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"Do Proxies for Informed Trading Measure Informed Trading? Evidence from Illegal Insider Trades"

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Abstract

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ABSTRACT

This paper exploits hand-collected data on illegal insider trades to provide new evidence of the ability of standard measures of illiquidity to detect informed trading. Controlling for unobserved cross-sectional and time-series variation, sampling bias, and strategic timing of insider trades, I find that when information is short-lived, absolute order imbalance and the autocorrelation of order flows are statistically and economically robust predictors of insider trading. However, when information is long-lasting, insiders strategically time their trades to avoid illiquidity and none of the measures I consider are reliable predictors of insider trading, including bid-ask spreads, Kyle's λ, and Amihud illiquidity.

Conflict of interest disclosure statement

Kenneth R. Ahern has no conflicts of interest to disclose.

Information asymmetry plays a central role in fundamental theories in finance (Kyle, 1985; Glosten and Milgrom, 1985). Identifying reliable proxies for information asymmetry from observable market data "ranks as one of the most important goals of empirical microstructure research" (Hasbrouck, 2007, p. 53). To achieve this goal, finance scholars have developed a battery of proxies for information asymmetry based on measures of illiquidity, such as bid-ask spread decompositions (Madhavan, Richardson, and Roomans, 1997), order imbalance (Easley, Kiefer, O'Hara, and Paperman, 1996), and price impact (Glosten and Harris, 1988). Though the theory is compelling and these proxies are widely used, there is little credible empirical evidence that the proxies are valid. This is because validating the proxies requires the rare opportunity to directly observe informed trading. Given the importance of information asymmetry in finance, it is crucial to empirically assess which proxies of informed trading are valid and under what circumstances.

In this paper, I exploit detailed data on illegal insider trading to provide new evidence on the validity of a host of proxies for informed trading. Though illegal insider trading does not represent all informed trading, these data help to overcome a number of empirical obstacles. First, the legal documents in insider trading cases provide direct observations of the timing of information flows and trades. Second, the trades documented in illegal insider trading cases are, by definition, based on material non-public information, not speculation or public information. Third, the data include observations of informed trading in a wide range of firms and events. Finally, the longevity of information varies in the data, allowing a comparison of short-lived versus long-lasting information.

The observations of insider trading are hand-collected from all insider trading cases filed by the Securities and Exchange Commission (SEC) and the Department of Justice (DOJ) between 2009 and 2013, as collected in Ahern (2017). The sample includes 312 different firms in 410 different insider trading events over the period 1996 to 2013. Mergers and acquisitions are the most common event in the sample (52%), followed by earnings announcements

(28%), news about drug regulation (9%), and other announcements about operations, security issuance, and financial distress. Firms range from recently public firms to the largest firms in the economy, including Microsoft, Procter & Gamble, and Berkshire Hathaway, and the median sample firm is comparable to the median firm on the NYSE.

Using intraday data from the NYSE Trades and Quotes (TAQ) database, I calculate the following widely-used measures of illiquidity: quoted, effective, and realized spreads, price impact, absolute order imbalance, Kyle's λ, and parameters from the spread decomposition of Madhavan, Richardson, and Roomans (1997) (MRR). Using daily data, I also calculate the illiquidity measure of Amihud (2002).

There are two primary obstacles to using illegal insider trading to test the predictive power of illiquidity measures: omitted variables and sampling bias. First, informed trading does not occur in a random set of firms and dates. This means that omitted variables could cause a spurious relationship between illiquidity and informed trading. For example, high-tech firms might have higher illiquidity for reasons unrelated to information asymmetry and at the same time, high-tech firms might also have greater informed trading. Similarly, omitted variables could drive both the time series pattern of illiquidity and informed trading.

To address these concerns, I use event fixed effects, event-day fixed effects, and time-varying macroeconomic and firm-level factors to absorb omitted variables. The event fixed effects control for all time-invariant properties of the firm and event, such as the type of event, the firm's industry, financial policies, and geographic location. The event-day fixed effects control for the average runup of informed trading and illiquidity before the public announcement of the event. The time-varying factors control for systematic risk and a firm's daily returns and trading volume.

A related concern caused by non-random trading is strategic timing by informed traders. The theoretical model of Collin-Dufresne and Fos (2016) predicts that insiders with long-lived information will strategically trade on days with low levels of illiquidity, generating a negative relationship between insider trading and illiquidity. To address this concern, I exploit the variation of inside traders' lead time before their private information becomes public. When lead times are shorter, inside traders have less freedom to strategically time their trades, which makes the timing of their trades closer to random.

The second obstacle of using illegal insider trading data is potential sampling bias. If regulators use illiquidity to detect illegal insider trading, then my tests would show that illiquidity is related to the likelihood of detection rather than the likelihood of insider trading itself. To address this concern, I first show that regulators use many methods to detect insider trading, of which abnormal market data is just one. Second, I use empirical proxies to measure the likelihood that the SEC used market data to detect insider trading in a particular case. The proxies are based on the network structure of inside traders and the involvement in a case by regulatory agencies that do not monitor the stock market.

My first results show that in tests that do not control for strategic timing, none of the measures of illiquidity are statistically correlated with insider trading except Kyle's λ. However, the effect of Kyle's λ is negative, consistent with the prediction of Collin-Dufresne and Fos (2016), but contrary to conventional wisdom.

When I control for strategic timing, the results are strikingly different. I find that the greater is the urgency of trading, the stronger is the positive correlation between measures of illiquidity and insider trading. Given that urgency is high, a one standard deviation increase in the quoted bid-ask spread is associated with an increase in the likelihood of insider trading by 26% of the average likelihood. Other measures, including the effective spread and Kyle's λ, have similar economic magnitudes. Using a stricter criteria for urgency reduces the magnitude of these effects by about half, but they remain highly statistically significant. These results imply that when information is short-lived, standard measures of illiquidity are reliable predictors of insider trading.

However, only two measures of illiquidity are robust to controls for sampling bias: absolute order imbalance and the autocorrelation of order flow. Kyle's λ and Amihud Illiquidity are slightly less robust. The remaining measures of illiquidity either lose their statistical significance or are directly correlated with the SEC's detection mechanism. In particular, all of the quote-based measures of spreads and price impact are related to the likelihood of detection through abnormal market data.

Robustness tests provide a number of additional results. First, in regressions that simultaneously include multiple illiquidity variables, absolute order imbalance and the autocorrelation of order flows are still the most statistically significant and economically meaningful predictors of informed trading. Second, the predictive power of Kyle's λ is sensitive to modeling assumptions. Third, I find that illiquidity is driven by the trades of sophisticated buy side managers and analysts. Fourth, intraday price range and volatility and dollar-based spreads do not predict insider trading in any tests. Finally, the results of the paper do not change when conditioning on firm size, year, type of event, the source of the information, and whether the event was positive or negative.

This paper's results have a number of important implications. First, though standard illiquidity measures have the power to predict informed trading, they are only effective when information is short-lived and traders cannot strategically time their trades to avoid illiquid markets. Second, the illiquidity measures that are the most reliable predictors are based on order flows, not prices or quotes. This suggests that market makers do not adjust prices and quotes on a daily basis in response to informed trading, contrary to the assumptions of prior theoretical models. Finally, for practical purposes, the relative efficacy and simplicity of calculation makes Amihud Illiquidity an attractive option to measure information asymmetry, assuming information is short-lived.

The central contribution of this paper is to provide credible evidence on the validity of standard measures of illiquidity using direct observations of informed trades. The direct observations of illegal insider trading over a wide range of events provides an advantage over prior studies that rely on indirect proxies for informed trading in specialized settings, such as institutional holdings and R&D expenses (Van Ness, Van Ness, and Warr, 2001), analyst coverage (Kelly and Ljungqvist, 2012), family-firms (Anderson, Reeb, and Zhao, 2012), geographic proximity (Coval and Moskowitz, 2001), and retail short-sellers (Kelley and Tetlock, 2017). Cornell and Sirri (1992) and Chakravarty and McConnell (1999) also use direct observations of informed trading, but only for single trading events. Petchey, Wee, and Yang (2016) study a single measure of informed trading from illegal trading before merger announcements, but do not control for strategic timing, event-day fixed effects, or sampling bias.

This paper is most closely related to Collin-Dufresne and Fos (2015) and Kacperczyk and Pagnotta (2017). Collin-Dufresne and Fos show that activist hedge funds who privately increase their ownership in target firms before they publicly disclose their ownership positions strategically time their trades to avoid illiquidity. Thus, they find a negative correlation between measures of illiquidity and informed trading. However, when activists face time constraints, the correlation is less negative, consistent with my findings. Compared to Collin-Dufresne and Fos, my setting of illegal insider trading provides evidence from a wider set of firms, events, traders, and information longevity. A second difference is that activist hedge funds control when their private information is revealed to the public, whereas the typical inside trader in my sample has no control over the release of the information.

Kacperczyk and Pagnotta (2017) study similar data on illegal insider trading as this paper, but use a different research design. In particular, Kacperczyk and Pagnotta (2017) do not control for event-day fixed effects and provide fewer tests of sampling bias and strategic timing of trades. Thus, they find small negative correlations between insider trading and measures of stock illiquidity as in Collin-Dufresne and Fos (2015). Instead, Kacperczyk and Pagnotta (2017) focus on the predictive power of options, as do Augustin, Brenner, and Subrahmanyam (2015). I do not study options in this paper to avoid endogenity problems. Ahern (2017) suggests that sophisticated inside traders strategically avoid options because options make it easier for regulators to convict inside traders. In particular, in some of its cases, the SEC notes that buying short-maturity out-of-the-money options is strong evidence of insider trading.

I. Research Design

In an ideal research setting, informed trading is directly observable by the researcher and occurs randomly across firms and time. In this setting, if illiquidity is higher on firm-days with informed trading than firm-days without informed trading, we could conclude that informed trading causes illiquidity to increase. In reality, informed trading is not randomly assigned to firms or days and is not directly observable. Below, I discuss how I address these two limitations.

A. Non-Random Selection of Firms and Dates

The firms in which informed traders invest are not randomly assigned in the real world. First, informed trading requires private valuable information. The existence of valuable information varies across firms in non-random ways. For instance, information that a firm is a takeover target is valuable, but firms are not randomly assigned to be takeover targets. Second, the spread of information is not random. For instance, some firms might rely more heavily on outside contractors who are more likely to spread private information.

Because informed trading is not randomly assigned to firms, an omitted variable could cause both illiquidity and informed trading. For example, high-tech firms might have a high likelihood of informed trading because they are more likely to be takeover targets. At the same time, high-tech firms might also be more illiquid for reasons unrelated to informed trading, such as a lack of institutional investors. Thus, omitted variables could generate a spurious relationship between illiquidity and informed trading.

To control for omitted variables, I use event fixed effects, where an event is a specific piece of information, such as a takeover or earnings announcement. The event window includes the period from 120 days to two days before the public announcement of the information. Any characteristic that does not change over the 119 days in the event window is absorbed in the fixed effects. Such characteristics include the type of information and time-invariant characteristics of the firm, such as industry, firm policies, location, etc.

Second, in the real world, the timing of informed trading is not random. For example, private information about an upcoming earnings announcement might spread faster as the announcement date approaches, leading to an increase in informed trading. At the same time, illiquidity might increase as investors respond to earnings announcements of peer firms. Therefore, both illiquidity and informed trading could increase as the announcement date approaches, creating a spurious relationship.

To control for non-random timing of informed trading, I use event-day fixed effects and time-varying macro and firm-level variables. The event-day fixed effects are dummy variables for each event day from -120 to -2. These event-day fixed effects normalize each event's time series pattern by the average time series pattern across all events. I also include the event firms' daily trading volumes and absolute stock returns as well as the absolute values of the Fama French factors.[1] These variables control for time-varying factors that could be correlated with both informed trading and illiquidity. Altogether, event fixed effects, event-day fixed effects, and time-varying factors absorb a wide range of potential omitted variables.

The final concern with non-random assignment is the strategic timing of informed trading. Collin-Dufresne and Fos (2016) show that if private information is long-lived, informed investors will choose to trade when liquidity is high to limit the price impact of their trades. This will cause a negative relationship between illiquidity and informed trading.

To control for the strategic timing of insider trading, I create two variables to measure the urgency of trading. When urgency is high, informed investors have less freedom to strategically time their trades and the timing of trades is closer to random. The first measure, *Daily Urgency*, is a daily measure of the time remaining before the private information is

[1] All of the results in this paper are robust to using turnover instead of ln(1+volume). Using turnover decreases the R^2s in the regressions and increases the significance of the main variables.

publicly announced. In particular,

$$\textit{Daily Urgency}_\tau = -\frac{1}{\tau} \qquad \text{for event dates } \tau = -120, \ldots, -2.$$

Daily Urgency increases as the public announcement date gets closer to the current date. Because *Daily Urgency* is identical across all events, it is perfectly correlated with the event-day fixed effects, though an interaction with daily illiquidity is not. Unlike fixed effects, the impact of *Daily Urgency* can be interpreted with an economic meaning.

Using insider trading as a proxy of informed trading, I estimate the following econometric model,

$$\begin{aligned} \textit{Insider trading}_{\iota\tau} = \; & \alpha + \beta \cdot \textit{Illiquidity}_{\iota\tau} + \phi \cdot \textit{Daily Urgency}_\tau + \psi \cdot \textit{Illiquidity}_{\iota\tau} \times \textit{Daily Urgency}_\tau \\ & + \gamma_1 \cdot |ret_{\iota\tau}| + \gamma_2 \cdot \ln(1 + volume_{\iota\tau}) \\ & + \gamma_3 \cdot |MKT_\tau| + \gamma_4 \cdot |SMB_\tau| + \gamma_5 \cdot |HML_\tau| \\ & + \kappa_\iota + \varepsilon_{\iota\tau}, \qquad \forall \iota \in \mathcal{I} \text{ and } \tau = -120, \ldots, -2, \end{aligned} \tag{1}$$

where κ_ι are event fixed effects; $|ret_{\iota\tau}|$ is the absolute value of event-firm ι's stock return on event-date τ; $\ln(1 + volume_{\iota\tau})$ is the logged trading volume of event-firm ι on day τ; and MKT, SMB, and HML are Fama-French daily risk factors. A positive estimate of ψ indicates that illiquidity is positively related to informed trading in situations where trading is more likely to be randomly timed.

A drawback to *Daily Urgency* is that it does not account for the lead time from the day of the original information leak to the day of the public announcement. For example, *Daily Urgency* at $\tau = -4$ is the same whether the original leak happened at $\tau = -5$ or $\tau = -20$. However, when the original leak occurs with a long lead time, strategic traders may have already timed their trades to reduce their price impact.

To overcome this limitation, I also use a stricter measure of urgency, *Event Urgency*, that is the inverse of the lead time between the original leak and the public announcement. If we let $\overline{\tau}_\iota$ be the event-date of the original leak in event ι, then

$$\textit{Event Urgency}_\iota = -\frac{1}{\overline{\tau}_\iota}.$$

Because *Event Urgency* is constant across all event-dates for a given event, it is perfectly correlated with the event fixed effects. However, the interaction with daily illiquidity is not. Therefore, I estimate the following econometric model,

$$\begin{aligned}\textit{Insider trading}_{\iota\tau} = \; & \alpha + \beta \cdot \textit{Illiquidity}_{\iota\tau} + \kappa_\iota + \delta_\tau \\ & + \phi \cdot \textit{Illiquidity}_{\iota\tau} \times \textit{Event Urgency}_\iota \\ & + \gamma_1 |ret_{\iota\tau}| + \gamma_2 \ln(1 + volume_{\iota\tau}) \\ & + \gamma_3 |MKT_\tau| + \gamma_4 |SMB_\tau| + \gamma_5 |HML_\tau| \\ & + \varepsilon_{\iota\tau}, \qquad \forall \iota \in \mathcal{I} \text{ and } \tau = -120, \ldots, -2,\end{aligned} \tag{2}$$

where δ_τ are event-day fixed effects. As above, the coefficient ϕ in Equation 2 measures the difference-in-difference marginal effect of illiquidity on insider trading when the real world setting is closer to the ideal randomized setting.

B. Non-random Selection of Observations

The second major deviation from the ideal setting is that I cannot directly observe informed trading. Instead, I must rely on illegal insider trading cases filed by regulators. While these data provide exceptional detail on individual trading behavior, sampling bias is a concern. If regulators use illiquidity to identify illegal insider trading, I could misinterpret my results to mean illiquidity predicts informed trading when the true relation is that illiquidity causes regulators to detect insider trading.

To address this concern, it is important to understand how illegal insider trading is detected by regulators. If regulators use methods that are less related to illiquidity, then sampling bias is less of a concern. For brevity, I provide a short summary of the regulatory process below. For a longer description, see the Internet Appendix of Ahern (2017).

Illegal insider trading is detected by a number of different entities, including the SEC, the DOJ, the Financial Industry Regulatory Authority (FINRA), and the Federal Bureau of Investigation (FBI). Each entity relies on various detection methods including computerized monitoring of trading behavior, tips submitted by the public, and traditional investigation methods. FINRA has the primary responsibility for monitoring abnormal market behaviors. It uses a computer program called SONAR that monitors news feeds, SEC filings, and market data to identify suspicious trades.[2] If SONAR flags an event as suspicious, human investigators at FINRA collect additional information from trading records. If the investigations produce sufficient evidence, FINRA refers the case to the SEC.

In addition to referrals from FINRA, the SEC initiates its own investigations and commonly receives information from other sources, including the DOJ, the FBI, and tips submitted directly from individuals. If the SEC's investigation identifies evidence of insider trading in a particular event by a particular individual, it expands its investigation to other trades by the same individual in different events and to other traders in the same event. The SEC identifies other potential traders through phone and email records, in-person interrogations, and in some cases, wire taps. Therefore, the detection of a single episode of insider trading can lead regulators to identify other insider trading events and other inside traders. Even if FINRA's algorithm detects one event based on illiquidity, the SEC's subsequent investigations might detect other events that FINRA missed. Therefore, many cases in the data are likely to be detected by methods other than illiquidity.

[2]SONAR's algorithm is not publicly available so I do not know if it uses illiquidity as a determinant of illegal trading. My tests control for trading volume and absolute stock returns, which are likely included in SONAR's algorithm.

To provide empirical analysis of potential sampling bias, I use four proxies to control for the likelihood that insider trading is detected by market data. The first two proxies rely on the argument that illegal inside traders in larger networks are more likely to have been detected through traditional investigations than through abnormal market data. To calculate the size of insider trading networks, I first count the number of inside traders involved in an event. I then calculate the total number of people in the traders' complete network, including people who traded in other events. I use dummy variables for above-median network size and number of traders.

The other two proxies rely on the argument that different regulators use different detection methods. If FINRA is involved in an investigation, the trades are more likely to have been detected by abnormal market data. In contrast, if the FBI is involved in the investigation, the trades are more likely to have been detected by traditional investigation techniques rather than abnormal market data. I record dummy variables for events that are investigated by FINRA and the FBI, using data from SEC press releases that acknowledge the assistance of other organizations in their investigations.

Using the detection proxies, I estimate the following triple interaction regression:

$$\begin{aligned} \textit{Insider trading}_{\iota\tau} = \;& \alpha + \beta \cdot \textit{Illiquidity}_{\iota\tau} + \kappa_\iota + \delta_\tau \\ & + \phi \cdot \textit{Illiquidity}_{\iota\tau} \times \textit{Event Urgency}_\iota \\ & + \pi \cdot \textit{Illiquidity}_{\iota\tau} \times \textit{Detection}_\iota \\ & + \omega \cdot \textit{Illiquidity}_{\iota\tau} \times \textit{Event Urgency}_\iota \times \textit{Detection}_\iota \\ & + \gamma_1 |ret_{\iota\tau}| + \gamma_2 \ln(1 + volume_{\iota\tau}) \\ & + \gamma_3 |MKT_\tau| + \gamma_4 |SMB_\tau| + \gamma_5 |HML_\tau| \\ & + \varepsilon_{\iota\tau}, \qquad \forall \iota \in \mathcal{I} \text{ and } \tau = -120, \ldots, -2. \end{aligned} \tag{3}$$

The coefficient ω measures whether the central results of Equation 2 are significantly different when the insider trading case is more likely detected by abnormal market data or traditional investigations. If ω is zero, it implies that the main results are not driven by sampling bias.

Finally, an additional concern is that though the presence of an illegal insider trading case indicates the presence of informed trading, the absence of a case does not imply the absence of informed trading. This means that I do not observe a sample of alternative events that have no informed trading to use as a counterfactual. Randomly choosing events as a counterfactual sample would not help because the events could involve illegal insider trading that was undetected by regulators. Thus, I could not separate the hypothesis that illiquidity is unrelated to informed trading from the hypothesis that insider trading is equally common in the control sample as in the main sample.

The lack of a counterfactual sample is alleviated by the event fixed effects. The event fixed effects isolate the difference between days with insider trading and days without insider trading within the event-firm's time series. Once regulators identify insider trading in an event, they are likely to search for additional insider trading on all of the days preceding the event. Therefore, it is reasonable to assume that within an event's time series, the days which the regulators do not identify illegal insider trading actually have a substantially smaller likelihood of informed trading.

II. Data

A. Measures of Informed Trading

Data on illegal insider trading comes from the data used in Ahern (2017). These data are collected from legal filings by the Securities and Exchange Commission and the Department of Justice in connection with civil and criminal complaints of illegal insider trading. The data are from cases filed between 2009 and 2013, but include insider trading dates from 1996 to 2013. The data set includes the nature of the inside information, the date on which the original source leaked the inside information, the dates on which the information was shared with others, with whom the information was shared, and the days on which insiders traded. For a full description of the data, see Ahern (2017).

The advantage of using illegal insider trading data is that they provide direct observations of informed trading. Moreover, though the data come from filings that are allegations brought by the SEC and DOJ, they are most likely to be accurate. Most cases are not challenged by the defendants. In the cases that are challenged, the defense is usually based on a technicality of the legal definition of insider trading, not a dispute about the information or trading.

A second advantage is that the data include the dates and the number of shares that the insiders traded. In a minority of cases, the filings only provide a range of trading dates, not specific trading dates. In these cases, I assign an equal fraction of the total number of shares to each of the days in the range. This limitation will weaken the ability of measures of informed trading to correctly identify days with increased informed trading.[3]

I use two measures of informed trading. The first measure, *Trade dummy*, is a dummy variable that equals one if any insider trading occurred on a particular date. The second measure is *ln(1+Number of shares traded)*, which measures the total number of shares traded by all inside traders on a particular date. Though this variable is measured in levels, the fixed effects in the regressions convert them into deviations from the event's average.

B. Measures of Liquidity

In the main set of tests, I investigate the performance of seven popular measures of illiquidity based on intraday data. In robustness tests, I consider different calculations of these seven measures and alternative illiquidity measures.

[3] I attempted to get the specific trading dates from the SEC for the cases that didn't provide specific dates, but the SEC denied my Freedom of Information Act requests.

To calculate intraday measures of illiquidity, I use the NYSE Monthly Trades and Quotes (TAQ) database. Holden and Jacobsen (2014) show that compared to the more accurate Daily TAQ database, the monthly database produces biased estimates of illiquidity measures. Therefore, I follow Holden and Jacobsen's three recommendations designed to reduce errors in the monthly TAQ database: 1) set withdrawn quotes to missing, rather than omitting the quotes altogether, 2) estimate time stamps within the second using interpolated times based on the ordering of trades and quotes in the database, and 3) omit quotes and trades that occur when quotes are crossed or locked (where the national best bid is equal to or greater than the national best offer). In particular, I follow the programming code provided in the Internet Appendix of Holden and Jacobsen (2014) to generate National Best Bid and Offer (NBBO) quotes and matched trades. To identify a trade as a buy or sell, I follow the Lee and Ready (1991) algorithm commonly used in the literature.

B.1. Bid-Ask Spread

The percent quoted bid-ask spread is $\frac{A_t - B_t}{M_t}$, where A_t is the National Best Ask quoted at time t, B_t is the National Best Bid quoted at time t, and M_t is the midpoint of A_t and B_t. Following Holden and Jacobsen (2014), I aggregate the spread to the daily level by taking the weighted average of the intraday quoted spreads, where the weights on each observed intraday spread is the amount of time the spread is in force. In robustness checks, I also use the dollar quoted spread, which is the time-weighted dollar spread, $A_t - B_t$.

B.2. Effective Spread

The quoted bid-ask spread implicitly assumes that trades occur at the ask or bid price. The effective spread accounts for trades that occur within the quoted spread. The effective spread subtracts the midpoint of the NBBO quotes from the trading price to estimate the difference between the proxy for the true price (the midpoint) and what the trader actually pays. This difference is multiplied by two to give a full spread.

In particular, the percent effective spread is calculated per trade, k, as $\frac{2D_k(P_k - M_k)}{M_k}$. D_k is a trade indicator that equals $+1$ if the trade is a buy and -1 if it is a sell. P_k is the trade price of trade k, and M_k is the midpoint of the NBBO quotes prevailing when trade k occurs. Following Holden and Jacobsen (2014), I aggregate the effective spread to the daily level by taking the dollar-volume weighted average of the effective spread across all trades per day. In robustness checks, I also use the dollar effective spread, which is not normalized by M_k.

B.3. Price Impact

The price impact is an estimate of the permanent change in a stock price following a trade. It is calculated as the change in the current quoted midpoint to the quoted midpoint five minutes in the future. In particular, the percent price impact per trade k is $\frac{2D_k(M_{k+5} - M_k)}{M_k}$. M_{k+5} is the midpoint of the NBBO quotes prevailing five minutes after trade k. Again following Holden and Jacobsen (2014), I aggregate the price impact to the daily level by taking the dollar-volume weighted price impact across all trades per day. As above, I also calculate a dollar price impact that does not normalize by M_k.

B.4. Realized Spread

The realized spread is the effective spread minus the price impact. The market maker receives the effective spread, which accounts for trades within the bid and offer prices. However, if the trade moves the stock price, the market maker's gain is reduced as the trading price moves closer to the true price. In other words, the realized spread is the effective spread net of price impact.

The percent realized spread is calculated per trade k as $\frac{2D_k(P_k - M_{k+5})}{M_k}$. Again following Holden and Jacobsen (2014), I aggregate the realized spread to the daily level by taking the dollar-volume weighted realized spread across all trades per day. As above, I also calculate a dollar realized spread that does not normalize by M_k.

B.5. Absolute Order Imbalance

Following Holden and Jacobsen (2014), I calculate absolute order imbalance as $\left|\frac{Buys-Sells}{Buys+Sells}\right|$, where $Buys$ and $Sells$ are the number of buys and sells per day, and buys and sells are identified using the algorithm of Lee and Ready (1991). Absolute order imbalance has been used as a proxy for the probability of informed trading (PIN) measure of Easley, Kiefer, O'Hara, and Paperman (1996). Aktas, de Bodt, Declerck, and Oppens (2007) shows that if the probability of information arrival is constant, PIN is equivalent to the absolute order imbalance.

B.6. Kyle's Lambda

I estimate Kyle's (1985) lambda as the coefficient λ in the following regression,

$$\Delta p_k = \lambda \cdot S_k + u_k,$$

where Δp_k is the change in the transaction price, $S_k = D_k\sqrt{DollarVolume_k}$ is the signed dollar volume of the trade, and u_k is an unobserved error term. Observations include all price changes per day and I require at least 10 price changes to estimate λ. Alternative estimators of Kyle's lambda are discussed below.

B.7. MRR Bid-Ask Spread Decomposition

Madhavan, Richardson, and Roomans (1997) (MRR) presents a structural model to decompose the bid-ask spread into transaction costs and adverse selection costs of informed trading. Following MRR, I estimate five parameters using five moment conditions. The parameter of interest is θ, which measures the degree of information asymmetry between the market maker and investors. A higher value of θ is predicted to lead to a larger change in beliefs about the true stock price for a given change in order flow.

C. Summary Statistics

Table I presents summary statistics of the sample by event. The table also provides average characteristics separately for events with low urgency and high urgency, where high urgency is defined as event urgency greater than the median event urgency of nine days. There are 410 events in the sample over the period 1996 to 2013. The average time from the original leak of the information to the public announcement of the event is 22.9 days. In low urgency events, the average is 43 days, and in high urgency events, the average is four days.

I assign an event to be positive if the inside traders took long positions before the public announcement. In the entire sample, 75% of events are positive events. Positive events are significantly more common when urgency is low (82%) compared to when urgency is high (67%).

Panel B provides the fraction of events by the nature of the news. The most common type of event is a merger or acquisition, at 52% of the sample. Earnings news is the next most common type of event, with 28% of the sample. The remaining types of events are much less common: drug regulation news (9%), sale of securities (7.5%), and operations (2.4%). Financial distress, fund liquidation, and events with various news each account for less than one percent of the total. Mergers are significantly more likely to be low urgency events, while earnings news are significantly more likely to be high urgency events.

Panel C shows that the median target firm is large, with market equity equal to about $1 billion, though the sample includes a wide range of firm sizes. Firms with urgent events tend to be larger firms, with an average market equity of $16.6 billion, compared to an average market equity of $3.5 billion for low urgency firms. The other characteristics of firms reported in the table, Tobin's Q, R&D, and intangible assets, are not significantly different in high urgency events than in low urgency events.

Panel D provides the industry breakdown of the firm-events in the sample. The most common industries are chemical manufacturing and computers and electronics manufacturing.

Ahern (2017) shows that the sample is biased towards high-tech industries relative to the industry distribution of all public listings and also targets in acquisitions. However, neither chemicals or computers and electronics manufacturing firms exhibits a prevalence for high or low urgency events.

Finally, Panel E provides background characteristics of the inside traders in the sample. The average trader age is 46 years, and the average trader wealth (proxied by home values) is \$1.2 million. Traders in high urgency events tend to be slightly older and wealthier. In addition, traders in urgent events tend to have more insider trading connections (network degree) and also tend to be members of larger networks (network size). Unreported results show that across all days with insider trading, hedge fund managers are more likely to trade in urgent events, whereas low-level employees are more likely to trade in non-urgent events. These patterns are consistent with the findings in Ahern (2017) that showed that hedge fund managers tend to be wealthier, older, have larger insider trading networks, and receive information later compared to the average inside trader.

Table II presents summary statistics using roughly 48,000 observations of daily data from $\tau = -120$ to $\tau = -2$ over the 410 events in the sample. These statistics can be used to interpret the coefficients in the regressions presented below. All illiquidity measures and stock returns are winsorized at the 1st and 99th percentiles. Insider trading occurred in 4.6% of all days in the sample. The quoted spread is 0.31%, on average and 0.13% at the median. The effective spread is 0.31% on average, and 0.12% at the median. The realized spread is 0.16% and the median is 0.05%. The average price impact is 0.15% and the median is 0.06%. The spreads are slightly higher than those reported in the random sample of Holden and Jacobsen (2014), though the price impact is less. Absolute order imbalance is 11.1%, slightly less than reported in Holden and Jacobsen (2014). Kyle's λ is 8.5 on average, with a median of 6.3. At the median, this implies that a \$10,000 order will lead to a price change of 0.0063. At the median stock price of \$19.39 in the sample, this is equivalent to 3.25 basis points. MRR θ is 0.223% at the average, and 0.080% at the median.

Panel D reports the number of traders per day that hold a particular occupational title. Buy side analysts and managers are the most common type of trader, followed by small business owners, low-level employees (such as IT technicians and secretaries) and specialized occupations (such as medical doctors and engineers).

Finally, Panel E reports summary statistics of the event firms' daily stock returns, volume, and market-wide factors. The median absolute value of daily returns is 1.5%. The average log volume of shares traded is -0.623, which equates to 536,332 shares. The median is slightly less at 543,893 shares.

III. Empirical Results

This section of the paper first presents time-series patterns of returns, volume, measures of illiquidity, and insider trading, then it presents estimates of the empirical models described above.

A. Time-Series Evidence of Illiquidity and Insider Trading

Figure 1 presents the time series of insider trading activity in event time from $\tau = -40$ to $\tau = -1$. The solid line represents the fraction of positive events in the sample in which insider trading occurs, per event day. The dashed line represents the same concept for negative events. The figure shows that insider trading occurs in 5% of events as far back as 40 days before the announcement. As the public announcement date draws closer, the fraction of events with insider trading increases steadily to around 30% in the few days before the announcement. For negative events, insider trading is less common overall, but increases substantially in the last 15 days before the announcement. Thus, insider trading activity ramps up close to the public announcement date, but positive events have wide variation in the timing of trades.

Figure 2 presents cumulative abnormal returns of event firms in the period before the public announcement. Abnormal returns are calculated as the difference between a firm's daily return and the firm's average return in the period $\tau = -120$ to $\tau = -41$. This figure shows that insider trading is moving prices to their full-information value, as positive events have large runups of roughly 8% over the period $\tau = -40$ to $\tau = -1$, while negative events have declines of about 2.5%. The asymmetry reflects that negative events in the sample tend to be less extreme earnings events, compared to the large positive gains in merger events. Short-sell constraints might also contribute to the asymmetry. For this study, the most relevant fact is that stock prices are moving considerably before the public announcement. Likewise, Figure 3 shows that abnormal volume is increasing substantially during the period before the public announcement. Therefore, there is clear evidence that insiders are trading, stock prices are moving, and volume is increasing in advance of the public announcement of an event.

Figure 4 presents the time series of abnormal illiquidity for the seven illiquidity variables. Abnormal illiquidity is calculated as the daily illiquidity measure minus the firm's average illiquidity measure during the period from $\tau = -120$ to $\overline{\tau}_i - 1$, the day before the information was originally leaked. Grey areas indicate the 95th percentile confidence interval.

The patterns in these figures are in stark contrast to the steep runup in insider trading, volume, and prices. Instead of increasing, the measures of abnormal illiquidity are nearly flat, or slightly decreasing. For example, quoted spread and effective spread are significantly less than average for a number of days in the period $\tau = -10$ to $\tau = -1$ and never significantly positive. Likewise, order imbalance and Kyle's λ become significantly more negative as the event gets closer. These figures suggest that a firm's illiquidity decreases slightly at the same time that insider trading in the firm increases greatly.

B. Benchmark Regressions

Table III presents benchmark regression estimates of informed trading on return and volume measures without including measures of illiquidity. Observations include trading days from $\tau = -120$ to $\tau = -2$. All regressions in the paper cluster standard errors at the event level.

Column 1 presents a regression of absolute returns, trading volume, and absolute values of market-wide asset pricing factors on a dummy variable for insider trading. This specification does not include event fixed effects or event-day fixed effects. The results show that when the absolute return is higher and trading volume is lower, insider trading is more likely. None of the market-wide factors are significantly related to informed trading in this test or any of the other tests in the table. This shows that insider trading is based on idiosyncratic information, not systematic risk.

Next, column 2 includes event fixed effects to control for time-invariant factors over the 120 days before the announcement. After controlling for event fixed effects, absolute returns become insignificant and volume becomes positively significant. This suggests that returns and volume are correlated with the cross-sectional event fixed effects.

Column 3 includes event-day fixed effects to control for any explanatory variables that are common across events on a particular event day. The relationship between volume and insider trading decreases but remains significantly positive. Returns and market factors remain unrelated to informed trading. These same sets of results are found in columns 4–6 where the dependent variable is the size of insider trading volume.

The results in Table III show that controlling for cross-sectional and time-series variation, trading volume is positively related to the intensity of informed trading, whereas absolute returns and market factors are not. The positive relationship between trading volume and insider trading is consistent with the idea that insider trading increases trading volume above its historical firm-average and cross-sectional event-day average.

C. Naïve Tests

Table IV presents the relationship between insider trading and illiquidity without controlling for the strategic timing of trades. In all of the these and the remaining tests, I include all of the explanatory variables in the regressions in Table III, though for brevity, I do not report their coefficient estimates. The dependent variable in panel A of Table IV is an indicator for insider trading. In panel B, it is the volume of insider trading.

In Panel A, almost none of the measures of illiquidity have a statistical relationship with insider trading, whether fixed effects are included or not, consistent with the flat time-series of illiquidity shown in Figure 4. In Panel B, when no fixed effects are included in the regressions, the three spreads, price impact, and MRR θ are significantly related to insider trading. However, once event fixed effects are included, these relationships become insignificant. This suggests that time-invariant cross-sectional heterogeneity across event firms explains some of the variation in illiquidity and informed trading.

Consistent with Collin-Dufresne and Fos (2015), Kyle's λ is negatively related to informed trading after controlling for event fixed effects. However, once I control for event-day fixed effects, none of the measures of illiquidity are significantly related to insider trading except a weak relationship of the realized spread. The fact that Kyle's λ becomes insignificant after including event-day fixed effects implies that a common daily factor explains variation in abnormal liquidity and insider trading across events.

In summary, the results presented in this section show that measures of illiquidity are unrelated to informed trading after accounting for unobserved cross-sectional variation across events and unobserved time-series variation across event-dates. This is consistent with inside traders strategically timing their trades to avoid price impact.

D. Tests that Control for Strategic Timing

Table V presents estimates of Equation 1 in which the measures of illiquidity are interacted with *Daily Urgency*. As stated above, *Daily Urgency* is perfectly correlated with event-day fixed effects, so I only include event fixed effects. Panel A presents results for the incidence of insider trading; Panel B presents results for the volume of insider trading.

In all specifications, the main effect of *Daily Urgency* is positive and statistically significant. This reflects that as the event day draws closer, insider trading is more prevalent. The interaction between *Illiquidity* and *Daily Urgency* is positive and significant for all measures of illiquidity except MRR θ. At the same time, the main effects of the illiquidity measures tend to be negative and significant.

These findings imply that when urgency is low, illiquidity is negatively related to insider trading, but when urgency is high, illiquidity is positively related to insider trading. Thus, when the timing of trading is closer to random, as in the ideal research setting, illiquidity signals an increase in informed trading, as they are intended to do. However, when insiders can time their trades, illiquidity signals a decrease in informed trading.

The magnitudes of the coefficients are economically meaningful. A one-standard deviation increase in the quoted spread, using a base-level of daily urgency of zero is associated with a negative change in the likelihood of insider trading of -0.006. When urgency is one standard deviation higher, a standard deviation increase in the quoted spread is associated with a positive change in the likelihood of insider trading of $+0.006$. Therefore, the difference-in-difference estimate of the marginal effect of an increase of the quoted spread when urgency is low compared to when urgency is high is 0.012. Compared to the average of 0.046, this estimate represents an increase of 26% from the average. The magnitude of the difference-in-difference of the effective spread, the realized spread, and Kyle's λ are also 0.012. Price impact has a smaller effect at 0.009 and order imbalance is 0.007.

A similar pattern is found in Panel B, where the difference-in-difference estimate of the marginal effect is 0.127 for the quoted spread, 0.126 for the effective spread, 0.121 for the realized spread, 0.089 for price impact, 0.074 for order imbalance, and 0.072 for Kyle's λ, compared to the average of 0.316, or roughly 22% to 38% of the average of the informed trading volume. These results indicate that the shorter is the remaining time before the public announcement of an event, the stronger is the positive relationship between measures of illiquidity and the presence of insider trading.

Table VI provides estimates of Equation 2 using *Event Urgency* to provide a stricter test of the relationship between illiquidity and informed trading. Without controlling for event-day fixed effects, only order imbalance, Kyle's λ, and MRR θ are positive and significant across Panels A and B. When, controlling for both event and event-day fixed effects, quoted spread, effective spread, order imbalance, Kyle's λ, and MRR θ are all positive and significantly related to informed trading in Panels A and B. These results provide strong evidence that when measures of illiquidity are higher, the likelihood of insider trading is higher, as long as insiders cannot strategically time their trades to mitigate price impact.

The economic magnitude of these results are between 40% and 60% smaller than the magnitudes of the prior tests. For example, for a one-standard deviation increase in the quoted spread, the difference-in-difference of the likelihood of insider trading is 0.007, or about 15% of the average probability of insider trading. Kyle's λ has a similar magnitude, but order imbalance is smaller. In the tests of trading volume, quoted spread and Kyle's λ have the largest economic magnitude of about 15% of the average, whereas order imbalance and MRR θ have smaller economic magnitudes of about 7% of the average.

E. Tests that Control for Sampling Bias

Table VII presents the estimates of Equation 3 that control for sampling bias based on regulators' detection methods. Panel A includes the network size dummy, panel B includes the number of traders dummy, panel C includes the FBI dummy, and panel D includes the FINRA dummy.

In panels A and B, order imbalance and Kyle's λ are still statistically significant when trading is urgent, as before. In addition, the interaction terms are not statistically significant, indicating that the results are not different for events with large networks and many traders versus small networks and few traders. In panel C, nearly all of the illiquidity measures are positively correlated with urgent insider trading, but none of the interactions with the FBI dummy are statistically significant. This means that the results do not differ whether the FBI participated in the investigation or not.

Finally, panel D shows that only order imbalance is significant, as in the main results, and its interaction terms with the FINRA dummy is not significant. This means that order imbalance has the same predictive power regardless of FINRA's participation in the investigation. In contrast, all of the quote-based measures (spreads and price impact) are significantly related to FINRA's involvement and Kyle's λ and MRR θ lose their significance when events are not investigated by FINRA. These result suggest that FINRA's detection algorithm is correlated with these measures, but not with order imbalance.

Taken together, these results show that order imbalance is robust in all of the tests and Kyle's λ is robust in most of the tests that control for urgency and sampling bias. These results also suggest that the SONAR algorithm uses inputs related to spreads to detect insider trading.

F. Strategic Timing by Occupation

To provide more detail on which insiders trade in urgent situations and which strategically time their trades, I study the relationship between insider trading and Kyle's λ by inside traders' occupations. Ahern (2017) shows that inside information tends to flow from relatively unsophisticated corporate insiders who makes small trades, including executives, mid-level managers, and low-level employees, through a sequence of social connections to sophisticated buy side managers and analysts who make large trades.

Figure 5 presents the magnitudes of difference-in-difference tests for nine classifications of occupations of insider traders. The vertical axis of each subfigure measures ln(1+insider trading volume). The horizontal axis represents a one-standard deviation increase in Kyle's λ from Low to High.[4] The solid lines represent the economic magnitude of the relationship between Kyle's λ and insider trading volume for urgent events. The dashed lines represent the same relationship when events are not urgent. A positive slope indicates that Kyle's λ increases when insider trading increases. The relationships are separated into occupations by including interactions with occupational indicators per day in unreported regressions.

Focusing on urgent events, where traders have less freedom to strategically time their trades, we see a strong positive relationship between Kyle's λ and informed trading of buy side managers and analysts. This shows that buy side traders drive the positive relationship in the full sample between illiquidity and informed trading. In contrast, the flat slope for corporate executives and managers suggests that their trades do not influence Kyle's λ, even when trading is urgent.

Focusing on non-urgent events reveals which traders are most likely to strategically time their trades. This is indicated by a negative slope in the dashed line. Buy side analysts and corporate managers are the most strategic traders in non-urgent events. In contrast, low level employees and small business owners trade equally whether liquidity is high or low.

These results show that the relationship between illiquidity and insider trading depends on both urgency of trading and the sophistication of the inside traders.

IV. Alternative Measures of Illiquidity

In this section, I discuss alternative parameters of MRR, Amihud illiquidity, alternative calculations of Kyle's λ, and dollar-based spreads.

[4]The relationships are similar using other measures of illiquidity.

A. Structural Parameters of MRR's Bid-Ask Spread Decomposition

The bid-ask spread decomposition of Madhavan, Richardson, and Roomans (1997) provides five parameter estimates in the following three equations,

$$p_t - p_{t-1} = (\phi + \theta)x_t - (\phi + \rho\theta)x_{t-1} + \alpha_t + \xi_t - \xi_{t-1} \tag{4}$$

$$\mid x_t \mid = 1 - \lambda + \nu \tag{5}$$

$$x_t x_{t-1} = x_t^2 \rho + \iota. \tag{6}$$

From MRR's model, θ is an asymmetric information parameter, ϕ measures transaction costs, ρ is the autocorrelation of order flow, λ is the probability a trade occurs between the quotes, α is a constant drift assumed to be zero in expectation, and ξ, ν, and ι are error terms.

In the main tests, I use θ as an indicator of informed trading, as formulated in the theoretical framework of MRR. In Table VIII, I include each of the estimated parameters of MRR in the same regression specifications in Panel A of Tables V and VI as a placebo test. The results of the adverse selection parameter, θ, is repeated here for comparison. It is positive and significant when event urgency is high. In contrast, the three parameters, α, λ, and ϕ are insignificant in each of the various tests, as expected.[5]

However, ρ is also significant in nearly all specifications. In particular, when an event is not urgent, ρ is positively related to insider trading, but it is negatively related to insider trading as an event becomes more urgent. Moreover, the marginal effect of a one-standard deviation change in ρ is double the effect of θ in tests of informed trading volume. In addition, in unreported tests, MRR ρ is significant in all of the sampling bias tests, whereas θ is not.

[5]Internet Appendix Table I presents results using the continuous dependent variable.

These results follow from the framework in MRR. In particular, Equation 15 on page 1057 of MRR describes the revision in ask prices as follows,

$$p_t^a - p_{t-1}^a = \theta(1-\rho)x_{t-1} + \epsilon_t + \xi_t^a - \xi_{t-1}^a,$$

where ϵ_t is a mean zero random variable that captures changes in beliefs due to new public information and ξ_t^a is a rounding error from price discreteness. This equation states that θ has a positive impact on prices and ρ has a negative impact on prices for a given order x_{t-1}. The intuition behind ρ's negative impact is that if $\rho > 0$, then a buy order is most likely followed by another buy order. Thus, another a new buy order conveys less information and prices do not increase as much as if $\rho < 0$.

While MRR define θ, not ρ, to reflect informed trading, in the relationship between trading and price impact, θ and $-\rho$ have the same marginal effect. The advantage of using ρ is that it is directly estimated from the data, whereas θ is extracted as a structural parameter from other estimated parameters. Therefore, smaller measurement error might lead to ρ having a stronger marginal effect than θ.

B. High Level Measures of Illiquidity

I also consider three additional measures of illiquidity based on primary summary statistics. Following Kacperczyk and Pagnotta (2017), I calculated *Price Range* as the maximum national best offer minus the minimum best bid per day. *Realized Variance* is the realized variance of all five-minute window returns per day. Finally, I calculate a daily measure of Amihud Illiquidity, following Amihud (2002). This is the daily absolute value of the stock return divided by the dollar volume of trading in the stock.

Table IX presents the results using these measures of illiquidity with a dummy dependent variable.[6] All three measures are positively related to informed trading controlling for daily

[6]Internet Appendix Table II presents the results using the continuous dependent variable.

urgency. However, controlling for event urgency, price range and realized volatility are no longer significantly related. Amihud Illiquidity is significantly related once event-day fixed effects are included. The marginal effect of Amihud Illiquidity is about a third less than Kyle's λ and the quoted spread. In unreported tests, Amihud Illiquidity is significant in three of the four sampling bias tests. These are important results because Amihud Illiquidity does not require intraday trading day observations for estimation, but yet it still provides a relatively strong indicator of informed trading when trading is urgent.

C. Modeling Assumptions of Kyle's λ

In the main tests, I calculate Kyle's λ using a regression model with price changes as the dependent variable, with no intercept, and using transactions as observations. In subsequent tests, I denote this measures as $\lambda_{1,k}$. Two alternative specification are,

$$r_k = \alpha + \lambda_{2,k} \cdot S_k + u_k, \tag{7}$$

$$r_k = \lambda_{3,k} \cdot S_k + u_k, \tag{8}$$

where r_k is the stock return at trade k, S_k is the signed dollar volume of the trade, and u_k is an unobserved error term. Both of these specifications use the stock return instead of price changes as the dependent variable. The second alternative includes an intercept term.

Next, following the model of Glosten and Harris (1988) and as calculated in Brennan and Subrahmanyam (1995), Kyle's λ can be calculated in the following regression,

$$\Delta p_k = \lambda_{4,k} q_k + \psi[D_k - D_{k-1}] + u_k, \tag{9}$$

where Δp_k is the price change, q_k is the signed order flow of the number of shares traded, and D_k is a trade direction indicator. This estimate does not account for the dollar volume of a trade, and it includes a separate term ψ to measure the fixed costs associated with a trade.

The four calculations of Kyle's λ can also be calculated using 5-minute windows as observations, rather than using transactions as observations. In the calculations that use five-minute windows, q_k and D_k are aggregated to the five-minute window level and Δp_k is the change in price from the start to the end of the window. I denote these as $\lambda_{n,5}$ for $n = 1, 2, 3, 4$.

Internet Appendix Table III presents results using these eight different versions of Kyle's λ. The tests are identical to the tests in Tables V and VI of the main paper. Column 1 of Table III replicates the results using $\lambda_{1,k}$, the same version used in the main tests. Controlling for daily urgency, $\lambda_{1,k}$, $\lambda_{2,k}$, and all of the five-minute λs are positively related to informed trading, though the magnitude and statistical significance of the five-minute λs are weak. Controlling for the urgency of the event to reduce strategic timing bias, only $\lambda_{1,k}$ and $\lambda_{4,k}$ remain statistically significant. In particular, when there is no urgency in the event, these λs are negatively related to informed trading, and positively related when urgency is higher. The economic magnitude of the difference-in-difference of $\lambda_{1,k}$ is about 15% of the average likelihood of insider trading and the average insider trading volume. The magnitude of the other statistically significant version, $\lambda_{4,k}$ is about 5% of the average likelihood and volume.

These results show that the relationship between Kyle's λ and insider trading depends critically on the way that λ is calculated. In particular, the relationship between λ and insider trading is stronger using price changes, rather than returns, and using transaction-level observations, rather than five-minute windows.

D. Dollar Spreads

Internet Appendix Table IV presents tests using dollar-based measures of the quoted spread, effective spread, realized spread, and price impact. With the exception of the dollar quoted spread, none of these measures is significantly related to informed trading. The marginal difference-in-difference impact of the dollar quoted spread is slightly less than the percent quoted spread used in the main tests. These results show that on average, percent spreads provide a stronger signal of informed trading than dollar spreads.

V. Horse Race Between Measures of Illiquidity

Including Amihud Illiquidity and MRR ρ, the above results show that nine measures of illiquidity have varying statistical power to identify informed trading. In this section, I directly compare the measures' statistical significance and economic magnitudes to determine which measure has the most explanatory power.

First, since all of the measures have some degree of power to predict insider trading, they are likely correlated with each other. Internet Appendix Table V presents pair-wise correlations between the nine measures. The majority of the correlations are large and statistically significant. In particular, quoted spread, effective spread, realized spread, and price impact are highly correlated with each other, with correlations above 60%. Order imbalance is most closely correlated with quoted spread. Kyle's λ is also highly correlated with quoted spread, effective spread, and MRR θ, and negatively correlated with MRR ρ. MRR θ and ρ exhibit the least average correlation across the other measures, except they are negatively correlated with each other. Finally, Amihud Illiquidity has relatively low correlations with the spread measures and almost no correlation with the MRR measures.

In Table X, I run the same set of regressions as before using event urgency, but now I include the measures simultaneously in the same specification. Because the spread measures and price impact are so highly correlated, I run four different specifications where I only include one of the four measures at a time. The coefficients in Table X reflect the marginal effect of each variable on insider trading, while partialing out variation that is correlated with the other measures of illiquidity.

To assess the independent strength of each measure of illiquidity, I compare the statistical significance and economic significance. Statistically, order imbalance and MRR ρ have the strongest predictive power for insider trading. However, statistical significance is affected by measurement error in the independent variables. A variable that is precisely measured, could have strong statistical significance even if its economic significance is low.

To compare economic magnitudes, I compare the difference-in-difference marginal effect of a one-standard deviation increase in each measure of illiquidity. MRR ρ has the largest economic significance, followed by the quoted spread, effective spread, order imbalance, price impact, Kyle's λ, Amihud Illiquidity, MRR θ, and realized spread. Therefore, MRR ρ has the strongest independent predictive power for identifying insider trading, both economically and statistically. Of the statistically significant measures, order imbalance has the next strongest power, followed by price impact.

These results do not necessarily imply that other measures are ineffective. In regressions that include the quoted spread or Kyle's λ separately, these variables are also statistically significant and have economic magnitudes similar to MRR ρ. However, these results show that MRR ρ has a stronger independent signal of informed trading than the other variables.

VI. Robustness Tests

In unreported tests, I also consider a number of different mitigating factors in the relationship between illiquidity and insider trading. Conditioning on firm size, event year, and type of event have no significant impact on the results. Second, the relationship between insider trading and illiquidity is independent of whether traders received tips from family members, business associates, or friends. Finally, I find no effect when I include a control for whether the event is positive or negative.

VII. Conclusion

Foundational theories in finance predict that informed trading impacts stock prices and transaction costs. In turn, researchers have designed many empirical proxies based on observable price and quote data to capture the presence of informed trading. However, testing the efficacy of these proxies has been inhibited by the inability to observe the behavior of informed traders.

This paper helps to overcome some of the limitations of prior work by using direct observations of insider trading as a laboratory to test whether standard measures of illiquidity detect insider trading. A key contribution of this paper is the research design that reduces confounding effects from cross-sectional and time-series variation unrelated to illiquidity. This allows me to isolate the connection between private information and illiquidity, while controlling for omitted factors.

The results show that controlling for sampling bias and strategic timing, only absolute order imbalance and the autocorrelation of order flow are positively and significantly related to informed trading. These results provide some of the cleanest evidence to date to support the theoretical predictions of models of adverse selection in financial markets. However, these results only hold when information is short-lived. When information is long-lived, none of the illiquidity measures correctly detect informed trading.

This paper shows that though proxies for informed trading are ubiquitous in finance research, they are unlikely to be valid in many situations. Future theoretical research on informed trading should consider informed investors' ability to strategically time their trades to avoid price impact, as in Collin-Dufresne and Fos (2016). In addition, future research should reconsider the assumption that market makers' correctly infer informed trading from order flows. Addressing these concerns will shed light on information asymmetry in financial markets and may aid practitioners who bear the cost of adverse selection and regulators whose task is to detect insider trading.

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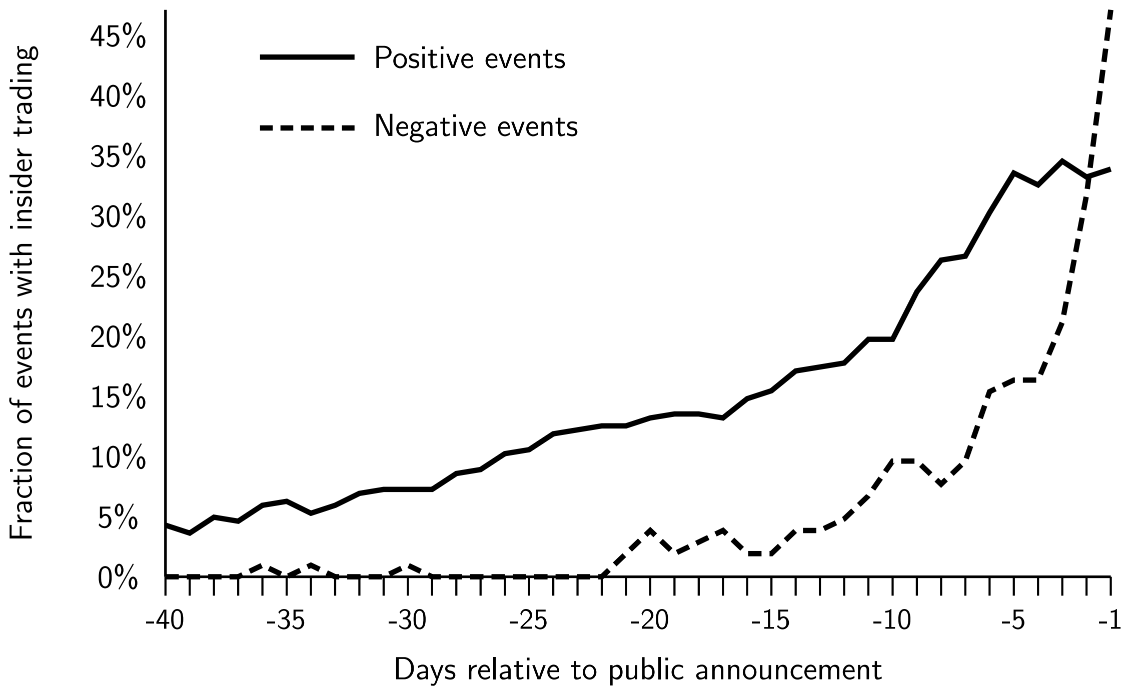


Figure 1. **The Prevalence of Insider Trading Before an Event**
This figure presents the fraction of events per day in which insider trading occurred. There are 410 events in the sample over the period 1996 to 2013. The horizontal axis includes dates in event time from $t = -40$ to $t = -1$, where the public announcement is on day $t = 0$. Positive events are events in which inside traders took long positions. Negative events are events in which inside traders took short positions.

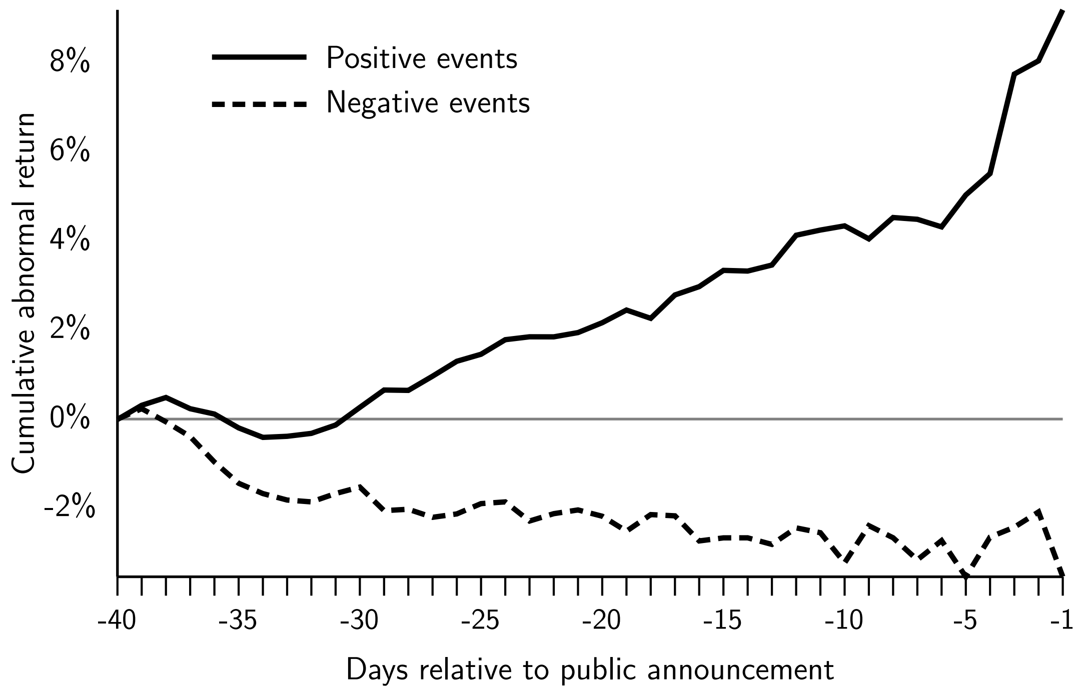


Figure 2. **Cumulative Abnormal Returns Before an Event**
This figure presents the cumulative abnormal returns of 410 firm-events over the period 1996 to 2013. Abnormal returns are a firm's daily return minus the firm's average return over the period $t = -120$ to $t = -41$. Cumulative abnormal returns are the cumulative sum of abnormal returns. The horizontal axis includes dates in event time from $t = -40$ to $t = -1$, where the public announcement is on day $t = 0$. Positive events are events in which inside traders took long positions. Negative events are events in which inside traders took short positions.

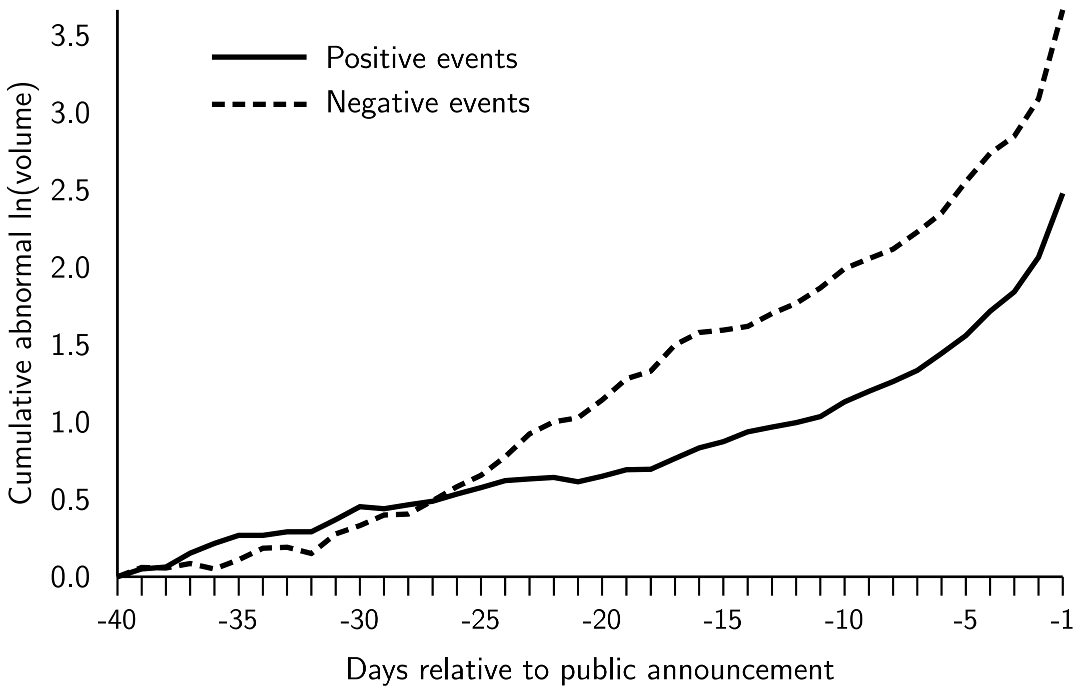
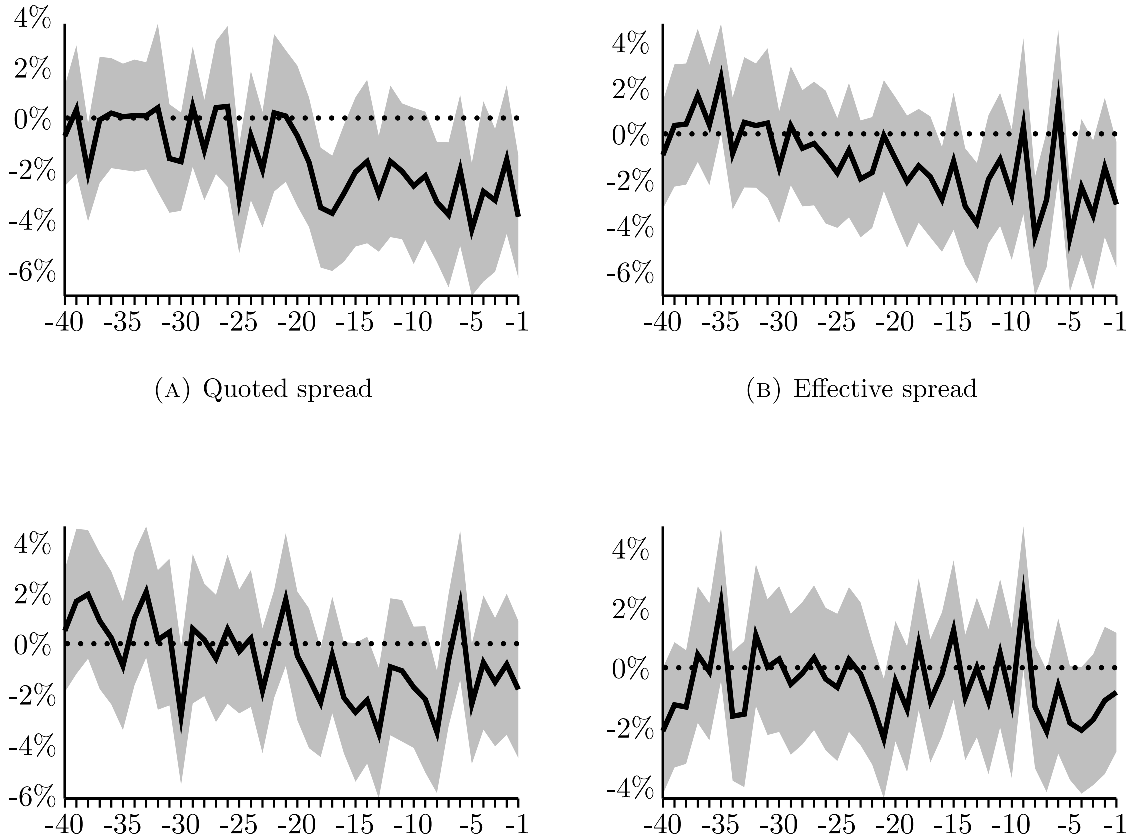


Figure 3. **Cumulative Abnormal Trading Volume Before an Event**
This figure presents the cumulative abnormal trading volume of 410 firm-events over the period 1996 to 2013. Abnormal volume is a firm's daily ln(1+volume) minus the firm's average ln(1+volume) over the period $t = -120$ to $t = -41$. Cumulative volume is the cumulative sum of abnormal volume. The horizontal axis includes dates in event time from $t = -40$ to $t = -1$, where the public announcement is on day $t = 0$. Positive events are events in which inside traders took long positions. Negative events are events in which inside traders took short positions.



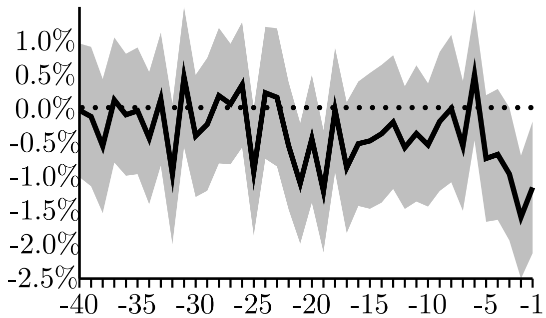
(A) Quoted spread

(B) Effective spread

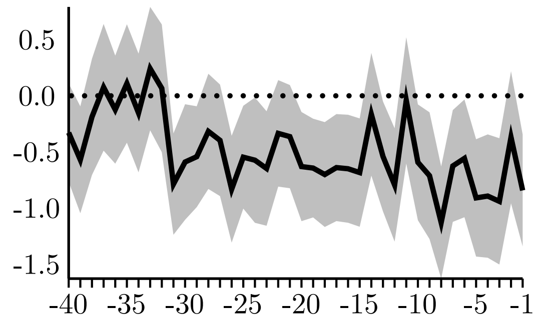
(C) Realized spread

(D) Price impact

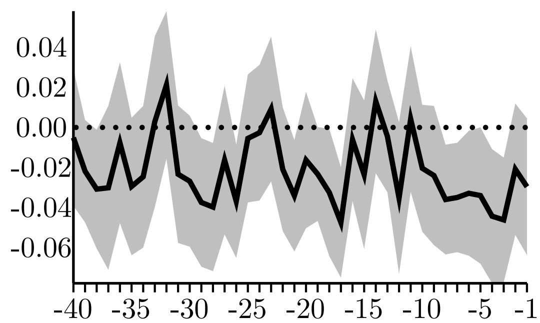
Figure 4. **Event-time Series of Abnormal Illiquidity.**
These figures present the time-series from $t = -40$ to $t = -1$, relative to the public announcement of the event at $t = 0$. Abnormal illiquidity is calculated as the daily illiquidity minus the average illiquidity of the firm over the period from $t = -120$ to the day before the original insider leak of the information. The gray bands indicate the 95th percent confidence interval.



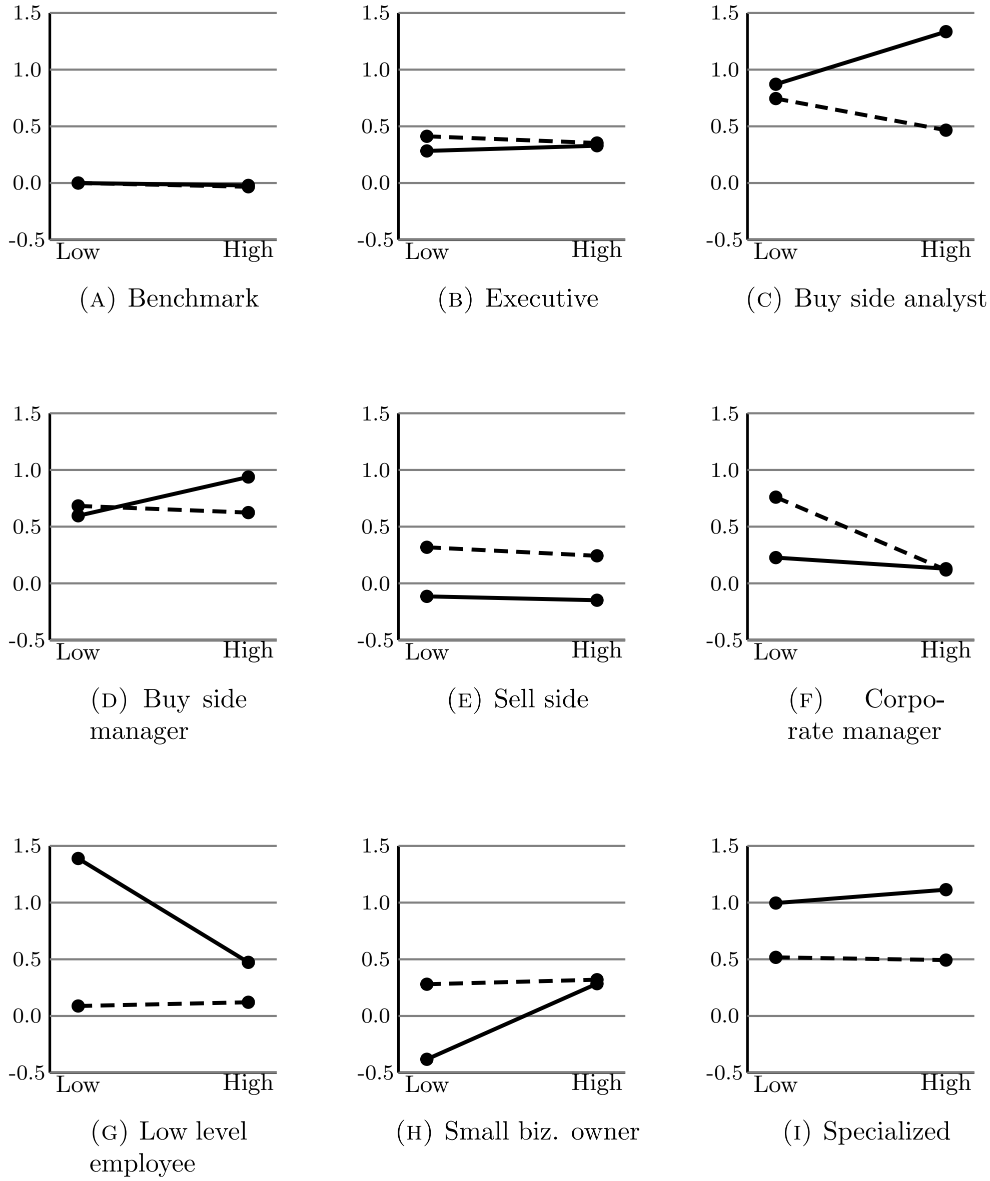
(E) Order imbalance



(F) Kyle's λ



(G) MRR θ



(A) Benchmark (B) Executive (C) Buy side analyst

(D) Buy side manager (E) Sell side (F) Corporate manager

(G) Low level employee (H) Small biz. owner (I) Specialized

Figure 5. **Kyle's λ, Occupation, and Event Urgency**
These figures present the marginal effects on ln(1+volume of insider trading) of a one standard deviation increase in Kyle's λ (Low to High on the horizontal axis), when an event is urgent (solid line) compared to when an event is not urgent (dashed line), by inside trader occupation. Urgent events have lead times from the original leak to the public announcement of less than the median (9 days). The results are based on a regression model with a triple interaction between Event urgency, Kyle's λ, and insider trader occupation.

Table I.

Summary Statistics by Event

Observations are 410 events over the period 1996–2013. *High Urgency* events have lead times from the original leak to the public announcement of the event greater than the median of nine days. *Average trader network degree* is the average number of connections an inside trader has to other insiders across all insiders trading in an event. *Average trader network size* is the average of the traders' network sizes who trade in an event. *Average tip source* is the average over a dummy variable indicating the source of an insider tip, family, business, or friends, with one tipper possibly having multiple roles. The last column presents the p-value of a t-test of the equality of the means of the low and high urgency subsamples. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

	All Events		Urgency			
	Mean	Median	Low	High	Difference	p-value
Panel A: Event timing						
Information leak to event lag (days)	22.88	9.00	42.59	3.73	38.86***	< 0.001
Positive event (%)	74.51	100.00	82.18	66.99	15.19***	< 0.001
Panel B: Event type (% of total)						
Mergers & Acquisitions	51.71	100.00	64.36	39.42	24.93***	< 0.001
Earnings	28.05	0.00	19.31	36.54	−17.23***	< 0.001
Drug Regulation	9.02	0.00	7.43	10.58	−3.15	0.266
Sale of Securities	7.56	0.00	6.44	8.65	−2.22	0.396
Operations	2.44	0.00	0.99	3.85	−2.86*	0.059
Financial Distress	0.49	0.00	0.99	0.00	0.99	0.158
Fund Liquidation	0.24	0.00	0.00	0.48	−0.48	0.318
Various	0.49	0.00	0.50	0.48	0.01	0.984
Panel C: Firm Characteristics						
Market equity ($billions)	10.09	1.01	3.45	16.58	−13.13***	< 0.001
Tobin's Q	2.54	1.87	2.46	2.61	−0.14	0.560
R&D/Assets (%)	10.54	1.24	12.32	8.80	3.53	0.147
Intangibles/Assets (%)	17.88	9.90	19.56	16.31	3.25	0.102
Panel D: Industry (% of total)						

	All Events		Urgency			
	Mean	Median	Low	High	Difference	p-value
NAICS 21: Mining, Oil, and Gas	3.17	0.00	1.49	4.81	−3.32*	0.053
NAICS 22: Utilities	0.49	0.00	0.99	0.00	0.99	0.158
NAICS 23: Construction	0.49	0.00	0.50	0.48	0.01	0.984
NAICS 31: Manufacturing (Food, Apparel, Leather)	9.76	0.00	5.45	13.94	−8.50***	0.004
NAICS 32: Manufacturing (Chemical)	21.95	0.00	21.29	22.60	−1.31	0.750
NAICS 33: Manufacturing (Computers and Electronics)	22.20	0.00	23.76	20.67	3.09	0.453
NAICS 42: Wholesale	4.39	0.00	4.95	3.85	1.10	0.587
NAICS 44: Retail (Electronics, Food, Clothing)	2.93	0.00	1.49	4.33	−2.84*	0.086
NAICS 45: Retail (Sports, Books, and General)	1.46	0.00	0.50	2.40	−1.91	0.105
NAICS 48: Transportation	0.98	0.00	1.49	0.48	1.00	0.306
NAICS 51: Publishing	9.51	0.00	11.88	7.21	4.67	0.109
NAICS 52: Finance and Insurance	6.59	0.00	5.94	7.21	−1.27	0.605
NAICS 53: Real Estate	1.95	0.00	3.47	0.48	2.98**	0.031
NAICS 54: Professional, Scientific Services	4.88	0.00	6.93	2.88	4.05*	0.059
NAICS 56: Administrative and Support Services	1.95	0.00	1.49	2.40	−0.92	0.501
NAICS 61: Educational Services	0.24	0.00	0.50	0.00	0.50	0.319
NAICS 62: Health Care	1.95	0.00	2.97	0.96	2.01	0.145
NAICS 72: Accomodation and Food Service	0.98	0.00	0.99	0.96	0.03	0.977
NAICS 99: Unknown	0.24	0.00	0.00	0.48	−0.48	0.318
Panel E: Trader Characteristics						
Average trader network degree	1.48	1.00	1.25	1.78	−0.53**	0.038
Average trader network size	8.37	2.00	7.05	10.03	−2.98*	0.099
Average tip source: family (%)	32.05	0.00	34.58	28.92	5.66	0.421
Average tip source: business (%)	54.67	15.00	55.26	53.94	1.33	0.894
Average tip source: friends (%)	58.96	60.00	56.43	62.08	−5.65	0.518
Average trader age	46.13	45.25	45.27	47.20	−1.94*	0.087
Average trader wealth ($100,000s)	12.33	5.98	10.53	14.96	−4.42**	0.047

Table II.
Summary Statistics by Trading Day

Daily observations in $t = -120, \ldots, -2$, relative to the public announcement date ($t = 0$), across 410 firm-events. *Trade dummy* equals one if there is any insider trading on a given day. *ln(1+Number of shares traded)* is based on inside trades. *Daily Urgency* is the inverse of the number of days to $t = 0$. *Event Urgency* is the inverse of the number of days from the original information leak to $t = 0$. The statistics for the occupations listed in Panel D reflect the number of inside traders on a given day with the occupation. MKT, HML, and SMB are daily Fama-French factor returns.

	Mean	Std. Dev.	25th	Median	75th	Observations
Panel A: Informed trading measures						
Trade dummy	0.046	0.208	0.000	0.000	0.000	48,021
ln(1+Number of shares traded)	0.316	1.591	0.000	0.000	0.000	47,748
Panel B: Trading window measures						
Daily Urgency	0.037	0.064	0.011	0.017	0.032	48,021
Event Urgency	0.182	0.255	0.031	0.091	0.200	42,904
Panel C: Illiquidity measures						
Quoted spread (%)	0.314	0.521	0.072	0.132	0.300	44,660
Effective spread (%)	0.307	0.526	0.067	0.121	0.285	46,701
Realized spread (%)	0.156	0.369	0.012	0.048	0.140	46,701
Price impact (%)	0.148	0.287	0.022	0.064	0.155	46,701
Order imbalance	0.111	0.115	0.032	0.074	0.146	42,819
Kyle's λ	8.500	7.850	3.680	6.280	10.280	44,042
MRR θ	0.223	0.514	0.012	0.080	0.235	46,429
Panel D: Trader Occupations						
Executive	0.006	0.076	0.000	0.000	0.000	48,021
Buy side analyst	0.015	0.157	0.000	0.000	0.000	48,021
Buy side manager	0.013	0.138	0.000	0.000	0.000	48,021
Sell side	0.004	0.068	0.000	0.000	0.000	48,021
Corporate manager	0.006	0.077	0.000	0.000	0.000	48,021
Low level employee	0.011	0.120	0.000	0.000	0.000	48,021
Small business owner	0.018	0.158	0.000	0.000	0.000	48,021
Specialized occupation	0.011	0.123	0.000	0.000	0.000	48,021
Panel E: Stock Returns						
|Return|	2.311	2.605	0.644	1.490	2.978	48,035
Ln(1+Volume)	−0.623	2.009	−1.861	−0.609	0.686	48,035
|MKT|	0.957	1.094	0.270	0.614	1.244	48,035
|SMB|	0.440	0.414	0.160	0.320	0.600	48,035
|HML|	0.429	0.510	0.110	0.270	0.530	48,035

Table III.

Benchmark Regressions

This table presents OLS regression coefficients where the dependent variable in columns 1–3 is a dummy equal to one if any insider trading occurred on a given day, and in columns 4-6 is the logged volume of shares traded by insiders on a given day. Observations are from a panel of 410 firm-events with trading days $t = -120$ to $t = -2$, relative to the announcement date of $t = 0$. *Return* is the firm's daily stock return, MKT, SMB, and HML are the Fama-French daily factor returns. *Event fixed effects* are fixed effects for each of the 410 events. *Event-day fixed effects* are fixed effects for each of the trading days, $t = -120, \ldots, -2$. Adjusted R^2 is the within-R^2. p-values from standard errors clustered at the event level are presented in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Dependent variable:	Informed trading dummy			Ln(1+volume of informed trading)		
	(1)	(2)	(3)	(4)	(5)	(6)
|Return|	0.002***	< 0.001	< 0.001	0.022***	< 0.000	0.006
	(0.003)	(0.501)	(0.611)	(< 0.001)	(0.948)	(0.264)
ln(1+volume)	−0.010***	0.015***	0.008**	−0.059***	0.117***	0.060**
	(< 0.001)	(< 0.001)	(0.011)	(< 0.001)	(< 0.001)	(0.013)
|MKT|	0.001	< 0.001	0.001	0.014	−0.006	< 0.001
	(0.601)	(0.900)	(0.766)	(0.411)	(0.704)	(0.982)
|SMB|	−0.002	−0.002	−0.001	−0.024	−0.035	−0.029
	(0.667)	(0.652)	(0.788)	(0.372)	(0.176)	(0.224)
|HML|	0.001	0.001	0.001	0.002	−0.017	−0.014
	(0.754)	(0.774)	(0.802)	(0.952)	(0.626)	(0.637)
Constant	0.033***	0.056***	0.005	0.224***	0.415***	0.055
	(< 0.001)	(< 0.001)	(0.377)	(< 0.001)	(< 0.001)	(0.173)
Event fixed effects	No	Yes	Yes	No	Yes	Yes
Event-day fixed effects	No	No	Yes	No	No	Yes
Observations	48,035	48,035	48,035	47,762	47,762	47,762
Adjusted R^2	0.010	0.003	0.148	0.007	0.003	0.144

Table IV.

The Naïve Relationship Between Illiquidity and Insider Trading

This table presents OLS regression coefficients where the dependent variable in Panel A is a dummy equal to one if any insider trading occurred on a given day, and in Panel B is the logged volume of shares traded by insiders on a given day. Observations are from a panel of 410 events with trading days $t = -120$ to $t = -2$, relative to the announcement date of $t = 0$. Each regression includes the independent variables in Table III. These coefficients are not reported. Each regression specification includes a measure of illiquidity, listed at the top of the column. For each panel and each column, I estimate three different regressions, differing by the fixed effects included in the regression (event fixed effects, event-day fixed effects, or both). p-values from standard errors clustered at the event level are presented in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Illiquidity measure:	Quoted spread	Effective spread	Realized spread	Price impact	Order imbalance	Kyle's λ	MRR θ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Dependent variable: Informed trading dummy							
No fixed effects							
Illiquidity	0.019	0.015	0.020	0.010	0.012	0.001	−0.013***
	(0.156)	(0.182)	(0.146)	(0.300)	(0.677)	(0.306)	(0.004)
Event fixed effects							
Illiquidity	−0.003	0.004	0.009	−0.004	−0.014	−0.001**	−0.007
	(0.826)	(0.690)	(0.331)	(0.540)	(0.235)	(0.041)	(0.146)
Event and event-day fixed effects							
Illiquidity	0.007	0.012	0.013*	0.001	−0.001	0.000	−0.001
	(0.562)	(0.192)	(0.090)	(0.892)	(0.943)	(0.407)	(0.774)

Panel B. Dependent variable: Ln(1+informed trading volume)

No fixed effects							
Illiquidity	0.193**	0.180**	0.202**	0.169**	0.101	0.003	−0.094***
	(0.030)	(0.028)	(0.032)	(0.028)	(0.575)	(0.378)	(0.001)
Event fixed effects							
Illiquidity	−0.014	0.055	0.059	0.007	−0.142	−0.007**	−0.048
	(0.913)	(0.544)	(0.424)	(0.880)	(0.142)	(0.019)	(0.143)
Event and event-day fixed effects							
Illiquidity	0.064	0.112	0.091	0.043	−0.035	−0.003	−0.003
	(0.532)	(0.136)	(0.139)	(0.321)	(0.687)	(0.265)	(0.932)

Table V.

Daily Urgency and the Relationship Between Illiquidity and Insider Trading

This table presents OLS regression coefficients where the dependent variable is a dummy equal to one if any insider trading occurred on a given day (Panel A) or the logged volume of shares traded by insiders on a given day (panel B). Observations are from a panel of 410 events with trading days $t = -120$ to $t = -2$, relative to the announcement date of $t = 0$. *Daily Urgency* is the inverse of the number of days from a given day to $t = 0$. Each regression includes the independent variables in Table III and event fixed effects. These coefficients are not reported. Each regression specification includes a measure of illiquidity, listed at the top of the column. p-values from standard errors clustered at the event level are presented in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Illiquidity measure:	Quoted spread	Effective spread	Realized spread	Price impact	Order imbalance	Kyle's λ	MRR θ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Dependent variable: Informed trading dummy							
Illiquidity	−0.011	−0.003	−0.006	−0.017*	−0.038*	−0.001***	−0.002
	(0.349)	(0.697)	(0.446)	(0.068)	(0.072)	(0.005)	(0.771)
Daily Urgency	0.887***	0.899***	0.929***	0.938***	0.935***	0.830***	1.014***
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)
Illiquidity×Daily Urgency	0.373**	0.353**	0.496**	0.458**	1.015*	0.024***	−0.053
	(0.024)	(0.022)	(0.019)	(0.036)	(0.060)	(0.006)	(0.598)
Panel B. Dependent variable: Ln(1+informed trading volume)							
Illiquidity	−0.108	−0.040	−0.101	−0.139*	−0.398**	−0.010***	0.015
	(0.270)	(0.564)	(0.138)	(0.052)	(0.016)	(0.006)	(0.666)
Daily Urgency	6.776***	6.859***	7.203***	7.281***	7.205***	6.920***	8.172***
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)
Illiquidity×Daily Urgency	3.824***	3.737***	5.147***	4.843**	9.955**	0.144**	−0.905
	(0.005)	(0.004)	(0.003)	(0.012)	(0.022)	(0.021)	(0.209)

Table VI.

Event Urgency and the Relationship Between Illiquidity and Insider Trading

This table presents OLS regression coefficients where the dependent variable in Panel A is a dummy equal to one if any insider trading occurred on a given day, and in Panel B is the logged volume of shares traded by insiders on a given day. Observations are from a panel of 410 events with trading days $t = -120$ to $t = -2$, relative to the announcement date of $t = 0$. *Event Urgency* is the inverse of the number of days between the day of the original leak of the information to $t = 0$. Each regression includes the independent variables in Table III. These coefficients are not reported. Each regression specification includes a measure of illiquidity, listed at the top of the column. p-values from standard errors clustered at the event level are presented in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Illiquidity measure:	Quoted spread	Effective spread	Realized spread	Price impact	Order imbalance	Kyle's λ	MRR θ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Dependent variable: Informed trading dummy							
Event fixed effects							
Illiquidity	−0.007	0.003	0.009	−0.006	−0.021	−0.001**	−0.013
	(0.708)	(0.822)	(0.441)	(0.507)	(0.186)	(0.020)	(0.116)
Illiquidity×Event urgency	0.027	0.008	−0.001	0.011	0.042	0.002***	0.020*
	(0.310)	(0.711)	(0.970)	(0.479)	(0.126)	(0.004)	(0.074)
Event and event-day fixed effects							
Illiquidity	−0.003	0.006	0.011	−0.005	−0.015	−0.001*	−0.008
	(0.846)	(0.622)	(0.269)	(0.502)	(0.305)	(0.058)	(0.291)
Illiquidity×Event urgency	0.053**	0.039**	0.014	0.037**	0.095***	0.003***	0.024**
	(0.039)	(0.045)	(0.492)	(0.024)	(0.002)	(0.003)	(0.046)
Panel B. Dependent variable: Ln(1+informed trading volume)							
Event fixed effects							
Illiquidity	−0.042	0.053	0.057	0.008	−0.208	−0.010***	−0.083
	(0.799)	(0.662)	(0.564)	(0.908)	(0.107)	(0.008)	(0.107)
Illiquidity×Event urgency	0.195	0.030	0.035	−0.004	0.388*	0.019***	0.132*
	(0.391)	(0.862)	(0.818)	(0.972)	(0.074)	(0.002)	(0.063)

Illiquidity measure:	Quoted spread	Effective spread	Realized spread	Price impact	Order imbalance	Kyle's λ	MRR θ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Event and event-day fixed effects							
Illiquidity	−0.008	0.073	0.073	0.011	−0.155	−0.007**	−0.045
	(0.949)	(0.457)	(0.368)	(0.843)	(0.183)	(0.023)	(0.319)
Illiquidity×Event urgency	0.394*	0.262*	0.140	0.201	0.795***	0.024***	0.168**
	(0.067)	(0.095)	(0.372)	(0.111)	(< 0.001)	(0.002)	(0.043)

Table VII.

The Relationship Between Illiquidity and Insider Trading Controlling for Sampling Bias

This table presents results from specifications identical to those in Table VI except for the inclusion of an interaction dummy variable to proxy for detection method by regulators. The dummy variables are *Large network* (Panel A) equal to one for events in which the traders belong to an above-median size trading network; *Many traders* (Panel B) equal to one for events in which the number of traders is above the median; *FBI* (Panel C) equal to one for events in which the FBI assisted in the investigation; and *FINRA* (Panel D) equal to one for events in which FINRA assisted in the investigation. Except for *FINRA* all interaction dummies indicated a higher likelihood that insider trading was detected by non-market patterns. p-values from standard errors clustered at the event level are presented in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Illiquidity measure:	Quoted spread	Effective spread	Realized spread	Price impact	Order imbalance	Kyle's λ	MRR θ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Traders belong to large vs. small network							
Illiquidity	0.006	0.013	0.014	0.001	0.001	0.000	0.001
	(0.745)	(0.335)	(0.193)	(0.950)	(0.966)	(0.569)	(0.885)
Illiquidity×Event urgency	0.041	0.028	0.012	0.025	0.065*	0.002**	0.015
	(0.196)	(0.223)	(0.598)	(0.166)	(0.056)	(0.039)	(0.187)
Illiquidity×Large network	−0.036	−0.034	−0.015	−0.024	−0.046	−0.002	−0.020
	(0.128)	(0.113)	(0.537)	(0.273)	(0.171)	(0.137)	(0.208)
Illiquidity×Urgency×Large nwk.	0.041	0.049	−0.001	0.068	0.106	0.001	0.022
	(0.441)	(0.220)	(0.983)	(0.156)	(0.152)	(0.602)	(0.478)
Panel B. Number of traders in event is large vs. small							
Illiquidity	0.006	0.013	0.014	0.001	0.001	0.000	0.001
	(0.745)	(0.335)	(0.193)	(0.950)	(0.966)	(0.569)	(0.885)
Illiquidity×Event urgency	0.041	0.028	0.012	0.025	0.065*	0.002**	0.015
	(0.196)	(0.223)	(0.598)	(0.166)	(0.056)	(0.039)	(0.187)
Illiquidity×Many traders	−0.036	−0.034	−0.015	−0.024	−0.046	−0.002	−0.020
	(0.128)	(0.113)	(0.537)	(0.273)	(0.171)	(0.137)	(0.208)

Illiquidity measure:	Quoted spread	Effective spread	Realized spread	Price impact	Order imbalance	Kyle's λ	MRR θ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Illiquidity×Urgency×Many	0.041	0.049	−0.001	0.068	0.106	0.001	0.022
	(0.441)	(0.220)	(0.983)	(0.156)	(0.152)	(0.602)	(0.478)
Panel C. Investigations included FBI participation or not							
Illiquidity	−0.001	0.007	0.011	−0.004	−0.017	−0.001*	−0.005
	(0.974)	(0.584)	(0.259)	(0.564)	(0.243)	(0.072)	(0.469)
Illiquidity×Event urgency	0.053**	0.038*	0.014	0.037**	0.103***	0.003***	0.021*
	(0.039)	(0.051)	(0.477)	(0.026)	(< 0.001)	(< 0.001)	(0.071)
Illiquidity×FBI	−0.058	−0.059	−0.018	−0.035	0.049	0.000	−0.040***
	(0.310)	(0.562)	(0.863)	(0.727)	(0.517)	(0.779)	(0.003)
Illiquidity×Urgency×FBI	−0.002	0.013	−0.030	0.021	−0.152	−0.004	0.061
	(0.983)	(0.943)	(0.862)	(0.875)	(0.305)	(0.629)	(0.493)
Panel D. Investigations included FINRA participation or not							
Illiquidity	0.037*	0.049***	0.040***	0.004	−0.036**	0.000	−0.006
	(0.097)	(0.003)	(0.003)	(0.708)	(0.038)	(0.769)	(0.695)
Illiquidity×Event urgency	−0.006	−0.020	−0.025	0.018	0.130***	0.002	0.019
	(0.865)	(0.421)	(0.321)	(0.371)	(< 0.001)	(0.174)	(0.309)
Illiquidity×FINRA	−0.077***	−0.082***	−0.057***	−0.021	0.038	−0.002	−0.005
	(0.008)	(< 0.001)	(0.003)	(0.191)	(0.181)	(0.109)	(0.797)
Illiquidity×Urgency×FINRA	0.215**	0.276***	0.133**	0.107**	−0.093	0.004	0.064
	(0.026)	(< 0.001)	(0.047)	(0.049)	(0.244)	(0.458)	(0.359)

Table VIII.
MRR Spread Decomposition Parameters
This table presents OLS regression coefficients where the dependent variable is a dummy equal to one if any insider trading occurred on a given day. Observations are from a panel of 410 events with trading days $t = -120$ to $t = -2$, relative to the announcement date of $t = 0$. Each regression specification includes a different parameter from Madhavan, Richardson, and Roomans (1997), listed at the top of the column. See the text for specific definitions. p-values from standard errors clustered at the event level are presented in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Parameter:	α	λ	ϕ	ρ	θ
	(1)	(2)	(3)	(4)	(5)
Event fixed effects					
Parameter	0.015	< 0.001	0.007	0.044*	−0.002
	(0.343)	(0.634)	(0.164)	(0.067)	(0.771)
Daily Urgency	1.003***	0.955***	1.040***	0.857***	1.014***
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)
Parameter×Daily Urgency	−0.096	0.002	−0.049	0.316	−0.053
	(0.818)	(0.732)	(0.510)	(0.426)	(0.598)
Event fixed effects					
Parameter	0.028	< 0.001	0.005	0.101***	−0.013
	(0.200)	(0.621)	(0.565)	(0.002)	(0.116)
Parameter×Event urgency	−0.046	0.001	−0.009	−0.199***	0.020*
	(0.142)	(0.459)	(0.417)	(< 0.001)	(0.074)
Event and event-day fixed effects					
Parameter	0.024	< 0.001	0.008	0.075***	−0.008
	(0.233)	(0.513)	(0.269)	(0.008)	(0.291)
Parameter×Event urgency	−0.042	0.001	−0.006	−0.168***	0.024**
	(0.194)	(0.386)	(0.609)	(0.007)	(0.046)

Table IX.
Alternative Illiquidity Measures
This table presents OLS regression coefficients where the dependent variable is a dummy equal to one if any insider trading occurred on a given day. Observations are from a panel of 410 events with trading days $t = -120$ to $t = -2$, relative to the announcement date of $t = 0$. Each regression specification includes a different illiquidity measure, listed at the top of the column. See the text for specific definitions. p-values from standard errors clustered at the event level are presented in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Illiquidity measure:	Price Range	Realized Variance	Amihud Illiquidity
	(1)	(2)	(3)
Event fixed effects			
Illiquidity	< 0.001	-0.017	-0.021
	(0.753)	(0.222)	(0.285)
Daily Urgency	0.844***	0.900***	0.972***
	(< 0.001)	(< 0.001)	(< 0.001)
Illiquidity×Daily Urgency	0.022***	0.847***	1.003*
	(0.007)	(0.002)	(0.096)
Event fixed effects			
Illiquidity	0.001	0.015	0.001
	(0.356)	(0.517)	(0.950)
Illiquidity×Event urgency	-0.001	-0.028	0.041
	(0.479)	(0.381)	(0.264)
Event and event-day fixed effects			
Illiquidity	0.001	0.029	< 0.001
	(0.132)	(0.132)	(0.997)
Illiquidity×Event urgency	-0.001	-0.049	0.090***
	(0.517)	(0.200)	(0.006)

Table X.

Multivariate Regressions

This table presents OLS regression coefficients where the dependent variable in columns 1–4 is a dummy equal to one if any insider trading occurred on a given day, and in columns 5–8 is the logged volume of shares traded by insiders on a given day. Observations are from a panel of 410 events with trading days $t = -120$ to $t = -2$, relative to the announcement date of $t = 0$. p-values from standard errors clustered at the event level are presented in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

	Dependent variable: Informed trading dummy				Dependent variable: Ln(1+informed trading volume)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Quoted spread	< 0.001				0.026			
	(0.978)				(0.844)			
Quoted spread×Urgency	0.034				0.240			
	(0.179)				(0.260)			
Effective spread		0.007				0.097		
		(0.534)				(0.332)		
Effective spread×Urgency		0.026				0.150		
		(0.167)				(0.324)		
Realized spread			0.015				0.106	
			(0.150)				(0.209)	
Realized spread×Urgency			−0.005				−0.003	
			(0.797)				(0.981)	
Price impact				−0.008				−0.002
				(0.382)				(0.972)
Price impact×Urgency				0.039**				0.209*
				(0.017)				(0.090)
Order imbalance	−0.038**	−0.032**	−0.033**	−0.032**	−0.358**	−0.321**	−0.331**	−0.318**
	(0.028)	(0.049)	(0.040)	(0.042)	(0.010)	(0.014)	(0.012)	(0.013)
Order imbalance×Urgency	0.112***	0.105***	0.110***	0.113***	0.966***	0.918***	0.944***	0.967***
	(0.003)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(< 0.001)	(< 0.001)
Kyle's λ	< 0.001	−0.001	−0.001	< 0.001	−0.002	−0.005	−0.005	−0.004
	(0.783)	(0.298)	(0.236)	(0.383)	(0.620)	(0.169)	(0.162)	(0.250)
Kyle's λ×Urgency	< 0.001	0.001	0.002*	0.002	0.004	0.012	0.016*	0.014*
	(0.707)	(0.203)	(0.095)	(0.142)	(0.620)	(0.142)	(0.075)	(0.099)

	Dependent variable: Informed trading dummy				Dependent variable: Ln(1+informed trading volume)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MRR θ	-0.006	-0.003	-0.003	-0.003	-0.046	-0.013	-0.008	-0.012
	(0.326)	(0.629)	(0.692)	(0.660)	(0.278)	(0.757)	(0.836)	(0.760)
MRR $\theta\times$Urgency	0.020*	0.011	0.012	0.011	0.150*	0.071	0.073	0.069
	(0.098)	(0.263)	(0.264)	(0.290)	(0.072)	(0.306)	(0.305)	(0.318)
MRR ρ	0.092***	0.078***	0.078***	0.080***	0.777***	0.675***	0.679***	0.689***
	(0.004)	(0.010)	(0.010)	(0.009)	(0.003)	(0.006)	(0.006)	(0.005)
MRR $\rho\times$Urgency	-0.177**	-0.143**	-0.141**	-0.153**	-1.426**	-1.167**	-1.148**	-1.236**
	(0.017)	(0.041)	(0.045)	(0.026)	(0.015)	(0.034)	(0.038)	(0.022)
Amihud	0.007	0.009	0.006	0.012	0.034	0.039	0.038	0.074
	(0.735)	(0.667)	(0.762)	(0.597)	(0.851)	(0.826)	(0.840)	(0.711)
Amihud$\times$Urgency	0.039	0.033	0.050	0.042	0.238	0.194	0.304	0.260
	(0.326)	(0.389)	(0.211)	(0.293)	(0.485)	(0.564)	(0.394)	(0.461)
|Return|	< 0.001	< 0.001	< 0.001	< 0.001	0.007	0.006	0.007	0.006
	(0.740)	(0.857)	(0.709)	(0.836)	(0.275)	(0.308)	(0.247)	(0.333)
ln(1+volume)	0.005	0.006	0.006	0.007	0.036	0.039	0.040	0.043
	(0.217)	(0.156)	(0.162)	(0.129)	(0.287)	(0.233)	(0.224)	(0.188)
|MKT|	-0.001	-0.001	-0.001	-0.001	-0.011	-0.012	-0.011	-0.011
	(0.572)	(0.626)	(0.638)	(0.665)	(0.359)	(0.339)	(0.366)	(0.383)
|SMB|	< 0.001	< 0.001	< 0.001	< 0.001	-0.026	-0.024	-0.023	-0.023
	(0.952)	(0.999)	(0.970)	(0.982)	(0.337)	(0.358)	(0.382)	(0.372)
|HML|	0.002	0.002	0.002	0.002	-0.015	-0.008	-0.007	-0.007
	(0.686)	(0.601)	(0.593)	(0.589)	(0.640)	(0.805)	(0.815)	(0.817)
Constant	-0.021	-0.017	-0.017	-0.015	-0.165	-0.149	-0.142	-0.134
	(0.171)	(0.244)	(0.253)	(0.294)	(0.153)	(0.187)	(0.208)	(0.229)
Event fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event-day fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	38,050	39,138	39,138	39,138	37,789	38,872	38,872	38,872
Adjusted R^2	0.166	0.167	0.167	0.167	0.160	0.163	0.163	0.163

Internet Appendix

"Do Proxies for Informed Trading Measure Informed Trading?

Evidence from Illegal Insider Trades"

Kenneth R. Ahern

Internet Appendix Table I.

MRR Spread Decomposition Parameters: Volume of Shares Traded

This table presents OLS regression coefficients where the dependent variable is the logged volume of shares traded by insiders on a given day. Observations are from a panel of 410 events with trading days $t = -120$ to $t = -2$, relative to the announcement date of $t = 0$. Each regression specification includes a different parameter from Madhavan, Richardson, and Roomans (1997), listed at the top of the column. See the text for specific definitions. p-values from standard errors clustered at the event level are presented in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Parameter:	α	λ	ϕ	ρ	θ
	(1)	(2)	(3)	(4)	(5)
Event fixed effects					
Parameter	0.080	−0.001	0.057*	0.250	0.015
	(0.483)	(0.788)	(0.075)	(0.207)	(0.666)
Daily Urgency	7.981***	8.949***	8.595***	5.185***	8.172***
	(< 0.001)	(< 0.001)	(< 0.001)	(0.002)	(< 0.001)
Parameter×Daily Urgency	0.235	−0.032	−0.818	6.115*	−0.905
	(0.934)	(0.436)	(0.122)	(0.089)	(0.209)
Event fixed effects					
Parameter	0.214	−0.004	0.021	0.828***	−0.083
	(0.184)	(0.341)	(0.726)	(0.002)	(0.107)
Parameter×Event urgency	−0.336	0.006	−0.051	−1.600***	0.132*
	(0.145)	(0.251)	(0.500)	(< 0.001)	(0.063)
Event and event-day fixed effects					
Parameter	0.184	−0.004	0.045	0.647***	−0.045
	(0.210)	(0.245)	(0.354)	(0.005)	(0.319)
Parameter×Event urgency	−0.304	0.009	−0.022	−1.366***	0.168**
	(0.205)	(0.223)	(0.769)	(0.007)	(0.043)

Internet Appendix Table II.
Alternative Illiquidity Measures: Volume of Shares Traded
This table presents OLS regression coefficients where the dependent variable is the logged volume of shares traded by insiders on a given day. Observations are from a panel of 410 events with trading days $t = -120$ to $t = -2$, relative to the announcement date of $t = 0$. Each regression specification includes a different illiquidity measure, listed at the top of the column. See the text for specific definitions. p-values from standard errors clustered at the event level are presented in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Illiquidity measure:	Price Range	Realized Variance	Amihud Illiquidity
	(1)	(2)	(3)
Event fixed effects			
Illiquidity	−0.003	−0.237**	−0.242
	(0.378)	(0.048)	(0.144)
Daily Urgency	6.345***	6.770***	7.637***
	(< 0.001)	(< 0.001)	(< 0.001)
Illiquidity×Daily Urgency	0.221***	9.836***	10.012**
	(0.002)	(< 0.001)	(0.047)
Event fixed effects			
Illiquidity	0.005	0.144	−0.005
	(0.395)	(0.475)	(0.978)
Illiquidity×Event urgency	−0.006	−0.188	0.291
	(0.517)	(0.473)	(0.367)
Event and event-day fixed effects			
Illiquidity	0.007	0.252	−0.011
	(0.150)	(0.133)	(0.943)
Illiquidity×Event urgency	−0.007	−0.358	0.648**
	(0.518)	(0.235)	(0.025)

Internet Appendix Table III.

Alternative Calculations of Kyle's λ

This table presents OLS regression coefficients where the dependent variable in Panel A is a dummy equal to one if any insider trading occurred on a given day, and in Panel B is the logged volume of shares traded by insiders on a given day. Observations are from a panel of 410 events with trading days $t = -120$ to $t = -2$, relative to the announcement date of $t = 0$. Each regression specification includes a different measure of Kyle's λ, listed at the top of the column. See the text for specific definitions. p-values from standard errors clustered at the event level are presented in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

	Trade level				5-minute window			
Illiquidity measure:	$\lambda_{1,k}$	$\lambda_{2,k}$	$\lambda_{3,k}$	$\lambda_{4,k}$	$\lambda_{1,5}$	$\lambda_{2,5}$	$\lambda_{3,5}$	$\lambda_{4,5}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Dependent variable: Informed trading dummy								
Event fixed effects								
Illiquidity	−0.001***	< 0.001	< 0.001	−0.009*	< 0.001*	< 0.001	< 0.001*	< 0.001
	(0.005)	(0.313)	(0.742)	(0.059)	(0.083)	(0.108)	(0.080)	(0.205)
Daily Urgency	0.830***	0.963***	1.025***	1.033***	0.999***	1.001***	1.000***	1.004***
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)
Illiquidity×Daily Urgency	0.024***	0.004**	< 0.001	0.039	0.015*	0.014*	0.014*	0.014*
	(0.006)	(0.043)	(0.261)	(0.697)	(0.051)	(0.052)	(0.053)	(0.084)
Event fixed effects								
Illiquidity	−0.001**	< 0.001	< 0.001	−0.012***	< 0.001	< 0.001	< 0.001	< 0.001
	(0.020)	(0.929)	(0.406)	(0.009)	(0.891)	(0.998)	(0.823)	(0.834)
Illiquidity×Event urgency	0.002***	< 0.001	< 0.001	0.021***	< 0.001	< 0.001	< 0.001	< 0.001
	(0.004)	(0.751)	(0.440)	(0.004)	(0.608)	(0.665)	(0.570)	(0.390)
Event and event-day fixed effects								
Illiquidity	−0.001*	< 0.001	< 0.001	−0.011***	< 0.001	< 0.001	< 0.001	< 0.001
	(0.058)	(0.853)	(0.322)	(0.005)	(0.817)	(0.724)	(0.871)	(0.976)

	Trade level				5-minute window			
Illiquidity measure:	$\lambda_{1,k}$	$\lambda_{2,k}$	$\lambda_{3,k}$	$\lambda_{4,k}$	$\lambda_{1,5}$	$\lambda_{2,5}$	$\lambda_{3,5}$	$\lambda_{4,5}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Illiquidity×Event urgency	0.003***	< 0.001	< 0.001	0.023***	0.001	0.001	0.001	0.001
	(0.003)	(0.325)	(0.240)	(0.002)	(0.513)	(0.544)	(0.494)	(0.287)
Panel B. Dependent variable: Ln(1+informed trading volume)								
Event fixed effects								
Illiquidity	−0.010***	−0.001	< 0.001	−0.032	−0.005**	−0.005**	−0.005**	−0.001
	(0.006)	(0.238)	(0.676)	(0.328)	(0.029)	(0.037)	(0.028)	(0.740)
Daily Urgency	6.920***	7.549***	8.132***	8.312***	7.787***	7.809***	7.798***	8.065***
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)
Illiquidity×Daily Urgency	0.144**	0.037**	< 0.001	−0.402	0.159**	0.150**	0.155**	0.068
	(0.021)	(0.018)	(0.409)	(0.592)	(0.020)	(0.021)	(0.021)	(0.247)
Event fixed effects								
Illiquidity	−0.010***	< 0.001	< 0.001	−0.078**	< 0.001	< 0.001	< 0.001	0.001
	(0.008)	(0.783)	(0.656)	(0.011)	(0.941)	(0.970)	(0.867)	(0.735)
Illiquidity×Event urgency	0.019***	< 0.001	< 0.001	0.135***	0.002	0.002	0.002	0.001
	(0.002)	(0.754)	(0.668)	(0.004)	(0.729)	(0.764)	(0.688)	(0.636)
Event and event-day fixed effects								
Illiquidity	−0.007**	< 0.001	< 0.001	−0.071***	0.001	0.001	< 0.001	0.001
	(0.023)	(0.706)	(0.501)	(0.005)	(0.803)	(0.735)	(0.865)	(0.577)
Illiquidity×Event urgency	0.024***	0.002	< 0.001	0.155***	0.005	0.004	0.005	0.003
	(0.002)	(0.307)	(0.349)	(0.002)	(0.578)	(0.597)	(0.555)	(0.390)

Internet Appendix Table IV.

Dollar Spreads and Informed Trading

This table presents OLS regression coefficients where the dependent variable is a dummy equal to one if any insider trading occurred on a given day (Panel A) or the logged volume of shares traded by insiders on a given day (Panel B). Observations are from a panel of 410 events with trading days $t = -120, \ldots, -2$, relative to the announcement date of $t = 0$. Each regression specification includes a different illiquidity measure, listed at the top of the column. See the text for specific definitions. p-values from standard errors clustered at the event level are presented in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Illiquidity measure:	Dollar bid-ask spread	Dollar effective spread	Dollar realized spread	Dollar price impact
	(1)	(2)	(3)	(4)
Panel A. Dependent variable: Informed trading dummy				
Event fixed effects				
Illiquidity	-0.001	< 0.001	< 0.001	< 0.001
	(0.573)	(0.830)	(0.946)	(0.782)
Daily Urgency	0.942***	0.969***	0.993***	0.990***
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)
Illiquidity×Daily Urgency	0.016	0.010	0.006	0.007
	(0.380)	(0.558)	(0.709)	(0.688)
Event fixed effects				
Illiquidity	-0.002	< 0.001	< 0.001	-0.001
	(0.235)	(0.898)	(0.906)	(0.551)
Illiquidity×Event urgency	0.004	< 0.001	< 0.001	0.001
	(0.132)	(0.842)	(0.728)	(0.669)
Event and event-day fixed effects				
Illiquidity	-0.001	0.001	0.001	< 0.001
	(0.390)	(0.530)	(0.428)	(0.779)
Illiquidity×Event urgency	0.006*	0.001	< 0.001	0.001

Illiquidity measures:	Dollar bid-ask spread	Dollar effective spread	Dollar realized spread	Dollar price impact
	(1)	(2)	(3)	(4)
	(0.082)	(0.620)	(0.934)	(0.416)
Panel B. Dependent variable: Ln(1+informed trading volume)				
Event fixed effects				
Illiquidity	−0.004	0.006	0.002	0.003
	(0.700)	(0.420)	(0.636)	(0.568)
Daily Urgency	7.959***	8.084***	8.076***	8.028***
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)
Illiquidity×Daily Urgency	−0.004	−0.029	−0.058	−0.025
	(0.977)	(0.832)	(0.660)	(0.855)
Event fixed effects				
Illiquidity	−0.020	−0.002	−0.003	−0.001
	(0.104)	(0.848)	(0.654)	(0.876)
Illiquidity×Event urgency	0.034*	−0.003	0.002	< 0.001
	(0.053)	(0.864)	(0.855)	(0.976)
Event and event-day fixed effects				
Illiquidity	−0.014	0.006	0.002	0.001
	(0.195)	(0.484)	(0.772)	(0.814)
Illiquidity×Event urgency	0.048**	0.011	0.006	0.005
	(0.042)	(0.568)	(0.611)	(0.667)

INTERNET APPENDIX TABLE V.

Pairwise Correlations of Illiquidity Measures

This table presents pairwise correlations using daily data from a panel of 410 events with trading days $t = -120$ to $t = -2$, relative to the announcement date of $t = 0$. p-values are presented in parentheses.

	Quoted spread	Effective spread	Realized spread	Price impact	Order imbalance	Kyle's λ	MRR θ	MRR ρ
Effective spread	0.883							
	(< 0.001)							
Realized spread	0.727	0.805						
	(< 0.001)	(< 0.001)						
Price impact	0.601	0.684	0.181					
	(< 0.001)	(< 0.001)	(< 0.001)					
Order imbalance	0.459	0.430	0.367	0.289				
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)				
Kyle's λ	0.487	0.445	0.373	0.313	0.263			
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)			
MRR θ	0.135	0.100	0.032	0.137	0.116	0.473		
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)		
MRR ρ	−0.039	−0.012	−0.015	< 0.001	0.039	−0.439	−0.260	
	(< 0.001)	(0.009)	(0.001)	(0.940)	(< 0.001)	(< 0.001)	(< 0.001)	
Amihud	0.225	0.225	0.187	0.113	0.094	0.145	−0.001	−0.009
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(0.903)	(0.053)