek 1992; Chakravarty and McConnell 1997, 1999) our final sample includes only announcements of earnings and mergers, and another possible future direction, therefore, is to expand the sample and test our results on different types of announcements.

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