

The Sound of Silence: What Do We Know When Insiders Do Not Trade?*

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Abstract

This paper examines the information content of insider silence, periods of no insider trading. We hypothesize that, to avoid litigation risk, rational insiders do not sell own-company shares when they anticipate bad news; neither would they buy, given unfavorable prospects; thus they keep silent. By contrast, insiders sell shares when they do not anticipate significant bad news. Future stock returns are significantly lower following insider silence than following insider net selling, especially among firms with higher litigation risk. We examine two quasi-natural experiments where new laws result in changes in shareholder litigation risks for insiders. In both cases, with higher shareholder litigation risks, stocks where insiders stay silent earn significantly lower returns than other stocks.

Keywords: Insider trading, Insider silence, Litigation risk

JEL Classification: G12, G14, G18

1. Introduction

Corporate insiders possess private information on their companies that is unknown to the public. Previous literature has found that insiders' buys can predict positive future stock returns, while insiders' sells provide only some weak signals to predict negative future stock returns.¹ But what if insiders neither buy nor sell, and instead, stay silent? Insider silence is a frequent phenomenon. In our sample of US common stocks from January 1990 through December 2012, 66 percent of firms do not have any insider trading activities in a given one-month period; 12 percent of firms do not have any insider trading activities in a given one-year period. Presumably, insiders do not trade because there is no positive or negative information to trade on, and hence they refrain from trading and stay silent. If this conjecture is true, then insider silence has no information content and should have no predictive power towards future stock returns.

In this paper, we document that insider silence is associated with future negative stock returns, ranging from over the next month to over the next year: for example, when insiders keep silent over a period of twelve months, the average abnormal return over the subsequent twelve-month horizon is 4.3% lower than the case when insiders sell. In a panel regression context, we show the silence-return effects are robust in both cross-section and time-series settings. Hence, insider silence is not associated with no news; rather, it is associated with bad news and negative future stock returns. Our main results survive a battery of robustness checks.²

¹ See Seyhun (1986, 1988); Lakonishok and Lee (2001).

² The silence-sell spread remains significantly negative in a more inclusive sample in which micro-cap and low-priced stocks are included, in a subsample of only micro-cap and low-priced stocks, when different horizons are used to measure insider trading activity, when the trading activity of all insiders, instead of only the managers and directors, is included, or when insider trading activity is time stamped by transaction date instead of report date. Further, the silence-sell spread widens for the next 18 months, and becomes lower after two years.

Why is insider silence bad news for the company? Our central hypothesis relates insider silence to litigation risk. In the United States, insider trading is subject to regulation and shareholder litigation. Limited by its resources, SEC investigations are few and uneven. As a result, the SEC rules are primarily enforced by shareholder litigation, which mostly follows stock price declines.³ In a typical litigation case, shareholders launch securities class action lawsuits following large stock price drops, alleging that insiders of the firm knew the adverse information that led to the price decline but failed to disclose it to the market promptly.⁴ Courts take insider selling prior to disclosure of the information as evidence that insiders knew the information.⁵ By contrast, the lack of insider selling undercuts plaintiffs' allegation that insiders knew the information. Thus, because of the fear of shareholder litigation, rational insiders do not sell own-company shares while possessing significant negative information. Neither would they buy, given the unfavorable prospects. Hence, insider silence, a period of no insider trading, is associated with bad news. Insiders sell shares only when they do not anticipate big negative news.⁶

The litigation risk hypothesis above implies that insider silence is associated with significant negative future returns, and insider net selling is associated with not-so-negative (or non-negative) future returns. That is, the spread in future returns between insider silence and insider net selling, or the silence-sell spread, is negative. We directly test this hypothesis using formal portfolio regressions.

³ Cox and Thomas (2003, 2009) find that the total volume of SEC enforcement actions is relatively modest compared to the number of complaints that it receives, and that it is unlikely that the SEC's recoveries will displace those obtained in private actions. They conclude that SEC enforcement historically is uneven, and the vast majority of private suits occur without parallel SEC enforcement actions. Also see Neihaus and Roth (1999), Cheng and Lo (2006), Rogers (2008), and Gande and Lewis (2009), as well as Arshadi (1998) and Bainbridge (2007) for overviews of insider trading laws and enforcement strategies. Indeed, as discussed later, Marin and Olivier (2008) reject SEC investigations as an explanation that large stock price decline follows low volume of insider trading.

⁴ For a recent example of securities class-action lawsuits following stock price declines, see the case against Expedia and its management, <http://securities.stanford.edu/filings-case.html?id=105096>.

⁵ See, e.g., Johnson et al. (2007), Rogers (2008), Rogers et al. (2011).

⁶ Numerous studies show that insider sales are inferior as a return predictor (Jaffe, 1974; Finnerty, 1976; Seyhun, 1986; Rozeff and Zaman, 1988; Lin and Howe, 1990; Seyhun, 1997; Noe, 1999), or insider sales have no predictive ability (Chowdhury et al. 1993; Lakonishok and Lee, 2001; Jeng et al. 2003).

We find that the silence-sell spreads are always negative and statistically significant for the holding periods ranging from one month to twelve months.

We conjecture that insiders stay silent due to fear of litigation risk coming from shareholders. To test the litigation risk hypothesis, we examine two quasi-natural experiments where new laws result in changes in shareholder litigation risks for insiders. In both cases, with higher shareholder litigation risks, stocks where insiders stay silent earn significantly lower returns than other stocks. We first use the Private Securities Litigation Reform Act (PSLRA, thereafter) as an exogenous shock to litigation risk. In 1995 Congress passed the PSLRA to address abuses in securities fraud class actions. Studies in accounting, finance, and law find that in the post-PSLRA period, the number of litigation cases increases, and merits matter more for both lawsuit incidence and outcomes. Specifically, firms are more likely to be sued when there are fraud indicators such as abnormal insider trading; the risk of litigation in connection with abnormal insider trading has increased following the enactment of the PSLRA (Cox et al., 2003; Perino, 2003; Choi and Thompson, 2006; Johnson et al., 2007; Choi et al., 2009).

If insider silence effect is due to litigation risk, then after PSLRA insider silence should lead to more negative stock returns. We conduct a difference-in-difference test on the portfolio performance before and after the PSLRA of 1995. Our results show that the “silence” firms have significantly more negative future returns during the post-PSLRA period. For the silence-sell spread, its abnormal returns relative to Daniel et al. (DGTW, thereafter, 1997) matched portfolios after the PSLRA of 1995 are 3.07% ($t=2.30$), 2.31% ($t=4.03$), and 2.71% ($t=6.68$) lower than those before the PSLRA of 1995, for the strategy of holding one, six, and twelve months, respectively. When we consider the Fama and French (1993) three-factor model and other benchmark models, the differences remain similar. The results support the litigation risk as the reason behind insider silence.

The second experiment is the passage of the universal demand law (UD law, thereafter) which imposes higher demand requirement on derivative lawsuits against corporate insiders in states that have adopted the laws. Between 1989 and 2005, 23 states in the U.S. implemented UD laws in a staggered fashion. The UD laws impose additional constraint that shareholders must first demand remedial action by the corporate board. Since the corporate board rarely accepts such a demand, it raises the hurdle for shareholders to overcome to file derivative lawsuits. Previous studies have confirmed that UD laws reduce litigation risks for corporate officers (Appel, 2015; Nguyen et al. 2018; and Lin et al. 2020).

We take the implementation of the UD laws as a quasi-natural-experimental setting where litigation risks are lowered for insiders exogenously. We conduct difference-in-difference tests on the portfolio performance before and after the passage of UD laws. Our evidence shows that portfolio of stocks with insider silence has significantly lower returns than other stocks before UD law was passed. Using the Fama-French (1993) three-factor model as the benchmark model, the (annualized) alphas on the “silence” stocks before the implementation of the UD law are 10.56%, 7.34%, and 10.00% lower than those after the implementation of the UD law (all the differences reach the conventional level of significance). The results are again consistent with the litigation risk concern interpretation on insider silence.

We confirm the results found in the two exogenous shocks by proxying litigation risk based on litigation cases in the same sector or in the same state the year before portfolios are formed. These results are also robust in panel regressions, with alternative fixed effects and alternative time horizons, for insider silence effect. As corroborative evidence, we document that insiders, anticipating class action lawsuits or stock price crash, choose to not trade on shares of their own firms.

In principle, our finding that silence predicts future returns might be driven by several alternative hypotheses. First, insider silence may be due to the lack of insiders (the *lack of insider hypothesis*). In a smaller firm with fewer insiders, there would be higher chance that there is no insider trading and reporting. In a given period, firms with no insider trading may represent a smaller firm than a period with insider trading. To the extent that there are time-varying return patterns associated with firm size, this could potentially explain our results. While we find that insider silence phenomenon is negatively correlated with both the number of insiders of a firm and the firm size, the lack of insiders cannot completely explain the insider silence phenomenon. Insider silence phenomenon exists among mid- and large-cap firms with market capitalizations above the median size of NYSE stocks as well as firms with many insiders. Importantly, the negative relation between insider silence and future stock return remains strong among these firms.

Alternatively, trading constraints could also lead to the insider silence effect (the *trading constraint hypothesis*). Insiders are not allowed to short sell own company shares, their holdings might be restricted stocks, and they might have to hold shares for corporate control concerns (Marin and Olivier, 2008). If insiders sold shares in the earlier period but do not trade any shares over the recent past, external investors interpret that insiders have sold shares to the extent that these trading constraints are binding and that insiders' lack of buying indicates negative information. Such interpretations prompt outside investors to reevaluate firm value downward, and thus stock price drops. This hypothesis predicts that the underperformance following insider silence exists where insider silence is preceded by insider net selling. Our empirical analysis finds no significant interaction effect between insider silence and insider past net selling, and importantly, insider silence itself remains strong in predicting future returns negatively. That is, the silence effect is not any different for firms whose insiders are with binding trading constraints (i.e. those with past net insider

selling and no recent trading activity). Thus, the insider silence effect cannot be explained by trading constraints.

A third possibility we consider is the *lockup hypothesis*: insider silence is due to the lockup policies following initial public offering (IPO). That is, for firms that have just gone public, the lockup period restriction prevents insiders from selling shares for a period of time.⁷ Corporate insiders of newly public firms are unlikely to buy own company shares due to diversification concerns. Thus, insider silence could arise because of the lockup restriction. At the same time, IPO firms underperform in the long term (Ritter, 1991; Loughran and Ritter, 1995). Hence, the poor performance associated with insider silence could be due to IPO firms whose lockup period has not expired. In our empirical tests, we find that insider silence predicts future negative returns even after we exclude recent IPO firms with non-expiring lockup restrictions. As a result, this hypothesis does not explain our results.

Finally, our finding might also be due to the lack of equity-based compensation for corporate insiders (the *no equity compensation hypothesis*). That is, when the equity-based component (stocks and options) is missing in corporate insiders' compensation plan, insiders have no shares to sell. At the same time, the lack of equity-based compensation is usually tied to a firm's poor performance, which shows up in future stock returns. The anticipated poor performance also discourages insiders from buying shares beforehand. The no equity compensation hypothesis implies that the poor stock performance following insider silence does not exist among firms with equity-based compensation. Empirically, we find that our result persists even among the subsample of firms with explicit equity compensation for corporate insiders. Hence, this hypothesis is not supported.

⁷ While the lockup period is typically 180 days, in some cases it could be as long as three years (Brav and Gompers, 2003, p.7).

This paper is broadly related to recent studies on the effect of the reduction in activity. Bagnoli et al. (2002) show that delays in earnings reports precede price drops. Giglio and Shue (2014) find that the passage of time after merger announcement is related to the probability of merger completion. Akbas (2016) reports that stocks experiencing unusually low trading volume over a week before earnings announcements tend to have more unfavorable earnings surprises. Our paper shows that insider silence, the lack of insider trading, signals bad news.

Our paper contributes to the insider trading literature, particularly on the role of litigation risk. In this regard, our paper is closely related to Jagolinzer (2009). Jagolinzer (2009) reports that insiders strategically time their entrance into 10b5-1 plan, which is enacted by SEC to provide a safe harbor for insiders who preplan trades when they do not possess material nonpublic information. He finds that participants' sales, the bulk of Rule 10b5-1 trade, tend to precede price declines and generate abnormal returns.⁸ Marin and Olivier (2008) document that lower insider selling volume over the recent past and heavy insider selling in the distant past predicts price crashes. They attribute this to trading constraint of insiders: insiders might have sold most of their shares and are constrained in their trading. We differ from these papers in two regards. First, we examine the absolute lack of insider trading, not just lower volume or the strategic timing of entering into a plan. Second, we show that such lack of insider trading is systematically related to insiders' fear of litigation risk. While Marin and Olivier (2008) reject that SEC investigations would explain their result, they have not considered shareholder litigation risk. Litigation risk is much more widespread than SEC investigations. Also, unlike SEC investigations which apply to both insider sales and purchases, litigation risk is especially pertinent for insider sales. Hence, insiders fearing litigation risk would

⁸ Due to data constraints, our main analyses do not filter out insiders' trades based on their 10b5-1 plans. Our unreported analyses indicate that, overall, insiders' trades from their 10b5-1 plans are not economically significant. Accordingly, for a sample of Russell 3000 stocks over a shorter time period that we have data on 10b5-1 trades, our main results hold intact after filtering out 10b5-1 trades.

stay silent when there is bad news for companies. In a recent paper, Adhikari, Agrawal, and Sharma (2020) report that insider sales become more profitable after the passage of UD laws, consistent with UD laws reducing litigation risk, especially for opportunistic insider sales. Our paper complements with theirs by documenting the role of insider silence in the context of UD law passage and the ensuing lower litigation risk.

While it is well known that litigation risk reduces insider trading during information rich events (e.g., Seyhun, 1992; Jagolinzer and Roulstone, 2009; Piotroski and Roulstone, 2008), there is no comprehensive study on how the lack of insider trading is related to stock returns. Our paper fills this gap. We show that lack of insider trading predicts negative abnormal future returns; such returns are even more negative than the returns after insider sales. Our findings also offer a potential explanation for the puzzling results documented in the literature that insider sales, *on average*, are not informative. In a recent work, Cohen et al. (2012) find some insider trades (the opportunistic ones) are informative for up to six months. Similarly, Biggerstaff, Cicero, and Wintoki (2020) identify sharpened information signals from insider trading after controlling for their trading patterns. Our paper is related to theirs in that we find another significant information signal from insider trading activity: the lack of it.

The rest of the article is organized as follows. We describe the sample and data in Section 2. We report our main results on the effect of insider silence on future stock returns in Section 3. We report results on the two quasi-natural experiments on litigation risk in Section 4. We test various alternative explanations in Section 5, and present robustness checks in Section 6. Section 7 concludes.

2. The Phenomenon of Insider Silence

In this section, we first describe our sample selection and measure of insider trading. Then we examine insider trading frequency and define insider silence.

2.1 Stock sample, insider trading data and measure

Our sample consists of the universe of all common stocks (with CRSP share code 10 or 11) listed on NYSE, AMEX, and NASDAQ from January 1990 (when insider trading data become available) through December 2012. We exclude stocks using the following criteria: (1) the share price is lower than \$5 at the prior month end; (2) the firm's market capitalization is classified into the lowest two deciles using the breakpoints from NYSE common stocks; and (3) the firm has missing or non-positive book value of equity for the prior fiscal year. We also require firms to be at least 18 months old so that the corporate insiders have at least 12 months to make insider trading decisions without the lockup period restriction.⁹ We obtain the stock price data from the CRSP and the firm accounting data from the Compustat XPF.

The U.S. insider trading data are from Thomson Reuters Insider Filing Data Feed. The Securities and Exchange Commission (SEC) mandates that officers and directors, large shareholders (those who own 10% or more of the outstanding shares), and affiliated shareholders report their transactions to the SEC by the 10th of the month following the transactions (prior to August 2002) or within two days following the transactions (after August 2002). Following the procedures in Lakonishok and Lee (2001), Piotroski and Roulstone (2005), and Sias and Whidbee (2010), we clean the insider trading data and consider only open-market transactions by managers and directors.¹⁰

⁹ For firms that have just conduct initial public offerings (IPOs), this filter allows an 18-month period for insiders of these firms to make insider trading decisions if the IPO lockup period is shorter than six months. For IPO firms with a lockup period longer than 180 days, insider trading might be restricted, at least partly, by lockup. (Brav and Gompers, 2003) Section 5.3 discusses this possibility when we look into the lockup hypothesis.

¹⁰ We follow the literature (e.g., Lakonishok and Lee, 2001; Sias and Whidbee, 2010) to clean the insider trading data. Specifically, we use the following filters. We delete duplicate and amended records and records with cleanse codes of "S" or "A". Transaction price must be available, and we delete records if the number of shares in a transaction is below 100. The transaction code is either "P" or "S" for stock transactions. We delete transactions that involve more than 20% of total shares outstanding, and delete records if the transaction price is outside the 80%–120% range of the CRSP end-of-day stock price.

As a conventional variable in the insider trading literature, we define the net insider demand for month j over the past m -month period ($NID_{j,m}$) as the total number of shares insiders buy minus the total number of shares insiders sell over the past m months, normalized by the total number of shares outstanding at month $j-1$:

$$NID_{j,m} = \frac{\# \text{ shares insiders buy}_{j-m,j-1} - \# \text{ shares insiders sell}_{j-m,j-1}}{\# \text{ shares outstanding}_{j-1}} \quad (1)$$

In our main analyses, we group stocks based on their insider trading activity over the past 12 months. Stocks with no insider trading activity reported over the past 12 months form the “silence” group; stocks with positive and non-positive net insider trading form the “buy” and “sell” groups, respectively. Future stock returns start from month j . While we follow the prior literature and use reporting dates to measure past insider trading activity (see Lakonishok and Lee, 2001, p.97), our main results remain the same if transaction dates are used instead of reporting dates (see section 6.4 for details).

2.2 Insider silence frequency

To set up the stage, we first examine how frequent the insider silence phenomenon is. At the end of each month from January 1990 through December 2012, we sort firms by their prior m -month ($m=1, 6, 12$, and 24) net insider demand into “silence”, “buy”, and “sell” groups, and calculate the proportion of firms that fall into these groups. We report the time-series averages of the proportions of these groups in Table 1.

When insider trading activity is measured over the past one month, the proportion of firms that fall into the “silence” group is 66%, suggesting that insiders do not trade frequently. On average, 6% of firms have net insider buying and 28% have net insider selling. When the measuring window is expanded into six and twelve months, the proportion of insider silence firms becomes 22% and 12%, respectively. The proportion of silence firms decreases while the length of window increases.

Strikingly, however, even with a window measured as long as 24 months, there are still 7% of firms in the silence group. The evidence suggests the phenomenon of insider silence is quite frequent.

[Insert Table 1 here]

3. Insider Silence and Future Stock Returns

Table 2 presents our baseline results using a set of portfolio regressions. We form monthly portfolios based on the stocks' insider trading activities over the past 12 months. Stocks whose insiders do not trade over the past 12 months are grouped into the "silence" portfolio. For stocks with insider trading activities over the past 12 months, we rank them into two groups – stocks with positive net insider demand (NID) are grouped into the "buy" portfolios, while stocks with non-positive NID are grouped into the "sell" portfolios. Portfolios are equally weighted. We then examine each portfolio's future returns over the next one, six, and twelve months. The returns are annualized for ease of comparison. On average, there are 204, 284, and 1271 stocks in silence, buy, and sell portfolios, respectively. Hence each portfolio is well diversified.

In Panel A, we report portfolio returns in excess of T-bill rate during portfolio holding periods. The silence portfolio generates substantially lower returns than the sell portfolio. The silence portfolio generates equal-weight monthly excess returns of 6.36%, 6.62%, and 7.40% (after annualizing) for the strategy of holding one, six, and twelve months, respectively, which are much lower than the returns of the sell portfolios of 9.36%, 9.74% and 10.66%. The silence-sell spreads are -3.00%, -3.12%, and -3.26% for the strategy of holding one, six, and twelve months, with Newey-West *t*-statistics of -3.09, -4.56, and -4.20, respectively.

In Panel B, we report monthly abnormal returns relative to matched portfolios as in DGTW (1997), which are formed on market capitalization and book-to-market ratios. The silence portfolio generates equal-weight abnormal returns of -2.76%, -2.80%, and -2.68%, for the strategy of holding

one, six, and twelve months, respectively. All the abnormal returns reach the conventional level of significance. By contrast, the monthly abnormal returns of the sell portfolio are positive, though none is statistically significant. As a result, the silence-sell spreads are all negative and significant for the three holding periods.

We find similar results when we estimate alphas based on the Fama and French (1993) three-factor model and other alternative benchmark factors that are recently developed. Specifically, we calculate alphas based on the Pastor and Stambaugh (2003) liquidity factor, the Novy-Marx (2013) profitability factor, the Fama and French (2015) five factors, and the Asness et al. (2019) quality-minus-junk factor. In each case, future monthly abnormal returns are more negative for the silence portfolio than for the sell portfolio. For example, the Fama-French-Carhart six-factor alpha of insider silence-minus-sell portfolio is -4.89% (with a Newey-West t -statistic of -4.87) for the strategy of holding twelve months as shown in Panel E of Table 2.

[Insert Table 2 here]

4. Insider Silence and Litigation Risk

We conjecture insiders stay silent due to fear of litigation risk coming from shareholders. As shown in Figure 1, shareholder litigations are even more widespread than SEC investigations. Here we identify cases violating any law or rule related to insider trading through the Stanford Securities Class Action Clearinghouse (SCAC). Securities Exchange Act of 1934 was the first law against insider trading. The Act had given the U.S. Securities and Exchange Commission (SEC) the authority to set the rules. Section 20(a), in particular, of the act provides a private right of action against persons engaged in insider trading. Thus, for each case, within all its filing documents, we search for keywords mentioning this rule using regular expressions or any details on the insider trading

behavior.¹¹ If either the rule or the content is mentioned in any of its filing documents, then this case is annotated as positive. We apply the same search rule to the SEC litigation dataset. When we search for Section 20 (a), the number of cases mentioned in the SCAC has risen from 83 in 1996 to 452 in 2001, and then constantly goes above 100 till the end of the sample period. By contrast, the number of cases mentioned in the SEC litigation barely exceeds one hundred during the entire sample period. When we search for any content mentioned in the documents related to insider trading, the number of cases recorded in the SCAC is still constantly more than that in the SEC litigation dataset. When we consider either of these search criteria, shareholder litigations are more prevalent than SEC investigations. Especially, when we compare the records in terms of the percentage out of the entire sample respectively, the average frequency of insider trading cases in the SCAC is as high as 88%, much higher than 17% for the SEC litigation.¹²

[Insert Figure 1 here]

Under such litigation risk, insider silence seems intuitive. However, it is hard to establish causality. To examine the litigation risk hypothesis in detail, we document insider silence results based on two quasi-natural experiments that changed litigation risks.

4.1 First quasi-natural experiment: PSLRA

We first use the PSLRA adopted by Congress in 1995 as an exogenous shock to litigation risk. The PSLRA erected a series of procedural barriers that have resulted in a higher percentage of securities fraud class actions being dismissed. Studies in accounting, finance, and law find that in the post-PSLRA period, the number of litigation cases increases, and merits matter more for both lawsuit incidence and outcomes. Specifically, firms are more likely to be sued when there are fraud indicators

¹¹ Appendix A provides details on the text analysis that we conducted on the SCAC files and the SEC Litigation cases.

¹² Note that the insider trading cases mentioned in the SEC litigation are not limited to public companies, but also include individuals, investment companies, and private firms.

such as abnormal insider trading; the risk of litigation in connection with abnormal insider trading has increased following the enactment of the PSLRA (Cox et al., 2003; Perino, 2003; Choi and Thompson, 2006; Johnson et al., 2007; Choi et al., 2009). For example, Johnson et al. (2007) document that the PSLRA's stringent pleading requirements encourage plaintiffs' lawyers to focus more on objective evidence that the firm and its managers acted with fraudulent intent to mislead investors. They find a significantly greater correlation between litigation and abnormal insider selling after the PSLRA. In particular, lawsuit filings and insider trading allegations correlate more significantly with abnormal insider selling than before the PSLRA.¹³

We use PSLRA of 1995 as a quasi-exogenous shock where concerns on litigation risk increased after PSLRA was enacted. If insider silence effect we documented is due to litigation risk, then after the rise of litigation risk, insider silence should be associated with more negative stock returns. We conduct a difference-in-difference test on the portfolio performance before and after litigation risk. We report the portfolio regression results in Table 3.

We find that the "silence" firms have significantly more negative future returns after the PSLRA of 1995. The silence portfolio after the PSLRA of 1995 generates equal-weight monthly excess returns of 0.20%, -0.81%, and 0.77% for the strategy of holding one, six, and twelve months, respectively, which are much lower than the returns of the silence portfolios of 8.51%, 8.65%, and 9.97%, respectively, before the PSLRA of 1995. The differences are economically large and statistically significant (-8.31% with a $t=-3.37$, -9.46% with a $t=-3.84$, -9.20% with a $t=-3.73$, for the strategy of holding one, six, and twelve months, respectively).

¹³ The number of lawsuits being filed, however, has not declined. After an initial dip, the number of securities fraud class actions has returned to, and even exceeded, its pre-PSLRA level (Buckberg et al., 2005). The larger number of filings suggests that the PSLRA may have done little to discourage the filing of frivolous suits, although it may have increased their likelihood of dismissal. Plaintiffs' lawyers respond that the suits have always been merit driven and that the only thing that has changed post-PSLRA is that meritorious suits are now being dismissed. They argue that the upsurge in filings simply reflects a massive expansion in the amount of fraud being committed (Lerach, 2001), perhaps due to reduced exposure to liability (Bernardo et al., 2000). A higher dismissal rate means that plaintiffs' lawyers need to file more cases in hopes that a reasonable number will make it through to discovery.

Silence-sell returns spreads have also increased after PSLRA. Before the PSLRA of 1995, the silence-sell spreads are -2.41%, -3.14%, and -3.39% for the strategy of holding one, six, and twelve months, respectively. In contrast, after the PSLRA of 1995, the silence-sell spreads are -5.48%, -5.70%, and -6.26%, with Newey-West *t*-statistics of -2.48, -4.51, and -4.52 for the holding periods of one, six, and twelve months, respectively. Again, the differences are economically large and statistically significant.

Panel B of Table 3 shows a similar picture in the monthly abnormal returns adjusted by the DGTW (1997) matched portfolios. The silence-sell spreads measured by the DGTW-adjusted abnormal returns after the PSLRA of 1995 are 3.07% ($t=2.30$), 2.31% ($t=4.03$), and 2.71% ($t=6.68$) lower than those before the PSLRA of 1995, for the strategy of holding one, six, and twelve months, respectively. The other panels of Table 3 report the regression alphas when we use the Fama and French (1993) three-factor model, the Fama and French (1993) and Carhart (1997) four-factor model, and the extended Fama-French-Carhart six-factor model, respectively. In all these cases, future monthly abnormal returns on both the silence portfolio and the silence-sell spread are more negative after the PSLRA of 1995 than those before the PSLRA of 1995.¹⁴

Overall, the PSLRA results are highly consistent with the litigation risk concern interpretation on insider silence.

[Insert Table 3 here]

¹⁴ All the differences are economically large and statistically significant. Take the trading strategy of holding 12 months as the example, the alphas have decreased 4.24% ($t=4.24$), 3.58% ($t=3.28$), and 3.90% ($t=3.57$) between the pre- and post-PSLRA periods, based on Fama and French (1993) three-factor model, the Fama and French (1993) and Carhart (1997) four-factor model, and the Fama and French (1993) extended six-factor model, respectively.

4.2 Second quasi-natural experiment: the UD law

The second experiment is the passage of the UD law, which imposes higher demand requirement on derivative lawsuits against corporate insiders in states that have adopted the laws. As explained earlier, the UD Law reduced litigation risks for corporate officers (Appel, 2015; and Nguyen et al., 2018; and Lin et al., 2020). We take the implementation of the UD laws as a quasi-natural-experimental setting where litigation risks are lowered for insiders exogenously.

The introduction of UD laws is staggered over multiple years across multiple states. The earliest states to adopt the laws were Georgia and Michigan in 1989 and the most recent states to adopt them were Rhode Island and South Dakota in 2005. This allows us to conduct difference-in-difference experiments on the impact of litigation risk across different states over the period before and after the implementation of the UD law. If insider silence effect is due to litigation risk, then we expect that portfolio of stocks with insider silence would have lower returns before the UD law was passed.

Table 4 presents results of portfolio analyses. For the 23 states that have adopted the UD law during the sample period, we separate stocks with headquarters located in those states into two periods (Before UD Law and After UD Law) and examine their future returns. Panel A is based on cumulative excess returns, and the first two blocks (with headers “I. Before UD Law” and “II. After UD Law”) are for the portfolios based on stocks from the 23 states. For the one-month holding period, the silence portfolio for the 23 states earns an average annualized excess return of 3.36% before UD law and 12.57% after UD law. Similarly, the silence-sell spread is much lower before UD law (-6.49% with a $t=-2.54$) than after UD law (-0.36% with a $t=-1.13$). The pattern is similar for the six- and twelve-month holding periods.

We find similar results after we adjust for risks in multiple ways. We present abnormal returns relative to DGTW (1997) matched portfolios (Panel B), Fama and French (1993) three-factor model

alphas (Panel C), Fama and French (1993) and Carhart (1997) four-factor alphas (Panel D), and monthly alphas based on a six-factor model (Panel E). The portfolio return patterns hold in all these cases. That is, the silence portfolio returns are lower before UD law than after UD law, and the silence-sell spread is wider (more negative) before UD law than after UD law. The differences in the abnormal returns between the pre- and post-UD law periods are all statistically significant. Using the Fama-French (1993) three-factor model as the benchmark model, the alphas on the “silence” stocks before the implementation of the UD law are 10.56%, 7.34%, and 10.00% lower than those after the implementation of the UD law, with a *t*-statistic of 4.24, 2.89, and 4.49, respectively for the holding periods of one, six, and twelve months. The significant differences on the abnormal returns between the pre- and post-UD law periods are consistent with the notion that litigation risk is higher before UD law.

For stocks from states other than the 23 states that have adopted the UD law over the sample period, we examine their portfolio return as well. The results are presented in the third block in each of the panels with panel header “III. Other States.” Shown in Panel A, based on excess returns, the silence portfolio earns significantly lower returns than the sell portfolio over the one-, six-, and twelve-month holding periods. Based on abnormal returns as presented in Panels B through E, the silence portfolio earns negative returns, which are in most cases statistically significant, and the silence-sell spreads are all negative and statistically significant. These stocks are to some extent similar to those in the first block (with the header “I. Before UD Law”) in that they both represent a period of higher litigation risk, relative to the period after UD law for those 23 states. Thus, in the fourth block with header “All States” we combine stocks from the first and the third blocks and examine their portfolio returns. The return pattern is very similar to our baseline results. That is,

silence portfolio earns negative abnormal returns and the silence-sell spreads are negative and significant, consistent with the litigation risk hypothesis.

[Insert Table 4 here]

We conduct further test based on a “peer effect” experiment. That is, we proxy litigation risk a firm’s insiders face based on litigation cases in the same sector or in the same state over the year before portfolios are formed. Table 5 shows the portfolio regression results on the experiment of Case by State/Sector. Here we construct the spread portfolio that longs stocks in which, for each given stock, there is at least one class action occurred in the same state or at least one class action occurred in the same sector last year and shorts stocks in which, for each given stock, there is no class actions either in the same state or in the same sector. As shown in Table 5, the silence portfolios affected by their peers in the class action have earned substantially lower risk-adjusted returns compared to those not affected by their peers. Take the alpha benchmarked against the Fama-French (1993) three-factor model, the silence spread portfolio generate annual abnormal returns of -2.76%, -3.08%, and -4.25% for the strategy of holding one, six, and twelve months, respectively, all statistically significant. By contrast, the alphas for the sell spread portfolio are not significantly different from zero. Consequently, the silence-sell spreads are negative and significant.

[Insert Table 5 here]

It is worth noting that these results are consistent with recent work by Adhikari, Agrawal, and Sharma (2020), who examine the profitability of insider trading before and after UD law passages. Using daily insider trading for a sample tilted toward smaller growth firms, Adhikari, Agrawal, and Sharma (2020) report that insider sales are more profitable after UD laws while most insider purchases are not after UD laws. They argue that UD laws reduce litigation risk, especially for the opportunistic insider sales.

4.3 Robustness checks

We estimate panel regressions to check robustness. Panel A of Table 6 presents the panel regression results, in which the dependent variables are the cumulative excess returns over the subsequent one-month, three-month, and six-month, one-year, and two-year periods. In addition to the main independent variables silence and buy, we also control for market capitalization, book-to-market, past return, and net issues. With one-month holding period, the coefficient of silence is -0.30, with a t -statistic of -4.96, and the coefficient of buy is positive. The same pattern in coefficients exists for other holding periods as well. Thus, Panel A results are consistent with the litigation risk hypothesis.

[Insert Table 6 here]

To exploit the implications of the quasi-natural experiments on the litigation risk, we consider adding interaction terms in panel regressions. Results are presented in Panel B of Table 6. For the PSLRA of 1995 experiment, when the interaction term of silence and the “Post 95” dummy variable is added to the panel regression, the coefficient on silence remains negative and statistically significant. In addition, the interaction term has a negative coefficient (-0.20 with $t=-1.95$). This latter result is consistent with our portfolio results in that the silence effect is stronger after the PSLRA, when the litigation risk is higher. This conclusion holds for the other holding periods as well.¹⁵

Similarly, using UD laws as the experiment, the silence coefficients are all negative and significant, and the coefficients on the interaction term “Silence \times UD Law” are all positive and significant, consistent with the notion that the adoption of UD law reduced litigation risk, thus weakening the silence effect. When using “peer effect” experiment, we reach the same conclusion.

¹⁵ Ideally, it would also be good to examine silence-sell differential. Since this test is done on a stock level, silence is well-defined but it is not clear how silence-sell differential should be defined.

The overall evidence in Table 6 suggests that our litigation risk effect is robust to alternative estimation models.¹⁶

4.4 Corroborative evidence

Corporate insiders possess private information unknown to the public. In the United States, insider trading is subject to regulation and shareholder litigation. The litigation risk hypothesis posits that when insiders possess significant negative information, for the fear of shareholder litigation they keep silent by abstaining from trading shares. To intuitively test such a hypothesis, we first investigate whether insiders refrain from trading before their firm has securities class action litigations. According to the previous literature, most litigations follow big downside movements in stock price.¹⁷ Based on the same reasoning, we then look at whether insiders refrain from trading before their firm's stock crash (i.e. large price drop).

To test whether insider silence predicts subsequent litigation and/or stock price crash, we estimate probit regression models. In Panels A, B, and C of Table 7, the binary dependent variables are equal to one if a firm is involved in a securities class action litigation over the subsequent one-, six-, and twelve-month period, respectively, while in Panels D and E, the binary dependent variables are stock price crash over the subsequent one month and one year periods, respectively. As can be seen clearly, the coefficients on silence (silence over past 1 year) has positive and significant coefficients, consistent with the litigation risk hypothesis of insider silence in that insiders,

¹⁶ As additional robustness checks, we also examine this hypothesis using three measures of litigation risk: Kim and Skinner (2012)'s measure of litigation risk, firm-level volatility, and market-level volatility. In all three cases, not only does firm silence per se significantly predict negative future returns, but the interaction effect between silence and litigation risk is significantly negative as well. These findings hold in both time series dimension using double-sorted portfolios and cross-sectional dimension using panel regressions. We report these results in the Internet Appendix.

¹⁷ See Bettis et al. (2000, p.208), Ke et al. (2003, p.316), Cheng and Lo (2006, p. 821), Piotroski and Roulstone (2008, p.410), Rogers (2008, p. 1269), Lee et al. (2014), Cohen et al. (2012, p. 1040, model 4; Panel B of Table IX), among others.

anticipating class action lawsuits or stock price crash, choose to not trade on shares of their own firms.¹⁸ We also examine whether insider silence predicts future price jumps. As shown in the lower panels of Table 7, we find it does not. Thus, insider silence is not merely related to stock volatility. Rather, it precedes large price drops only.

[Insert Table 7 here]

5. Alternative Explanations

In this section, we test some alternative hypotheses that could potentially explain the insider silence effect. The negative returns associated with insider silence can be also consistent with the lack of insider hypothesis, the trading constraint hypothesis, the no equity compensation hypothesis, and the lockup hypothesis. In this section we examine the implications of these hypotheses, check whether they are supported in the data, and investigate whether our baseline result holds.

5.1 The lack of insider hypothesis

A simple explanation for the phenomenon of insider silence is the lack of insiders. A small firm with few insiders is more likely to report no insider trading. If firm size is associated with time-varying returns, then this could potentially explain the insider silence effect. In sum, the lack of insider hypothesis implies that return predictability of insider silence is mostly likely driven by firms with few insiders and small size.

To test this hypothesis, we first sort firms into terciles by the number of insiders.¹⁹ Within each tercile, we then calculate the proportion of firms whose insiders do not trade over the past year.

¹⁸ An alternative perspective is that insider silence prevents future class action lawsuits. The argument goes that shareholders, by observing insider silence, choose to not file a class action lawsuit.

¹⁹ These numbers are estimated as the number of insiders of the firm that are covered in the Thomson Reuters Insider Filing Data over the window from three years before the current month to three years after the current month. Here we consider all insiders.

Panel A of Table 8 presents the time-series average of the proportion of silence firms for each of these terciles. As expected, the proportion of silence firms is higher when there are fewer insiders. When insider trading is measured over the past one-year period, the proportion of insider silence is 20% for firms with small number of insiders (bottom 1/3). However, there still remains a fair amount of proportions of insider silence associated with firms with medium (8%) and large (7%) number of insiders (the middle and top 1/3 correspond to medium and large groups, respectively).

[Insert Table 8 here]

We also rank firms into size groups and calculate the proportion of insider silent firms. Throughout our analysis in the paper, recall we exclude firms with market capitalization within the smallest two deciles of all NYSE common stocks. The medium size group consists of firms with market cap between NYSE deciles 3 and 5, and the large size group consists of the remaining firms with market cap above NYSE decile 5. Although smaller firms are more likely to experience insider silence (14% of medium-cap firms are silent over the past one-year period), there is also fair amount of insider silence among larger firms (10% of large-cap firms are silent).

If the lack of insider hypothesis mostly drives our result about insider silence and future negative returns, we should not find any significant silence effect in our sample if we exclude firms with small number of insiders or small size. We perform such an analysis and report the results in Panel B of Table 8. Even after we exclude firms with few insiders, silence significantly predicts future negative returns among all five time horizons. For example, the silence coefficient is -0.37% with a robust *t*-statistic of -4.73 for the dependent variable of future one-month stock return. Conclusions are similar when we exclude firms with their size equal or below NYSE median breakpoints. Overall, we find the effect of insider silence on predicting future negative stock return is not solely driven by small firms or firms with few insiders. The lack of insider hypothesis cannot explain the phenomenon

of insider silence. Among large firms and firms with many insiders, the proportions of insider silence are also sizable, and the silence-return effect remains strong.

5.2 The trading constraint hypothesis

Insiders cannot short sell and can only sell the shares they already own. According to the trading constraint hypothesis (Marin and Olivier, 2008), if insiders sold shares in the past but do not buy or sell recently, they may have sold all their shares and are constrained in their trading. Insiders' lack of buying then indicates negative information and negative future returns. If trading constraint hypothesis were to explain the insider silence effect, then the negative abnormal return following insider silence exists only when it is preceded by insider net selling.

To test this hypothesis, we first calculate the NID over the twelve-month period ending twelve months before the current month t , that is, NID ($t - 24, t - 13$). We then construct the "Past Sale" variable, which is the net insider selling (shares sold minus shares bought) normalized by the number of shares outstanding over this time period. The "Silence" variable is still defined by the insider trading activity over the past twelve months relative to the current month t . The interaction term between "Silence" and "Past Sale" should be negative if the trading constraint hypothesis holds.

Results in Panel C of Table 8 suggest the future negative returns associated with insider silence are unlikely to be driven by the preceding net selling. The silence effect is not any different for firms whose insiders have reached trading constraints (with past net selling and no recent trading): the coefficient of the interaction term is not statistically significant from zero (at most one standard error away from zero).²⁰ In all scenarios, the "Silence" coefficients are negative and significant (and

²⁰ As a robustness check, we also construct "Past Sale" variable by using the net insider selling over the past 3- and 6-month horizons. Conclusions are similar. None of these specifications yields significant interaction terms between "Silence" and "Past Sale". In contrast, the silence-return effect is always significantly negative. For brevity, these results are not reported but are available upon request.

very similar to those estimates in our baseline results of Table 6). The evidence suggests that the silence effect is not explained by trading constraints.²¹

5.3 The lockup hypothesis

The lockup hypothesis argues that the (long) lockup period drives the lack of insider trading among firms that just conduct initial public offering (IPO). Since IPO firms underperform in the long run (Loughran and Ritter, 1995), insider silence would then precede subsequent negative returns.

The IPO lockup hypothesis implies that the silence effect is strong among firms that just went public and whose insiders are locked up for selling shares. In Panel D of Table 8, we examine the results when we exclude firms that have IPO lockups. We collect the lockup period information of IPO firms from the Securities Data Company (SDC) database. Specifically, we exclude firm/month observations with any non-expiring lockup restrictions in our sample and then estimate a similar set of panel regressions as before. After excluding such firms, the silence effect on predicting future return is still negative and significant. This result is inconsistent with the lockup hypothesis.

5.4 The no equity compensation hypothesis

The no equity compensation hypothesis posits that a lack of equity-based compensation may drive both insider silence and subsequent poor stock performance. Because of the lack of equity-based compensation, insiders have no shares to sell. At the same time, the lack of equity compensation is likely related to a firm's poor performance (one would expect equity compensation as an economic incentive for insiders when the firm has good performance and shows upside gains in stock price). This hypothesis implies the negative returns associated with insider silence exist only among firms with no equity-based compensation.

²¹ We acknowledge that this proxy is not perfect. Insiders might have sold their shares before month $t-24$, in which case the trading constraint argument might still be at work.

To test this hypothesis, we classify firms into two groups: “Have equity” and “No equity.” Firms in the former group have explicit equity-based ownership by corporate insiders; firms in the latter group do not. Specifically, for firms covered in the Compustat ExecuComp database, a firm belongs to the “Have equity” group if in the two fiscal years prior to the current month t the firm has a positive aggregate number of shares owned by the insiders covered in the database. Thus, the insiders of firms in the “Have equity” group do have shares to trade at the beginning of the 12-month period during which we examine insider trading activity. The rest of the firms belong to the “No equity” group.²² Due to data restriction, our test uses the sample period from 1995 through 2012, when approximately 46% of the firms fall within the “No equity” group.

In Panel E of Table 8, we report regression results by excluding the firms that have no equity-based compensation. The no equity compensation hypothesis predicts an insignificant effect on the “Silence” variable. However, we find that even after excluding such firms, silence still predicts future negative returns. For example, when we consider the one-month future return scenario, the coefficient on silence is -0.26% which is similar to the coefficient to -0.30% in our baseline results. The results are robust when we consider other future return scenarios.

There are at least two reasons we would expect larger standard errors in estimated regression coefficients. First, a lot of firms do have equity based compensation. By excluding such firms, we lose almost half the observations (from 485,526 to 262,982), and as a result the statistical power is greatly reduced in the regression. Second, we note that the sample of firms with no equity based compensation is noisy due to limited firm coverage in the ExecuComp database. Nevertheless, we find a negative and significant insider silence effect among firms with equity based compensation,

²² “No Equity” group contains firms that are not covered in the ExecuComp database as well.

reinforcing our key story about the litigation risk explanation for insider silence and future negative stock return.

Collectively, the evidence in Table 8 provide little support for the alternative explanations. The lack of insiders, trading constraints, IPO lockup, and the absence of equity compensation do not seem to explain the insider silence effect.

6. Additional Robustness Checks

In this section, we perform additional analyses on the insider silence effect. First, we examine insider silence effects before M&A. Second, we examine earnings announcement returns to understand market reactions. Third, we investigate the length of insider silence and return predictability. Last, we present additional results on several variations of our baseline analysis.

6.1 Insider Silence Effects during Mergers and Acquisitions

While it is true that insiders are less likely to sell before negative information is released, it is still illegal to buy shares on private information that the stock will increase in value. It is reasonable therefore to predict that insiders would also avoid trading when they have private information that would cause a very large price increase. In this section, we examine whether insiders stay silent before an M&A.

Figure 2 plots the cumulative abnormal returns and the proportion of firms whose insiders net buy. Consistent with the prior literature (e.g., Jensen and Ruback, 1983; Jarrell et al. 1988; Andrade et al., 2001), the abnormal return of target firms is large in the announcement month (25.2%) and is statistically significant at the 1% level. Also consistent with the literature (e.g., Schwert, 1996), there

is evidence of price run-up in the months immediately leading to the announcement, and the target stock price stays flat on average after the announcement.²³

During the months before the public announcement, the proportion of targets whose insiders net buy is around 8-10%, which is slightly higher than but generally in line with the grand sample average of 7.6% (reported in a separate appendix table). This percentage starts to drop when it approaches the announcement month and reaches 2.9% during the announcement month. Unreported tests show that the proportion of firms with insider net buying during any of the three months right before the announcement is statistically different (at the 1% level) from that during any of the months from the 24th to the sixth months before the announcement.²⁴

In unsolicited or hostile deals, insiders of targets are less likely to be aware of the upcoming takeover offer and are less likely to have abstained from buying shares of their own companies, as compared to friendly deals in which target insiders have been involved in the private deal process. In an unreported analysis, we conduct the same investigation as in Figure 2 for the subsample of 275 hostile (or unsolicited) deals, and find no significant evidence of decreasing proportion of targets with net insider buying during the months prior to the announcement. On the other hand, the subsample of 3,992 friendly deals exhibits the exact pattern as shown in Figure 2.

Agrawal and Jaffe (1995) analyze the six-month short-swing rule and report evidence of its deterrent effect. The short-swing rule dictates that insiders are not allowed to profit from buying and selling shares within a six-month period. The short-swing rule will be particularly relevant for targets

²³ In the internet appendix, we provide a detailed report of the average monthly abnormal return, *p*-value of the abnormal return, cumulative abnormal return, percent of target firms whose insiders net buy, and percent of firms whose insiders net sell for the 24 months before and 12 months after the announcement.

²⁴ Note that an interesting pattern also exists in the proportion of targets with insider net selling. During the period before the public announcement, the proportion of net selling is between 18% and 20%, which is in line with the grand sample average of 18.3% (see Table 1). This proportion reduces to 10.4% in the month before the announcement. Thus, insiders delay selling, with the knowledge of the upcoming price appreciation upon the announcement. This delayed selling is consistent with the passive buying behavior analyzed in Agrawal and Nasser (2012).

with substantial upside. We explore this possibility by separating the merger sample into two equal groups based on their offer premium and conduct the same analysis as in Figure 2 for the two subsamples. For the high-premium subsample, insiders reduce their buying from the sixth month prior to the announcement, while for the low-premium subsample, the main reduction in buying starts about two months prior to the announcement month. These results confirm the analysis in Agrawal and Jaffe (1995) regarding the deterrent effect of the short-swing rule and further support our litigation risk hypothesis.

Overall, however, insiders are more likely sued in a class action lawsuit if they sell on bad news than if they buy on good news. Thus, insider silence does not predict overall good returns, even though it does precede very high positive returns for a very special event – corporate takeovers.

6.2 Earnings announcement returns

The results so far suggest that investors fail to fully incorporate the information contained in the preceding insider trading activity. To further investigate whether investors fail to incorporate the negative information in insider silence and whether they are systematically surprised when the relevant information is subsequently disclosed to the market, we examine the abnormal returns over the earnings announcement periods. Specifically, we extract quarterly earnings announcement dates from Compustat and calculate three-day announcement period abnormal returns adjusted by CRSP equal-weight daily market returns (i.e., an event window $[-1, +1]$ covering one day before and one day after the earning announcement). For each December, we first form portfolios based on the insider trading activity over the prior 12 months.²⁵ Within an insider trading/silence portfolio, we then calculate the average three-day abnormal returns of its constituent firms for the four quarterly

²⁵ Most of firms have December fiscal year end. The December portfolio formation generates straightforward results by using non-overlapped monthly returns over the subsequent four quarterly earnings announcement periods (and such a design mitigates the inflated test statistics). Results are similar when we use monthly portfolio formation.

earnings announcements following portfolio formation. For each of the four quarterly announcements and the four-quarter average, we estimate the time-series averages of the abnormal returns for three portfolios (“silence,” “buy,” and “sell”) and also the return differences among them.²⁶

We find that the silence portfolio is associated with negative abnormal returns in each of the four earnings announcements, with the average return of -0.08%. In contrast, the sell portfolio is associated with positive abnormal returns in each of the four earnings announcements, with the average return of 0.14%. The silence-sell spreads are economically large (three-day abnormal returns range from -0.31% to -0.16%) and statistically significant in the first and second earnings announcements. The average silence-sell spreads over four quarterly earnings announcements are -0.22% (with a robust *t*-statistic of -3.11). For brevity, these results are not reported but are available upon request. Overall, insider silence sounds bad news and investors are systematically surprised when such information comes to the market through earnings announcements.

6.3 The length of insider silence

Our empirical results so far are based on insider silence defined by the insider trading activity over the past twelve-month horizon. How is the length of silence related to future stock performance? In this subsection, we investigate how future negative returns are related to insider silence defined by insider trading activity over different horizons.²⁷

Our central story is that insiders keep silent because they possess negative information and they fear litigation, but *a priori* it is unclear how the future negative returns should vary with the past silence period. Our view is that insider silence formed over a very short time or a very long time will

²⁶ We follow the methodology developed in Chopra et al. (1992), which is used in Jegadeesh and Titman (1993), La Porta et al. (1997), and Titman et al. (2004), among others.

²⁷ The short-swing rule as studied in Agrawal and Jaffe (1995) prevents any individual to buy and sell shares within a six-month period. We argue that this rule does not materially affect our study because we aggregate individuals' trades over a period longer than six months.

less likely to be informative about future return. Insiders do not trade all the time. As Table 1 shows, two-thirds of the firms report no insider trading over the past month and over 22% of the firms report no trading over the past six months. If we define insider silence over too short a time period, then the insider silence may happen just by chance and not capture insider's deliberate decision not to trade. At the other extreme, if we define insider silence over too long a time period, then there are very few firms left with such insider silence. Since measuring expected returns over long horizons is a noisy process, there may not be enough power in a regression setting to relate these firms to future returns in either direction.

Table 9 presents the panel regression results on silence variable when we specify different lengths (number of months) to measure the past insider trading activity. Silence negatively predicts future returns, but the magnitudes and significance differ across various lengths of insider silence. We find no significant effects of silence on future returns when we measure past insider trading over a length that is too short (three months) in which the estimate is barely one standard error from zero. Silence predicts negative future returns the strongest when we measure insider trading over the past eighteen to twenty-four months: the silence effects are -0.33% and -0.38% with robust *t*-statistics of -5.71 and -5.06, respectively.

[Insert Table 9 here]

6.4 Other robustness checks

We have further conducted our empirical analysis with a couple of variations to check the robustness of the insider silence effect. First, we include all types of insiders (instead of only managers and directors) to measure insider trading activity, and rerun our main analysis. The key result is similar as before: silence firms underperform firms with insider sales by -1.33% (with a robust *t*-statistic of -2.33) in the panel regression with fixed effects. Second, we measure insider

trading by transaction dates rather than report dates. This also does not make any difference in the main result: the coefficient of silence variable is -1.01% and is statistically significant in the panel regression.

We also look at future stock performance over a longer horizon. While in the main results we find that insider silence strongly predicts future cumulative excess returns in the next twelve months, we find that the predictability is much weaker over the next two years. This is intuitive since insiders' information may be less informative for stock price over a long horizon, and also insiders may not worry too much about being sued if they sell a long enough time prior to a news event.

7. Conclusions

This paper examines stock returns following insider silence, periods of no insider trading. We hypothesize that, concerning litigation risk, rational insiders do not sell own-company shares when they withhold bad news. Neither would they buy, given the unfavorable prospects. Thus they keep silent. By contrast, rational insiders sell shares when they do not anticipate significant bad news to come. Consistent with this litigation risk hypothesis, we find insider silence is related to subsequent shareholder litigations and large stock price drop. Regarding the information content of insider silence, we show future returns are significantly lower following insider silence than following insider net selling. The silence-return effects are robust in both cross-section and time-series settings. Using two quasi-natural experiments, we confirm our hypothesis that insiders stay silent due to fear of litigation risk coming from shareholders.

We propose and test a number of alternative explanations for insider silence, including the lack of insiders, the trading constraint, the IPO lockup, and the no equity compensation. Our empirical evidence strongly rejects these alternative hypotheses. The litigation risk hypothesis more consistently explains the insider silence effect.

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Table 1: Insider Silence Phenomenon

Months to measure insider trading	Net Insider Demand	Silence	Buy	Sell
1	-0.103	66%	6%	28%
6	-0.301	22%	15%	63%
12	-0.564	12%	16%	73%
24	-1.042	7%	15%	79%

This table shows the average net insider demand and the proportion of firms in the silence, buy and sell groups. We report the time-series averages of the cross-sectional mean values of the net insider demand based on past insider trading activity over the past m months ($m=1, 6, 12$, or 24). A firm is classified as being in the “silence” group if the insiders of the firm do not trade over the past m months. A firm is in the “buy” group if the insiders of the firm have net buys over the past m months. Similarly, a firm is in the “sell” group if the insiders of the firm have net sells over the past m months. We report the time-series averages of the proportions of firms assigned to the “silence,” “buy,” and “sell” groups. The sample includes an average of 1,759 firms for a total of 276 monthly cross-sections from 1990 through 2012.

Table 2: Portfolio Analysis of Insider Silence

Panel A: Cumulative excess return

Portfolio	Holding Period		
	1 month	6 months	12 months
Silence	6.36 (1.65)	6.62 (2.08)	7.40 (2.52)
Buy	10.8 (2.81)	10.6 (3.19)	11.56 (3.90)
Sell	9.36 (2.44)	9.74 (3.18)	10.66 (3.82)
Silence - Sell	-3.00 (-3.09)	-3.12 (-4.56)	-3.26 (-4.20)

Panel B: Abnormal return related to matched portfolios in the spirit of DGTW (1997)

Portfolio	Holding Period		
	1 month	6 months	12 months
Silence	-2.76 (-2.84)	-2.80 (-3.89)	-2.68 (-4.08)
Buy	0.60 (0.72)	-0.36 (-0.53)	-0.15 (-0.24)
Sell	0.36 (0.53)	0.30 (0.47)	0.48 (0.77)
Silence - Sell	-3.12 (-3.94)	-3.10 (-5.28)	-3.17 (-4.90)

Panel C: Fama-French three-factor alpha

Portfolios	Holding Period		
	1 month	6 months	12 months
Silence	-2.64 (-3.24)	-3.26 (-3.38)	-3.84 (-3.60)
Buy	0.60 (0.56)	-0.22 (-0.24)	-0.05 (-0.06)
Sell	0.48 (0.71)	0.82 (1.02)	0.88 (0.98)
Silence - Sell	-3.12 (-3.46)	-4.08 (-4.59)	-4.72 (-5.53)

Table 2: Portfolio Analysis of Insider Silence, continued

Panel D: Fama-French-Carhart four-factor alpha

Portfolios	Holding Period		
	1 month	6 months	12 months
Silence	-2.04 (-2.58)	-2.20 (-2.34)	-1.86 (-1.89)
Buy	2.16 (2.30)	1.26 (1.55)	1.67 (2.19)
Sell	0.84 (1.29)	1.80 (2.39)	2.39 (3.12)
Silence - Sell	-3.00 (-3.08)	-4.00 (-4.23)	-4.25 (-4.54)

Panel E: Extended Fama-French-Carhart six-factor alpha

Portfolios	Holding Period		
	1 month	6 months	12 months
Silence	-2.52 (-2.90)	-2.70 (-2.70)	-2.31 (-2.21)
Buy	2.40 (2.43)	1.34 (1.50)	1.87 (2.25)
Sell	0.96 (1.17)	1.94 (2.28)	2.58 (3.00)
Silence - Sell	-3.36 (-3.33)	-4.64 (-4.59)	-4.89 (-4.87)

This table presents the portfolio returns of trading strategies associated with insider silence. Portfolios are formed based on the insider trading activity over the prior 12 months. The “silence” portfolio includes firms with no insider trading activity. The “buy” and “sell” portfolios include firms whose insiders have, on the net, bought and sold shares respectively. For each of the portfolios we calculate their cumulative annualized excess returns (net of T-bill rates) over the holding periods of one, six, and 12 months. Panel A shows the time-series averages of cross-sectional means of the portfolio returns. The row “silence – sell” presents return spreads between the corresponding portfolios. Panel B shows the abnormal returns adjusted for the matched portfolios in the spirit of DGTW (1997), in which the matched portfolios are matched with stocks held in the evaluated portfolio on the basis of market capitalization and book-to-market ratios of these stocks. Panel C shows the Fama-French (1993) three-factor alphas of the portfolios. The annual alphas are computed as 12 times the monthly estimates. Panel D shows the Fama-French-Carhart four-factor alphas of the portfolios, and Panel E shows the extended Fama-French-Carhart six-factor alphas of the portfolios in which the benchmark model includes Fama-French three factors, the momentum factor, the Pastor and Stambaugh (2003) liquidity risk factor, and the Sadka (2006) liquidity risk factor. We report Newey-West *t*-statistics in parentheses.

Table 3: Portfolio Analysis of Insider Silence -- the PSLRA of 1995 Experiment

Panel A: Cumulative excess return

Portfolio	Before the PSLRA of 1995			After the PSLRA of 1995		
	1 month	6 months	12 months	1 month	6 months	12 months
Silence	8.51 (1.60)	8.65 (1.95)	9.97 (3.88)	0.20 (0.03)	-0.81 (-0.18)	0.77 (0.19)
Buy	11.73 (2.28)	11.51 (2.43)	13.16 (4.19)	7.57 (1.07)	5.97 (1.22)	8.53 (2.12)
Sell	10.92 (1.84)	11.80 (2.52)	13.35 (4.88)	5.68 (0.72)	4.89 (1.03)	7.03 (1.58)
Silence - Sell	-2.41 (-1.53)	-3.14 (-2.73)	-3.39 (-3.06)	-5.48 (-2.48)	-5.70 (-4.51)	-6.26 (-4.52)

Panel B: Abnormal return related to matched portfolios in the spirit of DGTW (1997)

Portfolio	Before the PSLRA of 1995			After the PSLRA of 1995		
	1 month	6 months	12 months	1 month	6 months	12 months
Silence	-2.01 (-1.76)	-2.60 (-2.37)	-2.69 (-2.65)	-3.28 (-1.49)	-3.32 (-2.75)	-3.94 (-4.78)
Buy	0.81 (0.94)	-0.58 (-0.87)	-0.77 (-1.38)	1.79 (1.05)	0.46 (0.29)	1.10 (0.79)
Sell	0.40 (0.57)	0.42 (0.90)	0.62 (1.68)	2.19 (1.29)	2.01 (1.94)	2.08 (1.71)
Silence - Sell	-2.40 (-1.84)	-3.02 (-2.90)	-3.31 (-3.36)	-5.47 (-3.33)	-5.33 (-5.05)	-6.02 (-6.93)

Panel C: Fama-French three-factor alpha

Portfolio	Before the PSLRA of 1995			After the PSLRA of 1995		
	1 month	6 months	12 months	1 month	6 months	12 months
Silence	-1.92 (-1.62)	-3.12 (-2.83)	-3.09 (-2.88)	-5.40 (-2.75)	-6.52 (-2.64)	-8.03 (-2.24)
Buy	1.92 (1.59)	0.44 (0.38)	-0.32 (-0.31)	-1.32 (-0.53)	-2.20 (-1.00)	-1.20 (-0.53)
Sell	0.48 (0.51)	0.78 (0.93)	0.28 (0.34)	-0.84 (-0.44)	-0.16 (-0.06)	-0.03 (-0.01)
Silence - Sell	-2.40 (-1.70)	-3.90 (-3.04)	-3.38 (-2.87)	-4.56 (-1.85)	-6.36 (-2.91)	-8.01 (-3.78)

Table 3: Portfolio Analysis of Insider Silence -- the PSLRA of 1995 Experiment, continued

Panel D: Fama-French-Carhart four-factor alpha

Portfolio	Before the PSLRA of 1995			After the PSLRA of 1995		
	1 month	6 months	12 months	1 month	6 months	12 months
Silence	-2.04 (-1.59)	-2.90 (-2.46)	-2.38 (-2.17)	-4.32 (-2.35)	-4.06 (-1.96)	-3.26 (-1.49)
Buy	3.24 (2.30)	1.64 (1.27)	1.03 (0.87)	0.72 (0.35)	0.00 (0.00)	1.48 (0.88)
Sell	0.60 (0.53)	1.22 (1.12)	1.19 (1.17)	0.00 (0.03)	2.54 (1.32)	3.89 (2.10)
Silence - Sell	-2.64 (-1.67)	-4.14 (-3.02)	-3.57 (-2.90)	-4.32 (-1.65)	-6.60 (-2.64)	-7.15 (-3.01)

Panel E: Extended Fama-French-Carhart six-factor alpha

Portfolio	Before the PSLRA of 1995			After the PSLRA of 1995		
	1 month	6 months	12 months	1 month	6 months	12 months
Silence	-1.92 (-1.35)	-2.90 (-2.23)	-2.58 (-2.28)	-4.92 (-2.74)	-4.36 (-2.20)	-3.52 (-1.73)
Buy	3.24 (2.44)	1.92 (1.51)	1.40 (1.16)	0.60 (0.26)	-0.10 (-0.05)	1.48 (0.88)
Sell	0.48 (0.41)	1.12 (1.03)	1.01 (0.96)	-0.12 (-0.09)	2.48 (1.24)	3.96 (2.07)
Silence - Sell	-2.40 (-1.38)	-4.02 (-2.72)	-3.58 (-2.82)	-4.80 (-1.87)	-6.84 (-2.79)	-7.48 (-3.23)

This table presents the portfolio returns of trading strategies associated with insider silence on the PSLRA of 1995 experiment. Portfolios are formed based on the insider trading activity over the prior 12 months. The “silence” portfolio includes firms with no insider trading activity. The “buy” and “sell” portfolios include firms whose insiders have, on the net, bought and sold shares respectively. For each of the portfolios we calculate their cumulative annualized excess returns (net of T-bill rates) over the holding periods of one, six, and 12 months. Panel A shows the time-series averages of cross-sectional means of the portfolio returns. The row of “silence – sell” present return spreads between the corresponding portfolios. Panel B shows the abnormal returns adjusted for the matched portfolios in the spirit of DGTW (1997), in which the matched portfolios are matched with stocks held in the evaluated portfolio on the basis of market capitalization and book-to-market ratios of these stocks. Panel C shows the Fama-French (1993) three-factor alphas of the portfolios. The annual alphas are computed as 12 times the monthly estimates. Panel D shows the Fama-French-Carhart four-factor alphas of the portfolios, and Panel E shows the extended Fama-French-Carhart six-factor alphas of the portfolios in which the benchmark model includes Fama-French three factors, the momentum factor, the Pastor and Stambaugh (2003) liquidity risk factor, and the Sadka (2006) liquidity risk factor. We report Newey-West *t*-statistics in parentheses.

Table 4: Portfolio Analysis of Insider Silence -- the UD Law Experiment

Panel A: Cumulative excess return

Portfolio	23 States						III. Other States			All States		
	I. Before UD Law			II. After UD Law						I & III		
	1 month	6 months	12 months	1 month	6 months	12 months	1 month	6 months	12 months	1 month	6 months	12 months
Silence	3.36 (0.74)	4.60 (1.06)	6.63 (1.58)	12.57 (2.20)	7.02 (1.42)	8.03 (1.99)	6.77 (1.71)	7.43 (2.28)	8.18 (2.76)	6.49 (1.65)	7.29 (2.21)	8.17 (2.70)
Buy	10.79 (2.55)	9.71 (2.52)	11.74 (3.13)	11.82 (2.18)	11.41 (2.36)	13.20 (3.59)	10.88 (2.82)	10.48 (3.18)	11.66 (3.92)	10.92 (2.85)	10.54 (3.20)	11.73 (3.91)
Sell	9.86 (2.61)	9.65 (3.04)	11.27 (3.79)	12.93 (2.06)	11.34 (3.76)	12.05 (4.42)	9.48 (2.44)	9.87 (3.18)	10.88 (3.83)	9.56 (2.49)	9.93 (3.22)	10.93 (3.88)
Silence - Sell	-6.49 (-2.54)	-5.05 (-2.34)	-4.64 (-2.21)	-0.36 (-1.13)	-4.32 (-1.07)	-4.02 (-0.94)	-2.70 (-2.26)	-2.45 (-3.02)	-2.70 (-2.92)	-3.08 (-2.70)	-2.65 (-3.42)	-2.77 (-3.46)

Panel B: Abnormal return related to match portfolios in the spirit of DGTW (1997)

Portfolio	23 States						III. Other States			All States		
	I. Before UD Law			II. After UD Law						I & III		
	1 month	6 months	12 months	1 month	6 months	12 months	1 month	6 months	12 months	1 month	6 months	12 months
Silence	-5.95 (-2.59)	-3.88 (-1.97)	-3.35 (-1.70)	3.20 (0.72)	-2.32 (-0.86)	-1.01 (-0.47)	-2.15 (-1.72)	-1.86 (-1.78)	-1.77 (-2.01)	-2.52 (-2.12)	-2.02 (-2.02)	-1.81 (-2.02)
Buy	1.44 (0.81)	0.04 (0.02)	0.18 (0.11)	1.26 (0.35)	0.59 (0.18)	1.82 (0.59)	0.53 (0.56)	-0.51 (-0.66)	-0.10 (-0.14)	0.63 (0.70)	-0.40 (-0.52)	-0.01 (-0.02)
Sell	0.57 (0.47)	0.24 (0.21)	0.49 (0.46)	3.00 (0.53)	1.25 (0.67)	1.77 (1.17)	0.63 (0.88)	0.56 (0.82)	0.84 (1.05)	0.64 (0.91)	0.54 (0.81)	0.82 (1.13)
Silence - Sell	-6.52 (-2.66)	-4.11 (-1.95)	-3.84 (-1.89)	0.20 (1.00)	-3.57 (-1.03)	-2.78 (-0.53)	-2.78 (-2.64)	-2.42 (-2.81)	-2.61 (-3.16)	-3.16 (-3.16)	-2.56 (-3.07)	-2.63 (-3.42)

Table 4: Portfolio Analysis of Insider Silence -- the UD Law Experiment, continued

Panel C: Fama-French three-factor alpha

Portfolio	23 States									All States		
	I. Before UD Law			II. After UD Law			III. Other States			I & III		
	1 month	6 months	12 months	1 month	6 months	12 months	1 month	6 months	12 months	1 month	6 months	12 months
Silence	-5.28 (-2.45)	-6.04 (-2.57)	-6.51 (-3.01)	5.28 (1.16)	1.30 (0.30)	3.49 (0.83)	-1.92 (-1.98)	-2.28 (-2.27)	-2.87 (-2.57)	-2.28 (-2.46)	-2.60 (-2.66)	-3.11 (-2.89)
Buy	1.92 (1.06)	0.22 (0.13)	0.02 (0.01)	-0.12 (-0.04)	2.44 (0.70)	5.75 (1.82)	0.72 (0.60)	-0.26 (-0.26)	0.08 (0.08)	0.84 (0.76)	-0.16 (-0.15)	0.15 (0.16)
Sell	1.56 (1.31)	1.64 (1.38)	1.73 (1.39)	5.52 (1.22)	4.38 (1.27)	5.27 (1.54)	0.72 (1.01)	0.96 (1.21)	1.06 (1.20)	0.72 (1.13)	1.04 (1.32)	1.13 (1.29)
Silence - Sell	-6.84 (-2.91)	-7.68 (-3.42)	-8.24 (-4.03)	-0.24 (-0.77)	-3.08 (-0.68)	-1.78 (-0.32)	-2.64 (-2.32)	-3.24 (-3.07)	-3.92 (-3.81)	-3.00 (-2.86)	-3.64 (-3.70)	-4.23 (-4.47)

Panel D: Fama-French-Carhart four-factor alpha

Portfolio	23 States									All States		
	I. Before UD Law			II. After UD Law			III. Other States			I & III		
	1 month	6 months	12 months	1 month	6 months	12 months	1 month	6 months	12 months	1 month	6 months	12 months
Silence	-4.8 (-2.25)	-5.56 (-2.23)	-5.23 (-2.37)	5.52 (1.22)	1.38 (0.33)	3.38 (0.81)	-1.68 (-1.60)	-1.36 (-1.30)	-1.05 (-0.96)	-2.04 (-1.99)	-1.68 (-1.68)	-1.32 (-1.29)
Buy	3.24 (1.76)	1.82 (1.07)	2.17 (1.26)	1.80 (0.51)	3.58 (1.01)	6.37 (1.94)	2.28 (2.13)	1.22 (1.30)	1.69 (2.01)	2.40 (2.34)	1.36 (1.49)	1.82 (2.20)
Sell	1.80 (1.48)	2.44 (2.08)	3.15 (2.68)	4.08 (1.10)	4.12 (1.32)	5.14 (1.64)	1.2 (1.67)	1.96 (2.62)	2.55 (3.39)	1.20 (1.73)	2.00 (2.69)	2.60 (3.46)
Silence - Sell	-6.60 (-2.83)	-8.00 (-3.36)	-8.38 (-3.97)	1.44 (0.96)	-2.74 (-0.70)	-1.76 (-0.32)	-2.88 (-2.26)	-3.32 (-2.94)	-3.60 (-3.19)	-3.24 (-2.70)	-3.68 (-3.53)	-3.92 (-3.86)

Table 4: Portfolio Analysis of Insider Silence -- the UD Law Experiment, continued

Panel E: Extended Fama-French-Carhart six-factor annualized alpha

Portfolio	23 States						III. Other States			All States		
	I. Before UD Law			II. After UD Law						I & III		
	1 month	6 months	12 months	1 month	6 months	12 months	1 month	6 months	12 months	1 month	6 months	12 months
Silence	-5.04 (-2.24)	-5.36 (-2.12)	-4.82 (-2.09)	5.52 (1.20)	1.52 (0.36)	3.51 (0.84)	-1.80 (-1.64)	-1.70 (-1.57)	-1.45 (-1.24)	-2.04 (-2.01)	-1.94 (-1.84)	-1.61 (-1.46)
Buy	3.00 (1.50)	1.52 (0.84)	1.96 (1.07)	2.28 (0.65)	3.30 (0.95)	5.09 (1.60)	2.64 (2.41)	1.40 (1.37)	1.87 (2.05)	2.64 (2.54)	1.46 (1.48)	1.94 (2.17)
Sell	1.68 (1.25)	2.46 (1.88)	3.15 (2.40)	3.84 (1.09)	4.10 (1.32)	4.88 (1.52)	1.20 (1.61)	2.16 (2.56)	2.84 (3.35)	1.20 (1.63)	2.18 (2.60)	2.86 (3.37)
Silence - Sell	-6.72 (-2.76)	-7.82 (-3.24)	-7.98 (-3.63)	1.68 (0.95)	-2.58 (-0.67)	-1.37 (-0.29)	-3.00 (-2.35)	-3.88 (-3.30)	-4.28 (-3.60)	-3.36 (-2.75)	-4.14 (-3.80)	-4.46 (-4.13)

This table presents the portfolio returns of trading strategies associated with insider silence on the UD law experiment. Portfolios are formed based on the insider trading activity over the prior 12 months. The “silence” portfolio includes firms with no insider trading activity. The “buy” and “sell” portfolios include firms whose insiders have, on the net, bought and sold shares respectively. For each of the portfolios we calculate their cumulative annualized excess returns (net of T-bill rates) over the holding periods of one, six, and 12 months. Panel A shows the time-series averages of cross-sectional means of the portfolio returns. The row of “silence – sell” present return spreads between the corresponding portfolios. Panel B shows the abnormal returns adjusted for the matched portfolios in the spirit of DGTW (1997), in which the matched portfolios are matched with stocks held in the evaluated portfolio on the basis of market capitalization and book-to-market ratios of these stocks. Panel C shows the Fama-French (1993) three-factor alphas of the portfolios. The annual alphas are computed as 12 times the monthly estimates. Panel D shows the Fama-French-Carhart four-factor alphas of the portfolios, and Panel E shows the extended Fama-French-Carhart six-factor alphas of the portfolios in which the benchmark model includes Fama-French (1993) three factors, the momentum factor, the Pastor and Stambaugh (2003) liquidity risk factor, and the Sadka (2006) liquidity risk factor. We report Newey-West *t*-statistics in parentheses.

Table 5: Portfolio Analysis of Insider Silence -- The Peer Effect Experiment

Portfolio	Excess Return			DGTW			FF three-factor model			FF six-factor model		
	1 month	6 months	12 months	1 month	6 months	12 months	1 month	6 months	12 months	1 month	6 months	12 months
Silence	2.82 (0.68)	2.59 (0.73)	1.37 (0.43)	-2.47 (-1.98)	-2.41 (-2.64)	-2.40 (-2.93)	-2.76 (-2.52)	-3.08 (-2.34)	-4.25 (-2.89)	-2.76 (-2.42)	-3.16 (-2.17)	-3.82 (-2.47)
Buy	7.37 (1.70)	5.97 (1.65)	3.95 (1.29)	0.84 (0.87)	-0.15 (-0.16)	0.36 (0.42)	0.48 (0.39)	-0.28 (-0.23)	-0.42 (-0.34)	1.92 (1.47)	0.74 (0.57)	0.84 (0.69)
Sell	5.72 (1.36)	5.28 (1.54)	3.61 (1.17)	0.25 (0.29)	0.07 (0.08)	0.32 (0.38)	-0.12 (-0.12)	0.34 (0.30)	0.15 (0.12)	0.12 (0.09)	1.16 (0.92)	1.39 (1.05)
Silence - Sell	-2.90 (-2.70)	-2.69 (-3.91)	-2.24 (-4.24)	-2.73 (-2.79)	-2.47 (-3.62)	-2.72 (-3.41)	-2.64 (-2.42)	-3.42 (-3.33)	-4.40 (-4.49)	-2.88 (-2.45)	-4.32 (-3.76)	-5.21 (-4.58)

This table presents the spread portfolio returns of trading strategies associated with insider silence. The spread portfolio longs stocks in which, for each given stock, there is at least one class action occurred in the same state or at least one class action occurred in the same sector last year and shorts stocks in which, for each given stock, there is no class actions either in the same state or in the same sector. Portfolios are formed based on the insider trading activity over the prior 12 months. The “silence” portfolio includes firms with no insider trading activity. The “buy” and “sell” portfolios include firms whose insiders have, on the net, bought and sold shares respectively. For each of the portfolios we calculate their cumulative annualized excess returns (net of T-bill rates) over the holding periods of one, six, and 12 months. The rows “silence – sell” present return spreads between the corresponding portfolios. This table shows the time-series averages of cross-sectional means of the portfolio returns (“Excess Return” in the table). Also reported are the abnormal returns adjusted for the matched portfolios in the spirit of DGTW (1997), in which the matched portfolios are matched with stocks held in the evaluated portfolio on the basis of market capitalization and book-to-market ratios of these stocks (“DGTW” in the table), and the alphas respect to the Fama-French (1993) three-factor model and the extended Fama-French-Carhart six-factor model of the portfolios in which the benchmark model includes Fama-French three factors, the momentum factor, the Pastor and Stambaugh (2003) liquidity risk factor, and the Sadka (2006) liquidity risk factor. The annual alphas are computed as 12 times the monthly estimates. We report Newey-West *t*-statistics in parentheses.

Table 6: Panel Regression Analysis of Litigation Risk Hypothesis

Panel A: The Base Experiment

Variables	1 Month	3 Month	6 Month	1 Year	2 Year
Silence	-0.30 (-4.96)	-1.09 (-10.18)	-2.18 (-14.20)	-4.64 (-19.40)	-4.18 (-16.47)
Buy	0.12 (2.08)	0.21 (2.03)	0.27 (1.83)	0.62 (2.71)	0.71 (3.25)
Market capitalization	-0.04 (-1.70)	-0.14 (-3.40)	-0.21 (-3.89)	-0.46 (-5.62)	-0.46 (-6.02)
Book-to-market	0.27 (5.12)	0.90 (9.74)	1.80 (13.56)	3.20 (17.09)	2.54 (13.95)
Past Return	0.06 (0.47)	0.09 (0.41)	-0.28 (-1.09)	-2.05 (-6.09)	-4.07 (-21.59)
Net Issues	-1.94 (-8.69)	-5.80 (-14.67)	-11.48 (-20.79)	-18.56 (-22.82)	-5.68 (-6.89)
Adj. R-squared	0.16	0.18	0.16	0.13	0.14

Panel B: Quasi-natural Experiments

Variables	1 Month	3 Month	6 Month	1 Year	2 Year
<i>I. The PSLRA of 1995 Experiment</i>					
Silence	-0.20 (-2.30)	-0.84 (-5.45)	-1.87 (-8.21)	-4.04 (-10.89)	-1.40 (-4.12)
Post 95	4.47 (9.00)	12.64 (9.98)	13.68 (5.98)	45.19 (13.51)	-63.19 (-15.15)
Silence × Post 95	-0.20 (-1.95)	-0.54 (-2.57)	-0.75 (-2.44)	-1.39 (-2.82)	-4.54 (-8.93)
<i>II. The UD Law Experiment</i>					
Silence	-0.35 (-5.51)	-1.27 (-11.61)	-2.51 (-15.84)	-5.29 (-21.04)	-4.78 (-17.79)
UD Law	-0.04 (-0.92)	-0.12 (-1.50)	-0.17 (-1.51)	-0.59 (-3.23)	0.15 (0.80)
Silence × UD Law	0.29 (2.11)	1.00 (4.43)	1.83 (5.52)	3.65 (7.49)	3.39 (5.96)
<i>III. The Peer Effect Experiment</i>					
Silence	-0.17 (-2.16)	-0.73 (-5.10)	-1.57 (-7.42)	-3.47 (-10.42)	-1.61 (-5.08)
Peer Event	0.00 (0.03)	-0.42 (-1.26)	-1.41 (-3.14)	-2.48 (-3.74)	-1.35 (-1.81)
Silence × Peer Event	-0.22 (-1.95)	-0.61 (-3.03)	-1.03 (-3.46)	-1.95 (-4.15)	-4.48 (-8.98)

Table 6: Panel Regression Analysis of Litigation Risk Hypothesis, continued

This table presents panel regression results of insider silence where silence is considered independently (Panel A), and where silence is interacted with the proxy on the PSLRA of 1995 (Panel B, Case I), (Panel B, Case II), and the peer event (Panel B, Case III). The dependent variable is the subsequent 1-month, 3-month, 6-month, 1-year, and 2-year cumulative excess return (in percent). The independent variables include insider silence (Silence), insider buy (Buy), log of market capitalization (Market capitalization), log of book-to-market ratio (Book-to-market), past 12-month returns (Past returns), and net stock issues (Net issues). We include the interaction term between Silence and the post 1995 dummy variable (Panel B, Case I), the one between Silence and the UD Law dummy variable (Panel B, Case II), and the one between Silence and the peer event dummy variable (Panel B, Case III) in which the dummy variable on the peer event takes value of one if, for a given stock, there is at least one class action occurred in the same state or at least one class action occurred in the same sector last year. More details on the definitions of the variables can be found in Appendix B. Fixed effects are considered in each model. The robust *t*-statistics in parentheses are accordingly based on clustered standard errors.

Table 7: Insider Silence, Subsequent Securities Class Action Litigation and Stock Price Crash (and Rally)

	Parameter	Estimate	Standard Error	Pr > ChiSq
Panel A: Litigation in the following Month	Intercept	-2.8692	0.3547	<.0001
	Silence over past 1 Year	0.1415	0.0418	0.0007
	Book to Market	0.1469	0.0174	<.0001
	Log(Size)	0.0350	0.0075	<.0001
Panel B: Litigation in the following 6 Months	Intercept	-3.1036	0.1880	<.0001
	Silence over past 1 Year	0.1116	0.0172	<.0001
	Book to Market	0.1501	0.0076	<.0001
	Log(Size)	0.0323	0.0031	<.0001
Panel C: Litigation in the following Year	Intercept	-3.3039	0.1766	<.0001
	Silence over past 1 Year	0.1073	0.0124	<.0001
	Book to Market	0.1488	0.0053	<.0001
	Log(Size)	0.0312	0.0022	<.0001
Panel D: Price crash in the following month	Intercept	-2.5688	0.1878	<.0001
	Silence over past 1 Year	0.0478	0.0271	0.0772
	Book to Market	0.1375	0.0131	<.0001
	Log(Size)	0.0227	0.0055	<.0001
Panel E: Price crash in the following 6 months	Intercept	-2.5419	0.0871	<.0001
	Silence over past 1 Year	0.0513	0.0125	<.0001
	Book to Market	0.1314	0.0061	<.0001
	Log(Size)	0.0241	0.0026	<.0001
Panel F: Price crash in the following year	Intercept	-2.6641	0.0641	<.0001
	Silence over past 1 Year	0.0765	0.0081	<.0001
	Book to Market	0.1170	0.0038	<.0001
	Log(Size)	0.0232	0.0017	<.0001
Panel G: Price rally in the following month	Intercept	-2.5272	0.1631	<.0001
	Silence over past 1 Year	0.0101	0.0222	0.6481
	Book to Market	0.1121	0.0097	<.0001
	Log(Size)	0.0056	0.0047	0.2335
Panel H: Price rally in the following 6 months	Intercept	-2.5899	0.0824	<.0001
	Silence over past 1 Year	-0.0005	0.0105	0.9658
	Book to Market	0.1058	0.0044	<.0001
	Log(Size)	0.0089	0.0022	<.0001
Panel I: Price rally in the following year	Intercept	-2.5460	0.0505	<.0001
	Silence over past 1 Year	0.0100	0.0066	0.1341
	Book to Market	0.1063	0.0028	<.0001
	Log(Size)	0.0076	0.0014	<.0001

This table reports the probit regressions results on the prediction that firms have securities class action litigation in the following month (Panel A), following 6 months (Panel B), following year (Panel C), or whether their stock price crashes in the following month (Panel D), following 6 months (Panel E), or following year (Panel F), or whether their stock price rallies in the following month (Panel G), following 6 months (Panel H), or following year (Panel I). At the end of each month from January 1997 through December 2012, we first identify firms classified as “litigated” if it is recorded in the securities class action litigation of the Stanford and Cornerstone Research database. Industry (Fama-French 49 industries) fixed effects are considered.

Table 8: Alternative Hypotheses

Panel A: Proportion of silent firms (silence defined by no insider trading activity over the past 12 months)

	Small	Medium	Large
Number of insiders	20%	8%	7%
Firm size (NYSE size decile 3 and above)	—	14%	10%

Panel B: Lack of Insider

Variables	1 Month	3 Month	6 Month	1 Year	2 Year
<i>Sample excluding firms with small number of insiders</i>					
Silence	-0.37 (-4.73)	-1.26 (-9.47)	-2.52 (-12.86)	-5.41 (-17.95)	-5.31 (-16.08)
Buy	0.15 (2.22)	0.30 (2.47)	0.47 (2.66)	1.11 (4.00)	0.92 (3.47)
Market Capitalization	-0.05 (-1.73)	-0.15 (-3.19)	-0.22 (-3.51)	-0.57 (-5.89)	-0.51 (-5.70)
Book-to-market	0.26 (4.11)	0.86 (7.97)	1.78 (11.44)	3.17 (14.80)	2.54 (11.78)
Past Return	0.18 (1.17)	0.44 (1.72)	0.29 (0.88)	-1.49 (-3.42)	-4.59 (-21.29)
Net Issues	-1.95 (-7.95)	-5.91 (-13.77)	-11.87 (-19.58)	-19.12 (-20.75)	-4.18 (-4.43)
Adj. R-squared	0.17	0.18	0.16	0.13	0.14
<i>Sample excluding firms with size below NYSE median</i>					
Silence	-0.20 (-2.63)	-0.69 (-5.27)	-1.25 (-6.65)	-2.93 (-10.44)	-3.66 (-11.64)
Buy	0.05 (0.74)	0.10 (0.78)	0.22 (1.25)	0.76 (2.89)	0.45 (1.85)
Market Capitalization	-0.05 (-1.66)	-0.21 (-4.41)	-0.44 (-6.39)	-1.03 (-8.91)	-0.97 (-10.16)
Book-to-market	0.25 (3.98)	0.84 (7.41)	1.61 (10.01)	2.88 (13.10)	2.05 (10.41)
Past Return	-0.02 (-0.07)	0.12 (0.38)	0.02 (0.05)	-1.67 (-3.37)	-4.71 (-19.42)
Net Issues	-1.79 (-6.59)	-5.64 (-11.96)	-10.54 (-15.91)	-16.29 (-16.08)	-2.45 (-2.41)
Adj. R-squared	0.18	0.20	0.18	0.15	0.17

Table 8: Alternative Hypotheses, continued

Panel C: Trading Constraint

Variables	1 Month	3 Month	6 Month	1 Year	2 Year
Silence	-0.30 (-4.96)	-1.10 (-10.23)	-2.17 (-14.14)	-4.63 (-19.40)	-4.20 (-16.41)
Silence \times Past Sale	-0.51 (-0.28)	-3.72 (-1.29)	1.65 (0.42)	-4.98 (-0.82)	-15.99 (-1.33)
Past Sale	0.05 (0.30)	-0.08 (-0.33)	-0.36 (-0.90)	-1.03 (-1.78)	-2.36 (-3.43)
Buy	0.12 (2.08)	0.21 (2.03)	0.28 (1.84)	0.62 (2.74)	0.72 (3.29)
Market Capitalization	-0.04 (-1.70)	-0.14 (-3.39)	-0.20 (-3.88)	-0.46 (-5.60)	-0.46 (-5.94)
Book-to-market	0.27 (5.11)	0.90 (9.74)	1.80 (13.55)	3.20 (17.08)	2.55 (13.94)
Past Return	0.06 (0.47)	0.09 (0.41)	-0.28 (-1.09)	-2.06 (-6.11)	-4.07 (-21.60)
Net Issues	-1.94 (-8.68)	-5.80 (-14.67)	-11.46 (-20.75)	-18.56 (-22.81)	-5.73 (-6.94)
Adj. R-squared	0.16	0.18	0.16	0.13	0.14

Panel D: IPO lockup hypothesis

Variables	1 Month	3 Month	6 Month	1 Year	2 Year
Silence	-0.30 (-4.98)	-1.09 (-10.23)	-2.19 (-14.24)	-4.65 (-19.47)	-4.18 (-16.44)
Buy	0.12 (2.07)	0.21 (2.02)	0.27 (1.81)	0.62 (2.72)	0.71 (3.25)
Market Capitalization	-0.04 (-1.69)	-0.14 (-3.40)	-0.20 (-3.89)	-0.46 (-5.63)	-0.46 (-6.04)
Book-to-market	0.27 (5.12)	0.90 (9.73)	1.79 (13.55)	3.20 (17.06)	2.53 (13.94)
Past Return	0.06 (0.47)	0.09 (0.41)	-0.28 (-1.09)	-2.06 (-6.11)	-4.07 (-21.60)
Net Issues	-1.94 (-8.67)	-5.80 (-14.66)	-11.45 (-20.72)	-18.54 (-22.78)	-5.71 (-6.92)
Adj. R-squared	0.16	0.18	0.16	0.13	0.14

Table 8: Alternative Hypotheses, continued

Panel E: No equity compensation hypothesis

Variables	1 Month	3 Month	6 Month	1 Year	2 Year
Silence	-0.26 (-3.02)	-0.78 (-5.35)	-1.50 (-7.08)	-3.15 (-9.58)	-5.43 (-13.93)
Buy	0.11 (1.39)	0.28 (2.03)	0.35 (1.74)	0.86 (2.95)	1.02 (3.10)
Market Capitalization	-0.09 (-3.64)	-0.30 (-6.95)	-0.51 (-8.58)	-0.94 (-9.75)	-0.81 (-8.90)
Book-to-market	0.11 (1.77)	0.43 (4.03)	0.91 (6.02)	1.73 (8.15)	1.72 (8.85)
Past Return	0.03 (0.17)	0.11 (0.44)	-0.17 (-0.44)	-2.31 (-4.48)	-4.67 (-13.45)
Net Issues	-1.61 (-5.54)	-4.82 (-9.80)	-9.31 (-13.42)	-14.73 (-14.44)	-2.32 (-2.28)
Adj. R-squared	0.19	0.21	0.20	0.17	0.18

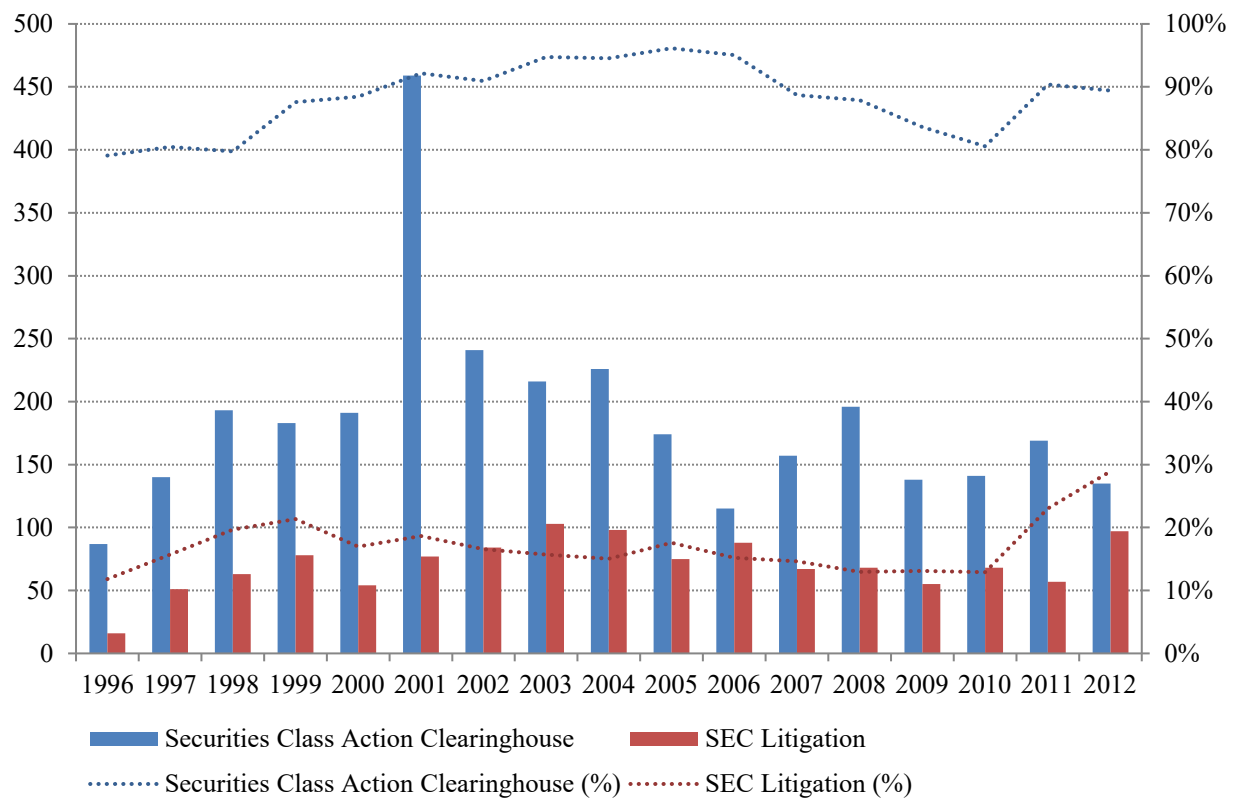
Panel A presents the proportion of insider-silence firms among firms with small (bottom third), medium (middle third) and large (upper third) number of insiders, and among firms with small, medium and large firm size. Throughout our analysis in the paper, we exclude small firms with size below NYSE decile 3. The medium-size firms have size between NYSE deciles 3 and 5; the large-size firms have size above NYSE decile 5. Panel B presents subsample panel regression results where 1) firms with small number of insiders are excluded, and 2) firms with size below NYSE median are excluded. In each model, a constant term is included but not reported. Panel C presents the panel regression results where Silence is interacted with Past Sale. “Past Sale” is defined as net insider sales over the second past one-year period ($t - 24$, $t - 13$), relative to the current month t . Panels D and E presents the panel regression results with subsample of firms so as to investigate the IPO lockup and no equity compensation hypothesis. In Panel D, we exclude IPO firms with more than 30-month lockup period. In Panel E, we exclude firms that are covered in Compustat ExecuComp database and have no equity compensation (the sample includes an average of 1,153 firms for a total of 228 months from January 1994 through December 2012). In each model, the dependent variable is the future one-year cumulative excess return (in percent). The independent variables include insider silence (Silence), insider buy (Buy), log of market capitalization (Market capitalization), log of book-to-market ratio (Book-to-market), past 12-month returns (Past returns), and net stock issues (Net issues). More details on the definitions of the variables can be found in Appendix B. A constant term is included but not reported. The robust t -statistics in parentheses are accordingly based on clustered standard errors.

Table 9: Length of Insider Silence and Return Predictability

Variables	3 months	6 months	12 months	18 months	24 months
Silence	-0.04 (-1.03)	-0.20 (-4.15)	-0.30 (-4.96)	-0.33 (-5.71)	-0.38 (-5.06)
Buy	0.18 (3.20)	0.16 (2.90)	0.12 (2.08)	0.13 (2.29)	0.13 (2.32)
Market Capitalization	-0.04 (-1.50)	-0.04 (-1.70)	-0.04 (-1.70)	-0.04 (-1.73)	-0.04 (-1.57)
Book-to-market	0.26 (4.88)	0.26 (5.05)	0.27 (5.12)	0.27 (5.14)	0.26 (5.06)
Past Return	0.06 (0.48)	0.06 (0.46)	0.06 (0.47)	0.06 (0.47)	0.06 (0.49)
Net Issues	-1.94 (-8.69)	-1.94 (-8.69)	-1.94 (-8.69)	-1.94 (-8.68)	-1.94 (-8.66)
Adj. R-squared	0.16	0.16	0.16	0.16	0.16

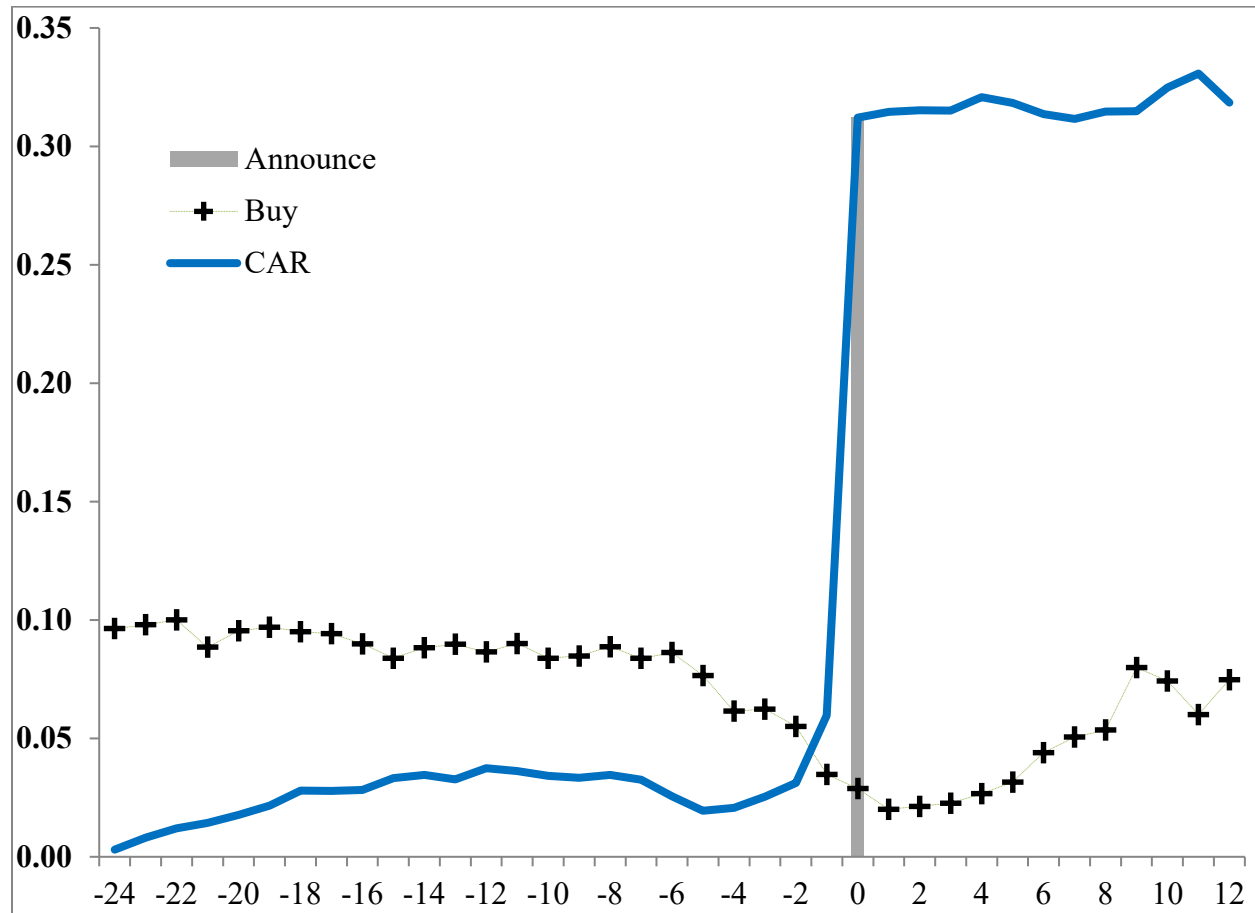
Table 9 presents the panel regressions associated with insider silence activities measured over different time horizons. Each column represents a panel regression, in which the dependent variable is the future one-month excess return (in percent). Column head specifies the length (number of months) of time over which past insider trading activity is measured. The data sample includes observations from January 1990 through December 2012. The independent variables include insider silence (Silence), insider buy (Buy), log of market capitalization (Market capitalization), log of book-to-market ratio (Book-to-market), past 12-month returns (Past returns), and net stock issues (Net issues). Constant terms are estimated but not reported. More details can be found in Appendix B. Fixed effects are considered in each model. The robust *t*-statistics in parentheses are accordingly based on clustered standard errors.

Figure 1: Insider Trading: Securities Class Action Clearinghouse vs. SEC litigation



This figure plots the number and the percentage out of the entire sample of insider trading cases in the Stanford Securities Class Action Clearinghouse filed in U.S. federal court and those in the SEC litigation dataset by year. The summary statistics are based on the actual filing documents on each of the cases. It is based on the specific rules that the claims arose under.

Figure 2: Cumulative Abnormal Returns and Insider Net Buying in Merger Targets



The sample includes 4,267 targets involved in merger deals announced during 1992–2010. Month 0 is the announcement month. The graph shows the proportions of targets whose insiders buy in net (Buy) from 24 months before to 12 months after the announcement. The thick solid line draws the cumulative abnormal returns.

Appendix A: Text Analysis on the Class Action Lawsuits and the SEC Litigation Cases

In particular, we use the Securities Class Action Clearinghouse (SCAC) file at Stanford University to identify every securities class action suit meeting the filing date criteria. In each of these 3,561 cases, we examine the following documents in the court record for each case: (1) Case Summary; (2) Company & Securities Information, “company” information provides the industry and sector classification and headquarters state for the primary company-defendant in the litigation, and “securities” information provides the ticker symbol, market, and market status for the underlying securities at issue in the litigation; (3) all filings for First Identified Complaint (FIC); (4) all filings for Reference Complaint (REF); and (5) Related District Court Filings.

While Case Summary (item 1 above) offers a brief case discussion, it does not always specify if the allegation involves any insider trading or if the securities law violation is related with illegal insider trading. Hence, we conduct a much broader search on the comprehensive filing data (items 1, 2, 3, 4, and 5 above).

Filings in (3) , (4) and (5) include but are not limited to class action complaint for violation of the federal securities laws, the U.S. District Court Civil Docket, all motions for appointment of lead plaintiff and lead counsel, supporting memoranda, affidavits, declarations, order transferring action documents, court orders appointing lead plaintiffs and approving lead counsel, any filed objections and responses to the settlement, court orders granting final approval to the settlement, and filed documents of dismissal.

We consider a case to be related to insider trading if any of the document includes Section 20(a) in or SEC rule 10b-5, or it specifically mentions insider trading. As a background, Securities Exchange Act of 1934 was the first law against insider trading. The Act had given the U.S. Securities and Exchange Commission (SEC) the authority to set the rules. Section 20(a), in particular, of the act provides a private right of action against persons engaged in insider trading. Rule 10b-5 is the SEC’s principal instrument to launch investigations of insider trading cases. This rule prohibits not only insider trading involved in listed companies, but also any act or omission resulting in fraud or deceit in connection with the purchase or sale of any security.

We employ a machine learning algorithm to search for specific description on illegal insider trading activities in the filing documents. As a way to cross-check the quality of the algorithm, we employ research assistants to manually go through random samples of cases. We find the results are robust. We apply the same search rule to the SEC litigation dataset.

Appendix B: Variable Definitions

The data sources are the Center for Research in security Prices (CRSP), Compustat, Thomson Reuters Insider Filing Data Feed, and 13f. Time t in Compustat refers to fiscal year end in calendar year t . The main variables are defined below.

Buy:	Equal to one if NID over the past 12 months is positive, zero otherwise.
Cumulative excess return:	Monthly excess return (net of the three-month T-bill rate) is calculated first for each stock. For each stock/month, the one-year cumulative excess return is the cumulative excess return over the one-year period from months 1 to 12.
Book-to-market:	Book to market ratio, the natural log of the ratio of the book value of equity to the market value of equity. Book value B is total assets (Compustat item AT) for year $t-1$, minus liabilities (LT), plus balance sheet deferred taxes and investment tax credit (TXDIC) if available, minus preferred stock liquidating value (PSTKL) if available, or redemption value (PSTKRV) if available, or carrying value (PSTK). Market value M is price times share outstanding at the end of December of $t-1$, from CRSP.
Market capitalization:	Market capitalization, the natural log of price times number of shares outstanding at the end of June of year t , from CRSP.
Market volatility:	Standard deviation of the value-weighted CRSP market return over the prior 252 trading days.
Net issues:	Net stock issues, the natural log of the split-adjusted shares outstanding at the fiscal year end in $t-1$ minus the natural log of the split-adjusted shares outstanding at the fiscal year end in $t-2$. The split-adjusted shares outstanding is CSHO times AJEX in Compustat.
NID:	Net insider demand, NID of month j is defined as the number of shares that insiders buy minus the number of shares that insiders sell over the past six months, normalized by the total number of shares outstanding at the end of month $j-1$.
Past Return:	The buy-and-hold return from month $j-12$ to $j-1$, where $j-1$ is the month of portfolio formation and j is the first month of forecasted stock returns. This variable is monthly rebalanced.
Past Sale:	Net insider sales over the one-year period from -24 to -13 months, relative to the current month.
Silence:	Equal to one if there is no insider trading activity over the past 12 months, and zero otherwise.