Information Asymmetry and Insider Trading*

Wei Wu
Texas A&M University

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Abstract

I investigate the impact of information asymmetry on insider trading by exploiting a quasi-experimental design: the brokerage closure-related terminations of analyst coverage, which exogenously increase the information asymmetry of the affected firms. Using a difference-in-differences approach, I find that after the terminations of analyst coverage, corporate insiders obtain significantly higher abnormal returns and enjoy larger abnormal profits. The magnitudes of the increase are large economically. For firms with five or fewer analysts, losing one analyst increases the six-month abnormal returns by 16.0% for insider purchases, and by 10.7% for insider sales (both in absolute terms). My paper highlights the role of information asymmetry as a critical determinant of insiders' abnormal profits, and calls for regulatory attention to corporate insiders' transactions associated with high levels of information asymmetry.

JEL classification: G12, G14, G18, G24, G30, G38

Keywords: information asymmetry, insider trading, analyst coverage, informed trading models, abnormal returns, enforcement, difference-in-differences.

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1. Introduction

Informed traders (e.g., hedge funds and corporate insiders) in the financial market have better information regarding the traded assets than uninformed traders (e.g., retail investors). The informational advantage of the informed traders over uninformed traders is termed information asymmetry in this paper. Informed traders exploit information asymmetry through their transactions. During this process, they impound their private information into asset prices and can make the capital markets more efficient. Thus, investigating the impact of information asymmetry on the behavior and outcome of informed trading can help researchers better understand the price discovery process. It can also help investors evaluate the performance of active management as we would like to know how much abnormal returns one can earn if he or she possesses valuable private information. On the other hand, because informed traders obtain abnormal profits at the expense of uninformed traders, large abnormal profits of certain types of informed traders (such as corporate insiders and government employees) can raise alarm regarding fairness and integrity of the financial market, and thus discourage capital market participation.[1] Therefore, understanding the relation between information asymmetry and informed trading is also of interest to policymakers who aim to preserve market integrity.

The theoretical literature has made substantial progress in characterizing the trading behavior of informed traders (e.g., Grossman and Stiglitz 1980, Kyle 1985, Copeland and Galai 1983, Spiegel and Subrahmanyam 1992, Back 1992). For example, Kyle's seminal model predicts a positive relation between information asymmetry and the abnormal profits of informed traders. However, testing this relation is an empirical challenge. This is because information asymmetry is time varying and more importantly unobservable. Previous studies have relied on proxies for information asymmetry, such as the level of institutional ownership and the number of analysts covering a stock, to study the correlation between information asymmetry and insider trading. These studies (e.g., Huddart and Ke, 2007) have reported mixed results across proxies. Moreover, for a given proxy, the results are often inconsistent across insider purchases and sales samples. Up to now, our understanding regarding the relation between information asymmetry and insider trading remains limited.

A critical problem associated with the proxies for information asymmetry is the omitted variables issue. In particular, both the proxies for information asymmetry and the outcome and

[1] See Leland (1992) and Bhattacharya (2014) for the commonly argued pros and cons of insider trading.

behavior of insider trading can be driven by private information regarding the prospects of the firms. For instance, studies have used the number of analysts as a proxy for information asymmetry because research analysts are shown to be an important information source for outsiders. Analysts analyze, interpret, and disseminate information to capital market participants, and thus help reduce the informational advantage of the insiders (Womack 1996, Barber et al. 2001, Gleason and Lee 2003, Jegadeesh et al. 2004, Brown et al. 2014). However, variation in the number of analysts such as the termination of an existing coverage or the initiation of a new coverage is likely influenced by the analysts' private information about the prospects of the covered firm. Meanwhile, insiders can trade based on their private information about the firms' prospects. Therefore, an OLS regression between the number of analysts and insiders' abnormal returns will yield biased estimates because the omitted variable, the prospects of the covered firms, is correlated with both the proxy and insiders' returns.

To deal with the endogeneity problem, I exploit a quasi-experimental design and use a difference-in-differences (DiD) approach to establish a causal link between the number of analysts and insider trading. The identification strategy I use in this paper relies on the closure-related coverage terminations, which are reductions of analyst coverage due to the fact that 43 brokerage firms close their research departments between 2000 and 2008. Unlike typical changes in analyst coverage, closure-related terminations are driven by the unfavorable economic condition of the brokerage firms and are shown to be neither economically nor statistically related to the subsequent performance of the covered stocks (Hong and Kacperczyk 2010, Kelly and Ljungqvist 2012). Therefore, closure-related terminations of analyst coverage increase the informational advantage of the insiders exogenously, and thus provide me with a clean environment to identify the causal impact of coverage reduction on insider trading.

I merge the corporate insider trading data with the closure-related coverage termination data to construct the sample in my study. I focus on corporate insiders because they are a group of informed traders who possess firm-specific private information and meanwhile are required to disclose their transactions. Because the coverage terminations have stronger impact on information asymmetry in firms with lower levels of initial coverage[2], I focus on the subsample

[2] For example, losing one analyst in a firm with three analysts prior to the coverage reductions is much more likely to have a strong impact on firms' information environment compared to losing one analyst in a firm with 20 analysts prior to the coverage reductions.

with treated firms that have five or fewer analysts prior to the coverage reductions in most analysis of my paper.[3]

I first examine the changes in insiders' abnormal returns around the terminations of analyst coverage. Consistent with the predictions of various informed-trading models (e.g., Grossman and Stiglitz 1980, Kyle 1985, Copeland and Galai 1983, Spiegel and Subrahmanyam 1992, Back 1992), I find insiders' abnormal returns increase significantly after terminations of analyst coverage. This increase takes place in both the insider purchases sample and insider sales sample, in which the changes in stock abnormal returns exhibit opposite signs. The six-month cumulative abnormal returns increase by 16.0% in absolute terms following the terminations of analyst coverage in the insider purchases sample, suggesting that insiders enjoy higher abnormal returns from their purchases, whereas the six-month cumulative abnormal returns decrease by 10.7% in absolute terms following the terminations of analyst coverage in the insider sales sample, suggesting that insiders avoid more losses from their sales. These results are robust to the inclusion of firm fixed effects (or firm × insider fixed effects), transaction date fixed effects, and control variables, indicating the treatment effects are not due to systematic differences in firms, insiders, transaction dates, or control variables. The large magnitude of the treatment effects highlights the time-varying feature of insiders' abnormal returns and indicates that information asymmetry is a critical determinant of insiders' abnormal returns.

To better understand the source of the treatment effects, I systematically examine the change of insiders' abnormal returns cumulated in different time windows. Consistent with previous studies (Bettis, Vickrey, and Vickrey 1997, Lakonishok and Lee 2001), I found stock prices adjust slowly following insiders' trades. The majority of the increase in insiders' abnormal returns comes after the filing dates of their transactions. A significant portion of the increase in insiders' abnormal returns is concentrated in narrow time windows surrounding the release of corporate news such as earnings announcements and 8-K filings. This suggests the edge insiders have over uninformed traders lies in their firm-specific private information.

[3] In section 4.1, I relax this constraint and provide heterogeneity test across the levels of the initial coverage. I show the treatment effects are much weaker in firms with more than five analysts prior to the coverage terminations. Except for section 4.1, all analysis in my paper are performed in the subsample with treated firms that have five or fewer analysts prior to the coverage reductions.

After documenting the impact of coverage reductions on insiders' abnormal returns, I present evidence that shows the heterogeneity in the treatment effects. The increase in insiders' abnormal returns is more pronounced in firms with fewer analysts covering the firm, corroborating the identification strategy by showing that insider trading responds to larger percentage drops in analyst coverage.[4] The increase in insiders' abnormal returns is stronger in firms with a higher percentage of insiders that exhibit opportunistic trading patterns, whose trades are more likely driven by their private information. Moreover, background risks and the associated desires of diversification shape insiders' trading behavior. Insiders who have accumulated large positions of their own firm are much less likely to take advantage of the increase in information asymmetry through purchases, but are more likely to do so through sales. Finally, regulatory attention can shape insiders' abnormal returns. The increase in insiders' abnormal returns is much lower in time periods with higher intensity of legal enforcement, suggesting that insiders are concerned about litigation risks associated with their transactions.

I perform a range of robustness checks to confirm the validity of the empirical tests. First, I study the dynamics of the treatment effects. I confirm that no pre-trends are present in either the insider purchases sample or the insider sales sample. I also show that the duration of the treatment effects depends on the recovery pattern of the number of analysts. The increase in insiders' abnormal returns decays away six months after the coverage reductions in firms whose number of analysts rebounds rapidly. Next, I construct portfolios consisting of insiders' transactions and examine their performance. Consistent with the DiD analysis at the transaction level, I find the alphas of the insider-purchases portfolios increase significantly, whereas the alphas of the insider-sales portfolios decrease significantly after the terminations of analyst coverage. Finally, I confirm the treatment effects are robust to alternative measures of abnormal returns, the inclusion of liquidity measures, and the exclusion of tiny firms and low-price transactions, whereas they disappear in the placebo tests in which I falsely shift the termination dates or replace the treated firms with similar control firms.

Terminations of analyst coverage also alters insiders' trading behavior in both the intensive and extensive margins. In particular, I find insiders' trading volume, transaction value, and trading probability for liquid stocks increase significantly after the terminations of analyst

[4] I also use a specification that parametrically adjusts the treatment intensity by assuming the increase in information asymmetry is inversely proportional to the amount of initial coverage. I run this specification in the full sample and the results are consistent to those in the baseline analysis.

coverage. These results are consistent with the price-taking models (e.g., Grossman and Stiglitz 1980), which assume insiders' transactions have little influence on the stock prices. For illiquid stocks, I observe no significant changes in the insiders' trading volume, transaction value, and trading probability in response to the increase in information asymmetry. These results are more consistent with the imperfect-competition models (e.g., Kyle 1985, Copeland and Galai 1983, Spiegel and Subrahmanyam 1992), which take the price impact of insider transactions into consideration and hence predict little to no change in the expected trade size despite an increase in information asymmetry.

To assess the economic losses that outsiders can incur when information asymmetry increases, I estimate the change in insiders' abnormal profits after the terminations of analyst coverage. Conditional on holding his or her position for six months for all trades within the one-year post-termination period, an average insider makes $87,444 more profits from purchases, and avoids $896,916 more losses from sales. These changes in the insiders' abnormal profits are economically sizable compared to their compensation.[5] In fact, they are comparable to the abnormal profits in the illegal insider trading cases.[6] Despite their easy access to non-public information, corporate insiders have not been the primary targets of legal investigations.[7] My analysis calls for regulatory attention to the corporate insiders' transactions, especially for those associated with high levels of information asymmetry.

I then discuss two channels that can both lead to increase in information asymmetry after coverage reductions. The first channel is "information provider" channel. In this channel, information asymmetry increases because important information that would otherwise have been transmitted to investors by analysts is lost. I present evidence supporting this explanation. I show the precision of analysts' forecasts deteriorates after coverage reductions, which is consistent with Hong and Kacperczyk (2010), who find similar results in the merger-related terminations sample. Moreover, the increase in insiders' abnormal returns is stronger for firms

[5] According to ExecuComp data, the median total compensation of the top five executives is $778,686 for the treated firms in the insider purchases sample, and $930,112 for the treated firms in the insider sales sample.

[6] Del Guercio, Odders-White, and Ready (2013) show that, the mean of the profits associated with the illegal trading cases is $519,116 per trader, whereas the median of the profits is $61,189 per trader (both in 2011 dollars) during fiscal year 2003 to 2007.

[7] In all cases prosecuted by the SEC, only around 20% of defendants are employees of stocks they traded, most of which are not those subject to the filing requirement of the SEC (Del Guercio, Odders-White, and Ready 2013). In fact, none of the insiders in my data have been prosecuted by the SEC up till today.

that experience a larger reduction in the precision of the analysts' forecasts, supporting the hypothesis that analysts provide information to outside investors and hence reduce the informational advantage of insiders. The second channel is "discipline" channel. In this channel, analysts act as "insider trading police" and deter insiders from trading aggressively on their private information. After coverage reductions, the tradable information set of insiders expands, which effectively enlarges the informational advantage of the insiders over outsiders. I provide evidence showing the discipline channel *alone* cannot fully rationalize the data. Specifically, I show the performance of other informed agents (e.g., active mutual funds) that are not subjected to the governance of analysts also improves after coverage reductions, a result that is more consistent with the "information provider" channel.

One potential concern of my paper is there exists alternative explanations. It is possible that the increase of insiders' abnormal returns is not directly driven by the increase of information asymmetry, but instead by other changes caused by coverage reductions. Without specifying the alternative explanations, I cannot dismiss this possibility entirely. However, two results in my paper help to alleviate this concern. First, I find after coverage reductions the stock abnormal returns following insiders' transactions increase in the insider purchases sample while they decrease in the insider sales sample. Many alternative explanations, such as changes of risk premium, struggle to simultaneously explain this bidirectional changes in stock returns.[8] Second, I find the majority of the changes in insiders' abnormal returns concentrates in narrow time window surrounding the release of corporate news. This result supports information-based explanations but is at odds with many alternative explanations. For example, if the changes in insiders' abnormal returns were due to increase in firms' risks after coverage reductions, we would expect to observe gradual drifts in insiders' abnormal returns.[9]

My paper contributes to the literature that studies the relation between information asymmetry and insider trading. Aboody and Lev (2000) show that insiders from firms with higher R&D spending gain larger abnormal returns. Huddart and Ke (2007) study the relation between proxies of information asymmetry and insiders' abnormal returns. Although these

[8] For example, if increase of liquidity risk premium is the main driver for the results, one would expect to observe increase of stock abnormal returns in both the insider purchases sample and insider sales sample.

[9] The risk-premium based explanations are further weakened by robustness tests where I show the changes in insiders' abnormal returns are robust in the DiD specifications with liquidity controls and they are robust to the portfolio analysis in which I control for the risk exposure separately for time periods both before and after the coverage reductions.

papers study the cross-sectional correlation between information asymmetry and insiders' abnormal returns, the proxies they use are likely subject to omitted variable concerns. I overcome the endogeneity challenge by examining the impact of closure-related terminations of analyst coverage, which increases the information asymmetry exogenously. I show that after the terminations of analyst coverage, insiders' abnormal returns and profits within the same firms or the same firm-insider pairs can increase significantly, whereas the trading behavior of insiders from liquid firms can change in both the intensive margin and extensive margin. These results are consistent with a large body of theoretical research that models the trading behavior of informed investors (Grossman and Stiglitz 1980, Kyle 1985, Copeland and Galai 1983, Spiegel and Subrahmanyam 1992, Back 1992), and thus indicate the descriptive validity of the theory applied to corporate insiders' trades. A recent working paper (Ellul and Panayides 2016) also examines insiders' profitability after terminations of analysts' coverage. Consistent with my paper, they find insiders' profitability increases after firms their analysts. My paper differentiates with Ellul and Panayides (2016) in two dimensions. First, I focus on the exogenous reduction of analyst coverage due to brokerage closure and mergers, while Ellul and Panayides (2016) focus on the complete coverage terminations (i.e. firms lose all their analysts) which can potentially be driven by analysts' private information of the firms' future prospects. Second, besides examining the changes of insiders' abnormal returns, I also systematically study the sources of insiders' informational advantage, examine the changes of insiders' trading volume and abnormal profits, and differentiate channels via which analyst coverage reductions affect insider trading.

This paper also adds to the insider trading literature regarding the magnitude and source of corporate insiders' abnormal returns. Although much empirical work has examined the trading behavior of corporate insiders, the literature has focused on the average returns of the corporate insiders, and has in general reported small abnormal returns for insider purchases and zero return for insider sales (e.g., Seyhun 1986, Jeng, Metrick, and Zeckhauser 2003). Moreover, demonstrating whether insiders obtain their abnormal returns by trading on private information or simply by acting as contrarian investors has also been difficult (Rozeff and Zaman 1998, Lakonishok and Lee 2001, Ke, Huddart, and Petroni 2003, Piotroski and Roulstone 2005). In contrast to previous work, my paper highlights the time-varying nature of insiders' abnormal returns. I find that information asymmetry is a critical determinant of insiders' abnormal profits. Within the same firm or even the same firm-insider pair, the level of insiders' abnormal returns for both purchases and sales can increase by more than 10% in absolute terms within a short time window after losing one analyst, a result that has important implications for both trading and regulatory purposes. Moreover, I show that the increase in insiders' abnormal returns is associated with the release of corporate news such as earnings announcements and 8-K filings, which provides evidence showing that insiders obtain abnormal returns by trading on their private information, rather than simply by acting as contrarian investors.

Finally, my paper is also related to a growing body of literature that uses closure-related terminations of analyst coverage (or merger-related coverage reductions alone) as exogenous shocks to firms' information environment. This literature has studied the impact of coverage reductions on security analyst reporting bias (Hong and Kacperczyk 2010), credit ratings (Fong et al. 2011), asset pricing (Kelly and Ljungqvist 2012), cost of debt (Derrien, Kecskes, and Mansi 2012), corporate investment and financing policies (Derrien and Kecskes 2013), corporate disclosure (Balakrishnan et al. 2012, Irani and Oesch 2013), and corporate governance (Chen, Harford, and Lin 2013). My paper adds to this new strand of literature by investigating the impact of coverage reductions on insider trading.

The remainder of the paper is organized as follows. Section 2 describes the data and empirical design; section 3 illustrates the impact of the terminations of analyst coverage on insiders' abnormal returns; section 4 explores the heterogeneity in the treatment effects; section 5 provides a set of robustness checks; section 6 analyzes the impact of the terminations of analyst coverage on insiders' trading behavior and abnormal profits; section 7 discusses economic channels, alternative explanations, and regulatory implications; and section 8 concludes.

2. Data and Empirical Design

2.1. Closure-related Terminations of Analyst Coverage

The identification strategy of this paper is the closure-related termination of analyst coverage. My data set of closure-related terminations is identical to the one in Kelly and Ljungqvist (2012). The reduction of analyst coverage is a consequence of 43 brokerage firms closing their research departments between 2000 and 2008, resulting in a total of 4,429 coverage terminations, which affects 2,180 unique stocks. The data contain two types of coverage terminations. The first type of coverage termination is due to stand-alone brokerage closures, which account for 22 brokerage closures and more than 60% of the total coverage terminations. The second type of coverage termination occurs in the wake of brokerage mergers, similar to what is described in Hong and Kacperczyk (2010).[10]

Unfavorable economic conditions and regulatory changes in 2000s drive the closures and mergers of the brokerage firms. Research departments in the brokerage firms are cost centers. Because it is difficult to keep research reports as private information, research reports are usually provided to the clients for free by the brokerage firms. The research department are subsidized by revenue from trading activities ("soft dollar commissions"), market-making activities, and investment banking departments. Since the early 2000s, all three revenue sources have shrunk: soft dollar commissions came under attack from both the SEC and institutional clients; market-making revenue decreased because of competition for order flow; and new regulations (e.g., 2003 Global Settlement) made it difficult for brokers to use investment banking revenue to cross-subsidize research. As a result of the worsening economic condition, many brokerage firms exited the equity research industry. Unlike typical changes in the analyst coverage, closure-related terminations of analyst coverage have no predictive power over subsequent earnings surprises of the covered stocks (Kelly and Ljungqvist 2012).

Closure-related terminations of analyst coverage are also shown to increase the level of information asymmetry. Kelly and Ljungqvist (2012) show that the bid-ask spreads of the affected firms increase significantly after coverage reductions. Johnson and So (2014) develop a multimarket measure of information asymmetry (MIA) with many desirable empirical properties. They show that MIA increases significantly after closure-related terminations. Moreover, consistent with the impact of an increase in information asymmetry, closure-related terminations are shown to worsen stock liquidity (Kelly and Ljungqvist 2012), increase the cost of capital, and reduce firm investment and financing activities (Derrien and Kecskes 2013). Taken together, closure-related terminations of analyst coverage provide plausibly exogenous shocks to firms' information environment and therefore serve as a clean quasi-experimental design to study the relation between information asymmetry and insider trading.

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[10] The merger-related coverage termination can be further categorized into two types. In the first type of coverage termination, the affected stock is covered by both brokers before the merger, but is covered by only one analyst after the merger. This type of coverage termination is included in my sample. In the second type of coverage termination, the affected stock is covered by both brokers before the merger, but is not covered by the surviving broker after the merger. This type of coverage termination can be endogenous and thus is excluded from my sample. The findings in my paper are qualitatively similar if I only include the terminations due to the stand-alone closures and exclude all the merger-related terminations.

2.2. Sample Construction

Corporate insiders are defined broadly to include those that have "access to non-public, material, insider information," and they include officer,[11] directors, and any beneficial owners of more than ten percent of a class of the company's equity securities registered under Section 12 of the Securities Exchange Act of 1934. Corporate insiders are required to file the SEC forms 3, 4, and 5 when they trade their companies' stocks.[12] The insider trading data are collected from the Thomson Reuters Insiders Filings Database, which is designed to capture all corporate insider activities as reported on the SEC forms 3, 4, and 5. I exclude insider transactions that are not common stocks (share codes other than 10 or 11).

I merge the insider trading data with the closure-related terminations data, and construct both the insider purchases and inside sales samples containing insider transactions around the termination dates of analyst coverage. Treated firms are firms that experience closure-related terminations of analyst coverage. I match each treated firm with up to five control firms that do not experience coverage reductions one year before and after the termination dates of the treated firm. Following Kelly and Ljungqvist (2012), I require the control firms to be in the same Fama-French size and book-to-market quintile in the preceding month of June as those of the treated firms. If more than five candidate firms are in the Fama-French size and book-to-market quintile, I choose firms that are closest to the treated firm in terms of the average bid-ask spreads three months prior to the terminations of analyst coverage.[13] Here, the bid-ask spreads are the percentage bid-ask spreads calculated by $\frac{100*(ask-bid)}{(ask+bid)/2}$. To allow the comparison between the abnormal returns of insiders before and after the terminations of analyst coverage, I require both the treated firms and control firms to have

[11] The term officer means a president, vice president, secretary, treasury or principal financial officer, comptroller or principal accounting officer, and any person routinely performing corresponding functions with respect to any organization whether incorporated or unincorporated. 17 C.F.R. § 240.3B-2.

[12] Before August 2002, insiders need to file their trades within ten days after the close of the calendar month in which the transaction occurred, which could result in a delay of up to 40 days. After August 2002, the Sarbanes-Oxley Act requires insiders to file their trades within two business days. Insiders' transactions become public information after they file their trades.

[13] I match on the bid-ask spread in order to ensure the treated firms and control firms have similar levels of information asymmetry. The treated firms and control firms also have similar levels of analyst coverage after the matching procedure.

at least one insider purchase (sale), both three months before and after the termination dates in the insider purchases (sales) sample.[14]

Note that not all coverage reductions are expected to have the same impact on information asymmetry and hence on insider trading. In particular, the impact probably depends on the number of analysts covering the firms. If few analysts cover a stock prior to the terminations of analyst coverage, one coverage drop is likely to significantly increase the corporate insiders' informational advantage. However, one coverage drop is unlikely to have a substantial impact if many analysts cover this stock prior to the terminations of analyst coverage. I provide evidence of this treatment heterogeneity in section 4.1, in which I show strong treatment effects in the subsample with treated firms that have five or fewer analysts covering the firm prior to the coverage reductions, and much weaker effects in the subsample with treated firms that have higher amount of initial coverage. Thus, except in section 4.1, I perform all the analysis in this paper using the subsample with treated firms that have five or fewer analysts. The purchases dataset in this subsample consists of 658 unique firms (129 treated firms and 529 control firms). One year before the coverage reductions, 12,021 insider purchases occur (2,599 from treated firms and 9,422 from control firms), and 13,621 insider purchases occur one year after the coverage reductions (2,371 from treated firms and 11,250 from control firms). The sales dataset in this subsample consists of 989 unique firms (231 treated firms and 758 control firms). One year before the coverage reductions, 53,982 insider sales occur (11,809 from treated firms and 42,173 from control firms), and 57,367 insider sales occur one year after the coverage reductions (12,010 from treated firms and 45,357 from control firms). The insider transactions in both datasets span 1999 to 2008.

2.3. Dependent Variables and Control Variables

The main dependent variables are the cumulative abnormal returns, trading volume, transaction value, and the cumulative abnormal profits. The cumulative abnormal returns (CARs) over different horizons (one month, three months, and six months) are estimated by Carhart's four-factor model (Carhart 1997) for each insider transaction, using the event-study

[14] The results are qualitatively similar if I use six months instead.

approach (e.g., Seyhun 1986).[15] First, I estimate the parameters in Carhart's four-factor model by regressing the stock excess returns on the four factors. The parameter estimation window is from day -250 to day -50 (trading days) relative to the insider-transaction dates. I perform a thorough analysis to cross check the validity of the estimated parameters.[16]

$$R_{it} - R_{ft} = \alpha_i + \beta_i * (R_{mt} - R_{ft}) + s_i * SMB_t + h_i * HML_t + m_i * MOM_t + \varepsilon_{it} \qquad (1)$$

Here, R_{it} denotes the returns of stock i in the parameter-estimation window, R_{ft} denotes the risk-free rates, and R_{mt} denotes the market returns. SMB, HML, and MOM are factors downloaded from Kenneth French's website. Next, I calculate the abnormal returns in the event-study window by subtracting the expected returns from the realized stock returns:

$$AR_{it} = R_{it} - E(R_{it}) = R_{it} - \hat{\alpha}_i - \hat{\beta}_i * (R_{mt} - R_{ft}) - \hat{s}_i * SMB_t - \hat{h}_i * HML_t - \hat{m}_i * MOM_t \ (2)$$

The cumulative abnormal returns from day 0 to day T are simply

$$CAR_i(0,T) = \sum_{t=0}^{T} AR_{it} \qquad (3)$$

Here, T = 21, 63, and 126 correspond to the cumulative abnormal returns with one-month, three-month, and six-month investment horizons, respectively (assume 21 trading days per calendar month).

Insiders' transaction value is the product of trading volume and transaction price. $LnShares$ is the natural log of the transaction shares. $LnValue$ is the natural log of insider transaction value. I compute insiders' abnormal profits both at the transaction and insider-quarter levels. The cumulative abnormal profits ($Profit$) at the transaction level are the product between the cumulative abnormal returns and the transaction value. IQ_Profit are the cumulative abnormal profits aggregated at the insider-quarter level. Because the distributions of

[15] According to SEC section 16(b) rules, insiders are prohibited from "short-swing" transactions (i.e., a sale and purchase of company stock within a six-month period). Hence the six-month abnormal returns are the returns that insiders have the opportunity to earn. I present the abnormal returns at the one-month and three-month horizons in order to study the price discovery process associated with insiders' transactions.

[16] The slopes of the excess market returns are centered on 1 in both the insider purchases and the insider sales samples. The loadings on SMB, HML, and MOM show patterns that are consistent with firm size, book-to-market ratio, and momentum in both the insider purchases and the insider sales samples.

Profit and *IQ_Profit* exhibit heavy tails, I winsorize them at the 2.5th and 97.5th percentiles of their empirical distributions to mitigate the effect of outliers.

I include several variables that have predictive power over expected returns as control variables. *LnSize* is the natural log of the market cap (in millions) in year t-1, *LnBEME* is the natural log of the book-to-market ratio in year t-1, *LnLev* is the natural log of the debt-to-equity ratio in year t-1, and *Ret1mPrior* is the one-month (day -21 to day -1) cumulative raw returns prior to insider transactions. I also include two liquidity measures as control variables in one of the robustness checks. *AIM* is the average Amihud illiquidity measure one-month (day -21 to day -1) cumulative returns prior to insider transactions, whereas the Amihud illiquidity measure is calculated by $\ln(1+\frac{|return|}{|price|*volume})$*1,000,000 (Amihud 2002). *Liqbeta* is the historical liquidity beta, the coefficient of the innovations in aggregate liquidity in the regression of monthly returns (month -60 to month -1) on the Fama-French three factors, and the innovations in aggregate liquidity (Pastor and Stambaugh 2003).

In addition, I obtain analyst data from the Thomson Reuters I/B/E/S database, stock returns data from the Center of Research in Security Prices (CRSP), accounting data from COMPUSTAT, manager compensation data from Execucomp, earnings release and 8-K filing dates from the SEC EDGAR system, insider trading enforcement data from the SEC website, and mutual fund holding data from Thomson Reuters.

3. Impact of the Terminations of Analyst Coverage on Insider' Abnormal Returns

3.1. Summary Statistics and Validity of the Quasi-Experimental Design

Table 1 presents the ex-ante summary statistics for both the treated firms and control firms prior to the coverage reductions. The treated group and the control group have a similar amount of coverage and similar level of the bid-ask spreads prior to the terminations. Thus, I ensure that the treated firms and control firms have comparable levels of information asymmetry prior to the coverage reductions. Moreover, the covariates in both groups are similar after the matching procedure, with the exception of firm size, where the mean firm size is slightly higher (though significant) for firms in the treated group. The difference in firm size is unlikely to account for the changes in insiders' abnormal returns, because the magnitude of the size difference is stable around the termination dates. Finally, the abnormal returns, trading volume, and transaction value of the treated firms are similar to those of the control firms, which provides common baselines for the DiD design.

[Insert Table 1 about here]

Because the interpretations of my results critically depend on the identification strategy, I perform a number of tests that directly examine the validity of the quasi-experimental design in my sample (Table 2).[17] First, I examine whether firms that experience closures and mergers related coverage reductions have different performance compared to their matched control firms. I compute the DiD estimators for a set of performance variables: actual earnings, market cap, Tobin's Q, profitability, sales, and raw stock returns[18]. None of these DiD estimators are significantly different from zero, suggesting that the treated firms have similar performance to the control firms. These results are consistent with previous studies (Hong and Kacperczyk 2010, Kelly and Ljungqvist 2012), and they indicate that the closures and mergers of the brokerage firms are unrelated to the performance of the covered stocks. Second, I examine whether coverage terminations increase information asymmetry of the affected firms. I find the DiD estimator for the bid-ask spreads is significantly positive. Specifically, compared with the control firms, the bid-ask spreads of the treated firms increased by 9.3 basis points. This result is consistent with Kelly and Ljungqvist (2012), and suggests that coverage reductions lead to increase in firms' information asymmetry.

[Insert Table 2 about here]

3.2. Eyeball Tests

Mergers and closures of the brokerage firms shock firms' information environment by reducing the amount of coverage. In the ideal world, the pattern of the number of analysts should be a step function that changes its value at the time point of the treatment. Figure 1 plots the mean value of the number of analysts covering a stock around the closures and mergers of the brokerage firms. The number of analysts covering the treated firms drops sharply around

[17] I include both insider purchases sample and insider sales sample in the validity tests. Thus, these tests are not conditional on the trading directions of the insiders.

[18] The DiD estimators are estimated by DiD specifications with firm fixed effects and calendar quarter fixed effects.

the closures and mergers. However, the pattern of the number of analysts deviates from the ideal step function in two ways. First, the reduction of coverage actually starts in the quarter prior to the termination dates rather than immediately after the termination dates, because some brokerage firms may fire their analysts before announcing closures or mergers officially. Notice that because I define the treatment dates as the official announcement dates of the closures or mergers, the measurement errors in the actual termination dates will bias the DiD coefficients toward zero and hence bias against me in finding the treatment effects. Second, starting from the second quarter after the mergers and closures, the number of analysts gradually recovers. This recovery is due to the fact that other brokerage firms start to initiate coverage and fill the void left by the brokerage firms that exit the equity research industry.[19] The recovery of the analyst coverage suggests that the reduction of analyst coverage is unlikely driven by the fundamentals of the firms, which provides additional support to collaborate the identification strategy. Because the reduction of analyst coverage is not permanent, I pick a one-year time window before and after the terminations for the DiD analysis. The fact that the number of analysts partially recovers within the one-year window after the termination dates will also bias against me, because the intensity of treatment decreases over time.[20]

[Insert Figure 1 about here]

Figure 2 plots the covariates (*LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*) around the closures and mergers of the brokerage firms. Except for *LnSize*, the covariates of the treated firms are similar to those of the control firms both before and after the termination dates. The average size of the treated firms is slightly larger than that of the control firms. However, the size difference does not change after the termination dates. The pattern of the covariates shown in Figure 2 suggests these variables are unlikely to explain any major change in insiders' abnormal returns after terminations of analyst coverage.

[Insert Figure 2 about here]

[19] In many cases, the analysts who lose their jobs in the mergers and closures are hired again by other brokerage firms. These analysts often reinitiate the coverage that they have worked on in their previous firms.

[20] In section 5.1, I show that the treatment effects last longer for firms that experience slower recovery in the number of analysts.

Figure 3 plots the three-month and six-month cumulative abnormal returns around the closures and mergers of the brokerage firms. The abnormal returns in the insider purchases sample (left panels) are in general positive, suggesting insiders earn positive abnormal returns from their purchases. The magnitude of the abnormal returns for the treated firms and control firms are comparable prior to the termination dates. However, after the termination dates the abnormal returns for the treated firms increase sharply before they return back to the original level four quarters after the closures and mergers. The pattern of the abnormal returns indicates insiders earn more abnormal returns from their purchases after terminations of analyst coverage. The abnormal returns in the insider sales sample (right panels) are in general negative, suggesting insiders avoid losses from their sales. After the termination dates, we observe a downward shift in the abnormal returns for the treated firms after the termination dates, suggesting insiders avoid more losses from their sales after terminations of analyst coverage.

[Insert Figure 3 about here]

Figure 3 plots the mean values of the cumulative abnormal returns over time. However, the mean values can be noisy given the wide distribution of the cumulative abnormal returns. To better understand the impact of terminations of analyst coverage on insiders' abnormal returns, in Figure 4, I plot the kernel density functions of the six-month cumulative abnormal returns in the one-year time window before and after the coverage reductions. In the insider purchases sample, the distribution of the cumulative abnormal returns of the treated firms shifts rightward after the terminations of analyst coverage, suggesting insiders earn substantially larger abnormal returns in their purchases (the Kolmogorov-Smirnov test rejects the equality of the two distributions at the 1% level). In the insider sales sample, we observe exactly the opposite. The distribution of the cumulative abnormal returns shifts leftward after the terminations of analyst coverage, suggesting insiders avoid significantly more losses in their sales (the Kolmogorov-Smirnov test rejects the equality of the two distributions at the 1% level). Moreover, changes in the cumulative abnormal returns take place only in the treated firms. Figure 4 also plots the distributions of the cumulative abnormal returns of the control firms in both the insider purchases and insider sales samples. Neither displays a systematic shift after the terminations of analyst coverage (the Kolmogorov-Smirnov test does not reject the equality of the two distributions at the 10% level).

[Insert Figure 4 about here]

3.3. Changes in Insiders' Abnormal Returns

Figure 3 and Figure 4 provide visual evidence in showing the treatment effects of the coverage reductions. However, two important concerns prevent us from quantifying the treatment effects. First, the shifts in these plots might be due to systematic differences in insiders' abnormal returns across firms, insiders, and transaction dates. To address this problem, I include firm fixed effects (or firm × insider fixed effects) and calendar-date fixed effects in the DiD regressions. Second, changes in the abnormal returns illustrated in Figure 3 and Figure 4 might be due to variations in the covariates. For example, the abnormal returns may come from a contrarian investment strategy that insiders may employ. Research has shown that insiders purchase when stock prices have recently decreased and sell when stock prices have recently increased (Rozeff and Zaman 1998, Lakonishok and Lee 2001), and thus, changes in the recent stock returns prior to insider transactions may lead to the changes in the stock abnormal returns. To address this concern, I add the one-month cumulative raw returns prior to insider transactions ($Ret1mPrior$) to the DiD specifications as a control variable. Similarly, the leverage ratio can also be correlated with both insiders' abnormal returns and the terminations of analyst coverage. Previous studies have shown that leverage ratio has some predictive power over expected stock returns (Fama and French 1992), whereas more recent evidence suggests terminations of analyst coverage can lead to changes in firms' costs of debt (Derrien, Kecskes, and Mansi 2012) and financing policies (Derrien and Kecskes 2013). Therefore, I also include the natural log of the debt-to-equity ratio ($LnLev$) as a control variable. Finally, I add the natural log of the firm size ($LnSize$) and the natural log of the book-to-market ratio ($LnBEME$) as control variables, because they may still have some predictive power over insiders' returns because of noise in the estimation of abnormal returns.

$$y_{ift} = \alpha_{(i)f} + \alpha_t + \beta_1 * Treat_{ft} \times Post_{ft} + \gamma' X_{ift} + \varepsilon_{ift} \quad (4)$$

The DiD specification with fixed effects and control variables is illustrated by equation (4), which is the baseline specification in my paper. The outcome variable, y_{ift}, represents the cumulative abnormal returns of an insider transaction executed by insider i from firm f on date t. I compute cumulative abnormal returns with one-month, three-month, and six-month investment horizons. $\alpha_{(i)f}$ denotes firm fixed effects or firm × insider fixed effects, whereas α_t denotes calendar-date fixed effects. $Treat$ is a dummy variable that equals 1 if the insider transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after the terminations of analyst coverage. X_{ift} represents control variables, and β_1 is the DiD coefficient that captures the impact of the terminations of analyst coverage on the outcome variables. I include insider transactions one year before and after the terminations of analyst coverage in the analysis. To be conservative, I cluster the standard errors at the closure/merger groupings.[21] Insider transactions from the treated firms that experience coverage reductions in the same closure/merger event are clustered together. Insider transactions from the control firms are assigned to the same clusters as the corresponding treated firms. This method corrects for serial correlation in the insider transactions from the same firms, and cross correlation among insider transactions from firms affected by the same brokerage closures or mergers.

[Insert Table 3 about here]

Table 3 shows the regression results of the above DiD specification. The systematic changes of the cumulative abnormal returns shown previously in Figure 3 and Figure 4 survive after controlling for the fixed effects and the control variables. The DiD coefficients are significantly positive in the insider purchases sample (Panel A), whereas they are significantly negative in the insider sales sample (Panel B) across all investment horizons. Moreover, the magnitudes of the coefficients are economically remarkable. For example, according to the DiD specification with firm fixed effects, the six-month cumulative abnormal returns in the insider purchases sample experience a 16.0% increase (in absolute terms) after the terminations of analyst coverage, which roughly corresponds to one third of one standard deviation of the six-month cumulative abnormal returns. On the other hand, the six-month cumulative abnormal returns in the insider sales sample exhibit a 10.7% decrease (in absolute terms) after the terminations of analyst coverage, which roughly corresponds to one fourth of one standard deviation of the six-month cumulative abnormal returns. Coupled with the argument that the terminations of analyst coverage increase information asymmetry exogenously (Kelly and Ljungqvist 2012), the results in Table 3 indicate that information asymmetry is a critical determinant of insiders' abnormal returns. Insiders enjoy a large increase in their abnormal returns when the information asymmetry of their firms increases.

[21] The standard errors would be smaller in most cases had I clustered standard errors at the firm level.

The four covariates that I control directly cannot explain the changes in insiders' abnormal returns. However, one may argue that omitted variables such as unobserved firm characteristics may account for the treatment effects. One unique feature in my analysis alleviates this concern. The change in the abnormal stock returns has a positive sign in the insider purchases sample but a negative sign in the insider sales sample. The bidirectional changes in stock returns limit the scope of omitted-variables-based explanations, because these variables usually predict one-directional changes in stock returns. For example, one may argue that changes in the stock abnormal returns can be attributed to an increase in the risk premium after terminations of analyst coverage. However, this explanation will predict an increase in the stock abnormal returns in both the insider purchases sample and insider sales sample.

The magnitude of the treatment effect is large considering the level of insiders' abnormal returns documented previously in the literature. For example, Seyhun (1986) studies corporate insiders' transactions from 1975 to 1981 and finds that corporate insiders earn small abnormal returns from their purchases and these returns are no longer significant after taking into account the transaction costs. Jeng, Metrick and Zeckhauser (2003) use a portfolio analysis approach to compute the risk-adjusted returns for insider transactions from 1975 to 1996. They show that the risk-adjusted return is around 6% per year for insider purchases, whereas it is not significantly different from zero for insider sales. To compare my results with these studies, noting two differences between my paper and previous work is important. First, my analysis focuses on a set of firms with five or fewer analysts. These firms are mostly small-cap and micro-cap firms, in which insiders probably earn higher abnormal returns compared to those in larger firms. More importantly, the DiD terms in my paper do not represent the average level of the insiders' abnormal returns. Instead, they represent the magnitude of the changes in insiders' abnormal returns when information asymmetry increases. The large magnitude of the DiD terms highlights the time-varying feature of insiders' abnormal returns. Within the same firm or even the same firm-insider pair, the level of insiders' abnormal returns can increase dramatically when information asymmetry increases, a result that can have important implications for both trading and regulatory purposes.

In a broader sense, my paper separates out a subset of insider transactions that have a higher level of abnormal returns than others. In this regard, it is related to several recent papers that make similar attempts. For example, Cohen, Malloy, and Pomorski (2012) sort insiders into "opportunistic insiders" and "routine insiders" based on their trading patterns. They find opportunistic insiders earn around 10% abnormal returns per year through their transactions (including both purchases and sales), whereas routine insiders do not earn significant returns. Karamanou, Pownall and Prakash (2013) sort insider sales into liquidity-motivated sales and information-motivated sales based on the transactions of the traders who are insiders of multiple firms. They find that the average abnormal returns for information-motivated sales are higher than 10% per year in small firms, which is in sharp contrast to the common belief that insider sales contain no information.

3.4. Anatomy of the Changes in Insiders' Abnormal Returns

I have shown that insiders' abnormal returns increase significantly after terminations of analyst coverage. The large magnitude of the treatment effects provides a unique opportunity to examine how the informational advantage of the insiders gets incorporated into stock prices. Because corporate insiders are required to publicly disclose their transactions by filling the SEC forms, I break down the six-month cumulative abnormal returns into three components: cumulative returns from trading dates to filing dates, cumulative returns around the filing dates, and cumulative returns after the filing dates. I replace the dependent variables in equation (4) using the above cumulative returns and run the regressions with firm fixed effects. The DiD coefficients of these regressions are summarized in Table 4. The cumulative returns from trading dates to filing dates increase by 3.2% for insider purchases, and decrease by 1.8% for insider sales. Interestingly, the cumulative abnormal returns around the filing dates change only slightly (increase by 1.1% for insider purchases and decrease by 0.3% for insider sales). The majority of the changes in the insiders' abnormal returns actually come after the filing dates. The cumulative abnormal returns after filing dates increase by 11.1% for insider purchases, and decrease by 8.1% for insider sales.

[Insert Table 4 about here]

To better understand the source of the changes in the abnormal returns, I further single out the cumulative abnormal returns around the release of corporate news. In particular, I compute the cumulative abnormal returns around the earnings announcements and 8-K filings that take place after the insider filing dates but within six months of the trading dates. I focus on earnings announcements and 8-K filings because they are the main channels via which firms disclose information to the public. As Table 4 shows, the cumulative abnormal returns around the earnings announcements and 8-K filings both change significantly after terminations of analyst coverage. The change in the cumulative abnormal returns around these two types of information release events accounts for the majority if not all of the treatment effects after the filing dates. In the insider purchases sample, the cumulative abnormal returns in day [-2, 2] around the earnings announcements increase by 5.0%, whereas the cumulative abnormal returns in day [-2, 2] around the 8-K filings increase by 3.6%. In the insider-sales sample, the cumulative abnormal returns in day [-2, 2] around the earnings announcements decrease by 4.3%, whereas the cumulative abnormal returns in day [-2, 2] around the 8-K filings decrease by 6.0%.

The fact that a large portion of the increase in insiders' abnormal returns is concentrated in narrow time windows around earnings announcements and 8-K filings indicates that insiders' advantage lies in their private information of corporate news. This prominent feature of the price discovery process helps rule out several alternative explanations. First, the price discovery pattern limits the scope of the risk-premium-based explanations. If the changes in the abnormal returns were due to changes in the risk premium, we would expect to see a smooth drift rather than jumps in the price discovery process. Second, the price discovery pattern alleviates concerns regarding the methodology in estimating the abnormal returns. Because the treatment effects are concentrated in narrow time windows around earnings announcements and 8-K filings, the results are less sensitive to the choice of benchmarks. Finally, the price discovery pattern also challenges the view that insiders mainly gain their abnormal returns by acting as contrarian investors. If the main source of the abnormal returns were contrarian investing, that is, buying low and selling high, we would not expect the increase in abnormal returns to be concentrated around the release of corporate news.

From the anatomy of the treatment effects, we can see that incorporating the informational advantage of the insiders into stock prices takes a long time. The price discovery process remains slow after the adoption of the Sarbanes-Oxley Act (SOX), which accelerates the disclosure of insider trading by requiring insiders to report their transactions within two business days. As Table A1 in the appendix shows, the magnitude of the changes in insiders' abnormal returns after filing dates remains the same after SOX. Interestingly, previous empirical work also documents similar price discovery patterns for corporate insider trading, especially for the transactions associated with large abnormal returns. For example, Lakonishok and Lee (2001) study corporate insider trading activities during the 1975-1995 period. They find that insider purchases from small companies have strong predictive power for stock returns in long investment horizons (e.g. 12 months). However, they observe little stock price movement around the time of insider trading or around the reporting dates. Bettis, Vickrey, and Vickrey (1997) show that outsiders can earn significant abnormal returns by following insiders who make large volume purchases or sales. The abnormal returns for the mimickers in their paper also keep increasing over the one-year investment horizon.

The slow price discovery process may seem counter-intuitive given that insiders' transactions are public knowledge right after the filing dates. However, given the wide distribution of the insiders' abnormal returns, it is possible that limits to arbitrage may deter rational traders to arbitrage away the pricing inefficiencies. Of course, the slow price discovery process may also be explained by non-rational explanations. For example, investor inattention may play an important role. Despite the fact that insiders report their transactions publicly, investors may not pay enough attention to this information, especially in smaller firms, which are less likely to be under scrutiny. Alternatively, outsiders might fail to immediately recognize the increase in information asymmetry after coverage reductions. As a result, they fail to adjust their response to the transactions of corporate insiders and hence slow down the price discovery process. Understanding the mechanism that accounts for the slow price discovery process will help gauge the contribution of insider trading to market efficiency, and assist regulators to evaluate the effectiveness of insider trading policies such as disclosure rules. Thorough tests that examine the above mechanisms are beyond the scope of this paper and remain promising research topics for future studies.

4. Heterogeneity in the Treatment Effects

4.1. Heterogeneity across the Levels of Initial Coverage

In section 3, I study the impact of the coverage reductions on insiders' abnormal returns using a subsample containing treated firms with five or fewer analysts prior to the coverage reductions. Because a one-unit drop in analyst coverage accounts for a smaller proportion of the public information lost in firms with more analysts, I would reasonably expect to observe weaker treatment effects in these firms. To test this hypothesis, I extend the DiD tests to subsamples with treated firms that have medium levels (6 to 10 analysts) and high levels (more than 10 analysts) of initial coverage.[22] I present the results in Panel A of Table 5.

[Insert Table 5 about here]

In contrast to the results I have shown previously in the low-initial-coverage group, the DiD coefficients are no longer significant in either the medium-initial-coverage group or the high-initial-coverage group. For insider purchases, the magnitude of the DiD coefficients decreases to 12.6% in the medium-initial-coverage group and to 4.6% in the high-initial-coverage group. For insider sales, the magnitude of the DiD coefficient decreases to -5.5% in the medium-initial-coverage group and is statistically indistinguishable from zero in the high-initial-coverage group. These heterogeneous treatment effects across initial coverage confirm the hypothesis that insiders exhibit a larger response when their firms lose a greater percentage of analyst coverage. If we go one step further and assume the increase in information asymmetry is inversely proportional to the amount of initial coverage, we can quantify the treatment effects in the full sample by adjusting the treatment intensity parametrically.

$$y_{ift} = \alpha_{(i)f} + \alpha_t + \beta_1 * Treat_{ft} \times Post_{ft} \times (1/\,Initial\ Coverage_f) + \gamma' X_{ift} + \varepsilon_{ift} \qquad (5)$$

Here, the DiD coefficient β_1 should be interpreted as the treatment effects for firms that have only one analyst prior to the terminations. Panel B of Table 5 presents the results of this test. Consistent with the findings in the baseline tests, β_1 is significantly positive for insider purchases and significantly negative for insider sales. Notice that the coefficients are larger than those shown previously in the baseline analysis, because the DiD coefficients in this table represent the change in insiders' abnormal returns for firms that lose their only coverage.

4.2. Other Heterogeneity Tests

To better understand the nature of insiders' transactions associated with high levels of information asymmetry, I perform additional heterogeneity tests which are summarized in the appendix A of this paper. First, consistent with Cohen, Malloy, and Pomorski (2012) who find insiders that exhibit opportunistic trading patterns earn significant higher abnormal returns, I

[22] Section 4.1 is the only place where I use the sample with treated firms that have more than five analysts. I perform all the other analysis throughout the paper on a subsample with treated firms that have five or fewer analysts.

find the treatment effects are mainly driven by the opportunistic insiders (Table A2). Second, insiders' desire to diversify influences their response to increase in information asymmetry. The treatment effects in insider purchases sample are much weaker for insiders with heavy exposure to the stocks of their own companies, whereas the treatment effects in insider sales sample are much stronger for these insiders (Table A3). Finally, I find the treatment effects are much weaker in time periods with strong intensity of legal enforcement, suggesting that insiders do concern about litigation risks when they exploit information asymmetry (Table A4).

5. Robustness Checks

In the baseline DiD analysis, I have shown that insiders enjoy significantly larger abnormal returns after the terminations of analyst coverage. In this section, I provide robustness checks regarding the validity of the empirical analysis.

5.1.Dynamics of the Treatment Effects

The DiD analysis in the previous sections includes insider transactions one year before and after the termination of analyst coverage. Thus, the treatment effects I reported previously can be seen as the average treatment effects with a one-year duration in both the pre-treatment and post-treatment period. In this section, I systematically investigate the dynamics of the treatment effects using the following modified DiD specification:

$$y_{ift} = \alpha_{(i)f} + \alpha_t + \beta_1 * Treat_{ft} \times (6 \sim 12m\, before)_{ft} + \beta_2 * Treat_{ft} \times (0 \sim 6m\, after)_{ft} + \beta_3 * Treat_{ft} \times (6 \sim 12m\, after)_{ft} + \gamma' X_{ift} + \varepsilon_{ift} \qquad (6)$$

Here, *(6 ~ 12m before)*, *(0 ~ 6m after)*, and *(6 ~ 12m after)* are dummy variables that equal 1 if the transaction happens within [-12, -6), [0, 6), and [6, 12] months relative to the coverage reductions. Notice that I omit the term *Treat* × *(0 ~ 6m before)* in the regression. Thus, the coefficients of other terms can be interpreted as the DiD treatment effects relative to the baseline in the [-6, 0) months.

[Insert Table 6 about here]

Panel A of Table 6 presents the regression results. For both the insider purchases and sales sample, the coefficients are insignificant prior to the coverage reductions. These results suggest no pre-trends exist before the treatment takes places, which verifies the validity of the DiD analysis. In both samples, the DiD coefficients become significant with a large magnitude in the first six months after the terminations of analyst coverage and then decrease in magnitude in the next six months. These results suggest the impact of the coverage reductions on insiders' abnormal returns reaches its peak within six months and starts to decay six to 12 months after the terminations of analyst coverage.

The decay of the treatment effects is reminiscent of the recovery pattern of the number of analysts in Figure 1. After the closure-related terminations of analyst coverage, other brokerage firms initiate new coverage to fill the void left by the firms that exit the equity research industry, which may bring down the levels of information asymmetry and thus decrease insiders' abnormal returns. To test this hypothesis, I calculate the recovery in the number of analysts by subtracting the number of analysts right after the terminations from the number of analysts six months after the terminations. I then sort the treated firms into two groups based on this recovery measure. The sorting is performed annually and independently to the number of analysts prior to the terminations to ensure the two groups of firms have the same treatment intensity. I create a dummy variable *Strong_Analyst_Recovery* that equals 1 if a firm's recovery in the number of analysts is larger than the median values. I then interact this dummy variable with the DiD terms.

Panel B of Table 6 presents the results of the regressions. For firms that have stronger recovery of the number of analyst coverage during the first six months after the terminations, insiders earn significantly less abnormal returns. This reduction in the magnitude of insiders' abnormal returns is especially strong six months to 12 months after the mergers and closures, when the new analysts have initiated their coverage. These results provide collaborative evidence showing the direct impact of the number of analysts on insiders' abnormal returns. Other channels may also contribute to the decay of the treatment effects. For example, Balakrishnan et al. (2012) show that firm managers increase voluntary disclosure after the terminations of analyst coverage in order to improve the liquidity of the stocks. Voluntary disclosure can also lead to reductions in firms' information asymmetry, and thus can help explain the decay of the treatment effects.

5.2. Other Robustness Checks

In the appendix B of this paper, I confirm the increase in insiders' abnormal returns after closure-related terminations of analyst coverage is robust to calendar portfolio analysis (Table A5), alternative measures of abnormal returns (Table A6), the inclusion of liquidity measures (Table A7), and the exclusion of tiny firms and low-price transactions (Table A8), whereas it disappears in the placebo tests in which I falsely shift the termination dates or replace the treated firms with similar control firms (Table A9).

6. Impact of the Terminations of Analyst Coverage on Insider' Trading Behavior and Abnormal Profits

6.1. Changes in Insiders' Trading Behavior

Does the increase in information asymmetry alter insiders' trading behavior? In this section, I examine changes in insiders' trading volume, transaction value, and trading probability after terminations of analyst coverage. Although almost all informed-trading models predict a positive relation between insiders' abnormal returns and information asymmetry, predictions about insiders' trade size vary greatly. The key difference among these models is the assumption about the sensitivity of the expected stock prices to insiders' trade size. The price-taking models (e.g., Grossman and Stiglitz 1980) assume insiders' transactions do not alter the expected stock prices. In this world, after the terminations of analyst coverage, insiders will trade more aggressively to take full advantage of the increase in information asymmetry. By contrast, the imperfect-competition models (e.g., Kyle 1985, Copeland and Galai 1983, Spiegel and Subrahmanyam 1992) assume the volume of insiders' transactions can endogenously affect stock prices, and predict insiders choose an optimal trade size to maximize their expected profits. These models predict little to no change in the expected trade size despite an increase in information asymmetry. Therefore, a test of the validity of both types of models in the empirical setting of my study is interesting.

I use the DiD specification in equation (4) to study the impact of the coverage reductions on insiders' trading volume and transaction values. The dependent variables are the natural log of insiders' trading shares and the natural log of transaction value. Table 7 presents the results. The coefficients for the *Treat* × *Post* terms are positive (although not statistically significant) in both the insider purchases and insider sales samples. Given that price-taking models and imperfect competition models have different assumptions about the sensitivity of the expected stock prices to insiders' trading size, I further sort the treated firms into liquid and illiquid groups based on the bid-ask spreads prior to the coverage reductions. The sorting is performed annually and independently to the number of analysts to ensure the two groups of firms have the same treatment intensity. Consistent with the price-taking models, I find that after the terminations of analyst coverage, insiders' trading volume and transaction value increase significantly in stocks with high liquidity. In the insider purchases sample, insiders' trading volume increases by 49%, whereas insiders' trading value increases by 34%. In the insider sales sample, the increase is smaller (16% for insiders' trading volume and 17% for insiders' transaction value), possibly because insider sales are bounded by their stock endowment.[23] However, for stocks with low liquidity, insiders' trading volume and transaction value remain unchanged after the terminations of analyst coverage. This result is consistent with the imperfect-competition models.

[Insert Table 7 about here]

The increase in information asymmetry also alters insiders' trading behavior in the extensive margin. In appendix C, I show that after terminations of analyst coverage the probability of insider trading changes in a similar way to the trading volume. Insiders' trading probability increases significantly in stocks with high liquidity, whereas it remains the same in stocks with low liquidity (Table A10).

6.2. Changes in Insiders' Abnormal Profits

Since insiders obtain abnormal profits at the expense of outsiders, finding out how many additional profits insiders can earn when they have a larger informational advantage over uninformed investors is of interest to regulators aiming to preserve market integrity. I compute insiders' abnormal profits at both the transaction and the insider-quarter levels. Abnormal profits at the transaction level are calculated as the product between abnormal returns and

[23] Another reason is insider sales may be less information-driven compared to insider purchases.

insiders' transaction value. I then sum up these abnormal profits for each insider in each quarter to obtain the insider-quarter abnormal profits. Because the distribution of the abnormal profits exhibits heavy tails on both ends, I winsorize the profits at the 2.5th and 97.5th percentiles of their empirical distributions to mitigate the effect of outliers.[24] Table 8 presents the coefficients for the DiD tests at the transaction level. Insiders earn more abnormal profits in their purchases and avoid more losses in their sales after the terminations of analyst coverage. If insiders hold their positions for six months, they on average make $5,485 more profits per purchase and avoid $14,186 more losses per sale according to the DiD specifications with firm fixed effects. Similar results are found in specifications with firm × insider fixed effects.

[Insert Table 8 about here]

I perform the same analysis for insiders' abnormal profits aggregated at the insider-quarter level. Table 9 presents the coefficients for the DiD tests. Conditional on holding his or her position for six months, an average insider makes $21,861 more profits from purchases and avoids $224,229 more loses from sales per quarter after the terminations of analyst coverage. These two point estimations are statistically significant at the 1% level. Simple back-of-the-envelope calculations can illustrate the economic impact. If we multiply the estimated coefficients by four, we can see that conditional on holding his or her position for six months for all trades within the one-year post-termination period, an average insider makes $87,444 more profits from purchases and avoids $896,916 more losses from sales. These changes in insiders' abnormal profits are sizable compared to their compensation. Using ExecuComp data, I find the median total compensation[25] of the top five executives is $778,686 in the treated firms of the insider purchases sample, and is $930,112 in the treated firms of the insider sales sample. Thus, the abnormal profits an average insider earns from purchases are 11.2% of the median annual compensation of the top executives, whereas the losses an average insider avoids from sales are 96.4% of the median annual compensation of the top executives. The fact that the changes in abnormal profits are larger in the insider sales sample is not surprising. Insiders are usually endowed with stocks of their companies as part of their compensation plans, and thus they tend

[24] Log transformation is not an option here, because the abnormal profits can be both positive and negative. Results are robust to other thresholds of winsorization.

[25] The total compensation is TDC1 in ExecuComp, which includes salary, bonus, other annual, total value of restricted stock granted, total value of stock options granted (using Black-Scholes), long-term incentive payouts, and all other compensation.

to make sales both more frequently and of a larger value, hence explaining the larger abnormal profits in the insider sales sample.

[Insert Table 9 about here]

7. Discussion

7.1. Economic Channels

There are two channels via which coverage reductions can increase information asymmetry. First, the increase in information asymmetry can be due to deterioration of outsiders' information. Analysts analyze, interpret, and disseminate information to capital market participants, and thus serve as information providers to outside investors. After terminations of analyst coverage, outsiders suffer from information losses, which can enlarge the informational advantage of insiders. I term this channel "information provider" channel. The increase in the information asymmetry can also come from a different channel. Research has shown that analysts can act as whistle blowers on corporate frauds (Dyck, Morse, and Zingales 2010). Moreover, Chen, Harford and Lin (2013) have recently shown that corporate governance deteriorates after terminations of analyst coverage, suggesting that analysts play an important governance role in scrutinizing management. Therefore, it is possible that analysts act as "insider trading police" and directly discourage insiders from trading on valuable private information. In this channel, insiders' informational advantage also effectively increases after coverage reductions, because they can trade on more material information after the removal of disciplining pressure. I term this channel the "discipline" channel.[26] In the rest of this session, I first present evidence supporting the information provider channel and then show results suggesting the discipline channel cannot fully explain the results of my study.

In order to test the information provider channel, we need to know the information provided by the analysts that influences outsiders' trading decisions. Although I cannot pin down the exact information set, a reasonable assumption, however, is that outsiders make use of

[26] The difference between the discipline channel and the information provider channel is subtle. Similar to the information provider channel, the discipline channel also predicts an increase in insiders' abnormal returns. Moreover, the discipline channel will predict many of the heterogeneity results that I have shown previously: the removal of disciplining pressure probably has a larger impact on opportunistic insiders, in time periods with low intensity of legal enforcement, and in firms with fewer analysts to begin with.

the analyst forecasts in deciding their trades. Under this assumption, I present evidence supporting the information provider channel. Specifically, I show the precision of analyst earnings forecasts decreases after coverage reductions and the decrease in the precision is correlated with the increase in insiders' abnormal returns.

I examine the changes of a set of measures for the precision of analysts' earnings forecasts after termination of analyst coverage. These measures include the median EPS forecast errors, the mean EPS forecast errors, the dispersion of the forecasts, and the absolute value of the earnings surprises. Notice that larger value of the above measures indicate worse quality of earnings forecasts. Panel A of Table 10 presents the results. The DiD estimators are significantly positive for all the measures, suggesting that the quality of earnings forecasts deteriorates after coverage reductions. This result is consistent with Hong and Kacperczyk (2010), who find the precision of earnings forecasts decreases after merger-related coverage terminations.

[Insert Table 10 about here]

If the "information provider" channel is the underlying mechanism that accounts for the increase in insiders' abnormal returns, we expect to see stronger treatment effects in firms with larger reduction in the precision of earnings forecasts. To test this hypothesis, I compute the first principle component (*PCA1*) of the four precision measures. I then sort the treated firms into two bins based on the levels of reduction in the precision of earnings forecasts (i.e., the average *PCA1* one year after minus the average *PCA1* one year before coverage reductions). The sorting is performed annually and independently to the number of analysts prior to the coverage reductions to ensure the two groups of firms have the same treatment intensity. I create a dummy variable $High\_Precision\_Reduction$, that equals 1 if a firm's reduction in the precision of the analyst forecast is larger than the median values. I then interact this dummy variable with the DiD term and run the following regression:

$$y_{ift} = \alpha_{(i)f} + \alpha_t + \beta_1 * Treat_{ft} \times Post_{ft} + \beta_2 * Treat_{ft} \times Post_{ft} \times High\_Precision\_Reduction_{ft} + \gamma' X_{ift} + \varepsilon_{ift} \quad (8)$$

Panel B of Table 10 presents the results of the regressions. The treatment effects are significantly stronger in firms that experience a larger reduction in the precision of the earnings forecasts, supporting the hypothesis that analysts reduce the informational advantage of insiders, by providing information to outside investors.

To further differentiate the information provider channel and the discipline channel, thinking more about which types of informed traders are better off after coverage reductions is worthwhile. In the information provider channel, any informed agents who are either endowed with the information (e.g., corporate insiders) or who can acquire the information (e.g., active management funds) can be better off, because both types of informed traders can take more advantage of the uninformed traders who suffer from information losses due to coverage reductions. In the discipline channel, however, the enlarged informational advantage belongs solely to corporate insiders. Unlikely corporate insiders, other informed traders such as active management funds are not disciplined by the analysts, and hence their tradable information set does not expand after coverage reductions. Therefore, examining the performance of active funds in trading the stocks that experience coverage reductions will help differentiate the two channels. To do so, for every stock that experience coverage reductions (affected stock), I construct an aggregate active fund that consists of all the active mutual funds that hold this stock in the one-year window prior to the termination dates.[27] I then follow Kacperczyk, Sialm, and Zheng (2005) and determine the quarterly "trading" directions of the aggregate active fund based on the weights of the affected stock calculated from the quarterly holding data. Next, I compute the excess returns of the aggregate active fund by subtracting the returns of the characteristic-based benchmarks (Daniel et al. 1997, Wermers 2004) from the raw returns of the affected stock. The regression specification is:

$$(R_{ft} - R_{bft}) * H_{ft} = \alpha_f + \alpha_t + \beta_1 * Post_{ft} + \varepsilon_{ift} \qquad (9)$$

where

$$H_{ft} = 1 \text{ if } \omega_{f,t-1} > \frac{\omega_{f,t-2}(1+R_{f,t-1})}{\sum_k \omega_{k,t-2}(1+R_{k,t-1})}$$

$$H_{ft} = -1 \text{ if } \omega_{f,t-1} \leq \frac{\omega_{f,t-2}(1+R_{f,t-1})}{\sum_k \omega_{k,t-2}(1+R_{k,t-1})}$$

[27] I focus my analysis on actively managed equity mutual funds and eliminate balanced, bond, money market, international, and index funds. Active equity mutual funds are selected based on Lipper objectives following the approach in Huang, Sialm, and Zhang (2011).

Here, R_{ft} denotes the quarterly returns of affected stock f, R_{bft} denotes the quarterly returns of the corresponding characteristic-based benchmarks, and H_{ft} represents the "trading" directions of the aggregate active fund consisting all the active mutual funds that hold stock f in the one-year window prior to the termination dates. H_{ft} = 1 (or -1) if the weights of stock f ($\omega_{f,t-1}$) increase (or decrease) after adjusting for the price changes of all the stocks (denoted by k) in the aggregate active fund. α_f denotes the aggregate active fund fixed effects, and α_t denotes the year fixed effects. $Post$ is a dummy variable that equals 1 if quarter t contains the termination dates or takes place after the termination dates. I include the excess returns four quarters before and after the termination dates in the regression. β_1 is the coefficient of interest and it represents the changes in the performance of aggregate active funds in trading the affected stocks after coverage reductions.

[Insert Table 11 about here]

Panel A of Table 11 presents results of the regressions. The quarterly abnormal returns of the aggregate active funds increase by 1.2% after terminations of analyst coverage, which translates to a 4.7% increase in the annualized abnormal returns. Notice that the magnitude of increase in the abnormal returns is smaller than what I find for corporate insiders. This is not surprising given that insiders should have better information regarding their own firms compared to active mutual funds. Moreover, the abnormal returns of the active funds are probably underestimated because their trades are not observed directly and can only be inferred using holding data at quarterly frequency. To better understand the sources of the abnormal returns, I run the same regression for the subsample when the aggregate active funds increase their holdings (i.e., $H_{ft} = 1$) and the subsample when the aggregate active funds decrease their holdings (i.e., $H_{ft} = -1$), respectively. Conditional on active funds increasing their holdings, the quarterly abnormal returns increase by 1.9% after terminations of analyst coverage, which translates to a 7.5% increase in the annualized abnormal returns. However, conditional on the aggregate active funds decreasing their holdings, essentially no change occurs in the abnormal returns. These results make sense because mutual funds typically do not engage in short selling activities. Thus, their informational advantage is more likely to concentrate on the purchases side. Taken together, the above results suggest that corporate insiders are not the only party that is better off after terminations of analyst coverage. The performance of active funds also improves, suggesting that the discipline channel is unlikely to be the only mechanism that accounts for the increase in information asymmetry.

I should emphasize that my results do not rule out the discipline channel entirely, because the discipline channel and the information provider channel are not mutually exclusive. In fact, it is likely that these two channels both contribute to the increase in the information asymmetry. My results, however, indicate the discipline channel *alone* cannot fully rationalize the data.

7.2. Alternative Explanations

Since the reductions of analyst coverage may change other aspects of the firms besides the levels of the information asymmetry, alternative explanations may exist for my findings. One prominent alternative story centers around changes in firms' risk premium. Research has shown that terminations of analyst coverage worsen stock liquidity conditions and increase the cost of capital (Kelly and Ljungqvist 2012). Thus, the risk premium of affected firms can increase after coverage reductions, which in turn may lead to changes in stocks' abnormal returns. However, I argue the risk-premium based alternative story is unlikely to explain my findings for reasons listed below. First, the stock abnormal returns show bidirectional changes depending on the trading directions of the corporate insiders. Changes in risk premium cannot simultaneously explain these bidirectional changes in my results. Second, I show a large portion of the increase in insiders' abnormal returns takes place in narrow time windows surrounding the release of corporate news. If changes in risk premium were the main driving force of my results, we would expect to see a smooth drift of the cumulative abnormal returns. Third, I conduct portfolio analysis and control for the risk exposure separately for time periods both before and after the coverage reductions. Consistent with the transaction-level DiD analysis, I show that the alphas of insiders' purchases portfolios increase after terminations of analyst coverage, whereas the alphas of insiders' sales portfolios decrease after terminations of analyst coverage. Finally, the changes in insiders' abnormal returns exhibit similar patterns in specifications where I control for liquidity measures directly. Taken together, the risk-premium-based alternative story struggles to explain the results in my paper.

Besides the risk-premium explanation, one may come up with alternative stories based on other aspects of the firms that are influenced by coverage reductions. Without specifying these alternative stories and testing directly against them, I cannot dismiss these explanations. However, I note that the scope of these alternative stories are likely limited given that any alternative story will have to simultaneously explain the bidirectional changes in stocks' abnormal returns depending on the trading directions of insiders, and the fact that the treatment effects are concentrated around the release of corporate news.

7.3. Regulatory Implications

In contrast to the common belief that corporate insiders earn small abnormal returns, in this study, I find that insiders' abnormal returns can increase sharply in situations with high information asymmetry. In the insider purchases sample, the abnormal returns increase by 16.0% and the insiders earn an additional $87,444 in the one-year post-termination window, assuming they hold their positions for six months. In the insider sales sample, the abnormal returns decrease by more than 10.7% and the insiders avoid an additional $896,916 in losses in the one-year post-termination window assuming they hold their positions for six months. The magnitude of these abnormal returns and profits are in fact comparable to those in the illegal insider trading cases filed by the SEC. Table A11 lists the characteristics of illegal insider trading cases documented by previous studies (Meulbroek 1992; Del Guercio, Odders-White, and Ready 2013). The mean of the abnormal returns associated with the illegal trading cases (defined as the returns on the dates when information, such as takeovers, is announced publicly) is around 20%. In 2003 to 2007, the mean of the profits associated with the illegal trading cases is $519,116 per trader, whereas the median of the profits is $61,189 per trader (both in 2011 dollars).[28]

The impact of information asymmetry on insiders' abnormal profits has important regulatory implications. My analysis calls for regulatory attention to corporate insiders' transactions, especially those associated with high level of information asymmetry. Despite their easy access to non-public information, corporate insiders have not been the primary targets of legal investigations. In all cases prosecuted by the SEC, only around 20% of defendants are employees of stocks they traded, most of which are not those subject to the filing requirement of the SEC (Del Guercio, Odders-White, and Ready 2013). Recent efforts in regulating the corporate insiders have focused on the disclosure of their trades. The Sarbanes-Oxley Act

[28] Insiders can face severe punishment in illegal insider trading cases with these abnormal profits. For example, in 2004, Martha Stewart received five months of a prison sentence, five months of house arrest, and a full two years of probation afterward for avoiding a loss of $45,673 from her insider sales.
http://en.wikipedia.org/wiki/Martha_Stewart
In 2012, Noah J. Griggs Jr., a former executive at CKE Restaurants Inc., was barred for 10 years from serving as an officer of a public company for making $145,430 profits from his insider purchases.
http://www.latimes.com/business/la-fi-mo-cke-insider-trading-20120315-story.html

accelerates the disclosure of insiders by requiring them to file Form 4 with the SEC within two business days. Brochet (2010) finds that the abnormal returns around the filings dates increase after the SOX Act, and he concludes the information of insiders' transactions gets incorporated into stock prices in a more timely fashion. In this paper, however, I show that the price discovery process associated with insiders' transactions can remain slow in the post-SOX era when information asymmetry is high. My findings suggest timely disclosure of insiders' transactions is not enough to prevent insiders from taking advantage of their private information, and furthermore, it does not guarantee price efficiency.

I show that corporate insiders earn large magnitude of abnormal returns and profits when the level of information asymmetry is high. However, none of the insiders in my data have been prosecuted by the SEC up to today. This somewhat puzzling fact may be due to difficulty in detecting insiders' transactions associated with high level of information asymmetry. Previous studies have cast doubt on the effectiveness of traditional measures for information asymmetry (Huddart and Ke 2007, Johnson and So 2014). Consistent with these papers, I find proxies such as the bid-ask spreads, idiosyncratic volatility and the number of analysts all fail to explain the increase in insiders' abnormal returns after coverage reductions (Table A12). A naive regulator who relies on these measures would miss the opportunities to detect the change in insiders' abnormal returns. Thus, developing measures of information asymmetry that are easy to implement and empirically effective remain an important challenge for financial researchers.

8. Conclusion

Various informed-trading models in the literature have proposed information asymmetry as crucial in determining the insiders' abnormal returns and abnormal profits. However, testing the above relationship has been empirically challenging because of endogeneity concerns associated with the information asymmetry measures. My paper systematically studies the impact of closure-related terminations of analyst coverage, which exogenously increase the information asymmetry of the affected firms, on insider trading. I find that corporate insiders enjoy higher abnormal returns and gain larger abnormal profits after the terminations of analyst coverage. My paper highlights the role of information asymmetry as a critical determinant of insiders' abnormal profits, and indicates the descriptive validity of informed-trading models applied to corporate insiders' trades. It also calls for regulatory attention to corporate insiders' transactions associated with high levels of information asymmetry.

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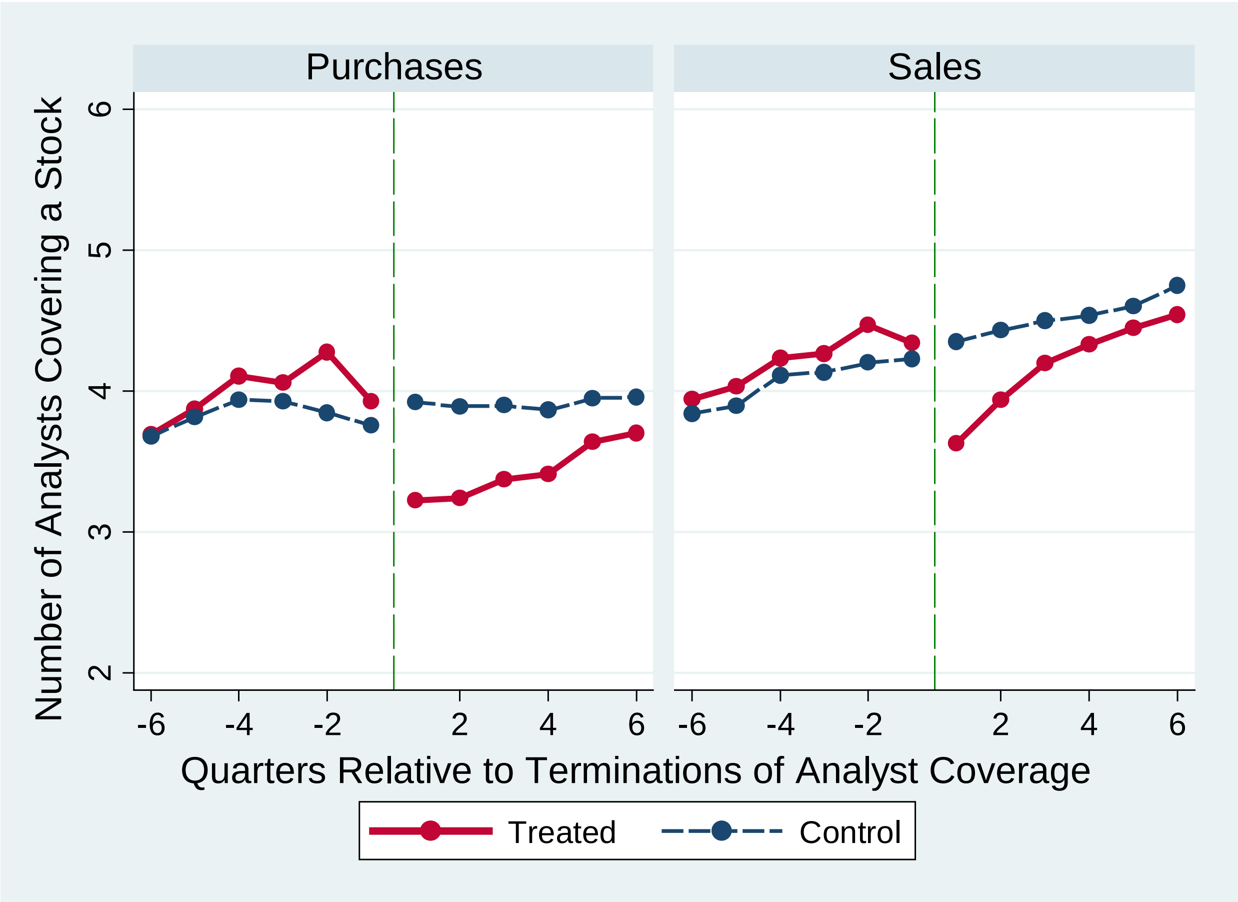
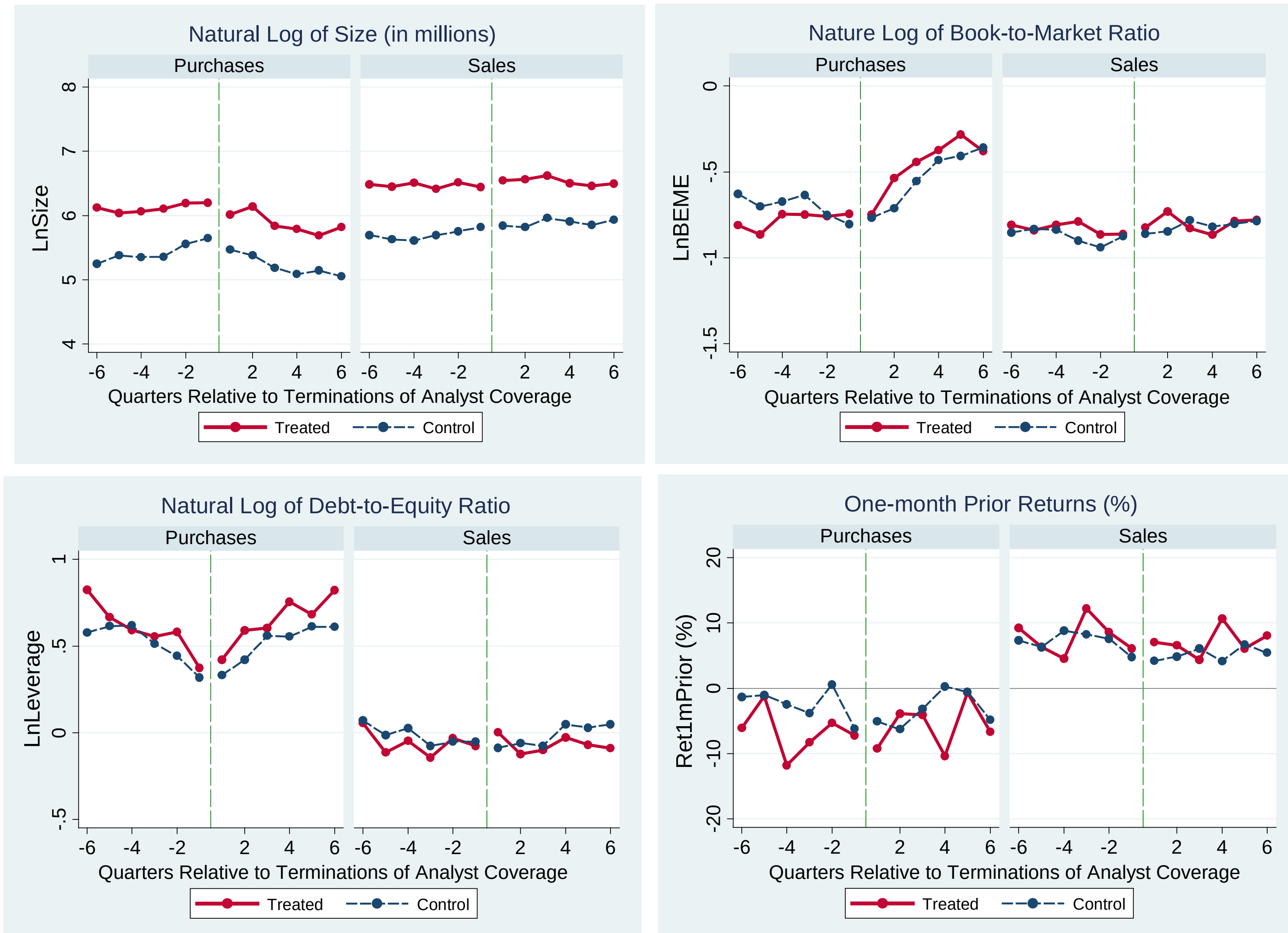
Figure 1. Number of Analysts around the Mergers and Closures of Brokerage Firms



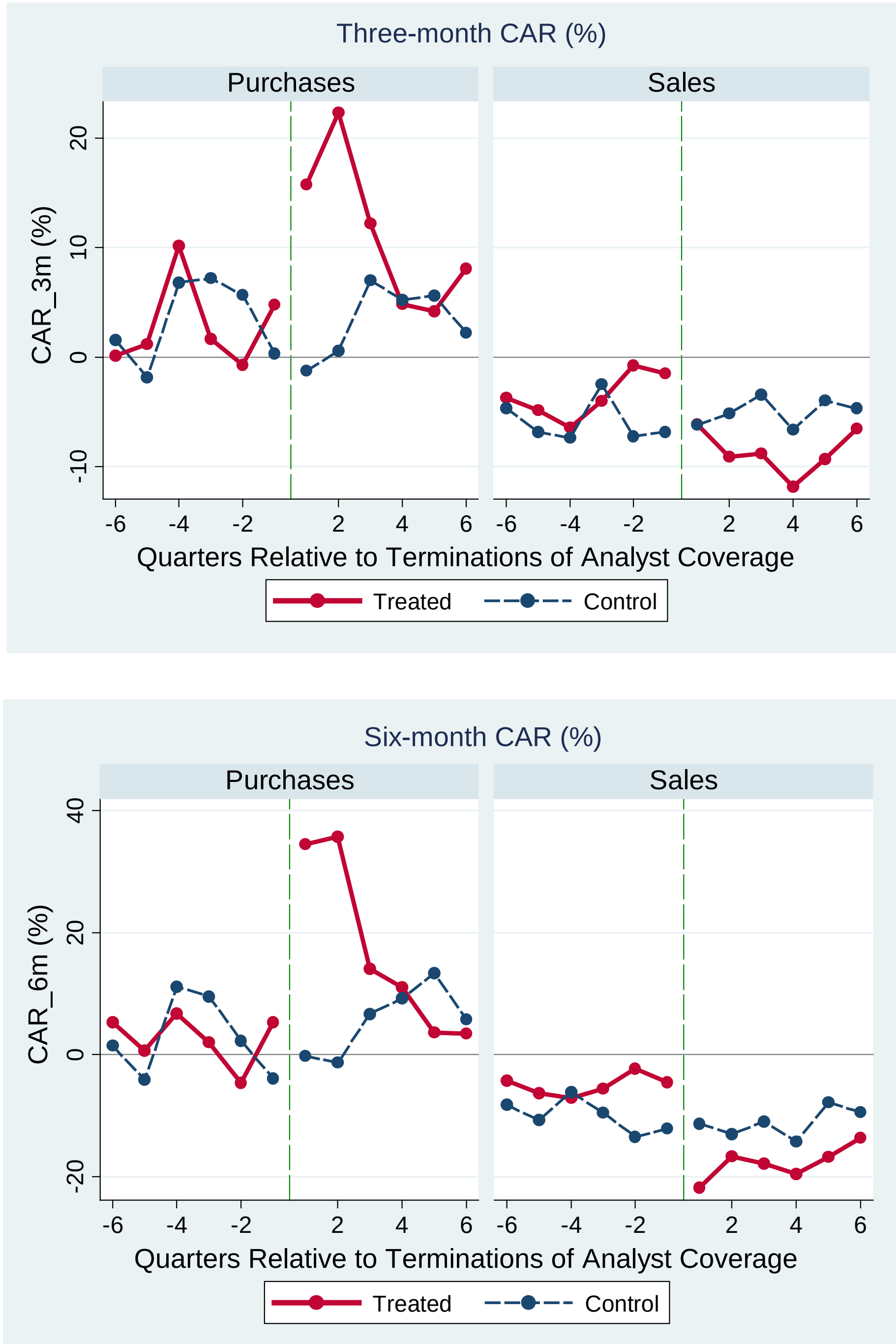
The above figure plots the mean value of the number of analysts covering a stock around the terminations of analyst coverage. Vertical dash lines represent the closures and mergers of the brokers. Red solid lines represent the average number of analysts for firms that experience terminations of analyst coverage (treated firms). Blue dash lines represent the average number of analysts for the matched control firms. The left panel plots results from the insider purchases sample, while the right panel plots results from the insider sales sample.

Figure 2. Covariates around the Mergers and Closures of Brokerage Firms



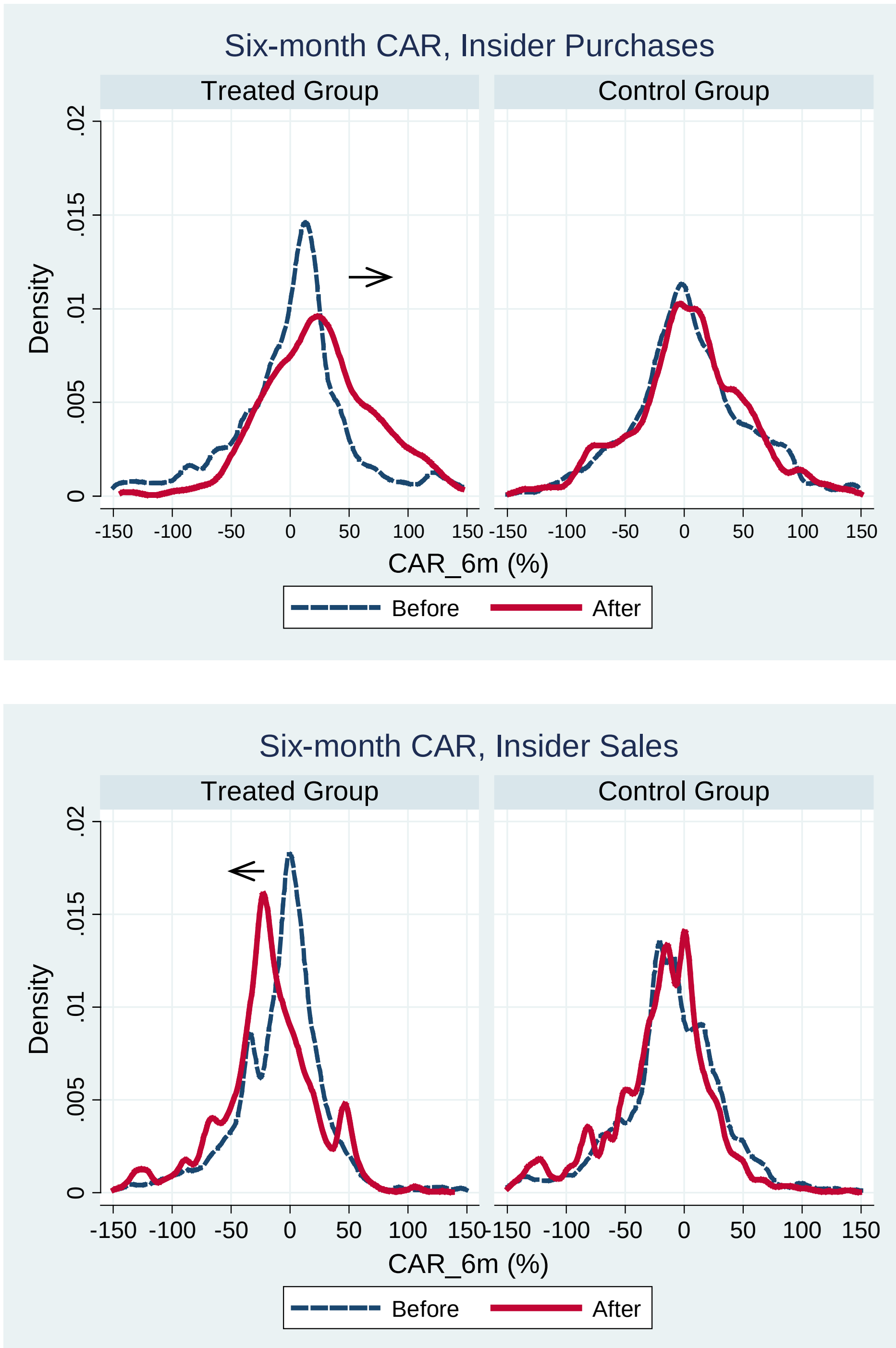
The above four figures plot the mean value of the covariates around the terminations of analyst coverage. The covariates are the natural log of the firm size (*LnSize*), the natural log of the book-to-market ratio (*LnBEME*), the natural log of the debt-to-equity ratio (*LnLeverage*), and the one-month returns prior to insiders' transactions (*Ret1mPrior*). Vertical dash lines represent the closures and mergers of the brokers. Red solid lines represent the covariates for firms that experience terminations of analyst coverage (treated firms). Blue dash lines represent the covariates for the matched control firms. The left panels plot results from the insider purchases sample, while the right panels plot results from the insider sales sample.

Figure 3. Insiders' Abnormal Returns around the Mergers and Closures of Brokerage Firms



The above two figures plot the average three-month (upper panel) and six-month (lower panel) cumulative abnormal returns for insider transactions that take place in different quarters relative to the terminations of analyst coverage. For example, the first red dot (from left) in the left upper panel represents the mean value of the three-month cumulative abnormal returns for all insider purchases that take place six quarters prior to the closures and mergers of the brokers and come from firms experiencing terminations of analyst coverage. The cumulative abnormal returns are estimated by Carhart's four-factor model. Vertical dash lines represent the closures and mergers of the brokers. Red solid lines represent the cumulative abnormal returns for firms that experience terminations of analyst coverage (treated firms). Blue dash lines represent the cumulative abnormal returns for the matched control firms. The left panels plot results from the insider purchases sample, while the right panels plot results from the insider sales sample. Notice that negative abnormal returns for insider sales represent losses the insiders avoid.

Figure 4. Kernel Density of Insiders' Abnormal Returns



The above two figures plot the kernel density of the six-month cumulative abnormal returns in both the insider purchases sample (upper panel) and in the insider sales sample (lower panel). The cumulative abnormal returns are estimated by Carhart's four-factor model. Red solid lines represent the kernel density of the six-month cumulative abnormal returns for insider transactions within the one year window after the closures and mergers of the brokers. Blue dash lines represent the kernel density of the six-month cumulative abnormal returns for insider transactions within the one year window prior to the closures and mergers of the brokers. The left panels plot results from the firms that experience terminations of analyst coverage (treated firms), while the right panels plot results from the matched control firms. Notice that negative abnormal returns for insider sales represent losses the insiders avoid.

Table 1. Ex-ante Summary Statistics

This table presents ex-ante summary statistics for both the treated group and the matched control group. The treated firms are firms that experience closure-related terminations of analyst coverage. Each treated firm is matched with up to five control firms in the same Fama-French size and book-to-market quintile. If more than five candidate firms exist, those with the closest bid-ask spreads prior to the terminations of analyst coverage are chosen. Summary statistics are calculated at the firm level using observations in the one-year period prior to the terminations of analyst coverage. *LnSize* is the natural log of market cap (in millions) in year t-1; *LnBEME* is the natural log of book-to-market ratio in year t-1; *LnLev* is the natural log of debt-to-equity ratio in year t-1; *Spreads* is the one-month (day -21 to day -1) average percentage bid-ask spreads prior to the insider transactions, where the percentage bid-ask spreads are calculated by $\frac{100*(ask-bid)}{(ask+bid)/2}$; *Ret1mPrior* is the one-month (day -21 to day -1) cumulative raw returns prior to insider transactions; *CAR1m*, *CAR3m*, and *CAR6m* are the cumulative abnormal returns adjusted by the Carhart four-factor model over one month, three months, and six months after the insider transactions; *LnShares* is the natural log of the transaction shares, and *LnValue* is the natural log of the transaction value. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses the insiders avoid. The data span 1999 to 2008.

Panel A. Insider purchases.

	Treated Group (# of firms = 129) (# of trades = 2599)		Control Group (# of firms = 529) (# of trades = 9422)		
	mean	std. dev.	mean	std. dev.	p-value
# of Coverage	4.03	2.07	3.78	2.95	0.36
Spreads (%)	1.81	1.92	1.81	1.83	0.94
LnSize	6.17	1.48	5.47	1.30	0.00***
LnBEME	-0.85	1.00	-0.85	0.95	0.95
LnLev	0.51	1.45	0.47	1.55	0.79
Ret1mPrior (%)	-7.44	19.56	-4.89	18.14	0.16
CAR1m (%)	2.10	13.39	2.42	15.63	0.83
CAR3m (%)	4.01	29.20	4.46	27.86	0.71
CAR6m (%)	2.45	48.82	5.31	47.96	0.64
LnShares	7.42	1.98	7.40	2.02	0.92
LnValue	9.94	1.86	9.89	1.78	0.80

Panel B. Insider sales.

	Treated Group (# of firms = 231) (# of trades = 11809)		Control Group (# of firms = 758) (# of trades = 42173)		
	mean	std. dev.	mean	std. dev.	p-value
# of Coverage	4.34	2.36	4.21	2.09	0.42
Spreads (%)	0.51	0.59	0.56	0.73	0.34
LnSize	6.56	1.31	5.88	1.11	0.00***
LnBEME	-0.99	0.79	-0.93	0.81	0.32
LnLev	-0.08	1.45	-0.04	1.47	0.72
Ret1mPrior (%)	5.79	12.72	6.60	15.85	0.48
CAR1m (%)	-1.26	9.23	-2.19	12.53	0.30
CAR3m (%)	-4.17	22.13	-6.21	24.66	0.26
CAR6m (%)	-6.25	35.77	-10.53	43.28	0.17
LnShares	7.53	1.84	7.44	1.73	0.50
LnValue	10.72	2.09	10.55	1.91	0.20

Table 2. Validity of the Quasi-Experimental Design

This table presents results from difference-in-differences specifications that evaluate changes in various measures after the terminations of analyst coverage. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. The data cover observations from both insider purchases sample and insider sales sample one year before and after the terminations of analyst coverage and span 1999 to 2008. *Actual Earnings* is the actual quarterly EPS normalized by the stock prices in the end of previous quarter. *Market Cap* is the market cap of the stocks. *Tobin's Q* is the total market cap normalized by total asset. *Profitability* is the operation income normalized by the total asset. *Log(Sales)* is the natural log of the revenue. *Monthly Returns* is the monthly stock return. *Bid-Ask Spreads* is the monthly average percentage bid-ask spreads, where the percentage bid-ask spreads are calculated by $\frac{100*(ask-bid)}{(ask+bid)/2}$. *Actual Earnings*, *Market Cap*, *Tobin's Q*, *Profitability*, and *Log(Sales)* are calculated at quarterly frequency, whereas *Monthly Returns* and *Bid-Ask Spreads* are calculated at monthly frequency. The difference-in-differences (DiD) coefficients are estimated by DiD specifications with firm fixed effects and calendar quarter fixed effects. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Variables	DiD Coefficients
Actual Earnings ($\times 10^{-4}$)	-6.447
	[19.558]
Market Cap (in millions)	-0.627
	[37.137]
Tobin's Q	-0.019
	[0.093]
Profitability	-0.004
	[0.124]
Log(Sales)	0.001
	[0.015]
Monthly Returns (%)	0.370
	[0.647]
Bid-Ask Spreads (%)	0.093**
	[0.043]

Table 3. Insiders' Abnormal Returns

This table presents results from difference-in-differences specifications that evaluate changes in insiders' abnormal returns after the terminations of analyst coverage. The dependent variables are insiders' cumulative abnormal returns adjusted by the Carhart four-factor model over one month (*CAR1m*), three months (*CAR3m*), and six months (*CAR6m*) after the insider transactions. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after terminations of analyst coverage. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses the insiders avoid. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

Panel A. Insider purchases.

	(1)	(2)	(3)	(4)	(5)	(6)
	CAR1m (%)	CAR1m (%)	CAR3m (%)	CAR3m (%)	CAR6m (%)	CAR6m (%)
Treat × Post	6.92***	7.97**	10.47***	9.86***	15.96***	18.35***
	[2.02]	[3.68]	[3.32]	[2.75]	[4.72]	[6.21]
LnSize	3.86	8.45*	3.42	10.55	-0.10	21.02*
	[3.88]	[4.87]	[4.58]	[7.16]	[9.57]	[10.83]
LnBEME	7.91**	10.46**	20.89***	21.02***	38.23***	45.65***
	[3.80]	[4.68]	[5.73]	[7.81]	[10.39]	[12.44]
LnLev	2.80	4.20	1.38	7.34	-3.32	5.05
	[2.09]	[2.90]	[5.72]	[7.10]	[8.59]	[9.81]
Ret1mPrior	-0.12***	-0.14***	-0.14***	-0.15***	-0.17***	-0.25***
	[0.02]	[0.03]	[0.04]	[0.05]	[0.05]	[0.06]
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider	firm	firm × insider
# of transactions	24078	24078	24008	24008	23726	23726
R-squared	0.47	0.65	0.57	0.74	0.67	0.82

Panel B. Insider sales.

	(1)	(2)	(3)	(4)	(5)	(6)
	CAR1m (%)	CAR1m (%)	CAR3m (%)	CAR3m (%)	CAR6m (%)	CAR6m (%)
Treat × Post	-2.49*	-2.89**	-6.11**	-6.05**	-10.73***	-10.17**
	[1.42]	[1.33]	[2.58]	[2.66]	[3.74]	[4.46]
LnSize	-5.79***	-5.01**	-7.25*	-6.45	-7.78	-9.39
	[2.01]	[1.96]	[4.02]	[4.76]	[6.30]	[6.94]
LnBEME	2.24	2.50	5.67*	4.09	12.62**	8.63
	[1.71]	[1.58]	[3.44]	[3.78]	[5.06]	[5.91]
LnLev	0.38	0.75	0.18	1.43	2.37	1.86
	[1.33]	[1.42]	[3.26]	[3.62]	[5.20]	[5.49]
Ret1mPrior	-0.08***	-0.10***	-0.09**	-0.09	-0.11*	-0.14**
	[0.03]	[0.03]	[0.04]	[0.06]	[0.06]	[0.07]
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider	firm	firm × insider
# of transactions	103330	103330	102838	102838	101908	101908
R-squared	0.48	0.63	0.59	0.74	0.64	0.77

Table 4. Anatomy of the Changes in Insiders' Abnormal Returns

This table presents the difference-in-differences coefficients from the regressions that evaluate changes in insiders' abnormal returns cumulated at different time windows. Firm fixed effects and calendar date fixed effects are included in the regressions. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The number of trading days in the corresponding CAR time windows are also listed in the table. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses the insiders avoid. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

Purchases Sample			Sales Sample		
Six-month window after trading dates (t)					
CAR window	DiD Coef. (%)	# of days	CAR window	DiD Coef. (%)	# of days
[t, t+126]	15.96***	127	[t, t+126]	-10.73***	127
From trading dates to filing dates (f)					
CAR window	DiD Coef. (%)	# of days	CAR window	DiD Coef. (%)	# of days
[t, f)	3.17*	17 (median)	[t, f)	-1.83**	3 (median)
Around filing dates (f)					
CAR window	DiD Coef. (%)	# of days	CAR window	DiD Coef. (%)	# of days
[f, f+1]	1.09**	2	[f, f+1]	-0.35	2
After filing dates (f)					
CAR window	DiD Coef. (%)	# of days	CAR window	DiD Coef. (%)	# of days
[f+2, t+126]	11.09*	108 (median)	[f+2, t+126]	-8.07**	122 (median)
Around earnings report release dates (after filing dates)					
CAR window	DiD Coef. (%)	# of days	CAR window	DiD Coef. (%)	# of days
[0, 2] around ER	4.26***	6 (median)	[0, 2] around ER	-2.63	6 (median)
[-2, 2] around ER	5.01***	10 (median)	[-2, 2] around ER	-4.32**	10 (median)
Around 8-K release dates (after filing dates)					
CAR window	DiD Coef. (%)	# of days	CAR window	DiD Coef. (%)	# of days
[0, 2] around 8K	2.58*	3 (median)	[0, 2] around 8K	-4.42*	12 (median)
[-2, 2] around 8K	3.63**	5 (median)	[-2, 2] around 8K	-5.97**	20 (median)
Dates not around release of earnings or 8-K (after filing dates)					
CAR window	DiD Coef. (%)	# of days	CAR window	DiD Coef. (%)	# of days
exclude [-2, 2] around ER/8K	2.51	93 (median)	exclude [-2, 2] around ER/8K	1.69	92 (median)

Table 5. Impact of the Initial Coverage

Panel A presents results from difference-in-differences specifications that evaluate the impact of the initial coverage on the changes of insiders' abnormal returns after the terminations of analyst coverage. *Initial Coverage* is the number of analysts covering a stock prior to the terminations of analyst coverage. The regressions are run in samples with low levels of initial coverage (<=5 analysts), medium levels of initial coverage (6~10 analysts), and high levels of initial coverage (>=11 analysts). *CAR6m* are insiders' cumulative abnormal returns over six months after the insider transactions. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after terminations of analyst coverage. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Negative abnormal returns for insider sales represent losses the insiders avoid. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

Panel A. Split sample analysis.

	(1)	(2)	(3)	(4)	(5)	(6)
	Insider Purchases			Insider Sales		
Initial Coverage	Low	Medium	High	Low	Medium	High
	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)
Treat × Post	15.96***	12.60	4.60	-10.73***	-5.50	6.30
	[4.72]	[8.73]	[7.41]	[3.74]	[3.56]	[3.88]
Controls	Y	Y	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm	firm	firm	firm	firm
# of transactions	23726	19729	19282	101908	120140	224812
R-squared	0.67	0.77	0.84	0.64	0.70	0.56

Panel B . DiD analysis with parametric adjustment in the treatment intensity.

	(1)	(2)	(3)	(4)
	Insider Purchases		Insider Sales	
	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)
Treat × Post × 1/Initial Coverage	19.10**	21.41**	-18.69***	-14.79**
	[7.94]	[10.84]	[6.06]	[6.75]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider
# of transactions	43990	43990	446860	446860
R-squared	0.67	0.82	0.55	0.69

Table 6. Dynamics of the Treatment Effects

This table presents results from specifications that evaluate dynamics of insiders' abnormal returns around the terminations of analyst coverage. The dependent variables are insiders' cumulative abnormal returns adjusted by the Carhart four-factor model over three months (*CAR3m*) and six months (*CAR6m*) after the insider transactions. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *(6 ~ 12m before)*, *(0 ~ 6m after)*, and *(6 ~ 12m after)* are dummy variables that equal 1 if the transaction happens within [-12, -6), [0, 6), and [6, 12] months relative to the terminations of analyst coverage. Notice that *Treat* × *(0 ~ 6m before)* is omitted in the regression. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior* which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses the insiders avoid. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

Panel A. Dynamics of the treatment effects.

	(1)	(2)	(3)	(4)
	Insider Purchases		Insider Sales	
	CAR3m (%)	CAR6m (%)	CAR3m (%)	CAR6m (%)
Treat × (6 ~ 12 m before)	2.23	1.65	-3.86	-5.10
	[5.07]	[2.94]	[5.82]	[5.60]
Treat × (0 ~ 6 m before)	omitted	omitted	omitted	omitted
Treat × (0 ~ 6 m after)	13.33***	20.72***	-8.31**	-13.38**
	[3.03]	[4.50]	[3.43]	[5.22]
Treat × (6 ~ 12 after)	5.99	7.28	-5.86*	-10.70**
	[5.09]	[6.87]	[3.27]	[5.28]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm	firm	firm
# of transactions	24008	23726	102838	101908
R-squared	0.57	0.67	0.59	0.64

Table 6. Dynamics of the Treatment Effects (continued)

Panel B. Impact of the recovery of the number of analysts on the dynamics of the treatment effects.

I calculate the recovery in the number of analysts by subtracting the number of analysts right after the terminations from the number of analysts six months after the terminations. Treated firms are sorted into two groups based on this measure. The sorting is performed annually and independently to the number of analysts prior to the terminations to ensure the two groups of firms have the same treatment intensity. *Strong_Analyst_Recovery* is a dummy variable that equals 1 if a firm's recovery in the number of analysts is larger than the median values.

	(1)	(2)	(3)	(4)	(5)	(6)
	Insider Purchases			Insider Sales		
	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)
Treat × (0 ~ 6 m after)	21.49**	19.23***	24.55**	-15.52**	-13.39**	-18.31*
	[9.37]	[4.39]	[10.12]	[7.54]	[6.65]	[10.41]
Treat × (0 ~ 6 m after) ×	-6.11		-16.17	6.26		13.68
Strong_Analyst_Recovery	[17.25]		[18.82]	[13.17]		[12.94]
Treat × (6 ~ 12 m after)	6.26	16.68*	18.92*	-12.09**	-18.44***	-20.61***
	[6.87]	[9.95]	[10.93]	[6.02]	[4.52]	[5.65]
Treat × (6 ~ 12 m after) ×		-25.48*	-27.42**		17.86*	23.82***
Strong_Analyst_Recovery		[13.43]	[13.52]		[9.26]	[8.56]
Controls	Y	Y	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm	firm	firm	firm	firm
# of observations	23726	23726	23726	101908	101908	101908
R-squared	0.67	0.67	0.67	0.64	0.64	0.64

Table 7. Insiders' Trading Volume and Transaction Value

This table presents results from difference-in-differences specifications that evaluate changes in insiders' trading volume and transaction value after the terminations of analyst coverage. The dependent variables are the natural log of the transaction shares (*LnShares*) and the natural log of insider transaction value (*LnValue*). *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after the terminations of analyst coverage. Treated firms are sorted into two groups based on the bid-ask spreads prior to the coverage reductions. The sorting is performed annually and independently to the number of analysts to ensure the two groups of firms have the same treatment intensity. *Liquid (LIlliquid)* is a dummy variable that equals 1 if a firm's average bid-ask spreads are smaller (larger) than the median values. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

Panel A. Insider purchases

	(1)	(2)	(3)	(4)
	LnShares	LnShares	LnValue	LnValue
Treat × Post	0.19		0.16	
	[0.14]		[0.14]	
Treat × Post × Liquid		0.49***		0.34***
		[0.12]		[0.12]
Treat × Post × Illiquid		-0.06		0.01
		[0.21]		[0.24]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm × insider	firm × insider	firm × insider	firm × insider
# of transactions	24857	24857	24857	24857
R-squared	0.77	0.77	0.74	0.74

Panel B. Insider sales

	(1)	(2)	(3)	(4)
	LnShares	LnShares	LnValue	LnValue
Treat × Post	0.07		0.11	
	[0.07]		[0.11]	
Treat × Post × Liquid		0.16***		0.17
		[0.05]		[0.10]
Treat × Post × Illiquid		-0.03		0.03
		[0.14]		[0.19]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm × insider	firm × insider	firm × insider	firm × insider
# of transactions	109183	109183	109183	109183
R-squared	0.62	0.62	0. 65	0.65

Table 8. Insiders' Abnormal Profits

This table presents results from difference-in-differences specifications that evaluate changes in insiders' abnormal profits after the terminations of analyst coverage. The dependent variables *Profit1m*, *Profit3m*, and *Profit6m* are the cumulative abnormal profits over one month, three months, and six months after the insider transactions. The cumulative abnormal profits are calculated by *CAR1m**TradeValue*, *CAR3m**TradeValue*, and *CAR6m**TradeValue*, respectively, and are winsorized at the 2.5th and 97.5th percentiles of their respective empirical distributions. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after terminations of analyst coverage. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal profits for insider sales represent losses the insiders avoid. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

Panel A. Insider purchases.

	(1)	(2)	(3)	(4)	(5)	(6)
	Profit1m ($)	Profit1m ($)	Profit3m ($)	Profit3m ($)	Profit6m ($)	Profit6m ($)
Treat × Post	2536**	2903	5500	5883*	5485	7230*
	[1162]	[2286]	[3488]	[3216]	[3628]	[4057]
Controls	Y	Y	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider	firm	firm × insider
# of transactions	24078	24078	24008	24008	23726	23726
R-squared	0.43	0.55	0.47	0.59	0.47	0.58

Panel B. Insider sales.

	(1)	(2)	(3)	(4)	(5)	(6)
	Profit1m ($)	Profit1m ($)	Profit3m ($)	Profit3m ($)	Profit6m ($)	Profit6m ($)
Treat × Post	-2432*	-2459	-9255**	-7148*	-14186**	-9654
	[1461]	[1798]	[4248]	[4077]	[7074]	[6299]
Controls	Y	Y	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider	firm	firm × insider
# of transactions	103330	103330	102838	102838	101908	101908
R-squared	0.28	0.46	0.33	0.53	0.38	0.58

Table 9. Aggregate Abnormal Profits

This table presents results from difference-in-differences specifications that evaluate changes in the aggregate abnormal profits after the terminations of analyst coverage. The cumulative abnormal profits over one month, three months, and six months after the insider transactions are aggregated at the insider-quarter level (*IQ_Profit1m*, *IQ_Profit3m*, and *IQ_Profit6m*). The cumulative abnormal profits are winsorized at the 2.5th and 97.5th percentiles of their respective empirical distributions. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after the terminations of analyst coverage. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal profits for insider sales represent losses the insiders avoid. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

	(1)	(2)	(3)	(4)	(5)	(6)
	Insider Purchases			Insider Sales		
	IQ_Profit1m ($)	IQ_Profit3m ($)	IQ_Profit6m ($)	IQ_Profit1m ($)	IQ_Profit3m ($)	IQ_Profit6m ($)
Treat × Post	4078	14914*	21861***	-32953**	-133325***	-224229***
	[3133]	[6805]	[5390]	[12822]	[41643]	[74546]
Controls	Y	Y	Y	Y	Y	Y
Fixed effect 1	year	year	year	year	year	year
Fixed effect 2	firm × insider	firm × insider	firm × insider	firm × insider	firm × insider	firm × insider
# of insider-quarters	6824	6824	6824	14288	14288	14288
R-squared	0.27	0.27	0.30	0.18	0.22	0.27

Table 10. Evidence Supporting the "Information Provider" Channel

This table presents evidence that suggests the terminations of analyst coverage increases information asymmetry through reducing the signal precision of the uninformed traders.

Panel A. Changes in the precision of the analyst forecast after terminations of the analyst coverage.

This panel presents results from difference-in-differences specifications that evaluate changes in the precision of the analyst forecast after the terminations of analyst coverage. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. The data cover observations from both insider purchases sample and insider sales sample six months (or one year) before and after the terminations of analyst coverage and span 1999 to 2008. Forecast error is the absolute difference between the analyst forecast and the actual EPS, normalized by the stock price in the end of previous quarter. *Median Earnings Forecast Error* is the median value of the forecast error across analysts covering a particular stock in each quarter. *Mean Earnings Forecast Error* is the mean value of the forecast error across analysts covering a particular stock in each quarter. *Dispersion of the Forecasts* is the standard deviation of the estimated EPS from all analysts covering a particular stock in each quarter, where the estimated EPS is normalized by the stock price in the end of previous quarter. *Absolute Value of the Earnings Surprises* is the absolute difference between the consensus and the actual EPS, where both the consensus and the actual EPS are normalized by the stock price in the end of previous quarter. The difference-in-differences (DiD) coefficients are estimated by DiD specifications with firm fixed effects and calendar quarter fixed effects. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	DiD Coefficients	
Sample Periods	[-6m, 6m]	[-12m, 12m]
Median Earnings Forecast Error ($\times 10^{-4}$)	24.12** [10.05]	16.56* [8.78]
Mean Earnings Forecast Error ($\times 10^{-4}$)	25.70** [10.42]	16.42* [9.32]
Dispersion of the Forecasts ($\times 10^{-4}$)	15.47** [6.07]	12.04** [5.87]
Absolute Value of the Earnings Surprises ($\times 10^{-4}$)	24.07** [9.62]	13.80* [8.31]

Table 10. Evidence Supporting the "Information Provider" Channel (continued)

Panel B. Heterogeneity across the reduction in the precision of the analyst forecasts.

This panel presents results from difference-in-differences specifications that illustrate the heterogeneity in the treatment effects across the level of reduction in the precision of the analyst forecasts. *CAR6m* are insiders' cumulative abnormal returns over six months after the insider transactions. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after the terminations of analyst coverage. Treated firms are sorted into two groups based on the level of reduction in the precision of the analyst forecasts. I compute the first principle component (*PCA1*) of the four measures of the precision of the analyst forecasts: *Median Earnings Forecast Error*, *Mean Earnings Forecast Error*, *Dispersion of the Forecasts*, and *Absolute Value of the Earnings Surprises*. I then sort the treated firms into two bins based on the level of reduction in the precision of the analyst forecasts (i.e., the average *PCA1* one year after the terminations of analyst coverage minus the average *PCA1* one year before the terminations of analyst coverage). The sorting is performed annually and independently to the number of analysts prior to the coverage reductions to ensure the two groups of firms have the same treatment intensity. *High_Precision_Reduction* is a dummy variable that equals 1 if a firm's reduction in the precision of the analyst forecast is larger than the median values. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior* which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses the insiders avoid. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

	(1)	(2)	(3)	(4)
	Insider Purchases		Insider Sales	
	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)
Treat × Post	7.17*	12.26*	-4.70	-3.94
	[3.70]	[6.55]	[3.90]	[4.22]
Treat × Post ×	12.55**	10.80*	-24.59**	-24.83**
High_Precision_Reduction	[5.01]	[5.64]	[11.49]	[11.19]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider
# of observations	20220	20220	99020	99020
R-squared	0.68	0.83	0.64	0.78

Table 11. Evidence against the "Discipline" Channel as the Sole Explanation

This table presents results from specifications that evaluate changes in the performance of active mutual funds in trading the affected stocks around the terminations of analyst coverage. For every stock that experiences coverage reductions, an aggregate mutual fund is constructed by aggregating the holdings of all the active funds that hold this stock in the one-year window prior to the terminations. I focus my analysis on actively managed equity mutual funds and eliminate balanced, bond, money market, international, and index funds. Active equity mutual funds are selected based on Lipper objectives following the approach in Huang, Sialm, and Zhang (2011). The quarterly "trading" directions (H_{ft}) of this aggregate fund are then determined based on the weights of the affected stock calculated from the quarterly holding data. H_{ft} = 1 (or -1) if the weights of the affected stocks increase (or decrease) after adjusting for the price changes of all the stocks in the aggregated fund. The dependent variables are the excess returns of the aggregate mutual funds calculated by subtracting the returns of the characteristic-based benchmarks (Daniel et al. 1997, Wermers 2004) from the raw returns of the affected stocks. *Post* is a dummy variable that equals 1 for quarters after the terminations of analyst coverage. I include the excess returns four quarters before and after the termination dates in the regression. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
Sample	All Sample (H_{ft} = ±1)	Increased Holdings (H_{ft} = 1)	Decreased Holdings (H_{ft} = -1)
	Quarterly Abnormal Returns for the Active Mutual Funds (%) $(R_{ft} - R_{bft}) * H_{ft}$		
Post	1.18**	1.86***	0.40
	[0.57]	[0.48]	[0.66]
Fixed effect 1	year	year	year
Fixed effect 2	fund	fund	fund
# of fund-quarters	5526	2533	2993
R-squared	0.31	0.82	0.76

Internet Appendix

A. Additional Heterogeneity Tests

A.1 Heterogeneity across Insiders' Trading Patterns

Corporate insiders trade for a variety of reasons. On one hand, they may trade on their private information to gain abnormal profits. On the other hand, personal liquidity, diversification, signaling and other motivations that are not necessarily information driven can drive their transactions. Cohen, Malloy, and Pomorski (2012) sort corporate insiders into "routine" traders and "opportunistic" traders based on their trading patterns. They find that "opportunistic" traders earn significant abnormal returns, whereas the returns for "routine" traders are essentially zero. Because "opportunistic" traders are more likely to be the insiders who trade actively on their private information, I expect them to be more aggressive in taking advantage of the increased information asymmetry after terminations of analyst coverage. To test this hypothesis, I follow Cohen, Malloy, and Pomorski (2012) and identify the insiders' type based on their historical trading patterns. Specifically, I define insiders as routine traders if they place a trade in the same calendar month in the three-year window prior to the terminations of analyst coverage. I define opportunistic traders as everyone else. Because the terminations of analyst coverage happen at the firm level, I compute the concentration of opportunistic traders at the firm level and then sort all the treated firms into two groups based on this measure. The sorting is performed annually and independently to the number of analysts to ensure the two groups of firms have the same treatment intensity. I create a dummy variable *High_Pct_Opportunistic_Insider* that equals 1 if a firm's concentration of opportunistic traders is larger than the median values. I then interact this dummy variable with the DiD term and run the following regression:

$$y_{ift} = \alpha_{(i)f} + \alpha_t + \beta_1 * Treat_{ft} \times Post_{ft}$$
$$+ \beta_2 * Treat_{ft} \times Post_{ft} \times High\_Pct\_Opportunistic\_Insider_{ft} + \gamma' X_{ift} + \varepsilon_{ift}$$

Here, β_1 represents the change in insiders' abnormal returns for firms populated with routine insiders. β_2 represents the additional treatment effects for firms populated with opportunistic insiders. The dummy variable *High_Pct_Opportunistic_Insider* by itself is not included in the regression, because I have already included firm fixed effects in the regression. Table A2 presents the results of the regressions. For firms populated with routine insiders, the changes in insiders' abnormal returns after terminations of analyst coverage are essentially zero. All the treatment effects come from firms populated with opportunist insiders. These results suggest that insiders who exhibit opportunist trading behaviors actively trade on the increased informational advantage brought by the coverage reductions in order to earn abnormal profits.

A.2 Heterogeneity across Insiders' Diversification Need

Insiders' stockholdings of their own companies can be a non-trivial portion of their wealth, because they are commonly compensated with stocks and options. Thus insiders' desire to diversify may affect their incentives to take advantage of the increased information asymmetry after coverage reductions. Insiders who have a higher level of firm stockholdings are probably more likely to sell and less likely to purchase when information asymmetry increases. To test this hypothesis, I compute the percentage of stocks and options in insiders' total compensations in the three-year time window prior to the terminations of analyst coverage using ExecuComp data. I define insiders as incentivized insiders if more than 75% of their compensation is paid using stocks and options.[1] I then compute the

[1] Results are robust to other cutoffs such as 50% and 90%.

concentration of incentivized insiders at the firm level and sort the treated firms into two groups based on this measure. The sorting is performed annually and independently to the number of analysts to ensure the two groups of firms have the same treatment intensity. I create a dummy variable $High\_Pct\_Incentivized\_Insider$ that equals 1 if a firm's concentration of incentivized insiders is larger than the median values. I then interact this dummy variable with the DiD term and run the following regression:

$$y_{ift} = \alpha_{(i)f} + \alpha_t + \beta_1 * Treat_{ft} \times Post_{ft} \\ + \beta_2 * Treat_{ft} \times Post_{ft} \times High\_Pct\_Incentivized\_Insider_{ft} + \gamma' X_{ift} + \varepsilon_{ift}$$

Table A3 presents the results of the regressions. In the insider purchases sample, the DiD coefficients are significantly positive for firms populated with non-incentivized insiders. However, the treatment effects are much weaker and are essentially zero for firms populated with incentivized insiders, suggesting that insiders who have heavy exposure to the stocks of their own companies avoid purchasing additional stocks when information asymmetry increases. The results in the insider sales sample are exactly the opposite. All the treatment effects come from firms populated with incentivized insiders in the insider sales sample, suggesting that insiders who have heavy exposure to the stocks of their own companies actively sell their stocks when information asymmetry increases. The above results confirm the hypothesis that insiders' desire to diversify influences their response to changes in information asymmetry.

A.3 Heterogeneity across Intensity of Regulatory Enforcement

If insiders trade on their private information to gain abnormal profits, they should be concerned about the potential litigation risks and adjust their trading behavior according to the intensity of legal enforcement. Del Guercio, Odders-White and Ready (2013) show that the SEC enforcement intensity deters illegal insider trading. Cohen, Malloy and Pomorski (2012) show that opportunistic corporate insiders reduce their trades followings waves of the SEC insider trading enforcement. In the context of my paper, insiders should be less aggressive in taking advantage of the increased information asymmetry during time periods with a higher intensity of legal enforcement. To test this hypothesis, I sort calendar months into two bins based on the number of litigation cases released by the SEC. The sorting is performed annually to remove any time trend in the SEC enforcement activities. I use the one-month-lagged number under the assumption that insiders adjust their behavior after observing the enforcement activities of the SEC in the previous month. I create a dummy variable $Low\_Enforcement\_Intensity$ that equals 1 if insider transactions take place in the calendar month with low enforcement intensity. I then interact this dummy variable with the DiD term and run the following regression:

$$y_{ift} = \alpha_{(i)f} + \alpha_t + \beta_1 * Treat_{ft} \times Post_{ft} \\ + \beta_2 * Treat_{ft} \times Post_{ft} \times Low\_Enforcement\_Intensity_t + \gamma' X_{ift} + \varepsilon_{ift}$$

Here, β_1 represents the change in insiders' abnormal returns during time periods with a high intensity of legal enforcement. β_2 represents the additional treatment effects during time periods with a low intensity of legal enforcement. Notice that I do not include the dummy variable $Low\_Enforcement\_Intensity$ itself in the regression, because I have already included time fixed effects in the regression. Table A4 presents the results of the regressions. In time periods with a high intensity of legal enforcement, the changes in insiders' abnormal returns after terminations of analyst coverage are not statistically significant. However, in both insider transaction samples, the treatment effects are significantly stronger in time periods with a low intensity of legal enforcement. These results suggest that litigation risks can deter insiders from taking advantage of the increase in information asymmetry after terminations of analyst coverage.

B. Additional Robustness Tests

B.1 Portfolio Return Analysis

I estimate the abnormal returns in the previous analysis at the individual transaction level. This approach effectively allows me to apply the DiD method, and controls for fixed effects and other variables at the transaction level. However, this approach warrants a couple of potential concerns. First, estimation of abnormal returns can be noisy at the individual transaction level. Second, in estimating cumulative abnormal returns, I implicitly assume the risk profiles (betas) of the stocks remain the same before and after the coverage reductions, which may not be true if the coverage reductions affect the riskiness of the stocks. Portfolio return analysis provides a useful tool for cross-checking the validity of the previous analysis, because it aggregates individual transactions to improve the signal-to-noise ratio, and allows for separate estimations of the portfolio risk profiles before and after the terminations of analyst coverage.

Because the abnormal stock returns at the individual transaction level increase significantly in the insider purchases sample, and decrease significantly in the insider sales sample after the terminations of analyst coverage, I expect to see the portfolio alphas of insider purchases become more positive and to see the portfolio alphas of insider sales become more negative after the terminations of analyst coverage. To test this hypothesis, I build insider portfolios using insider transactions of the treated firms 12 months before and after the terminations of analyst coverage. I assume that insiders hold their positions for three different horizons: one month, three months, and six months. Individual transactions are either weighted equally or by the treatment intensity (i.e., the reciprocal of the number of analysts initially covering the firm). The portfolio alphas are estimated using Carhart's four-factor model:[2]

$$R_{pt} - R_{ft} = \alpha_p + \beta_p * \left(R_{mt} - R_{ft}\right) + s_p * SMB_t + h_p * HML_t + m_p * MOM_t + \varepsilon_{pt}$$

Here, R_{pt} denotes the portfolio returns, R_{ft} denotes the risk-free rates, R_{mt} denotes the market returns, and SMB, HML, and MOM are factors downloaded from Kenneth French's website. Table A5 presents the alpha, Sharpe ratio, and annualized alpha for insider portfolios before and after the coverage reductions. As Panel A of Table A5 shows, the alphas of the insider purchases portfolio before the coverage reductions are not significantly different from zero. The Sharpe ratio ranges from 0.32 to 0.55 across the different combinations of holding periods and weighting methods. After the terminations of analyst coverage, the alphas of the insider purchases portfolio increase and are significantly positive (Panel B of Table A5). The annualized alphas range from 15.6% to 30.4%, whereas the Sharpe ratio ranges from 0.74 to 1.13 across the different combinations of holding periods and weighting methods. For insider sales, the portfolio alphas are positive (though not statistically different from zero) before the coverage reductions (Panel C of Table A5). However, they become negative and are significantly different from zero in many cases after the terminations of analyst coverage (Panel D of Table A5). Thus, consistent with the baseline analysis using abnormal returns at the transaction level, the portfolio return analysis also shows that insiders enjoy larger abnormal returns in their purchases and avoid more losses in their sales after the terminations of analyst coverage.

B.2 Alternative Measures of Abnormal Returns

[2] I also use the Fama-French three-factor model, five-factor model that adjusts exposures to aggregate liquidity (Pastor and Stambaugh 2003), and six-factor model that adjusts exposures to aggregate liquidity and short-term reversals. All of them yield similar results.

The abnormal returns used in the baseline specifications are cumulative abnormal returns (CARs). CARs ignore compounding, because they use arithmetic rather than geometric averages. Thus, CARs do not represent realized returns from an investor's point of view. Buy-and-hold abnormal returns (BHARs) are the alternative measure for abnormal returns. BHARs are the compound returns from a sample firm less the compound returns from a reference portfolio. Table A6 presents the coefficients in the DiD analysis using BHARs with the Fama-French 25 portfolio returns as the reference portfolio.[3] In the insider purchases sample, the six-month BHARs increase significantly after the terminations of analyst coverage. The DiD coefficients have a similar magnitude to those in the baseline analysis using CARs. In the insider sales sample, the coefficients are negative. However, their magnitude is significantly smaller compared to those from the baseline analysis. This difference is probably due to severe positive skewness, a prominent drawback of BHARs (Barber and Lyon 1997). In contrast to cumulative returns, compound buy-and-hold returns have lower bounds at -100%. Moreover, the returns of the individual stocks are more volatile compared to the returns of the reference portfolio in most cases. For example, it is common to observe a sample firm with an annual return in excess of 100%, but rare to observe a return on a Fama-French 25 portfolio in excess of 100%. Because BHARs are calculated as the compound sample firm return less the compound Fama-French 25 portfolio return, they are positively skewed. Therefore, we naturally have limited room to observe the negative shifts in BHARs after the terminations of analyst coverage. Whether CARs or BHARs are more appropriate for evaluating abnormal returns is still a topic up for debate in the literature. In the context of my paper, if we assume no portfolio rebalancing and calculate the gain or loss for those insiders within the sample, BHARs are perhaps better choices conceptually. However, if we are more interested in investigating the impact of information asymmetry on price efficiency, or assessing the profits for informed traders in a broader sense (e.g., insiders who can rebalance their portfolios), CARs probably constitute a better choice.

B.3 Control for Liquidity Measures

One potential concern about the analysis is that the terminations of analyst coverage will change stock liquidity, which in turn affects the stock returns. I acknowledge this possibility but would like to point out that the change in liquidity itself can be a consequence of an increase in information asymmetry (Kelly and Ljungqvist 2012, Vayanos and Wang 2011). Therefore, the ideal approach is to separate out the component of stock liquidity that is orthogonal to the change in information asymmetry, and then add it to the DiD specifications as a control. However, empirically this approach is extremely challenging, if not impossible. As a step toward this goal, I add two liquidity measures as additional controls in the analysis. The first measure is the Amihud illiquidity measure (Amihud 2002), which is the ratio of the absolute return to the dollar volume of trading. The second measure is historical liquidity beta (Pastor and Stambaugh 2003), which reflects firms' sensitivity to changes in aggregate liquidity. Table A7 presents the regression results after adding these two liquidity measures as additional controls. Consistent with previous literature, both the Amihud illiquidity measure and historical liquidity beta are positively correlated with expected stock returns (Amihud 2002; Chordia, Huh, and Subrahmanyam 2009; Pastor and Stambaugh 2003). The coefficients of the DiD terms remain positive in the insider purchases sample and negative in the insider sales sample, with magnitudes similar to those in the baseline analysis. Note that the coefficients of the DiD terms in Table A7 may still be biased because the liquidity measures contain signals of information asymmetry. However, given that I obtain similar coefficients in the specifications with and without liquidity measures, controlling for the liquidity component that is orthogonal to information asymmetry is unlikely to overturn the results in the baseline analysis.

B.4 Exclude Tiny Firms and Low-price Transactions

[3] $BHAR_i(0,T) = \prod_{t=0}^{T}(1 + r_{it}) - \prod_{t=0}^{T}(1 + r_{pt})$, where r_{it} is the return of the individual firms and r_{pt} is the return of the corresponding Fama-French 25 portfolio.

As the summary statistics show, the firm size in my sample is relatively small, which raises a concern that tiny firms are the primary driver of the treatment results, which if true, would cast doubts on the external validity of my analysis. To rule out this possibility, I first exclude all firms smaller than 100M, and then re-run the DiD analysis. The resulting dataset contains 78.2% of the purchases and 92.4% of the sales in the baseline analysis. Panel A of Table A8 presents the regression results. The coefficients of the DiD terms remain similar to those in the baseline specifications. Next I further exclude insider transactions with prices less than $5. The resulting dataset contains 62.6% of the purchases and 89.4% of the sales in the baseline analysis. Panel B of Table A8 shows the regression results for insiders' abnormal returns. Again, the coefficients of the DiD terms are similar to those in the baseline specifications. Taken together, these results suggest the treatment effects I find in the baseline analysis are not limited to tiny firms or low-price transactions.

B.5 Placebo Tests

Two pieces of information associated with the coverage reductions are essential for the DiD analysis: which firms receive the coverage reductions (identified by PERMNOs) and when the coverage reductions occur (termination dates). I falsely replace these two pieces of information one at a time to generate "placebo termination" and then re-run the analysis. In the first placebo test, I falsely shift termination dates backwards for three years while keeping the identities of the treated firms unchanged. In the second test, I falsely replace treated firms with control firms that are in the same Fama-French size and book-to-market quintile and have close bid-ask spreads, while keeping the termination dates unchanged. Panel A and Panel B of Table A9 present the regression results of the above two placebo tests, respectively. The coefficients of the *Treat* × *Post* terms are no longer significant in either of these two tests, suggesting the treatment effects I find in the baseline analysis are not driven by the identities of the treated firms or the termination dates alone, but rather by the combination of the two (i.e., the actual terminations of analyst coverage).

C. Changes in Insiders' Trading Probability

Does increases in information asymmetry alter insiders' trading behavior in the extensive margin? To address this question, I examine whether the probability of insider trading changes in response to an increase in information asymmetry. I construct a firm-day panel containing all trading dates one year before and after the terminations of analyst coverage for every firm in my sample. The dependent variable *Buy* (*Sell*) is a dummy variable that equals 1 if insiders purchase (sell) in a given firm-day. The mean value of *Buy* is 3.5% (i.e., insiders in a given firm buy in one day out of every 29 trading days), whereas the mean value of *Sell* is 6.2% (i.e., insiders in a given firm sell in one day out of every 16 trading days). I run the DiD analysis using linear probability models. Table A10 present the coefficients of the tests.

Consistent with previous studies (Rozeff and Zaman 1998, Lakonishok and Lee 2001, Jenter 2005), I find that insiders try to time the market when they trade. First, insiders appear to exploit the size premium. The coefficients of *LnSize* are significantly negative for insider purchases and significantly positive for insider sales, which indicates insiders are more likely to buy when firm size decreases and more likely to sell when firm size increases. Second, insiders also exploit the value premium. The coefficients of *LnBEME* are significantly negative for insider sales, suggesting insiders are less likely to sell when the book-to-market ratio increases. Furthermore, insiders bet heavily on short-term reversals. The coefficients of *Ret1mPrior* are significantly negative for insider purchases and significantly positive for insider sales, which means insiders are more likely to buy when stock price has recently decreased and more likely to sell when stock price has recently increased.

Similar to the trading volume, the increase in information asymmetry also alters insiders' trading probability in a way that depends on the liquidity of the stocks. For stocks with high liquidity (quartile 1 sorted by the bid-ask spreads), the trading probability increase significantly by 1.8% in the insider purchases sample, and by 4.0% (both in absolute terms) in the insider sales sample, whereas the trading probability remain the same for other stocks (quartile 2, 3, and 4 sorted by the bid-ask spreads). These results suggest that information asymmetry also influence insiders' trading behavior in the extensive margin.

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Table A1. Impact of the Sarbanes-Oxley (SOX) Act

This table presents results from difference-in-differences specifications that evaluate the impact of the SOX Act on the changes of insiders' abnormal returns after the terminations of analyst coverage. *SOX* is a dummy variable that equals 1 if the transaction happens in September, 2003 or after. *CAR6m* are insiders' cumulative abnormal returns over six months after the insider transactions. *CARTtoF* are insiders' cumulative abnormal returns from the trading dates to the filing dates. *CARFto6m* are insiders' cumulative abnormal returns from the filing dates to six months after the trading dates. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after the terminations of analyst coverage. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses the insiders avoid. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

	(1)	(2)	(3)	(4)	(5)	(6)
	Insider Purchases			Insider Sales		
	CAR6m (%)	CARTtoF (%)	CARFto6m (%)	CAR6m (%)	CARTtoF (%)	CARFto6m (%)
Treat × Post	16.92***	3.70*	12.39*	-12.59**	-3.60***	-8.65**
	[5.95]	[2.05]	[6.92]	[5.11]	[1.33]	[3.82]
	-6.10	-2.87	-1.81	3.41	3.76***	-0.24
Treat × Post × SOX	[12.94]	[2.31]	[16.56]	[7.45]	[1.45]	[7.21]
Controls	Y	Y	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm	firm	firm	firm	firm
# of transactions	23726	20995	20980	101908	100499	100494
R-squared	0.67	0.52	0.69	0.64	0.41	0.64

Table A2. Heterogeneity across Insiders' Trading Patterns

This table presents results from difference-in-differences specifications that illustrate the heterogeneity of the treatment effects. *CAR6m* are insiders' cumulative abnormal returns over six months after the insider transactions. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after the terminations of analyst coverage. Insiders are defined as routine traders if they place a trade in the same calendar month in the three-year window prior to the terminations of analyst coverage. Opportunistic traders are defined as everyone else. Treated firms are sorted into two groups based on the concentration of opportunistic traders. The sorting is performed annually and independently to the number of analysts to ensure the two groups of firms have the same treatment intensity. *High_Pct_Opportunistic_Insider* is a dummy variable that equals 1 if a firm's concentration of opportunistic traders is larger than the median values. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses the insiders avoid. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

	(1)	(2)	(3)	(4)
	Insider Purchases		Insider Sales	
	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)
Treat × Post	0.81	-1.16	3.01	7.14
	[6.67]	[7.84]	[6.68]	[9.01]
Treat × Post × High_Pct_Opportunistic_Insider	19.74***	24.82**	-16.92**	-21.20**
	[3.66]	[10.37]	[8.55]	[10.17]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider
# of transactions	23726	23726	101809	101809
R-squared	0.67	0.82	0.64	0.78

Table A3. Heterogeneity across Insiders' Diversification Need

This table presents results from difference-in-differences specifications that illustrate the heterogeneity of the treatment effects. *CAR6m* are insiders' cumulative abnormal returns over six months after the insider transactions. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after the terminations of analyst coverage. Insiders are defined as incentivized insiders if more than 75% of their total compensations are paid using stocks and options in the three-year time window prior to the terminations of analyst coverage . Treated firms are sorted into two groups based on the concentration of incentivized traders. The sorting is performed annually and independently to the number of analysts to ensure the two groups of firms have the same treatment intensity. *High_Pct_Incentivized_Insider* is a dummy variable that equals 1 if a firm's concentration of incentivized insiders is larger than the median values. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses the insiders avoid. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

	(1)	(2)	(3)	(4)
	Insider Purchases		Insider Sales	
	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)
Treat × Post	19.69**	25.46**	-5.72	-9.27
	[10.16]	[12.09]	[5.16]	[6.34]
Treat × Post ×	-16.14	-27.29**	-32.28***	-22.23***
High_Pct_Incentivized_Insider	[12.42]	[13.70]	[10.45]	[8.48]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider
# of transactions	9586	9586	55026	55026
R-squared	0.78	0.90	0.63	0.79

Table A4. Heterogeneity across Intensity of Regulatory Enforcement

This table presents results from difference-in-differences specifications that illustrate the heterogeneity of the treatment effects. *CAR6m* are insiders' cumulative abnormal returns over six months after the insider transactions. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after the terminations of analyst coverage. Calendar months are sorted into two bins based on the number of litigation cases released by the SEC. The sorting is performed annually to remove any time trend in the SEC enforcement activities. One-month-lagged numbers are used in the sorting with the assumption that insiders adjust their trading behavior after observing the enforcement activities of the SEC in the previous months. *Low_Enforcement_Intensity* is a dummy variable that equals 1 if insider transactions take place in the calendar month with low enforcement intensity. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses the insiders avoid. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

	(1)	(2)	(3)	(4)
	Insider Purchases		Insider Sales	
	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)
Treat × Post	5.05	11.38	1.86	-2.56
	[8.47]	[9.69]	[9.95]	[9.06]
Treat × Post × Low_Enforcement_Intensity	18.45***	16.06**	-15.01**	-15.76***
	[6.65]	[6.94]	[6.76]	[5.76]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider
# of transactions	11631	11631	69908	69908
R-squared	0.71	0.82	0.64	0.78

Table A5. Portfolio Return Analysis

This table presents the performance-evaluation results for insider portfolios before and after the terminations of analyst coverage. Insider transactions from firms that have five or fewer analysts prior to coverage reductions are included in the portfolios. The holding periods of the insider transactions are one month, three months, and six months. Insider transactions are weighted either equally or by treatment intensity (1/ the number of the initial coverage). The dependent variables are excess portfolio returns. The independent variables include *RMRF*, *SMB*, *HML*, and *MOM*, which are daily factors downloaded from Kenneth French's website. α is the regression intercept. *Annualized* α = 252*α and *Sharpe ratio* = $\frac{\sqrt{252} * \overline{\text{excess portfolio return}}}{\sigma\,(excess\ portfolio\ return)}$. Standard errors are robust to heteroskedasticity. ***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively. The negative alpha and Sharpe ratio for insider sales represent losses the insiders avoid. The sample is from 2000/5/1 to 2007/12/3 (a common period shared by all subsamples).

Panel A. Portfolios constructed based on insider purchases one year before coverage reductions.

	(1)	(2)	(3)	(4)	(5)	(6)
	Excess Portfolio Returns of Insider Purchases (%)					
Holding Period	1 month	1 month	3 months	3 months	6 months	6 months
Weighting Method	Equal	1/Coverage	Equal	1/Coverage	Equal	1/Coverage
α	0.03	0.03	0.03	0.06	0.02	0.02
	[0.04]	[0.05]	[0.04]	[0.05]	[0.04]	[0.04]
Sharpe ratio	0.37	0.34	0.42	0.55	0.34	0.32
Annualized α (%)	7.15	7.16	7.53	13.78	4.43	4.54
# of trading days	1909	1909	1909	1909	1909	1909
R-squared	0.23	0.16	0.29	0.20	0.36	0.28

Panel B. Portfolios constructed based on insider purchases one year after coverage reductions.

	(1)	(2)	(3)	(4)	(5)	(6)
	Excess Portfolio Returns of Insider Purchase (%)					
Holding Period	1 month	1 month	3 months	3 months	6 months	6 months
Weighting Method	Equal	1/Coverage	Equal	1/Coverage	Equal	1/Coverage
α	0.12***	0.11**	0.08***	0.06*	0.08***	0.07**
	[0.04]	[0.05]	[0.03]	[0.04]	[0.03]	[0.03]
Sharpe ratio	1.13	0.92	0.94	0.74	1.04	0.83
Annualized α (%)	30.43	28.00	19.49	15.62	20.19	16.57
# of trading days	1909	1909	1909	1909	1909	1909
R-squared	0.30	0.25	0.40	0.35	0.44	0.41

Table A5. Portfolio Return Analysis (continued)

Panel C. Portfolios constructed based on insider sales one year before coverage reductions.

	(1)	(2)	(3)	(4)	(5)	(6)
	Excess Portfolio Returns of Insider Sales (%)					
Holding Period	1 month	1 month	3 months	3 months	6 months	6 months
Weighting Method	Equal	1/Coverage	Equal	1/Coverage	Equal	1/Coverage
α	-0.01	0.02	0.03	0.05	0.02	0.03
	[0.04]	[0.04]	[0.03]	[0.04]	[0.02]	[0.03]
Sharpe ratio	0.07	0.17	0.40	0.40	0.34	0.24
Annualized α (%)	-1.38	4.38	8.53	13.00	6.06	6.39
# of trading days	1909	1909	1909	1909	1909	1909
R-squared	0.42	0.39	0.56	0.49	0.65	0.58

Panel D. Portfolios constructed based on insider sales one year after coverage reductions.

	(1)	(2)	(3)	(4)	(5)	(6)
	Excess Portfolio Returns of Insider Sales (%)					
Holding Period	1 month	1 month	3 months	3 months	6 months	6 months
Weighting Method	Equal	1/Coverage	Equal	1/Coverage	Equal	1/Coverage
α	-0.09**	-0.11***	-0.04	-0.06*	-0.03	-0.05
	[0.04]	[0.04]	[0.03]	[0.03]	[0.03]	[0.03]
Sharpe ratio	-0.56	-0.71	-0.19	-0.36	-0.14	-0.30
Annualized α (%)	-22.85	-28.03	-9.58	-14.15	-7.09	-11.23
# of trading days	1909	1909	1909	1909	1909	1909
R-squared	0.46	0.45	0.58	0.56	0.66	0.63

Table A6. Buy-and-Hold Abnormal Returns

This table presents results from difference-in-differences specifications that evaluate changes in insiders' abnormal returns after the terminations of analyst coverage using an alternative measure. *BHAR6m* are insiders' buy-and-hold abnormal returns over six months after the insider transactions. *BHAR6m* are the compound returns on the individual firms less the compound returns on the corresponding Fama-French 25 portfolio. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after terminations of analyst coverage. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses the insiders avoid. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

	(1)	(2)	(3)	(4)
	Insider Purchases		Insider Sales	
	BHAR6m (%)	BHAR6m (%)	BHAR6m (%)	BHAR6m (%)
Treat × Post	16.58***	14.42***	-3.79*	-4.91*
	[3.79]	[4.48]	[2.26]	[2.62]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider
# of transactions	23802	23802	103246	103246
R-squared	0.70	0.86	0.63	0.76

Table A7. Control for Liquidity Measures

This table presents results from difference-in-differences specifications that evaluate changes of insiders' abnormal returns after the terminations of analyst coverage with liquidity measures as additional controls. *AIM* is the Amihud illiquidity measure calculated by $\ln\left(1+\frac{|return|}{|price|*volume}\right)*1{,}000{,}000$ (Amihud, 2002), and *Liqbeta* is the historical liquidity beta (Pastor and Stambaugh, 2003). *CAR6m* are insiders' cumulative abnormal returns over six months after the insider transactions. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after terminations of analyst coverage. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses avoided by the insiders. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

	(1)	(2)	(3)	(4)
	Insider Purchases		Insider Sales	
	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)
Treat × Post	16.27***	16.39***	-8.85**	-9.38**
	[4.39]	[4.15]	[3.56]	[3.87]
AIM	0.79	0.63	2.27**	1.91**
	[0.84]	[0.42]	[1.09]	[0.95]
Liqbeta	41.25**	42.10**	12.43*	12.03
	[18.21]	[19.53]	[6.70]	[7.51]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider
# of transactions	16990	16990	75810	75810
R-squared	0.67	0.82	0.68	0.81

Table A8. Exclude Tiny Firms and Low-price Transactions

This table presents results from difference-in-differences specifications that evaluate changes in insiders' abnormal returns after the terminations of analyst coverage. Firms with size less than 100 M are excluded in the analysis in Panel A. Firms with size less than 100M and transactions with price less than $5 are excluded in the analysis in Panel B. *CAR6m* are insiders' cumulative abnormal returns over six months after the insider transactions. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after termination of analyst coverage. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses avoided by the insiders. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

Panel A. Exclude tiny firms (i.e. firm size less than 100M).

	(1)	(2)	(3)	(4)
	Insider Purchases (Firm Size >= 100M)		Insider Sales (Firm Size >= 100M)	
	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)
Treat × Post	16.72***	16.60**	-11.60***	-12.24***
	[4.36]	[6.47]	[4.13]	[4.56]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider
# of transactions	18557	18557	94166	94166
R-squared	0.75	0.86	0.66	0.79

Panel B. Exclude tiny firms and low-price transactions (i.e., firm size less than 100M or transaction price less than $5).

	(1)	(2)	(3)	(4)
	Insider Purchases (Firm Size >= 100M & Transaction Price >= $5)		Insider Sales (Firm Size >= 100M & Transaction Price >= $5)	
	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)
Treat × Post	14.65***	14.32**	-11.70***	-11.93**
	[5.54]	[6.53]	[4.16]	[4.64]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider
# of transactions	14861	14861	91107	91107
R-squared	0.78	0.87	0.67	0.79

Table A9. Placebo Tests

This table presents results from difference-in-differences specifications that evaluate changes in insiders' abnormal returns after the terminations of analyst coverage in two placebo tests. In Panel A, the treated firms remain the same as those in the real test, but the termination dates of the analyst coverage are falsely shifted back for three years. In Panel B, the termination dates of the analyst coverage remain the same as those in the real test, but the treated firms are falsely replaced by a control firm that has a similar bid-ask spread and is in the same Fama-French 25 portfolio. *CAR6m* are insiders' cumulative abnormal returns over six months after the insider transactions. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after terminations of analyst coverage. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses avoided by the insiders. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

Panel A. Falsely shift termination dates back for three years.

	(1)	(2)	(3)	(4)
	Insider Purchases		Insider Sales	
	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)
Treat × Post	-5.11	-7.39	2.13	-0.43
	[6.90]	[7.16]	[5.11]	[5.48]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider
# of transactions	9571	9571	35733	35733
R-squared	0.72	0.83	0.67	0.79

Panel B. Falsely replace treated firms with control firms in the same Fama-French 25 portfolio with close bid-ask spreads.

	(1)	(2)	(3)	(4)
	Insider Purchases		Insider Sales	
	CAR6m (%)	CAR6m (%)	CAR6m (%)	CAR6m (%)
Treat × Post	-13.10	-19.52	-0.49	0.39
	[14.52]	[19.71]	[5.92]	[6.93]
Controls	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm × insider	firm	firm × insider
# of transactions	51031	51031	164610	164610
R-squared	0.60	0.73	0.56	0.71

Table A10. Probability of Insider Trading

This table presents results from difference-in-differences specifications that evaluate changes in the probability of insider trading after the terminations of analyst coverage. The dependent variable *Buy* (*Sell*) is a dummy variable that equals 1 if insiders purchase (sell) in a given firm-day. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after the terminations of analyst coverage. Treated firms are sorted into quartiles based on the bid-ask spreads prior to the coverage reductions. The sorting is performed annually and independently to the number of analysts to ensure the four groups of firms have the same treatment intensity. Firms in Quartile 1 have smallest bid-ask spreads and hence are most liquid. *SpreadQ12, SpreadQ34, SpreadQ1 and SpreadQ234* are dummy variables that equal 1 if a firm's average bid-ask spreads are in the corresponding quartile groups. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. The data are a full panel containing all firm-days one year before and after the terminations of analyst coverage and span 1999 to 2008.

	(1)	(2)	(3)	(4)	(5)	(6)
	Buy*100 (mean = 3.45)			Sell*100 (mean = 6.22)		
Treat × Post	0.369			0.056		
	[0.352]			[0.560]		
Treat × Post × SpreadQ12		0.786			1.047	
		[0.660]			[1.002]	
Treat × Post × SpreadQ34		0.229			-0.112	
		[0.348]			[0.585]	
Treat × Post × SpreadQ1			1.787***			3.990***
			[0.619]			[0.505]
Treat × Post × SpreadQ234			0.176			-0.329
			[0.352]			[0.628]
LnSize	-0.421***	-0.418***	-0.420***	0.526***	0.523***	0.526***
	[0.145]	[0.143]	[0.145]	[0.152]	[0.150]	[0.145]
LnBEME	-0.339	-0.335	-0.337	-0.803***	-0.808***	-0.808***
	[0.214]	[0.213]	[0.215]	[0.201]	[0.201]	[0.203]
LnLev	0.132	0.127	0.117	-0.609***	-0.614***	-0.613***
	[0.231]	[0.232]	[0.235]	[0.158]	[0.160]	[0.159]
Ret1mPrior	-0.041***	-0.040***	-0.040***	0.082***	0.082***	0.082***
	[0.005]	[0.005]	[0.005]	[0.010]	[0.010]	[0.010]
Constant	5.476***	5.464***	5.478***	2.179**	2.193**	2.179**
	[0.838]	[0.834]	[0.839]	[0.910]	[0.905]	[0.904]
# of firm-days	318926	318926	318926	479258	479258	479258
R-squared	0.003	0.003	0.003	0.007	0.007	0.007

Table A11. Characteristics of Prosecuted Illegal Insider Transactions

This table lists the abnormal returns and profits associated with the illegal insider trading cases reported by Meulbroek (1992) and Del Guercio et al. (2013). Panel A lists the abnormal returns on the information announcement day. The abnormal returns are estimated by regressions of daily returns on the market return, and dummies equal to 1 on the information announcement day, insider trading day(s), and interim news day(s). Prior to calculating the statistics in Panel A, the estimates from regressions for bad news events are multiplied by -1. Panel B lists the SEC-reported trading profits for each trader in the illegal insider trading cases. Profits are either profits obtained or losses avoided as a result of the illegal transactions, and they are converted to constant 2011 dollars.

Panel A: Information announcement day (e.g. takeovers) abnormal returns

Sample Period	1980-1989	2003-2007 & 2010-2011
Source	Meulbroek (1992)	Del Guercio et al. (2013)
Mean	18.50%	22.75%
Median	n/a	15.19%

Panel B: SEC-reported profits per trader ($ in Year 2011)

Sample Period	1980-1989	2003-2007	2010 - 2011
Source	Meulbroek (1992)	Del Guercio et al. (2013)	Del Guercio et al. (2013)
Mean	n/a	$519,116	$1,746,102
Median	$55,722	$61,189	$177,297

Table A12. Ineffectiveness of Traditional Proxies for Information Asymmetry

This table presents results from difference-in-differences specifications that include the traditional proxies for information asymmetry as control variables. *LnSpreads* are the natural log of the percentage bid-ask spreads. *LnIVOL* is the natural log of idiosyncratic volatility. *AIM* is the Amihurd illiquidity measure. *LnCoverage* is the natural log of the number of analyst coverage. *CAR6m* are insiders' cumulative abnormal returns over six months after the insider transactions. *Treat* is a dummy variable that equals 1 if the transaction comes from treated firms. *Post* is a dummy variable that equals 1 if the transaction happens after the terminations of analyst coverage. Control variables include *LnSize*, *LnBEME*, *LnLev*, and *Ret1mPrior*, which are defined in Table 1. Standard errors are clustered at the closure/merger groupings and are reported in brackets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The treated firms are limited to firms that have five or fewer analysts covering the firm prior to coverage reductions. Negative abnormal returns for insider sales represent losses avoided by the insiders. The data cover insider transactions one year before and after the terminations of analyst coverage and span 1999 to 2008.

Panel A. Insider purchases.

	(1)	(2)	(3)	(4)	(5)	(6)
			CAR6m (%)			
Treat × Post	15.96***	14.17**	13.80***	15.88***	15.24***	13.02**
	[4.72]	[5.73]	[5.20]	[4.56]	[4.70]	[6.54]
LnSpreads		28.00***				28.10***
		[3.80]				[4.36]
LnIVOL			11.50***			4.96
			[3.34]			[3.63]
AIM				7.48**		-2.66
				[3.53]		[4.15]
LnCoverage					-3.58	-2.44
					[2.59]	[3.53]
Controls	Y	Y	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm	firm	firm	firm	firm
# of transactions	23726	23112	23726	23538	23726	22925
R-squared	0.67	0.69	0.67	0.67	0.67	0.69

Panel B. Insider sales.

	(1)	(2)	(3)	(4)	(5)	(6)
			CAR6m (%)			
Treat × Post	-10.73***	-10.33***	-10.55***	-9.98***	-10.77***	-9.77**
	[3.74]	[3.974]	[3.77]	[3.74]	[3.83]	[3.85]
LnSpreads		24.73***				19.25***
		[4.32]				[4.09]
LnIVOL			-1.91			-5.70***
			[2.30]			[2.14]
AIM				22.60***		15.75***
				[2.64]		[2.13]
LnCoverage					-2.21	-0.48
					[5.00]	[5.28]
Controls	Y	Y	Y	Y	Y	Y
Fixed effect 1	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates	calendar dates
Fixed effect 2	firm	firm	firm	firm	firm	firm
# of transactions	101908	100500	101908	101883	101908	100475
R-squared	0.64	0.66	0.64	0.65	0.64	0.66