Market Quality of Informed Trades*

Wan Soo Choi[a], Juha Joenväärä[b], Dominik Rösch[a,*], Cristian Tiu[a]

[a] *University at Buffalo*
[b] *Aalto University*

August 23, 2025

Abstract

We investigate prices around timestamped informed trades using approximately 500,000 13D transactions from activist investors matched to TAQ trades. Activists are more price sensitive than non-activists, and are more likely to attempt to hide their trading by strategically choosing when and how to trade. Activists have lower execution quality, higher price impact, and lower realized spreads, suggesting that activists, on average, fail to hide among the uninformed. Activists with less information (as measured by lower returns) are better at hiding (that is, they have better execution quality). These results are reversed for hedge funds: hedge funds with better execution quality generate higher returns.

Keywords: Informed Trading, Liquidity, Transaction Costs, 13D, Hedge Funds

JEL: G14, G23

*We thank seminar participants at Hanyang University for their comments and OneMarket Data for the use of their OneTick software. This work is supported in part by NSF ACI-1541215.

*Corresponding author

Email addresses: `wansooch@buffalo.edu` (Wan Soo Choi), `juha.joenvaara@aalto.fi` (Juha Joenväärä), `drosch@buffalo.edu` (Dominik Rösch), `ctiu@buffalo.edu` (Cristian Tiu)

1. Introduction

Impounding large amounts of firm-specific information into prices improves market and allocation efficiency (Wurgler, 2000). While firms' public announcements may generate such information, Grossman and Stiglitz (1980) argue that prices are most efficient in markets where profit opportunities motivate traders to trade on their private information. According to theory, these informed investors then create adverse selection, thereby increasing illiquidity, and with it, the cost of capital.[1]

However, empirical evidence suporting this theory is scant. As Kyle (1985, abstract) noted, "noise trading provides camouflage which conceals [informed] trading." Sophisticated investors may thus hide among the uninformed in ways that make it difficult for liquidity providers to detect their presence and react accordingly.[2] Consistent with this possibility, newer empirical evidence indicates that illiquidity and adverse selection measures are often lower on the days informed traders trade because they strategically chose when and how to trade.[3] For example, Henry and Koski (2017) investigate dividend arbitrage and provide evidence that informed trading is profitable only when traders manage to achieve superior execution quality and hide among the uninformed. Yet Henry and Koski (2017) focus on informed traders trading on public information that generates relatively low returns (0.172%, and may even turn negative after transaction costs, according to their Table 3).

How important is execution quality when one is trading on private information, with much larger potential returns? Can liquidity providers detect such informed traders' presence at the time of execution or after? To answer these questions, we focus on activist investors, who are informed as they earn large profits of 9% over 40-day intervals (Figure 2 of Collin-Dufresne and Fos, 2015).

We thus extract all individual trades from all 13D filings from 1994 to 2021. This sample gives us a comprehensive cross-section and time-series of trades by activist investors. We match stock-day 13D trades against time-stamped Transaction and Quotes (TAQ) trades

[1]For example, Glosten and Milgrom (1985), Kyle (1985) and Amihud and Mendelson (1986).

[2]E.g., see Brogaard, Hendershott, and Riordan (2019), Goyal, Reed, Smajlbegovic, and Soebhag (2025), Kwan, Philip, and Shkilko (2024), and Roşu (2020).

[3]E.g., see Collin-Dufresne and Fos (2015), Collin-Dufresne and Fos (2016), and Kacperczyk and Pagnotta (2019).

using the trade price and the number of shares traded. Having time-stamped 13D trades allows us to investigate actual trades by informed traders, including their timing and, more importantly, to investigate how other traders and liquidity providers react to these individual trades precisely when they occur. To do so, using our matched 13D and TAQ sample, we construct all common market microstructure variables at the trade level and compare their values for matched 13D versus non-13D trades within the same one-minute interval, and throughout the day.

Depending on how we match 13D trades to TAQ trades, our sample results in between 50k and 1,500k TAQ observations. More precisely, if we consider all TAQ trades matching with 13D trades and do not impose either a unique TAQ nor a unique 13D trade, we find a match for almost 1,500k TAQ trades and almost 180k out of around 1.5 million 1-minute intervals with at least one TAQ trade matching with a 13D trade. This represents our initial sample, which we call "TAQ Multiple Match." While the 13D filings report the accumulation of shares, they do not contain only buy trades. To separate these trades we further filter our data to only TAQ trades uniquely matching with 13D trades (while allowing each 13D trade to match with one or more TAQ trades). We call this refined sample, which contains about 500k TAQ trades, the "TAQ Simple Match." This restricted dataset allows for the identification of what type of trade the 13D filer executed (e.g., a buy). Interestingly, while the latter dataset is smaller, the number of one-minute intervals with matched 13D trades is close to that in the former, larger dataset, suggesting that most of the "TAQ Multiple Match" trades occur in batches.

Consistent with the premise that informed traders attempt to hide, we find that almost 40% of all trades reported in 13D filings are sell transactions. Selling likely serves the purpose of hiding activists ulterior motive to build up a large enough equity stake allowing them to influence management. [4] Furthermore, consistent with Collin-Dufresne and Fos (2015), we also identify around 50% of all 13D trades as using limit-orders.

We start with comparing illiquidity measures such as the quoted, effective, and realized spread, as well as price impact of 13D compared to non-13D trades. For that, we estimate

[4] We only include initial 13D reports and exclude amendments, which have to be filed after the initial filing and contain any changes in the position. We expect that amendments contain even more sells.

all illiquidity variables at the trade level and average them separately for 13D and non-13D trades within each minute. We then run fixed-effect panel regressions explaining illiquidity by an indicator variable equal to one for 13D trades. The estimated slope coefficient in these regressions measures the difference in illiquidity between 13D and non-13D trades. Throughout the paper, we estimate these panel regressions using our two different samples. We first use one-minute intervals throughout the day and therefore compare 13D trades to non-13 trades regardless of when they occur. To address potential time-of-the-day trading patterns (Admati and Pfleiderer, 1988), we also focus on using only one-minute intervals with both at least one 13D and at least one non-13D trade.

We find evidence that 13D filers trade at times when hiding is easier. Specifically, when we compare illiquidity in intervals with 13D trades to that in any other interval, we find that both quoted and effective spreads in 13D intervals are lower by 1.0 and, respectively, 0.4 basis points. Even when we compare 13D to non-13D trades within the same interval, we find that for 13D trades quoted and effective spreads are also lower by, respectively, 0.5 and 0.3 basis points. These results confirm the 13D filers' endogenous decision to trade when liquidity is high.

Three out of four of these estimates have a t-statistic (based on standard errors clustered by stock and date) below -2.0 and are therefore conventionally considered statistically significant. While these results might appear economically insignificant, we find that the median (average) 1-minute interval has a quoted spread of just around 8 (20) basis points. A difference of around one basis point in quoted spreads comparing 13D to non-13D trades is therefore reasonably large and aligns to other studies in terms of magnitude, such as, for example, illustrated by Table 4 of Rösch (2021). These results at the trade level confirm findings at the stock-day level by Collin-Dufresne and Fos (2015), who find that spreads are around 12 basis points lower on days when 13D traders trade (their Table 3).

Collin-Dufresne and Fos (2015) also find that adverse selection measures are lower on days when 13D traders trade. While Collin-Dufresne and Fos (2015) explain these findings using their low-frequency approach, at the trade level we should expect that 13D traders have higher adverse selection costs.

Indeed, we find that price improvements and realized spreads (commonly interpreted as earnings by liquidity providers, see, e.g, Huang and Stoll (1996) or Conrad and Wahal (2020)) are lower by around 0.5 to 1.0 basis points for 13D trades and three out of four estimates are statistically significant. Price impact is higher by around 0.7 (with a t-statistic of 3.46) or 0.5 (t-statistic of 2.34) basis points, depending on whether we compare 13D to non-13D trades throughout the day or within the same one-minute interval. Results on price improvements are only statistically significant when comparing 13D trades throughout the day. When comparing 13D trades to non-13D trades in the same one-minute interval, results on price improvements are not statistically significant, consistent with 13D traders being able to partially hide their trades.

Given the common approach in the literature to proxy informed traders by large trades,[5] we also investigate differences in the number of shares and the dollar volume traded of 13D and non-13D trades. When we compare 13D trades to other trades in all intervals we do not find a statistically significant difference. When we compare 13D to non-13D trades within the same interval, however, we find that 13D trades are smaller by almost 150 shares and $ 4.5k. These results emphasize that, in more recent years, size is a poor proxy for informed trading and 13D traders use smaller orders to hide their information.

Electing to trade when liquidity is high is not the only way 13D traders can hide their buying. First, these informed traders may detect hidden illiquidity (Bartlett, McCrary, and O'Hara, 2025) or may use limit orders when liquidity is low. Second, traders may sell to hide their buying intentions. Consistent with these possibilities, we find that a large part of our 13D trades consists of limit orders and of sells. We therefore investigate differences in illiquidity measures for these groups next. If traders use limit orders during periods of low liquidity, we expect the coefficient for 13D limit orders to be higher than that for market orders when explaining quoted and effective spreads. This is exactly what we find.

For 13D limit-orders, quoted and effective spreads are also lower than for non 13D trades, but are higher than when 13D use market orders. This indicates that when spreads are relatively high, 13D traders switch to limit orders. Consistent with the idea that 13D traders endogenously chose their trading strategy we also find that size and dollar volume are higher

[5]For example, see Barclay and Warner (1993). However, more recent papers question this approach. For an excellent overview we refer to Section 1.1.1 in Goldstein, Spatt, and Ye (2024).

when they use limit, versus market orders.

The sign of both price improvement and price impact are also consistent, but statistically insignificant for market orders. The results on price improvement and price impact are driven by limit-orders. Regarding limit-orders we find negative price improvement and a higher price impact of around 1.0 basis points. Several studies indicate that informed traders might use limit orders and that these might be even more informed than market orders, see, e.g., Kaniel and Liu (2006). Our results are consistent with these studies.

When we separate 13D trades by buys and sells we find similar results regarding illiquidity, except for size of trades. Given that bid and ask prices are set before a trade arrives, it is unsurprising that quoted spreads for buy or sell 13D trades are statistically similar to each other. More surprising is that, realized spreads are around 2.0 to 3.0 bps lower than for non-13Ds regardless of whether 13Ds are buys or sells. For non-13Ds, the differences in realized spreads are driven by whether a trade is buyer or seller-initiated. These results hold regardless of whether we further distinguish between limit and market orders (as shown in Appendix Table A.3).

So far we document that 13D traders attempt to hide among uninformed traders by strategically choosing when and how to trade: by performing smaller transactions; by selling as well as buying as they build up a position; by electing to trade when liquidity is high; and by using limit orders when liquidity is not propitious to using market orders. However, these results do not imply that 13D traders are perfectly hidden or cannot be identified. Indeed, we find that adverse selection measures react to the 13D trades. In particular, realized spread is lower and price impact is higher, regardless of which sample we use, indicating that prices impound the information from the 13D trades immediately afterwards.

To more formally test how prices and illiquidity are affected by informed traders and to address endogeneity issues, we estimate a Vector Autoregression model and Impulse response functions using quoted spreads, trade indicators for 13D and non-13D trades, and returns as endogenous variables. Consistent with previous results, we find that returns after 13D trades increase by 40 basis points on average over the next minutes. After non-13D trades, returns only go up within the next five minutes, but then revert to zero. This is consistent with the idea that 13D traders have positive information resulting in a permanent price impact, while non 13D traders are, on average, uninformed and cause only transitory price pressure.[6] We also find that quoted spreads after 13D increase by around 50 basis points, while quoted spreads after non-13D trades increase even more, potentially indicating that inventory concerns for liquidity providers are more important than adverse selection about longer-term fundamentals. Alternatively, price improvement and hidden liquidity might be differently affected after 13D trades.

Next, we investigate cross-sectional differences between 13D traders. In particular, we investigate whether our results differ depending on whether the 13D trader is a Hedge Fund. Hedge Funds (HFs) are often considered especially sophisticated.[7] As such, HFs might be able to hide especially well among non-13D traders. This is indeed what we find. Adverse selection is elevated (price improvement and realized spreads are lower, and price impact is higher) for HF 13D trades when compared to non-13D trades across all 1-minute intervals. However, these differences disappear when we compare HF 13D trades within the same interval. Compared to non-13D trades within the same interval, HF 13D trades have a statistically and economically marginal difference in price improvement, realized spreads, and price impact.

An alternative explanation for why HF are able to hide well among non-13D traders is that 13D HF are not that well informed, e.g., because they are not really activist but rather liquidity providers (Jame, 2018). To rule out this possibility, we also investigate the abnormal returns associated with each 13D filing. That is, while our analysis has been at the trade level so far, we now aggregate trades at the filing level. All trades reported in a filing can be interpreted as one "order" or as one "package," as Chan and Lakonishok (1995) put it.

We find that at the order-level, HF 13D traders, on average, have higher announcement day returns of 1.1% than non HF 13D traders with only 0.3%. After 20-days, both 13D HF and non-HF have equal abnormal returns of around 4%, on average. These results hold regardless of whether we measure raw holding period returns or estimate abnormal returns.

[6] see, e.g., Admati and Pfleiderer (1988); Glosten and Harris (1988); Kyle (1985).

[7] e.g., (Stein, 2009; Grinblatt, Jostova, Petrasek, and Philipov, 2020; Jagannathan, Malakhov, and Novikov, 2010; Wu and Chung, 2022; Chen, Kelly, and Wu, 2020).

Finally, we formally test the informed traders' ability to hide among uninformed. Informed traders face a trade-off between improving execution quality (and with it their ability to hide among the uninformed) and potential trading profits. For example, to improve execution quality, traders may switch to using limit orders, but in this case they risk non-execution and therefore might not be able to profit from their information. To estimate the impact of execution quality on profits, we separate our 13D filings into two groups of either high or low execution quality, and then track their profitability by comparing their trade prices to end-of-day prices on the announcement day, as well as five, and respectively ten days afterwards.

Using the whole sample of almost 2,000 filings, we find that returns on the announcement date do not statistically differ across low or high execution quality. However, longer-term profits after five or ten days are higher (4% vs 3%) for the low execution filings (with a t-statistic of 1.65 or 1.86), depending on which execution quality measure we use. When we distinguish between hedge funds and non-hedge funds, we find that for hedge funds, execution quality does not statistically significantly affect profits. For hedge funds, profits are even higher by more than 1% in the group of high execution quality, although, the difference in profits between the low and high execution quality hedge funds is not statistically significant.

We additionally distinguish between patient and impatient traders. Although 13D reports must be filed within 10 days after investors reach the threshold of owning more than 5% of all outstanding shares, following the methodology outlined by Bogousslavsky, Fos, and Muravyev (2024) we categorize patient trades as those executed 20 or more days before the filing date. Returns are around 1% higher for the low execution quality trades, but are not statistically significantly different from those coming from the high execution quality trades. When traders are impatient, longer-term returns are much higher, by almost 3%, for low execution quality filings.

In summary, for non-hedge funds execution quality seems to play a marginal role in explaining the profits of 13D traders. In fact, profits decrease by execution quality, indicating the trade-offs between profiting from private information and achieving the best possible price. On the other hand, hedge funds have higher profits with higher execution quality, potentially indicating that 13D hedge funds are both especially informed as well as skilled in hiding among uninformed traders compared to other 13D traders.

Our paper contributes to the literature on how informed traders affect market liquidity. Finding causal evidence for how informed investors affect market liquidity is challenging. It is difficult to identify informed traders, and even more difficult to identify informed trades. To address this challenge, the current literature often proxies informed traders by using crude trade specific proxies (such as trade size), disclosure requirements such as Form-4 filings by insiders, or 13D filings by activist investors. Most of the existing literature uses these filings only to identify the stock-day when the investor traded, precluding finding actual causal evidence. Only a few papers link stock-day levels where the trader is identified to intraday TAQ data. Some examples are Inci, Lu, and Seyhun (2010) who uses Form-4 data, and Jame (2018) who uses institutional trades provided by Ancerno/AbelNoser. We add to this literature by showing that adverse selection measures increase for informed investors so their trades can be identified—but only when we look at high frequency data.

Our paper is most closely related to those of Akey, Grégoire, and Martineau (2022), Kacperczyk and Pagnotta (2019), Kacperczyk and Pagnotta (2024), Collin-Dufresne and Fos (2015), Jame (2018), Shkilko (2020), and Inci et al. (2010). Both Akey et al. (2022) and Kacperczyk and Pagnotta (2024) investigate illegal informed trading. Akey et al. (2022) investigate informed trading on days on which earnings announcement news were hacked. While they find that effective spreads are higher, on average, the increase is due to an increase in realized spreads and not price impact contrary to idea of an increase in adverse selection risk (their Table 6). Both Kacperczyk and Pagnotta (2019) and Kacperczyk and Pagnotta (2024) investigate illegal trading schemes prosecuted by the Securities and Exchange Commission (SEC). Kacperczyk and Pagnotta (2019) find that on days insiders trade, quoted spreads and illiquidity are lower (see, e.g., their Figure 2). Collin-Dufresne and Fos (2015) use 13D filings to investigate whether "prices reveal the presence of informed trading." However Collin-Dufresne and Fos (2015) investigate measures of adverse selection and illiquidity at the stock-day level to find that adverse selection is low on days when 13D traders trade.[8] Like their study, we use 13D filings but we additionally match 13D trades to TAQ trades,

[8]Collin-Dufresne and Fos (2015) rely on daily observations for all their tabulated results. They use "time-stamped" 13D trades only to show that 13D traders use limit orders.

allowing us to estimate adverse selection at the trade level.

Jame (2018), Shkilko (2020) and Inci et al. (2010) use intraday trades. Jame (2018) use proprietary data containing hedge fund trades. He focuses on liquidity provision of hedge funds. He does not match his institutional trades with TAQ data and therefore cannot estimate how these trades affect prices. Both Shkilko (2020) and Inci et al. (2010) focus on insider trading. Inci et al. (2010) use TAQ matched trades by corporate insiders as reported in Form-4. Their main contribution is showing that corporate insiders are "informed". Shkilko (2020) uses Canadian audit trail data from 2004 to 2006, allowing him to identify trades and quotes from corporate insiders.

Compared to these studies, we focus on publicly available 13D filings containing activist trades and estimate their execution quality. Both Inci et al. (2010) and Shkilko (2020) use data before 2003 and 2006, respectively, i.e., data before (or just after) regulation NMS and the advent of algorithmic trading. As such it is not surprising that both Inci et al. (2010) and Shkilko (2020) find that insiders "execut[e] large trades" (Shkilko, 2020, p. 30). For example, Inci et al. (2010) find that more than half of their insider trades are relatively large with over 1,000 shares per trade (their Table 1). By contrast, our data extends to after 2006 and features multiple small institutional trades.

Finally, our paper contributes to the debate on whether the current 13D reporting rules harm uninformed investors. In particular, activists can generate returns at the expense of uninformed investors, but also, as in Chabakauri, Fos, and Jiang (2024), corporate insiders may detect informed traders trading in their stocks and may refrain from selling in order to profit from activists' activities while preserving their control of a company. We show that publicly available measures of adverse selection react to, and enable the uninformed to infer that an informed trade took place.

The rest of the paper is organized as following. In Section 2 we discuss our sample construction and provide summary statistics, in particular we discuss how we collect 13D trades and how we merge these with TAQ trades. Sections 3 and 4 discuss the results comparing execution quality and returns for 13D and non-13D trades. Section 5 concludes.

2. Sample Construction and Summary Statistics

Data on trades by Schedule 13D filers come from Schedule 13D filings, publicly available on EDGAR. We collected Schedule 13D filings from 1994 to 2021, excluding amendments. Those trades are then combined with data from the Center for Research in Security Prices (CRSP) and with the NYSE Trade and Quotes (TAQ) database. Further details on the data collection are described in Section 2.2.1.

We categorize 13D filers into two groups: hedge funds and other activist investors. We identify hedge funds by using the methodology of Brav, Jiang, Partnoy, and Thomas (2008a) and then expanding their list by hedge funds from major commercial databases merged by Joenväärä, Kauppila, Kosowski, and Tolonen (2021). Since some activist hedge funds do not report to these commercial databases (Brav, Jiang, Partnoy, and Thomas (2008b)), we address potential selection bias by collecting the non-reporting funds from Form ADV filings, following the approach of Barth, Joenväärä, Kauppila, and Wermers (2023).

2.1. Schedule 13D filings

Rule 13d-1(a) of the 1934 Securities Exchange Act requires investors who acquire over 5% of any class of security of a publicly traded company to file a report with the SEC within 10 days.[9] This requirement applies specifically when the investor expresses an interest in actively influencing the management of the company. If the investor is not interested in the activism, they must file Schedule 13G instead, which is a shorter version of Schedule 13D with fewer reporting requirements. Item 5 (c) of Schedule 13D require the filer to describe the identity of the person, date, price, and quantity of any transactions in the target company's securities during the past 60 days or since the most recent filing of Schedule 13D. In most cases, detailed information about the trades is attached to the appendix, in Item 7 (Schedule A). However, there appears to be no specific guideline regarding the reporting of transactions. Some filings provide detailed tables, while others offer sparse information in sentences. The

[9]This rule has been amended. The SEC has revised Schedule 13D rules, shortening the initial filing deadline from 10 calendar days to 5 business days after acquiring more than 5% beneficial ownership. This amendment took effect on February 5, 2024. Details on this change can be found at `https://www.sec.gov/files/33-11030-fact-sheet.pdf` and `https://www.sec.gov/newsroom/press-releases/2023-219`. Our sample period predates this amendment, and therefore, we use the 10-day deadline in our analysis.

reporting frequency also varies, with some aggregating daily and others detailing each trade. This diverse formatting poses a key challenge in our data collection.

2.2. Data Construction

2.2.1. Collection and Refinement of the Schedule 13D Trades

We download all 13D filings from EDGAR by extracting those with form types labeled "SC13D" from the SEC index file. Within each filing, we identify lines that contain a date, a decimal, and an integer, which represent the trade date, price, and size, respectively. For Schedule 13D filings that contain transaction data, we collect the filing date and event date, as well as the company name, CIK, and CUSIP for both the subject and the filing companies. To verify the identity of the target company, we use the WRDS CIK-CUSIP link table. Often, a single 13D filing involves multiple entities participating in the transaction, referred to as "Persons" in the filings. When available, we collect detailed information about the Persons involved in these 13D trades.[10]

We begin by extracting all trades from the filings and subsequently go through multiple filtering processes for refinement. The changes in the number of filings and trades at each stage is documented in Table 1. Of the 114,241 13D filings from the SEC, only a small fraction contain detailed transaction records. Additionally, upon closer inspection, we found that some index files contained duplicate 13D filings listed under different names. After removing filings without trade information and duplicated 13D filings, we are left with 8,945 filings, comprising a total of 863,773 13D trades as reported in the *Raw* column of Table 1. To ensure data accuracy, we cross-referenced the filings with a CIK-CUSIP link table. Filings lacking CIK for both the filing entity and the target company were excluded from our sample. Additionally, filings without a corresponding CUSIP for the target company were also removed. This refinement process resulted in 7,802 filings, encompassing 798,619 13D trades as reported in *CUSIP matched* column. We further narrowed down the sample to retain only assets with CRSP share codes 10 or 11, indicating common stocks. Also, we excluded stocks priced below $1 or above $1,000, following the methodology of Collin-Dufresne

[10]The filing format changes over time. From 1994 to the late 1990s, most filings were in plain text (.txt) format. However, beginning in 2005, the predominant format shifted to HTML. During the transitional period, both formats were in use, resulting in a mix of file types.

and Fos (2015). This resulted in a dataset of 4,132 filings,[11] comprising 504,843 13D trades as presented in column *Common Stock ($1 - $1000).*

2.2.2. Identification of Schedule 13D Trades

Matching 13D trades to the TAQ database presents several challenges due to varying reporting practices among filers. According to 17 CFR § 240.13d-101, a regulation detailing the filing requirements for Schedule 13D, filers must describe "any transactions in the class of securities reported on that were effected during the past sixty days or since the most recent filing of Schedule 13D ...".[12] However, interpretations of this requirement differ, resulting in diverse reporting formats. Some filers report transactions in a daily aggregated format, others provide details at the individual trade level, and some use a combination of both approaches.

Despite the inconsistency in trade reporting, it is unlikely that aggregated 13D trades can be matched one-to-one to a single TAQ trade. This is because aggregated trades typically show exceptionally large dollar volumes with average prices that extend to four decimal places. For example, in the second quarter of 2018, GAMCO Investors, Inc. (CIK 807249) filed a 13D indicating their 5% ownership of EnPro Industries, Inc. (CIK 1164863) motivated by activism. The filing details the average price, reflecting the cost to accumulate the designated number of shares, clearly noted in the price column. Specifically, on May 3, 2018, GAMCO Asset Management Inc. reported purchasing 45,298 shares at an average price of $68.4091, amounting to a dollar volume exceeding three million dollars—a figure that suggests aggregated trades rather than a single transaction.[13] Trades with such characteristics are unlikely to match with TAQ trades. On the other hand, some filers report their trading at the individual trade level.[14] These filings typically involve trade size of one round lot and prices with two decimal places, making these types of trades more likely to match with TAQ

[11] After applying a similar filtering process, Collin-Dufresne and Fos (2015) identify 3,126 filings from their sample period spanning 1994 to 2010.

[12] For legal details on Schedule 13D filings, see `https://www.law.cornell.edu/cfr/text/17/240.13d-101`. Item 5(c) requires the reporting of trades conducted within 60 days prior to filing.

[13] The full example of 13D filings by GAMCO Investors can be found at `https://www.sec.gov/Archives/edgar/data/1164863/0000807249-18-000114.txt`

[14] An example of a filing that reports trades at the individual trade level can be found at `https://www.sec.gov/Archives/edgar/data/105744/0001742576-18-000014.txt`

data.

Due to the non-uniqueness of date, size, and price, some matches result in multiple potential pairings. To address this, we implement a series of matching methodologies, detailed in columns *TAQ Multiple Match*, *TAQ Simple Match*, *TAQ Unique Match*, and *TAQ Unique Match 5 trade* in Table 1. The numbers mentioned in the previous section (and shown the third column of Table 1) indicate the number of 13D trades for which CRSP identifiers are found, while the numbers from the filtering process (columns 4 to 7 in Table 1) represent the number of TAQ trades that match 13D trades. Given that we match 13D trades to TAQ trades based on price and size per stock-day, trades with round lot sizes frequently result in duplicated matches. Different matching processes aim to restrict these duplicates in various ways.

The first approach, referred to as the *TAQ Multiple Match*, allows mutual multiple matching, where each 13D trade can match with multiple TAQ trades and each TAQ trade can match with multiple 13D trades. This strategy significantly broadens the scope for matches, thereby increasing the number of observations to about 1.5 million TAQ trades. However, the number of filings drops to about 2,000 after we match the 13D trades with TAQ. Within this subset, 527 filings come from hedge funds containing 198,919 TAQ trades.

Building on this, the *TAQ Simple Match* allows multiple TAQ trades to be matched to a single 13D trade, but restricts each TAQ trade to match with only one 13D trade. This methodology is particularly useful for classifying the direction of 13D trades and distinguishing them as either limit or market orders. Because 13D filings typically indicate whether a transaction is a buy or sell, the trade direction is already specified. Therefore, restricting each TAQ trade to only one match with a 13D trade avoids the confusion of a single TAQ trade being flagged as both a buy and a sell. We further identify limit orders following Collin-Dufresne and Fos (2015) by comparing the 13D buy and sell flags with the trade direction from the Lee and Ready (1991) algorithm. A 13D trade classified as buy-initiated and reported as buy is likely a market order; if reported as sell, it is likely a limit order. From this filtering process, the number of 13D trades dramatically reduces to 510,275, although the decrease in the number of filings is comparatively minor. The significant drop in the number of 13D trades occurs primarily because most trades are executed in round lot sizes within the same filing on a given stock-day. In this version of matching, we identify 273,755 sell trades, which slightly exceeds the 236,520 buy trades. Additionally, a larger number of trades are classified as limit orders (262,734) compared to market orders (247,541). There are 490 hedge fund 13D filers, and their filings correspond to 126,762 matched TAQ trades.

A more stringent approach, *TAQ Unique Match*, narrows the scope further by limiting matches to only one-to-one pairings between 13D and TAQ trades. When applying this method, which prohibits any duplicates, the total number of trades falls to 49,070, yet the number of filings remains constant. This is because each filing typically has at least one unique trade matched. Among these matches, we identify 49,070 TAQ trades from 1,841 filings, with approximately 62% being buy trades, and half classified as limit orders. Notably, 8,349 of these trades are from 490 hedge fund 13D filers.

Lastly, considering that even aggregated transactions can yield unique matches, we refine our sample by focusing on 13D filings that report at least five transactions on a single day, following the methodology of Collin-Dufresne and Fos (2015). This refinement is based on the assumption that filers reporting five or more trades in one day are more likely to provide detailed trade-level information. Details on the most stringent criteria are provided under the *TAQ Unique Match 5 Trade*. Because this method only includes filings with at least five trades on a single day, the number of filings is reduced to approximately 1,100. However, the number of trades remains relatively unchanged, as the method excludes only those filings that report fewer than five trades per day. Similar to the *TAQ Unique Match*, approximately 60% of these trades are buys, and about half are classified as limit orders. Using this refined matching criteria, we have identified 7,722 trades from 134 hedge fund 13D filers.

Table 1 around here.

2.3. Illiquidity Measures

In this study, we employ several key measures to assess market illiquidity, each capturing a distinct aspect of market microstructure. These include the quoted spread, effective spread, price improvement, realized spread, and price impact. All measures are calculated relative to the National Best Bid and Offer (NBBO), which we construct following the filtering method of Holden and Jacobsen (2014). Specifically, we use quotes with positive bid and ask prices and sizes, require the ask price to be at least as high as the bid, and exclude quotes with non-normal quote conditions [15]. Observations with a quoted spread above $5 are also excluded. For trades, we require positive prices. Trades and quotes are then merged with no time lag. We restrict the sample to observations between 9:30 a.m. and 4:00 p.m.

The quoted spread is measured as the difference between the best ask and bid prices available in the market. For each trade at time t, the $QuotedSpread_t$, is calculated as:

$$\text{Quoted Spread}_t = \frac{(Ask_t - Bid_t)}{\text{Midpoint}_t} \tag{1}$$

where Ask_t and Bid_t are the best ask and bid prices at time t, and $MidPoint_t$ is the bid-ask midpoint of the quote.

The effective spread quantifies trading costs by measuring how closely the execution price aligns with the bid-ask midpoint, serving as a proxy for fair value at the time of the transaction. We measure effective spread for trade m at time t as:

$$\text{Effective Spread}_{m,t} = \frac{2 \times \text{BuySell}_m(\text{Price}_m - \text{Midpoint}_t)}{\text{Midpoint}_t} \tag{2}$$

where $Price_m$ is the execution price of the trade and $BuySell_m$ is the buy-sell indicator following Lee and Ready (1991).

Price improvement occurs when an order executes at a price more favorable than the quoted price at order submission. It is quantified as the difference between the quoted spread and the effective spread. A positive price improvement indicates that the execution price is closer to the bid-ask midpoint—the proxy for fair value—than either the bid or ask quotes. We measure the price improvement of trade m at time t as:

$$\text{Price Improvement}_t = \text{Quoted Spread}_t - \text{Effective Spread}_t. \tag{3}$$

Realized spread measures the profitability of liquidity providers by comparing the trade price to the bid-ask midpoint at a fixed interval after the trade, typically one minute. Follow-

[15] We exclude quotes with non-normal quote conditions A, B, H, O, R, and W in Daily Trade and Quote (DTAQ), and 4, 7, 9, 11, 13, 14, 15, 19, 20, 27, and 28 in Monthly Trade and Quote (MTAQ).

ing Conrad and Wahal (2020), we use the midpoint price one minute post-trade to capture most of the price impact. If an exact one-minute midpoint is unavailable, we allow some flexibility by considering midpoints within a surrounding window. To balance precision and practicality, we exclude observations where the lagged available midpoint is recorded less than 10 seconds or more than 5 minutes after the trade. For trade m at time t, the realized spread is measured as:

$$\text{Realized Spread}_{m,t} = \frac{2 \times \text{BuySell}_m(\text{Price}_m - \text{Midpoint}_{t+1\text{min}})}{\text{Midpoint}_t} \tag{4}$$

where $\text{Midpoint}_{t+1\text{min}}$ is the bid-ask midpoint one minute after the trade.

Price impact measures the change in the bid-ask midpoint caused by a trade, reflecting how much the execution moves the market price. It can signal the presence of informed trading. The price impact for trade m at time t is measured as:

$$\text{Price Impact}_{m,t} = \frac{2 \times \text{BuySell}_m(\text{Midpoint}_{t+1\text{min}} - \text{Midpoint}_t)}{\text{Midpoint}_t} \tag{5}$$

In addition to these illiquidity measures, following common practice in the literature, we also include trade size (number of shares) and dollar volume in our analysis as proxies for informed trading.

2.4. Summary Statistics

Once the illiquidity measures are computed at the trade level, we aggregate them into one-minute intervals separately for both 13D and non-13D trades. If at least one 13D trade occurs within a given one-minute interval, we create two separate aggregates: one for 13D trades and another for non-13D trades within that interval. Otherwise, only a single aggregate is recorded for non-13D trades. We then compute summary statistics for the illiquidity measures associated with both non-13D and Schedule 13D trades. Additionally, we distinguish 13D trades executed by hedge fund 13D filers to analyze differences within informed trading groups. The reported statistics include the mean, median, standard deviation, and the number of one-minute intervals for each measure. The details are presented in Table 2. For consistency and better interpretability, all illiquidity measures—except trade size (SIZE) and dollar volume (DVOL)—are scaled by 10,000, to express them in basis points.

Panel A of Table 2 presents descriptive statistics for non-13D trades, aggregated at one-minute intervals throughout the trading day. This panel serves as a benchmark, providing a reference point for comparing 13D-related trades against non-13D trades. On average, the quoted spread for non-13D trades is 20.93 basis points on days when 13D filers trade. These trades receive an average price improvement of 2.67 basis points, resulting in an effective spread of 18.26 basis points, which is lower than the quoted spread. The average price impact for non-13D trades is 7.36 basis points, while the realized spread is 10.89 basis points, which is the part of the spread that liquidity providers keep as profit. Regarding trade size and dollar volume, non-13D trades have an average size of approximately 585 shares, with an average dollar volume of about $16,000. The distribution of illiquidity measures is right-skewed, as indicated by relatively lower medians compared to means and high standard deviations.

Panel B focuses on 13D trades using a *multiple match* approach, where mutual multiple matching is allowed—each 13D trade can be matched with multiple TAQ trades, and each TAQ trade can be matched with multiple 13D trades. In this panel, 13D trades are compared to non-13D trades executed within the same one-minute intervals. This methodology allows for a more precise comparison of market conditions when both trade types occur within a similar time frame. Additionally, summary statistics for 13D trades executed by hedge fund 13D filers are reported separately. Overall, 13D trades tend to be associated with better liquidity than non-13D trades when compared within the same one-minute interval. For instance, the quoted spread for 13D trades is 16.79 basis points, slightly lower than the 17.26 basis points observed for non-13D trades. Although 13D traders receive less price improvement on average (0.15 basis points compared to 0.3 basis points for non-13D trades), they incur lower trading costs, as indicated by a lower effective spread of 16.64 basis points versus 16.96 basis points for non-13D trades. Liquidity providers earn less profit from executing 13D trades, as evidenced by the lower realized spread. However, 13D trades exhibit a higher price impact (9.75 basis points compared to 9.26 basis points for non-13D trades), suggesting a greater degree of adverse selection. Additionally, 13D trades tend to be executed in smaller sizes than non-13D trades. Hedge fund 13D trades show even better liquidity conditions than other 13D trades. However, their price impact is lower than both overall 13D and non-13D trades. This unexpected result will be analyzed in later sections.

Panel C further refines this comparison using a *simple match* approach, where each TAQ trade is restricted to match with only one 13D trade. The results from this matching method follow a similar pattern to those observed in the *multiple match* approach. In general, 13D trades are associated with better liquidity but have a higher price impact than non-13D trades. Hedge fund 13D trades, however, display a distinct pattern—better liquidity with lower adverse selection. While this section provides a brief comparison of illiquidity measures, the next section formally examines these differences across trader groups using a fixed-effects panel regression.

Table 2 around here.

3. Execution Quality

Standard models in market microstructure argue that informed investors increase adverse selection, leading to higher illiquidity and greater costs of capital,[16] but empirical evidence is scarce or even the other way around.[17] In the sample studied by Henry and Koski (2017), informed trading is profitable only when informed traders blend effectively with uninformed participants to achieve superior execution quality. In other words, informed traders gain an advantage by mimicking uninformed traders to secure favorable execution. Collin-Dufresne and Fos (2015) provides further evidence, demonstrating that illiquidity and adverse selection measures tend to be lower on days with 13D filings.

To test whether this holds at the trade level, we analyze whether 13D trades exhibit higher illiquidity and adverse selection costs using time-stamped trade data of 13D filings from 1994 to 2021, matched to TAQ trades. We first run fixed-effect panel regression explaining illiquidity by an indicator variable as below.

$$IliquidityMeasure_{i,s,d,t} = \beta_{13D} 13D_{i,s,d,t} + FE + \epsilon_{i,s,d,t} \tag{6}$$

where the variable $IliquidityMeasure_{i,s,d,t}$ represents a trade-level illiquidity measure, av-

[16] e.g., Glosten and Milgrom (1985), Kyle (1985) and Amihud and Mendelson (1986)

[17] e.g., see Collin-Dufresne and Fos (2015), Collin-Dufresne and Fos (2016), Goyal et al. (2025), Rösch (2021), and Roşu (2020).

eraged separately for 13D and non-13D trades within one-minute intervals. The measures include quoted spread (QSP), effective spread (ESP), price improvement ($PIMPRV$), realized spread (RSP), price impact ($PIMPCT$), size ($SIZE$), and dollar volume ($DVOL$). The indicator variable $13D$ equals 1 for trades under 13D filings and 0 otherwise.

The model includes fixed effects for intraday 30-minute intervals, dates, stocks, and stock-by-date interactions. Standard errors are clustered at the stock and date levels to account for serial correlation and heteroskedasticity. The sample used for this analysis is the "TAQ Multiple Match".

Table 3 compares the illiquidity measures of 13D trades against non-13D trades using one-minute intervals. Panel A includes all these intervals, even those containing only non-13D trades, which results in more observations. In contrast, Panel B focuses solely on intervals that contain both 13D and non-13D trades.

In Table 3, the number of observations represents the count of one-minute aggregated intervals, which differs from the number of trades reported in Table 1. Notably, the number of observations is substantially larger—significantly so in Panel A and nearly double in Panel B—compared to Table 2. This discrepancy arises because Table 3 includes all one-minute intervals associated with non-13D trades as well. Specifically, Panel A reports approximately 1.6 million observations, as it includes both one-minute intervals with 13D trades and all intervals throughout the trading day for non-13D trades. The number of observations in Panel B is approximately double the number of one-minute intervals that contain 13D trades, as it includes a matched non-13D trade for each 13D trade within the same interval.

The results in Panel A show that 13D trades are associated with significantly lower spreads. On average, the quoted spread (QSP) for 13D trades is 0.96 basis points lower than that of non-13D trades, with a t-statistic of -3.82. Similarly, Panel B show a QSP reduction of approximately 0.47 basis points (t-statistic: -5.52), along with a 0.32 basis point decrease in the effective spread (ESP) (t-statistic: -2.33) relative to other trades. This evidence aligns with recent literature suggesting lower illiquidity in the presence of informed trading, even at the individual trade level—contradicting predictions from standard market microstructure models.

On the other hand, our analysis reveals an increase in adverse selection at the trade level, contrasting with the findings of Collin-Dufresne and Fos (2015) at the daily level. Specifically, when 13D trades occur, the price impact (PIMPCT) shows a 0.71 basis point increase (t-statistic: 3.46) compared to non-13D trades throughout the trading day and a 0.49 basis point increase (t-statistic: 2.34) when compared to non-13D trades within the same one-minute interval. Notably, this suggests that at the trade level, adverse selection measures detect the presence of informed traders, highlighting the significance of evaluating adverse selection on a more granular scale.

In addition to the increased price impact, 13D trades exhibit price improvements (PIMPRV) that are approximately 0.56 basis points lower (t-statistic:-3.23) compared to other trades throughout the day. Similarly, the realized spread (RSP), commonly interpreted as the earnings of liquidity providers, is reduced by about 0.81 (t-statistic: -2.95) to 1.12 basis points (t-statistic: -3.6), depending on the time interval used for comparison.

Following the common practice in the literature of using trade size as a proxy for informed trading, we examine differences in the number of shares (SIZE) and the dollar volume (DVOL) traded between 13D and non-13D trades. When comparing 13D trades to all trades throughout the day, we do not observe a statistically significant difference. However, when comparing 13D trades to non-13D trades within the same one-minute interval, we find that 13D trades are smaller by approximately 150 shares and $4.5k in dollar volume. These findings highlight that, in recent years, trade size has become an unreliable proxy for identifying informed trading.

A fixed-effects panel regression using stricter matching criteria is presented in Appendix Table A.1 and Appendix Table A.2. Table A.1 uses the TAQ simple match, while Table A.2 uses the TAQ unique match. The results in these tables are consistent with Table 3, confirming the robustness of our findings. Notably, as the sample becomes more precise, measures of adverse selection increase for 13D trades. Stricter matching leads to lower price improvement and lower realized spreads, but higher price impact. For example, the price impact in Panel A of Table 3 is 0.71 bp with a t-statistics of 3.46; in simple match, it rises to 1.61 bp (t-statistics: 6.64). Under the unique match, the price impact further increases to 5.68 bp (t-statistics: 8.06). This pattern suggests that isolating the true 13D trades, rather than including multiple potential matches, amplifies the estimated impact. The robust results also suggest that allowing multiple matches is reliable. While each 13D trade has only one true match in the TAQ data, our process includes multiple potential matches. The other potential matches, however, effectively act as noise that averages out over the dataset. This design ensures that our results remain consistent, regardless of how strictly we define our sample selection criteria.

Table 3 around here.

While our adverse selection measures detect the presence of informed traders at the trade level, illiquidity measures are lower when 13D traders are active. Collin-Dufresne and Fos (2015) propose two primary mechanisms through which illiquidity may decrease in the presence of informed traders. The first mechanism suggests that 13D traders strategically time their trades, entering when liquidity is high. The second mechanism involves the use of limit orders by informed traders. We investigate whether these channels hold in our context.

The first mechanism highlights the idea that 13D traders may strategically avoid trading during periods of low market liquidity and high spreads. While the negative coefficients reported in Table 3 for the 13D trade indicator—when modeling both quoted and effective spreads—support the idea, it remains unclear whether informed traders choose to trade during periods of high liquidity or if their activity itself drives changes in spreads. To formally examine this causal relationship and mitigate potential endogeneity concerns, we implement a Vector Autoregression (VAR) model as outlined below.

$$\begin{aligned}\text{LHS}_{s,d,t} = &\sum_{l=1}^{5} \beta_{1,l}\text{QSP}_{s,t-l,d} + \sum_{l=1}^{5} \beta_{2,l}\text{13D}_{s,t-l,d} + \sum_{l=1}^{5} \beta_{3,l}\text{non13D}_{s,t-l,d} \\ &+ \sum_{l=1}^{5} \beta_{4,l}\text{RET}_{s,t-l,d} + \text{FE} \end{aligned} \tag{7}$$

where the quoted spread (QSP), 13D indicator ($13D$), non-13D indicator ($non13D$), and log return (RET) are endogenous variables, with RET derived from midpoint prices at one-minute intervals. The model includes five lags of all endogenous variables and is estimated as a panel with fixed effects for both stock-day and intra-day 30 minute interval. Each stock-day is treated as an independent time series, so lagged values do not carry over overnight. Observations at the beginning of each trading day are assigned missing values for lagged terms, ensuring that intra-day dynamics are not influenced by prior-day information. The subscripts s, d, and t denote stock, day, and time, respectively. The trade indicators separately measure the presence of $13D$ and $non13D$ trades within each one-minute interval. Both are set to zero in intervals without any trades, where only quotes are observed. The reported coefficients represent the cumulative sum of lagged effects. When the cumulative effect of $13D$ or $non13D$ explains the quoted spread or log return, we scale them by 10,000 to express the impact in basis points for better readability. The sample covers January 1994 to December 2021, using data from TAQ Multiple Match.

In dynamic panel regressions, lagged dependent variables appear as regressors. As such, standard errors could be biased, as noted by Arellano and Bond (1991), the estimation requires using the Generalized Method of Moments (GMM). However, Judson and Owen (1999) show that this concern is primarily relevant when the time dimension (T) is small (less than 30). Given that our VAR model includes over 2.1 million observations, the bias concern is largely mitigated, making GMM less necessary in this context.

Table 4 presents the results of the Granger causality test, evaluating whether 13D traders strategically select the timing of their trades. The positive coefficients for the cumulative sum of five lags of both 13D and non-13D trades, with QSP as the left-hand side variable, indicate that quoted spreads tend to increase following trade execution. Specifically, the cumulative effect of 13D trades on QSP is 2.48 basis points, while for non-13D trades it is 2.75 basis points. To formally assess whether these cumulative effects are statistically meaningful, we conduct Wald tests that evaluate whether the sum of the five lagged coefficients for each variable is significantly different from zero. The chi-squared statistics for the 13D and non-13D coefficients are 269.09 and 840.81, respectively, and both are highly significant. On the other hand, we find that when the quoted spread rises over the previous five minutes, both 13D and non-13D traders reduce their trading activity. This is reflected in the negative cumulative coefficients on QSP when using the 13D and non-13D trade indicators as the dependent variables: –0.65 (with chi-squared statistics of 13.31) and –2.40 (with chi-squared statistics of 72.69), respectively. Together, the results support the idea that both 13D and non-13D traders time their activity based on liquidity conditions, trading when spreads are low and holding back when spreads widen.

The presence of 13D trades leads to higher subsequent returns, a pattern not observed for non-13D trades. The cumulative sum of the lagged effects of 13D trades on log returns (RET) is about 0.48 basis points, while the corresponding effect for non-13D trades is statistically insignificant. This provides strong evidence of the superior informativeness of 13D trades compared to non-13D trades.

Since Granger causality tests examine only one equation at a time, they do not fully capture the joint dynamics of all variables. Impulse Response Functions (IRFs) provide a clearer picture by showing how shocks to one variable transmit through the entire set of variables over time. We thus construct IRFs to analyze the dynamic effects of informed trading on market conditions. Figure 1 illustrates the cumulative Generalized Impulse Response Function (GIRF) estimates derived from the panel VAR model. Each cell in the figure displays the impulse response to a one-standard-deviation shock applied to each endogenous variable, capturing the system's dynamic response to these shocks across a 20-period horizon. In this version, we do not yet include error bands for the GIRF due to computational limitations in the R package `panelvar`.

Consistent with the VAR results, we observe an increasing cumulative response in quoted spreads following a one-standard-deviation shock to both the 13D and non-13D trade indicators. Moreover, consistent with previous findings, cumulative returns following 13D trades rise and then stabilize, indicating a permanent price impact. In contrast, cumulative returns after non-13D trades increase within the first five minutes but subsequently flatten out and revert to zero, suggesting only transitory price pressures. This pattern supports the idea that 13D traders possess long-lived private information that leads to lasting price adjustments, whereas non-13D traders, being largely uninformed, do not generate permanent price changes.

Table 4 around here.

Figure 1 around here.

One concern about the results from Table 3 and Table 4 is that they may be misleading without distinguishing between different order types, particularly limit and market orders. We identify a significant number of limit orders in our sample (as shown in 1), and it requires a closer look. For example, when a trader uses a market order, a high quoted spread and high effective spread typically indicate higher transaction costs. In contrast, for a limit order, such high spreads may reflect a strategic decision to earn the spread rather than a cost incurred. By placing limit orders, informed traders can provide liquidity while potentially profiting from the bid-ask spread. Therefore, distinguishing trades by order type is important for accurately evaluating the impact of 13D traders on market liquidity.

This naturally leads to the second mechanism introduced by Collin-Dufresne and Fos (2015), which involves the use of limit orders by informed traders. Traditional theories link informed trading, short-selling, and arbitrage to increased illiquidity. However, under specific conditions, limit orders can reduce it. To understand if this mechanism works, we first examine how often 13D filers use limit orders.

To do so, we first identify limit orders, following the methodology provided by Collin-Dufresne and Fos (2015). A 13D trade is classified as a limit order by comparing the trade direction reported in the filing with the direction determined using the Lee and Ready (1991) algorithm. If the two directions align, the trade is classified as a market order; if they differ, it is classified as a limit order. Specifically, if a trade is identified as buy-initiated according to Lee and Ready (1991) and the 13D filing reports it as a buy, it is likely a market order. Conversely, if the filing reports it as a sell, it is likely a limit order. Notably, nearly 40% of 13D trades involve selling, which is unexpected given that only initial filings (not amendments) are considered. Consistent with Collin-Dufresne and Fos (2015), we find that approximately 50% of 13D-reported trades are executed as limit orders.

A potential limitation of this methodology is that it may lead to misclassification. For example, if a 13D filer submits a hidden limit buy order at or near the best ask price, it remains invisible in the order book, preventing other traders from reacting to it. When a sell order arrives, it executes against the hidden buy order at the designated price. Since the execution occurs near the ask price, above the midpoint, the Lee and Ready (1991) algorithm classifies it as buy-initiated. The 13D filing also reports it as a buy, leading our method to categorize it as a market order. However, in fact, the 13D filer submitted a limit order.

For tables that require precise identification of buy and sell trades, such as those distinguishing 13D trades by order type (Table 5) or trade direction (Table 6), we switch to the *TAQ Simple Match* sample. This choice is necessary to ensure that the trade direction is clearly defined for each TAQ trade. In the *TAQ Multiple Match*, where a single TAQ trade can be matched with multiple 13D trades, there is ambiguity in classifying whether a given TAQ trade represents a buy or a sell. This is because the matched 13D trades may have conflicting trade directions. By restricting each TAQ trade to match with only one 13D trade, the *TAQ Simple Match* eliminates this ambiguity.

To check if limit orders by informed traders indeed lowers the illiquidity measures, we conduct a fixed-effect panel regression extending the analysis from Table 3 by distinguishing between market and limit orders for 13D trades. The regression model includes a MKT_13D indicator, set to 1 for 13D trades executed as market orders and 0 for non-13D trades, and a $LIMIT\_13D$ indicator, set to 1 for 13D trades executed as limit orders and 0 for non-13D trades. The specification is given by:

$$\begin{aligned} IlliquidityMeasure_{i,s,d,t} = \beta_{MKT\_13D} MKT\_13D_{i,s,d,t} \\ + \beta_{LIMIT\_13D} LIMIT\_13D_{i,s,d,t} + FE + \epsilon_{i,s,d,t} \end{aligned} \tag{8}$$

where $IlliquidityMeasure_{i,s,d,t}$ denotes a illiquidity measure, calculated as the average across trades for stock s on day d within the 1-minute interval t, and estimated separately for 13D trades ($i = 1$) and non-13D trades ($i = 0$). We separately indicate MKT_13D and $LIMIT\_13D$ instead of using an interaction between the 13D indicator and order type because order type is unobservable for non-13D trades.

Table 5 shows that 13D limit orders are associated with higher quoted and effective spreads than 13D market orders, although both types of 13D orders generally correspond to lower spreads relative to non-13D trades. While the regressions compare each 13D order type to non-13D trades, the underlying goal is to contrast 13D market and limit orders. In Panel A (all-day comparison), 13D market orders are associated with a 1.17 basis point lower quoted spread (t-statistic: -4.05), a 0.86 basis point lower effective spread (t-statistic: -2.44), and no statistically significant difference in price improvement relative to non-13D trades. In contrast, 13D limit orders show a 0.56 basis point lower quoted spread (t-statistic: -2.21)—a smaller effect than for 13D market orders—an effective spread that is not statistically different, and a 1.00 basis point lower price improvement (t-statistic: -4.04). Panel B, which focuses on a within-interval comparison, shows a similar pattern.

On price impact and realized spread, 13D limit orders exhibit stronger effects than 13D market orders, although both are compared against non-13D trades. In Panel A, 13D limit orders are associated with a 1.65 basis point higher price impact (t-statistic: 5.30) and a 1.21 basis point lower realized spread (t-statistic: -2.85) relative to non-13D trades. 13D market orders, while not statistically different in terms of price impact, are associated with a 1.15 basis point lower realized spread (t-statistic: -3.60), but the magnitude is smaller than that of 13D limit orders. Panel B shows a similar pattern.

These findings show that 13D traders are more likely to submit limit orders when spreads are wide, reflecting their sensitivity to transaction costs. This allows them to capture larger spreads while potentially improving liquidity. It also highlights the endogenous nature of order choice, as informed traders balance execution quality against the risk of non-execution. Compared to 13D market orders, 13D limit orders exhibit stronger price impact and lower realized spreads. These patterns are consistent with studies suggesting that limit orders from informed traders may carry more informational content than market orders (e.g., Kaniel and Liu (2006)). By posting bid or ask prices, informed traders strategically enhance liquidity while capturing spreads.

Table 5 around here.

The classification of 13D trades into buy or sell categories is critical, as it substantially affects effective spreads, price improvements, realized spreads, and trade size. While the proportion of sell trades subtly varies depending on the matching approach used between 13D trades and TAQ data, they consistently represent a significant share of our sample. In the *TAQ Simple Match* version, approximately 37.6% of the trades are classified as sells, while in the *TAQ Unique Match*, this proportion is around 38%. To assess whether the buy-sell classification influences the relationship between 13D trades and illiquidity measures, we extend the analysis from Table 3 by running a fixed-effect panel regression that differentiates between buy and sell orders. The model includes a $13D$ indicator, a Buy indicator (equal to 1 for buy trades and 0 for sell trades), and an interaction term $13D \times Buy$. The regression is specified as follows:

$$IlliquidityMeasure_{i,s,d,t,b} = \beta_{13D}13D_{i,s,d,t,b} + \beta_{Buy}Buy_{i,s,d,t,b} + \beta_{13D\times Buy}(13D \times Buy)_{i,s,d,t,b} + FE + \epsilon_{i,s,d,t,b} \tag{9}$$

where $IlliquidityMeasure_{i,s,d,t}$ represents one of the illiquidity measures, calculated as the average of all trades for stock s on day d within the 1-minute interval t. These measures are computed separately for 13D trades ($i = 1$) and non-13D trades ($i = 0$), as well as for different trade directions b. For 13D trades, the order direction is determined based on the details provided in the 13D filing, whereas for non-13D trades, it is identified using the Lee and Ready (1991) algorithm. This table extends the classifications in Table 3 and Table 5 by further distinguishing each 1-minute interval (t) based on 13D versus non-13D trades, as well as buy and sell directions. For 13D trades, the buy or sell indicator reflects the actual direction reported in the filing, while for non-13D trades, it reflects trade initiation (i.e., buyer- or seller-initiated).

Table 6 presents the results of the analysis. We find no significant difference in quoted spreads (QSP) between 13D buy and 13D sell trades. This outcome is expected, as we previously noted that bid and ask prices are set before 13D traders decide to trade. However, once trades are executed, notable differences arise across other key illiquidity measures, particularly when comparing 13D buy and sell trades to non-13D buy and sell trades within the same time intervals (Panel B).

For instance, 13D buy trades exhibit much higher effective spreads (ESP) than 13D sell trades, with a 2.3 basis point increase. The ESP for 13D buy trades is even higher than that of non-13D trades, as evidenced by the change in the slope direction when we sum the coefficients from the 13D indicator and the interaction term $13D \times Buy$, resulting in a positive slope. The elevated ESP for 13D buy trades is primarily driven by lower price improvement. Specifically, 13D buy trades experience 2.47 basis points less price improvement compared to 13D sell trades. Furthermore, the high effective spread for 13D buy trades translates into increased revenues for liquidity providers, as shown by a 2.12 basis point increase in the realized spread. On the other hand, the price impact of 13D buy trades is not statistically significantly different from that of 13D sell trades.

Table 6 around here.

4. Returns and Execution Quality

Next, we analyze cross-sectional differences among 13D traders, particularly distinguishing hedge funds from other activist investors. Hedge funds are widely regarded as sophisticated investors, distinguished by unique attributes that set them apart from other institutional investors. They operate under strong profit incentives—earning performance fees on excess returns in addition to fixed management fees—and benefit from a lighter regulatory burden, which grants them the flexibility to take large, concentrated stakes without facing typical diversification constraints. Prior studies provide evidence supporting hedge funds' superior trading ability. Kosowski, Naik, and Teo (2007) and Jagannathan et al. (2010) document their persistent outperformance, while Agarwal, Jiang, Tang, and Yang (2013) find significant abnormal returns associated with hedge funds' confidential 13F filings. Ha, Hu, and Tang (2024) highlight hedge funds' informational advantage, showing that their pre-event trades correlate with higher cumulative abnormal returns. Hedge funds are adept at processing both fundamental and non-fundamental news — an edge not observed in non-hedge fund institutions (Huang, Tan, and Wermers (2020)) — and may benefit from early access to information (Bolandnazar, Jackson, Jiang, and Mitts (2020)) or strategic media collaborations (Ljungqvist and Qian (2016)).

Similar to Table 3, we run fixed-effect panel regression that differentiates 13D filers into hedge fund ($HF13D$) and non- hedge funds NON_HF13D. Note that since all hedge fund trades in the sample are 13D trades, the 13D trades are further classified into non-hedge fund 13D trades NON_HF13D and hedge fund 13D trades(HF13D). The regression model is specifed as below:

$$IlliquidityMeasure_{i,s,d,t} = \beta_{Non\_HF13D} Non\_HF13D_{i,s,d,t} + \beta_{HF13D} 13D\_HF_{i,s,d,t} + FE + \epsilon_{i,s,d,t} \tag{10}$$

where $IlliquidityMeasure_{i,s,d,t}$ represents a liquidity measure, computed as the average across all trades for stock s on day d within the 1-minute interval t. This measure is calculated separately for 13D trades ($i = 1$) and non-13D trades ($i = 0$).

Table 7 presents the regression results. Panel A of the table presents a comparison of non-hedge fund (non-HF) 13D and hedge fund (HF) 13D trades against non-13D trades throughout the trading day. The results from Panel A are consistent with our previous findings in Table 3. For both non-hedge fund and hedge fund 13D trades, we observe an increased price impact of 0.66 basis points for non-hedge funds and 0.91 basis points for hedge funds relative to non-13D trades. Both types of 13D filers receive less price improvement compared to non-13D traders (-0.58 bps for non-hedge funds and -0.5 bps for hedge fund 13D filers). Additionally, the realized spread is lower relative to non-13D trades, with a reduction of -1.23 basis points for non-hedge funds and -0.72 basis points for hedge funds.

However, when comparing non-hedge fund 13D and hedge fund 13D trades to non-13D trades executed at the same time, as shown in Panel B, these differences disappear. For HF 13D trades, most illiquidity measures are not significantly different from those of non-13D trades. The only notable exception is that hedge fund 13D traders tend to trade under more favorable liquidity conditions. This is reflected in a 0.22 basis point lower quoted spread (QSP) relative to non-13D trades. Other adverse selection measures—price impact, realized spread, and price improvement—exhibit marginal differences, both statistically and economically. Hedge fund 13D traders seem indistinguishable from non-13D traders, possibly suggesting an attempt to conceal their informed trading.

Meanwhile, non-HF 13D traders remain distinguishable from non-13D traders. They trade when liquidity conditions are more favorable, as indicated by lower illiquidity measures—a pattern consistent with earlier findings in Table 3 and Table 4. At the same time, they exhibit higher adverse selection. Non-HF 13D trades show a 0.66 basis point higher price impact compared to non-13D trades. Their realized spread is 1.04 basis points lower.

Table 7 around here.

However, it is premature to conclude that hedge funds are inherently sophisticated and highly skilled at concealing their trades among non-13D traders. An alternative explanation for why they appear indistinguishable is that they may simply lack superior information, making their trades less noticeable. To explore this possibility, we next examine whether hedge funds were, in fact, informed. Specifically, we analyze the abnormal returns associated with 13D filings. While our previous analysis focused on individual trades, we now shift our perspective by aggregating trades at the filing level.

To do this, we construct an abnormal return measure—the value-weighted average abnormal return (VWAAR)—for 13D filers. As defined in Equation (11), VWAAR for each filing f, t days after the filing date is calculated as the value-weighted average of abnormal returns from individual 13D trades i. Abnormal returns, in this context, are measured as the difference between a 13D trade i's holding period return and the corresponding market return at time t. The holding period return for a 13D trade is determined based on the price paid at the time of the trade and the closing price of the respective stock for each day following the filing date.

$$VWAAR_{f,t} = \frac{\sum_{i \in f} V_i \cdot (HPR_{i,t} - MR_t)}{\sum_{i \in f} V_i} \tag{11}$$

where V_i represents the dollar volume of individual 13D trades i, $HPR_{i,t}$ is the holding period return at time t for the trade i, and MR_t is the market return at time t.

To ensure that VWAAR accurately reflects the value-weighted holding period return net of market returns across all trades within a filing, we exclude transactions that could distort the measure. Specifically, we remove sell trades, option-like trades (i.e., those executed outside the bid-ask spread), and stocks that were delisted within 20 days of the filing date. Conceptually, this approach treats all trades within a filing as part of a single "order" or, as Chan and Lakonishok (1995) described, a "package" of trades executed by the filer.

Table 8 presents the average VWAAR across all 13D filings for each day following the filing date. The results are categorized into three groups: all 13D filers (full sample), hedge fund filers, and non-hedge fund filers. On the filing date (Day 0), the full sample exhibits a mean return of 0.50%, with hedge fund filers earning a higher return of 1.10%, compared to 0.26% for non-hedge fund filers. VWAARs continue to rise in the following days, reaching a peak of 4.06% around Day 20. Hedge fund filers consistently exhibit higher abnormal returns than non-hedge fund filers, suggesting that hedge fund 13D filers are generally more informed than their non-hedge fund counterparts. These findings are robust to using raw returns, instead of abnormal returns, as documented in Table A.4.

Taken together, this evidence effectively rules out the alternative explanation that hedge funds are uninformed and therefore indistinguishable from non-13D traders. Instead, the findings suggest that hedge funds trade strategically to avoid noticeable impacts on liquidity and adverse selection while still earning higher returns than other institutional investors who file 13D disclosures.

Our findings are consistent with the literature on 13D returns, particularly Figure 2 of Collin-Dufresne and Fos (2015) and in Figure 1 of Brav et al. (2008a), which show that stock prices remain stable before 13D filings but rise significantly afterward. This pattern suggests that 13D traders act on information about potential improvements in firm performance, which is more closely related to expectations about future cash flows rather than short-term mispricing.

The return pattern observed in 13D filings differs from that associated with corporate insiders who file Form-4 disclosures, representing another type of informed trading. Research by Inci et al. (2010) shows that Form-4 filers typically purchase shares following price declines, with stock prices rebounding immediately after their trades. This pattern suggests that Form-4 traders primarily act on information related to discount rate mispricing—exploiting temporary undervaluation relative to fundamental value—rather than identifying firms with unrealized growth opportunities

These differences highlight how 13D and Form-4 traders rely on distinct types of information. While both discount rates and cash flow expectations influence stock prices (e.g., Bordalo, Gennaioli, Porta, and Shleifer (2024)), the timing and persistence of returns suggest that 13D traders anticipate long-term firm improvements, whereas Form-4 traders exploit short-term pricing anomalies.

Table 8 around here.

To formally examine whether informed traders can hide their presence among uninformed participants, we investigate the relationship between execution quality and profitability. If informed traders successfully disguise their trades, they may avoid detection by market participants who adjust quotes in response to informed trading activity. Execution quality plays a critical role in this process, as it determines how a trade impacts the market and whether it signals information to liquidity providers.

Informed traders typically face a trade-off between execution quality and profitability. On one hand, improving execution quality—by using strategies that minimize market impact, such as limit orders—allows them to blend in with uninformed traders and avoid detection. On the other hand, prioritizing execution quality may come at the cost of delayed or incomplete execution, limiting their ability to fully capitalize on their informational advantage. If informed traders prioritize rapid execution to exploit private information before it decays, they may accept lower execution quality and incur higher trading costs.

By examining how execution quality relates to profitability, we can determine whether high-profitability traders tend to minimize detection or whether their success is linked to more aggressive trading behavior. If they exhibit lower execution quality, it suggests they prioritize immediacy, sacrificing execution efficiency to exploit their informational edge. Conversely, if they achieve high execution quality without sacrificing profitability, this would imply they can effectively hide among uninformed participants. Given this trade-off, in general, we expect traders with higher profitability to exhibit lower execution quality, as execution quality is less critical for them relative to the size of their alpha.

However, this trade-off may not hold for hedge funds (HFs). Our findings suggest that HFs are not only adept at concealing their trading behavior but also possess superior information. This is evidenced by the fact that their illiquidity and adverse selection measures do not significantly differ from those of non-13D traders, yet they achieve higher abnormal returns than 13D filers who are not hedge funds. This pattern indicates that HFs may employ trading strategies that allow them to maintain execution quality without sacrificing profitability, effectively bypassing the typical trade-off observed among other informed traders. Therefore, when examining the relationship between execution quality and profitability, we expect to observe either no significant difference or a positive correlation within this group.

To empirically assess the impact of execution quality on profitability, we classify 13D filings based on execution quality (high vs. low) and fund type (full sample, non-hedge funds, and hedge funds). We then track their subsequent profitability, comparing trade prices to end-of-day prices on the announcement day, as well as five and ten days afterward. This approach allows us to determine whether execution quality plays a meaningful role in shaping trading outcomes and whether hedge funds, in particular, are able to sustain high profitability without compromising execution quality.

We employ two complementary measures to evaluate execution quality: the Effective Spread (ESP) and the percentile trader skill ranking proposed by Anand, Irvine, Puckett, and Venkataraman (2012), which we refer to as AIPV. The first measure, ESP, directly reflects the effective spread of execution, capturing how closely a trade executes relative to the fair value (FV). Lower effective spreads indicate better execution quality, as trades occur closer to the prevailing market price. The second measure, $AIPV(ESP)$, follows the percentile trader skill ranking methodology proposed by Anand et al. (2012). It captures the alpha derived from a regression where execution shortfall—proxied by the effective spread in this analysis—is regressed on key economic determinants of trading costs. Specifically, the regression includes stock return volatility, market return volatility, a buy indicator (equal to one for buy orders), previous-day order imbalance, short-term price trend (proxied by the prior day's return), the inverse of stock price, and interaction terms between order imbalance and buy orders, as well as price trend and buy orders. Filer-specific fixed effects account for systematic differences across institutional traders. This approach isolates the component of abnormal trading costs attributable to execution skill, beyond what is explained by market conditions and trade characteristics.

In typical trading environments, execution quality and execution skill may be distinct concepts, as execution costs can vary due to differences in adverse selection rather than traders' execution decisions. However, in our setting, this distinction is less pronounced. Since our sample consists exclusively of informed traders, they all face comparable adverse selection costs, minimizing the variation in execution quality that arises purely from differences in information asymmetry.

Given this, AIPV refines our assessment by isolating execution-related cost reductions that are not driven by broader market conditions or trade characteristics. While ESP captures execution quality directly, it does not adjust for factors like liquidity conditions, stock volatility, and order imbalances, which also influence execution costs. AIPV controls for these determinants, allowing us to better distinguish execution quality that stems from strategic execution decisions rather than external market forces. By using both measures, we ensure a comprehensive assessment of execution quality that accounts for both raw execution costs and their economic context.

Table 9 presents the results. Panel A reports findings for the full sample, which includes about 2,000 filings. The results indicate that announcement-day returns do not exhibit statistically significant differences between groups with high and low execution quality, regardless of the measure used. However, when examining long-term profitability ten days after the filing, traders with higher execution quality tend to generate lower profits—by approximately 1.05% or 1.19%—with t-statistics of 1.65 and 1.86, respectively, depending on the measure applied. This suggests that among 13D filers, those who prioritize execution quality may do so at the expense of profitability. Since execution quality and execution skill are closely related in our context, this finding aligns with the idea that traders who optimize execution costs may, in turn, limit their ability to capitalize fully on their informational advantage. Conversely, traders who accept higher execution costs—potentially prioritizing immediacy—achieve higher long-term profitability, suggesting a trade-off between execution efficiency and profit maximization.

When distinguishing between hedge funds and non-hedge funds, the pattern shifts. As shown in Panel B, informed traders outside of hedge funds appear to face a strong trade-off between execution quality and profitability. Those with higher execution quality exhibit significantly lower abnormal returns, ranging from 1.1% to 1.67% five days after the filing date and from 2.08% to 2.47% ten days after, depending on the measure used. This result suggests that among non-hedge funds, traders who prioritize good execution tend to earn lower returns, reinforcing the idea that execution-focused traders may sacrifice profitability in order to manage market impact and reduce detection risk.

Focusing on hedge funds, Panel C reveals a different pattern: execution quality does not have a statistically significant impact on profitability. In fact, hedge funds with higher execution quality tend to achieve higher profits—exceeding those of the lower execution quality group by more than 1%—though the difference is not statistically significant. This finding supports our hypothesis that hedge funds possess a unique ability to blend in with uninformed traders, allowing them to avoid the typical trade-off between execution quality and profitability. Unlike non-hedge fund 13D traders, who prioritize immediacy to capitalize on their private information, hedge fund 13D trades reflect a more balanced approach between execution efficiency and profitability.

These results highlight the strategic advantage that hedge funds may hold over non-hedge fund 13D traders. Rather than facing a strict trade-off between execution quality and profitability, hedge funds appear to leverage their ability to both manage execution efficiently and generate high abnormal returns. This reinforces the idea that their superior performance is not solely a result of execution but also of how effectively they conceal their trading intentions while capitalizing on their private information.

Table 9 around here.

Building on our previous analysis of execution quality and profitability, we now examine how the trade-off between execution quality and profitability varies between patient and impatient trades. For investors filing Schedule 13D, a key regulatory requirement is to submit the filing to the SEC within ten days after crossing the 5% ownership threshold, referred to as the event date. This requirement is mandated by Rule 13d-1(a) of the 1934 Securities Exchange Act.[18] The day they officially file is the filing date. This timeline naturally distinguishes between patient and impatient trades.

Following the methodology outlined by Bogousslavsky et al. (2024), we define patient trades as those executed at least 20 days before the filing date, meaning they occur well before the event date. These trades allow for greater flexibility in optimizing execution quality without urgency constraints. In contrast, impatient trades are those executed within the 20 days leading up to the filing date, when the impending disclosure deadline creates pressure to act quickly. As a result, impatient trades may prioritize capturing profits from private information over minimizing transaction costs, whereas patient trades have more room to manage execution quality carefully.

Table 10 provides supporting evidence for this trade-off. It presents the profitability of 13D trades (VWAAR) by execution quality group, distinguishing between high- and low-execution-quality traders, as well as patient and impatient trades. The overall structure

[18] As previously noted, this rule has been changed. The SEC updated Schedule 13D regulations and reduced the initial filing deadline from 10 calendar days to 5 business days after investors acquire more than 5% beneficial ownership. This change took effect on February 5, 2024. Our sample period precedes this amendment, so we apply the 10-day deadline in our analysis.

follows that of Table 9. Execution quality is measured using two approaches: effective spread (ESP) and AIPV (ESP). The results are reported across three time intervals—filing date (Day 0), five days after filing, and ten days after filing.

Panel A of the table shows a clear pattern among impatient trades: those with lower execution quality achieve significantly higher profits, with abnormal returns exceeding those of high-execution-quality traders across all three time intervals. On the filing date, low-execution-quality traders earn modest yet positive abnormal returns, while high-execution-quality traders, on average, generate negative returns. Five days after filing, the profitability gap widens, with low-execution-quality traders earning between 2.97% and 3.10%, compared to only 0.60% to 0.84% for high-execution-quality traders. By ten days after filing, the difference increases further, ranging from 2.85 to 3.77 percentage points, depending on the execution quality measure used. This effect is both economically meaningful and statistically significant, as indicated by t-statistics exceeding -3.8 in magnitude. These findings reinforce the presence of a strong trade-off between execution quality and profitability. Impatient traders appear willing to to take higher transaction costs in order to quickly capitalize on their private information before the 13D filing deadline.

Panel B of the table presents the results for patient trades, showing a different pattern. For these trades, execution quality does not appear to play a decisive role in profitability. Although low-execution-quality patient trades achieve slightly higher returns than their high-execution-quality counterparts, the differences are not statistically significant. Since patient traders have ample time to manage execution quality, they do not face the same urgency-driven trade-off observed in impatient trading. These results suggest that the trade-off between execution quality and profitability depends largely on time constraints, becoming more pronounced as traders approach disclosure deadlines.

Our findings from Table 9 and Table 10 reveal a clear trade-off between execution quality and profitability among Schedule 13D filers, though its impact varies by fund type and time sensitivity. Non-hedge funds face a strong negative relationship between execution quality and profitability, suggesting that prioritizing execution efficiency limits their ability to capitalize on private information. In contrast, hedge funds appear to mitigate this trade-off, maintaining high returns regardless of execution quality, likely due to strategies that help them blend in with uninformed traders while still effectively exploiting their informational advantage. Time constraints further shape this relationship, as execution quality becomes more critical when immediacy is required. Impatient traders, operating under disclosure deadlines, exhibit a pronounced trade-off, with lower execution quality linked to significantly higher profits. In contrast, patient traders, who have more time to optimize execution, do not face the same constraint.

Table 10 around here.

5. Conclusions

Our study provides uncovers casual evidence that informed traders influence market liquidity, thereby addressing a long-standing challenge in financial economics research. While previous studies rely on daily-level data aggregation or broad trade-based proxies (such as size), we analyze informed trading at trade level using high frequency data. This approach allows us to uncover how informed traders operate in real time and detect the immediate influence of their trades on liquidity and uncover.

Prior studies focusing on lower-frequency liquidity measures find a positive association between the presence of informed traders and market liquidity. Indeed, we confirm this evidence and further document that informed traders attempt to hide their presence by endogenously electing to trade when liquidity is high and by placing limit orders when bid-ask spreads are wide. This evidence suggests that informed traders are not merely liquidity takers, but also, for example by placing limit orders, capture large spreads while influencing liquidity subtly.

Our study refines how informed trading is identified. Specifically, we document that traditional adverse selection measures are effective and react to informed trades when used at high frequencies. Because we match informed trades by 13D filers with timestamped TAQ data, we are able to capture changes in liquidity caused by these trades precisely at the time they occur.

We also confirm that 13D trades generate positive and increasing cumulative abnormal returns after the filing date, reinforcing the idea that activists strategically trade based on expectations of future price improvements rather than temporary mispricing. Hedge fund

13D traders, in particular, achieve positive abnormal returns while remaining indistinguishable from uninformed traders. They do so without even electing to trade when liquidity is favorable, as the average 13D trader does. We thus document heterogeneous trading execution among the informed. Non-hedge fund 13D traders, on the one hand, attempt to hide, but adverse selection measures identify them at high frequencies. Yet they possess superior information and generate returns in the long run. On the other hand, hedge fund 13D traders do not attempt to trade under favorable conditions, yet do not trigger the adverse selection measures and, via this superior execution quality, generate abnormal returns. These findings suggest a fine balance between execution quality and profitability, which hedge funds can overcome via more sophisticated execution strategies.

While our study is important as it documents that high frequency liquidity measures respond to informed trading by the average 13D filer, it also points out a limitation of our study as these measures do not react to trading by a few hedge funds filing the 13D forms. While we show that high-frequency liquidity measures uncover trading patterns previously missed by daily-level research, we also point out that some traders - hedge funds - show a level of sophistication transcending these patterns. This invites more research into adverse selection measures better suited to capture the presence of hedge funds and, in general, more research linking the areas of hedge funds and market microstructure.

Table 1
Summary Statistics

The table presents descriptive statistics summarizing the trading and filing activities of Schedule 13D filers from 1994 to 2021. It details the filtering process of data, starting from raw counts reported in the filings to refined subsets based on specific criteria, including CUSIP matching, restrictions to common stocks priced between $1 and $1,000, and unique trade identification. Trade identification is conducted by matching 13D-reported trades with TAQ data, using four distinct methods. The *TAQ multiple match* approach allows both 13D trades to match multiple TAQ trades and TAQ trades to match multiple 13D trades. The *TAQ simple match* method restricts this by allowing 13D trades to match multiple TAQ trades but not vice versa. The *TAQ unique match* further limits to trades with a one-to-one match. Finally, the *TAQ unique match 5 trade* additionally limits the dataset to 13D filings with at least five transactions on a given day, following the methodology of Collin-Dufresne and Fos (2015). This filtering assumes filers with at least five daily transactions are more likely to report trade-level data, reducing the inclusion of aggregated transactions. Limit and market orders are classified based on whether the trading direction in 13D filings matches the direction inferred from trades using the Lee and Ready (1991) method. Aligned directions indicate a market order; misaligned directions indicate a limit order. Hedge fund filers' activities are highlighted separately for further analysis.

	Raw	CUSIP Matched	Common Stock ($1 - $1000)	TAQ Multiple Match	TAQ Simple Match	TAQ Unique Match	TAQ Unique Match 5 trade
13D Trade	863,773	798,619	504,843	1,474,101	510,275	49,070	46,934
13D Filings	8,945	7,802	4,132	2,031	1,841	1,841	711
13D Buy Trade	613,676	564,933	369,061	-	236,520	30,532	28,559
13D Sell Trade	250,097	233,686	135,782	-	273,755	18,538	18,375
Limit Order	-	-	-	-	262,734	24,908	23,778
Market Order	-	-	-	-	247,541	24,162	23,156
HF 13D Trade	144,835	131,555	85,351	198,919	126,762	8,349	7,722
HF 13D Filings	2,554	2,215	1,280	527	490	490	134

Table 2
Summary Statistics: Illiquidity Measures

This table presents descriptive statistics on illiquidity measures for non-13D and Schedule 13D trades, including the quoted spread (QSP), effective spread (ESP), price improvement ($PIMPRV$), realized spread (RSP), price impact ($PIMPCT$), trade size ($SIZE$), and dollar volume ($DVOL$). Illiquidity measures are scaled by 10,000 for interpretability, except $SIZE$ and $DVOL$, which retain their original units. Each panel reports the mean, median, standard deviation, and number of 1-minute aggregated intervals ($N.1min$) for non-13D trades, 13D trades, and hedge fund filers. Panel A presents summary statistics for non-13D trades using illiquidity measures pre-aggregated at one-minute intervals. Panels B and C apply two trade-matching methods: Multiple Match (Panel B) and Simple Match (Panel C), reporting statistics using the pre-aggregated measures as well.

	QSP	ESP	PIMPRV	RSP	PIMPCT	SIZE	DVOL
Panel A: Non-13D Trades Throughout a Day							
Mean	20.93	18.26	2.67	10.89	7.36	585.36	16,091.73
Median	8.24	6.95	0.00	3.81	0.00	169.70	4,425.58
Std.Dev	56.18	58.92	48.44	59.82	36.97	4,895.88	146,683.42
N.1min	1,410,884	1,410,884	1,410,882	1,410,882	1,410,882	1,410,884	1,410,884
Panel B: Illiquidity Measures: Multiple Match							
Non-13D Trades: 13D-Interval							
Mean	17.26	16.96	0.30	7.70	9.26	594.54	14,066.99
Median	8.67	8.30	0.00	4.03	0.67	225.95	4,997.50
Std.Dev	37.28	38.80	23.44	43.31	36.59	5,206.34	129,256.79
N.1min	176,633	176,633	176,633	176,633	176,633	176,633	176,633
13D Trades							
Mean	16.79	16.64	0.15	6.89	9.75	452.14	9,556.01
Median	8.60	8.35	0.00	4.03	0.52	100.00	2,704.00
Std.Dev	34.62	36.55	22.29	48.87	45.76	7,041.86	79,935.13
N.1min	176,633	176,633	176,633	176,633	176,633	176,633	176,633
Hedge Fund 13D Trades							
Mean	11.08	11.22	-0.14	4.91	6.31	566.26	13,296.53
Median	4.99	4.62	0.00	2.88	0.00	100.00	5,641.00
Std.Dev	27.66	31.26	19.53	42.58	39.73	8,992.76	112,762.45
N.1min	36,709	36,709	36,709	36,709	36,709	36,709	36,709
Panel C: Illiquidity Measures: Simple Match							
Non-13D Trades: 13D-Interval							
Mean	16.94	16.67	0.27	7.46	9.21	593.65	14,447.60
Median	8.35	7.99	0.00	3.97	0.75	225.00	5,166.90
Std.Dev	36.56	38.26	23.27	42.77	36.37	5,280.08	132,234.75
N.1min	166,678	166,678	166,678	166,678	166,678	166,678	166,678
13D Trades							
Mean	16.47	16.38	0.09	6.65	9.73	436.66	9,541.13
Median	8.31	8.10	0.00	3.96	0.74	100.00	2,767.00
Std.Dev	33.89	35.83	21.77	48.16	44.82	6,368.75	77,560.65
N.1min	166,678	166,678	166,678	166,678	166,678	166,678	166,678
Hedge Fund 13D Trades							
Mean	10.83	10.98	-0.15	4.10	6.87	623.36	14,220.88
Median	4.82	4.37	0.00	2.64	0.79	100.00	6,075.75
Std.Dev	28.18	32.17	20.64	42.91	41.72	9,896.67	110,017.67
N.1min	29,009	29,009	29,009	29,009	29,009	29,009	29,009

Table 3
Panel Regression of Illiquidity Measures on 13D Indicator with Stock-Day-Time Fixed Effects

This table reports the results from stock-day-time fixed-effect panel regressions to estimate the impact of 13D-reported trades on various illiquidity measures The regression model is specified as: $IlliquidityMeasure_{i,s,d,t} = \beta_{13D} 13D_{i,s,d,t} + FE + \epsilon_{i,s,d,t}$ where $IlliquidityMeasure_{i,s,d,t}$ denotes one of the illiquidity measures, estimated as the average of all trades for stock s on day d within the 1-minute interval t, calculated separately for 13D trades ($i = 1$) and non-13D trades ($i = 0$). The illiquidity measures include quoted spread (QSP), effective spread (ESP), price improvement ($PIMPRV$), realized spread (RSP), price impact ($PIMPCT$), size ($SIZE$), and dollar volume ($DVOL$). The indicator variable $13D$ equals 1 for trades reported under 13D filings and 0 otherwise. The model includes fixed effects for intraday 30-minute intervals, dates, stocks, and stock-by-date interactions to control for unobserved heterogeneity at various levels. Standard errors are clustered by stock and date to account for serial correlation and heteroskedasticity. The sample spans January 1994, to December 2021, with data aggregated at 1-minute intervals. The data used in this analysis is sourced from TAQ multiple match. Coefficients for $13D$ measure its marginal impact on illiquidity measures relative to the baseline and are scaled by 10,000 to improve interpretability, except for SIZE and DVOL, which retain their original units. Panel A compares 13D trades to all trades within the same stock-day, while Panel B focuses on trades occurring in the same 1-minute intervals. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and * respectively. t-statistics are in parentheses below the coefficients.

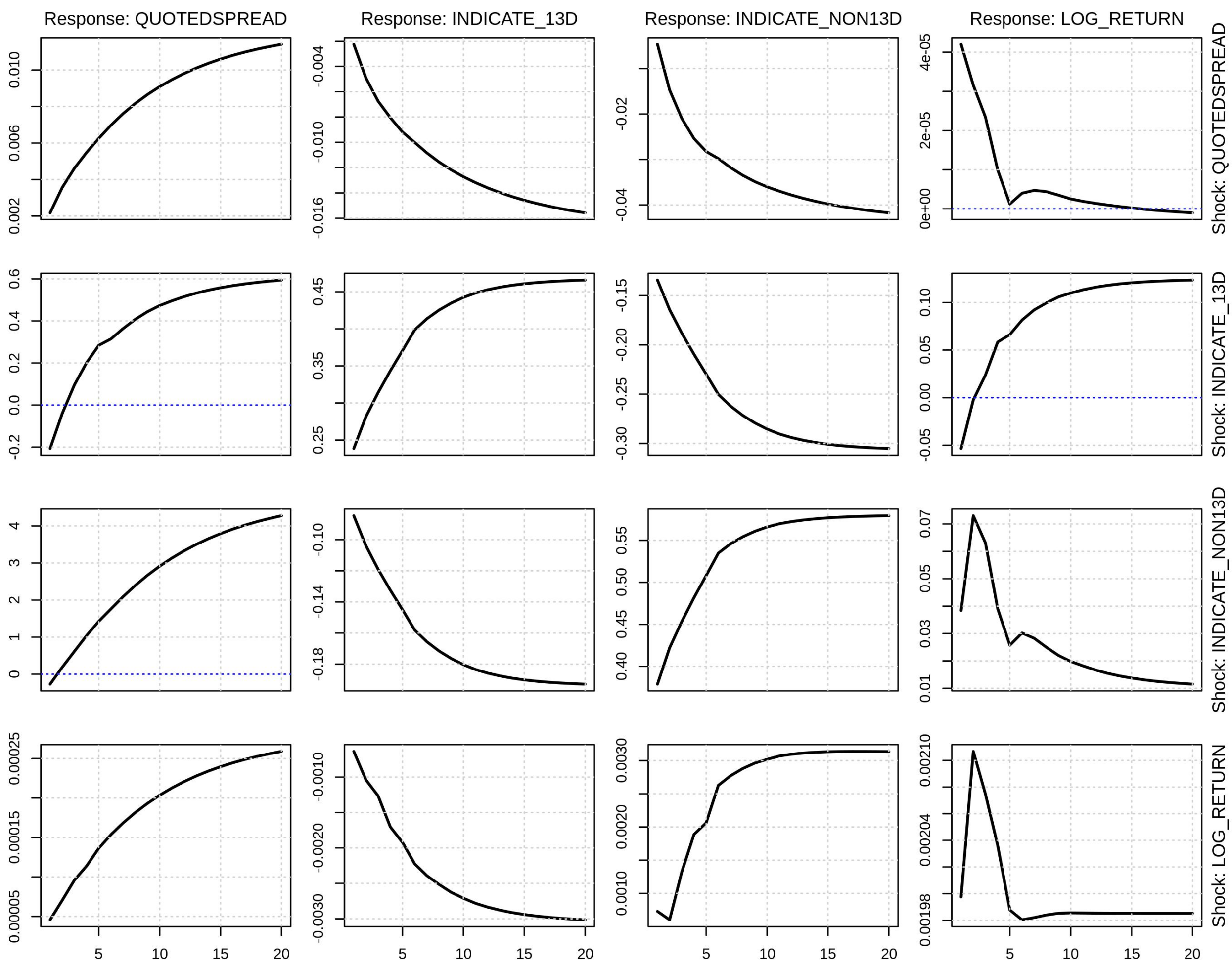
Variable	QSP	ESP	PIMPRV	RSP	PIMPCT	SIZE	DVOL
Panel A: 13D vs trades throughout the day							
$13D$	-0.96***	-0.40	-0.56***	-1.12***	0.71***	42.33	-1296.08
	(-3.82)	(-1.38)	(-3.23)	(-3.60)	(3.46)	(0.77)	(-1.26)
Adjusted-R2	0.56	-0.00	-0.01	-0.00	0.13	0.03	0.05
N	1,666,251	1,666,251	1,666,251	1,666,251	1,666,251	1,666,251	1,666,251
StockDayTimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: 13D vs trades within the same interval							
$13D$	-0.47***	-0.32**	-0.15	-0.81***	0.49**	-142.40***	-4510.98***
	(-5.52)	(-2.33)	(-1.23)	(-2.95)	(2.34)	(-3.21)	(-4.35)
Adjusted-R2	0.77	0.65	0.28	0.29	0.25	0.32	0.12
N	353,266	353,266	353,266	353,266	353,266	353,266	353,266
StockDayTimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 4
Intraday Vector Autoregressions and Granger Causality

This table presents results from panel vector autoregression VAR models that estimate the dynamic relationships among quoted spread QSP, 13D indicator $13D$, non-13D indicator $non13D$, and log return RET as endogenous variables. RET is calculated using midpoint prices at one-minute intervals. Each model includes five lags of all endogenous variables, structured as a panel with stock-day and time fixed effects. The regression model is specified as: $\text{LHS}_{s,d,t} = \sum_{l=1}^{5} \beta_{1,l}\text{QSP}_{s,t-l,d} + \sum_{l=1}^{5} \beta_{2,l}13\text{D}_{s,t-l,d} + \sum_{l=1}^{5} \beta_{3,l}\text{non13D}_{s,t-l,d} + \sum_{l=1}^{5} \beta_{4,l}\text{RET}_{s,t-l,d} + \text{FE}$. Here, the subscripts s, d, and t denote stock, day, and time, respectively. The endogenous variables are calculated over 1-minute intervals. To account for intervals without trades, where only quotes are available, the 13D trade indicators are divided into $13D$ and $non13D$ categories. The reported coefficients represent the cumulative sum of all lagged coefficients for each endogenous variable. Chi-squared statistics based on a Wald test are provided in parentheses to test the null hypothesis that the cumulative sum of all lagged coefficients is zero, indicating no cumulative effect. Coefficients are scaled by 10,000 in cases where the sum of coefficients from the five lags of either the $13D$ or $non-13D$ indicator explains the quoted spread or log return. The sample spans January 1994, to December 2021, and the data for this analysis is sourced from TAQ Multiple Match. Statistical significance is denoted by ***, **, and * for the 1%, 5%, and 10% significance levels, respectively.

Variable	QSP	13D	non13D	RET
$\sum_{l=1}^{5} QSP_{i,t-l,d}$	0.82***	-0.65***	-2.40***	-0.00**
	(261299.15)	(13.31)	(72.69)	(5.54)
$\sum_{l=1}^{5} 13D_{i,t-l,d}$	2.48***	0.47***	-0.18***	0.48***
	(269.09)	(80155.26)	(4606.53)	(11.88)
$\sum_{l=1}^{5} non13D_{i,t-l,d}$	2.75***	-0.03***	0.29***	0.14
	(840.81)	(885.93)	(30629.21)	(2.59)
$\sum_{l=1}^{5} RET_{i,t-l,d}$	0.00	-0.36**	0.81***	-0.01***
	(1.16)	(4.17)	(8.44)	(14.81)
Adjusted-R2	0.92	0.45	0.39	0.01
N	2,118,430.00	2,118,430.00	2,118,430.00	2,118,430.00
Fixed Effects	$Stock*Day+Time$	$Stock*Day+Time$	$Stock*Day+Time$	$Stock*Day+Time$

Figure 1. Cumulative Generalized Impulse Response Functions (GIRFs) from a Panel VAR Model



This figure shows cumulative Generalized Impulse Response Functions (GIRFs) estimated from a panel VAR model, as detailed in Table 7. The GIRFs correspond to 1-standard-deviation shocks applied to each variable ($QUOTEDSPREAD$, $INDICATE\_13D$, $INDICATE\_NON13D$, and LOG_RETURN), showing the system's dynamic responses to these shocks across a 20-period horizon. Each row represents the impact of a shock to a specific variable, while the columns display the corresponding responses of the target variables. Coefficients are scaled by 10,000 in cases where the impulse originates from either the $13D$ or $NON13D$ indicator and the response variables are the quoted spread or log return.

Table 5
Panel Regression of Illiquidity Measures on 13D Indicator and Limit Order Indicator with Stock-Day-Time Fixed Effects

This table extends the analysis in Table 3 by distinguishing between market and limit orders for 13D-reported trades. The regression model includes a MKT_13D indicator, set to 1 for 13D trades executed as market orders and 0 for non-13D trades, and a $LIMIT\_13D$ indicator, set to 1 for 13D trades executed as limit orders and 0 for non-13D trades. The regression model is specified as: $IlliquidityMeasure_{i,s,d,t} = \beta_{MKT\_13D} MKT\_13D_{i,s,d,t} + \beta_{LIMIT\_13D} LIMIT\_13D_{i,s,d,t} + FE + \epsilon_{i,s,d,t}$ where $IlliquidityMeasure_{i,s,d,t}$ denotes one of the illiquidity measures, estimated as the average of all trades for stock s on day d within the 1-minute interval t, calculated separately for 13D trades ($i = 1$) and non-13D trades ($i = 0$). The classification of $LimitOrder$ is determined by comparing the order direction reported in the filing with the direction identified using the Lee and Ready (1991) algorithm. A 13D trade is classified as a market order if the two directions align, and as a limit order if the two directions are opposite. For non-13D trades, we assume all transactions are executed as market orders. Since all trades classified as limit orders are 13D trades, the 13D trades are further classified into market 13D trades (MKT_13D) and limit 13D trades (LIMIT_13D). Methodology and scaling are consistent with Table 2. Specifically, all coefficients are scaled by 10,000, except those associated with SIZE and DVOL. Standard errors are clustered by stock and date to address serial correlation and heteroskedasticity. The sample spans January 1994 to December 2021, with data aggregated at 1-minute intervals, sourced from TAQ simple match. Panel A compares 13D trades (market and limit orders) to all trades within the same stock-day, while Panel B focuses on trades occurring in the same 1-minute intervals. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and * respectively. t-statistics are in parentheses below the coefficients.

Variable	QSP	ESP	PIMPRV	RSP	PIMPCT	SIZE	DVOL
Panel A: 13D vs trades throughout the day							
MKT_13D	-1.17***	-0.86**	-0.31	-1.15***	0.29	-70.65	-2690.51***
	(-4.05)	(-2.44)	(-1.59)	(-3.60)	(1.31)	(-1.50)	(-2.64)
$LIMIT\_13D$	-0.56**	0.44	-1.00***	-1.21***	1.65***	186.85**	-92.79
	(-2.21)	(1.43)	(-4.04)	(-2.85)	(5.30)	(2.46)	(-0.08)
Adjusted-R2	0.55	-0.00	-0.01	-0.00	0.13	0.03	0.05
N	1,526,657	1,526,657	1,526,657	1,526,657	1,526,657	1,526,657	1,526,657
StockDayTimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: 13D vs trades within the same interval							
MKT_13D	-0.54***	-0.53***	-0.01	-0.72***	0.19	-216.60***	-5700.45***
	(-5.61)	(-3.21)	(-0.04)	(-2.59)	(0.93)	(-5.31)	(-5.33)
$LIMIT\_13D$	-0.33***	0.16	-0.49***	-0.94**	1.09***	-56.20	-3730.67***
	(-2.70)	(0.90)	(-2.79)	(-2.49)	(3.35)	(-1.02)	(-3.22)
Adjusted-R2	0.75	0.63	0.27	0.28	0.24	0.29	0.11
N	334,668	334,668	334,668	334,668	334,668	334,668	334,668
StockDayTimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 6
Panel Regression of Illiquidity Measures on 13D Indicator and Buy Indicator with Stock-Day-Time Fixed Effects

This table extends the analysis from Table 3 by differentiating between buy and sell orders for 13D-reported trades. In addition to the $13D$ indicator, the model incorporates a Buy indicator set to 1 for buy trades and 0 for sell trades, along with the interaction term $13D \times Buy$. The regression model is specified as: $IlliquidityMeasure_{i,s,d,t,b} = \beta_{13D}13D_{i,s,d,t,b} + \beta_{Buy}Buy_{i,s,d,t,b} + \beta_{13D\times Buy}(13D \times Buy)_{i,s,d,t,b} + FE + \epsilon_{i,s,d,t,b}$ where $IlliquidityMeasure_{i,s,d,t}$ denotes one of the illiquidity measures, estimated as the average of all trades for stock s on day d within the 1-minute interval t, calculated separately for 13D trades ($i = 1$) and non-13D trades ($i = 0$), as well as for trade directions b. For 13D trades, order direction is classified based on the details provided in the filing. For non-13D trades, it is determined using the Lee and Ready (1991) algorithm. Unlike Tables 2 and 3, this table further segments each 1-minute time unit (t) by both 13D versus non-13D trades and buy versus sell trades. This additional categorization increases the number of observations compared to the earlier tables. All coefficients are scaled by 10,000, except those associated with SIZE and DVOL. The sample spans January 1994 to December 2021, with data aggregated at 1-minute intervals and sourced from TAQ simple match. Panel A compares 13D trades (buy and sell orders) to all trades within the same stock-day, while Panel B focuses on trades occurring in the same 1-minute intervals. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and * respectively. t-statistics are in parentheses below the coefficients.

Variable	QSP	ESP	PIMPRV	RSP	PIMPCT	SIZE	DVOL
Panel A: 13D vs trades throughout the day							
$13D$	-0.55**	-0.63	0.08	-2.51**	1.88***	-144.85***	-4679.28***
	(-2.31)	(-0.64)	(0.08)	(-2.40)	(6.32)	(-5.10)	(-4.37)
Buy	0.11	-0.99	1.10	-0.58	-0.41***	-43.28***	-880.63**
	(1.27)	(-0.52)	(0.58)	(-0.31)	(-2.97)	(-3.35)	(-2.04)
$13D \times Buy$	-0.19	1.68	-1.87	0.70	0.99**	554.60***	9860.19***
	(-0.74)	(0.85)	(-0.95)	(0.35)	(2.57)	(5.67)	(5.34)
Adjusted-R2	0.51	-0.00	-0.00	-0.00	0.07	0.02	0.03
N	2,082,519	2,082,519	2,082,519	2,082,519	2,082,519	2,082,519	2,082,519
StockDayTimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: 13D vs trades within the same interval							
$13D$	-0.30**	-1.18***	0.88***	-2.74***	1.56***	-235.11***	-7912.23***
	(-2.35)	(-5.87)	(6.54)	(-6.30)	(4.36)	(-8.15)	(-5.92)
Buy	-0.06	-2.16***	2.11***	-2.24***	0.08	-12.20	-1366.05*
	(-0.46)	(-7.45)	(8.23)	(-4.18)	(0.17)	(-0.42)	(-1.69)
$13D \times Buy$	-0.17	2.30***	-2.47***	2.12***	0.18	199.48***	5832.90***
	(-0.99)	(6.88)	(-7.48)	(3.98)	(0.42)	(2.79)	(3.35)
Adjusted-R2	0.68	0.60	0.20	0.15	0.09	0.15	0.07
N	408,411	408,411	408,411	408,411	408,411	408,411	408,411
StockDayTimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 7
Panel Regression of Illiquidity Measures on 13D Indicator and 13D Hedge Fund Indicator with Stock-Day-Time Fixed Effects

This table builds on the analysis from Table 2 by categorizing 13D filers into hedge funds and non-hedge funds. In addition to the $13D$ indicator, the model incorporates a $HF13D$ indicator, set to 1 for 13D-reported trades filed by hedge funds. The regression model is specified as: $IlliquidityMeasure_{i,s,d,t} = \beta_{NON\_HF13D} NON\_HF13D_{i,s,d,t} + \beta_{HF13D} 13D\_HF_{i,s,d,t} + FE + \epsilon_{i,s,d,t}$ where $IlliquidityMeasure_{i,s,d,t}$ denotes one of the liquidity measures, estimated as the average of all trades for stock s on day d within the 1-minute interval t, calculated separately for 13D trades ($i = 1$) and non-13D trades ($i = 0$). Since all hedge fund trades in the sample are 13D trades, the 13D trades are further classified into non-hedge fund 13D trades NON_HF13D and hedge fund 13D trades(HF13D). All coefficients are scaled by 10,000, except those associated with SIZE and DVOL. The sample spans January 1994 to December 2021, with data aggregated at 1-minute intervals and sourced from TAQ multiple match. Panel A compares 13D trades (buy and sell orders) to all trades within the same stock-day, while Panel B focuses on trades occurring in the same 1-minute intervals. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and * respectively. t-statistics are in parentheses below the coefficients.

Variable	QSP	ESP	PIMPRV	RSP	PIMPCT	SIZE	DVOL
Panel A: 13D vs trades throughout the day							
NON_HF13D	-1.14***	-0.56	-0.58***	-1.23***	0.66***	4.38	-458.23
	(-3.66)	(-1.58)	(-2.66)	(-3.21)	(2.65)	(0.07)	(-0.41)
$HF13D$	-0.30	0.20	-0.50***	-0.72**	0.91***	182.77	-4407.78*
	(-1.40)	(0.66)	(-2.87)	(-2.03)	(2.86)	(1.49)	(-1.77)
Adjusted-R2	0.56	-0.00	-0.01	-0.00	0.13	0.03	0.05
N	1,665,921	1,665,921	1,665,921	1,665,921	1,665,921	1,665,921	1,665,921
StockDayTimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: 13D vs trades within the same interval							
NON_HF13D	-0.53***	-0.37**	-0.16	-1.04***	0.66**	-172.79***	-3449.61***
	(-5.28)	(-2.24)	(-1.06)	(-3.06)	(2.53)	(-3.63)	(-3.34)
$HF13D$	-0.22*	-0.11	-0.11	0.04	-0.15	-27.92	-8524.79***
	(-1.76)	(-0.72)	(-0.97)	(0.16)	(-0.81)	(-0.27)	(-2.80)
Adjusted-R2	0.77	0.65	0.28	0.29	0.25	0.32	0.12
N	353,286	353,286	353,286	353,286	353,286	353,286	353,286
StockDayTimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 8
Value-Weighted Average Abnormal Returns (VWAAR) of 13D Filers: Post-Filing Comparison Across Full Sample, Hedge Funds, and Non-Hedge Funds

The table summarizes the mean value-weighted average abnormal return (VWAAR) for 13D filers, calculated for each day after the filing date. The VWAAR for each filing is the value-weighted average of abnormal returns from individual 13D trades, where abnormal returns are defined as the difference between a 13D trade's holding period return and the corresponding market return (see equation (11)). Daily average VWAARs are computed by averaging across all 13D filings, with returns expressed as arithmetic values. The analysis uses a sample of 13D trades classified as common stock, priced between $1 and $1,000. It is restricted to trades executed on stock-days with multiple TAQ matches, excluding sell trades, option-like trades (executed outside the bid-ask spread), and stocks delisted within 20 days of the filing date. Results are further grouped into three categories: all 13D filers, hedge fund filers, and non-hedge fund filers.

Days After Filing	Full Sample			Hedge Fund			Non HF		
	Ret	Median	n_obs	Ret	Median	n_obs	Ret	Median	n_obs
0	0.0050	-0.0050	1638	0.0110	-0.0031	466	0.0026	-0.0053	1172
1	0.0083	-0.0009	1638	0.0131	0.0006	466	0.0064	-0.0019	1172
2	0.0119	0.0021	1638	0.0158	0.0020	466	0.0104	0.0021	1172
3	0.0161	0.0044	1638	0.0199	0.0061	466	0.0147	0.0030	1172
4	0.0181	0.0059	1638	0.0221	0.0062	466	0.0165	0.0059	1172
5	0.0216	0.0080	1638	0.0255	0.0109	466	0.0201	0.0068	1172
6	0.0231	0.0093	1638	0.0280	0.0101	466	0.0212	0.0091	1172
7	0.0268	0.0118	1638	0.0294	0.0143	466	0.0258	0.0109	1172
8	0.0329	0.0150	1638	0.0357	0.0182	466	0.0317	0.0142	1172
9	0.0360	0.0180	1638	0.0396	0.0238	466	0.0345	0.0167	1172
10	0.0371	0.0196	1638	0.0412	0.0216	466	0.0354	0.0188	1172
11	0.0373	0.0208	1638	0.0408	0.0256	466	0.0359	0.0194	1172
12	0.0398	0.0230	1638	0.0439	0.0251	466	0.0381	0.0207	1172
13	0.0391	0.0192	1638	0.0430	0.0179	466	0.0375	0.0205	1172
14	0.0387	0.0191	1638	0.0407	0.0168	466	0.0379	0.0198	1172
15	0.0380	0.0202	1638	0.0391	0.0164	466	0.0376	0.0218	1172
16	0.0391	0.0211	1638	0.0390	0.0149	466	0.0391	0.0230	1172
17	0.0383	0.0164	1638	0.0383	0.0141	466	0.0384	0.0171	1172
18	0.0385	0.0172	1638	0.0382	0.0146	466	0.0386	0.0194	1172
19	0.0397	0.0183	1638	0.0415	0.0167	466	0.0390	0.0200	1172
20	0.0406	0.0192	1638	0.0410	0.0193	466	0.0404	0.0192	1172

Table 9
Profitability and Execution Quality of 13D Trades: A Comparison Across Fund Types and Skill Levels

This table summarizes the profitability of 13D trades by execution quality groups (high vs. low) and fund types (full sample, non-hedge funds, and hedge funds) across three time intervals: Day 0 (filing day), Day +5, and Day +10. Execution quality is evaluated using two measures. The first measure, ESP, directly reflects the effective spread of execution. The second measure, $AIPV(ESP)$, is based on the percentile trader skill ranking proposed by Anand et al. (2012). It captures execution quality adjustments after controlling for key economic determinants of trading costs, allowing for a refined assessment of execution cost reductions. Schedule 13D filers are divided into top and bottom halves based on these execution quality measures. Their value-weighted average abnormal return (VWAAR), as previously described (see Equation (11)), is then computed and compared across groups. Panel A reports results for the full sample, highlighting differences in profitability between high-execution-quality and low-execution-quality groups (High - Low). Panel B focuses on non-hedge funds, while Panel C isolates hedge funds, maintaining the same structure for comparison. Profitability is winsorized at the 1% and 99% levels. Returns are expressed in percentage terms.

		Filing Date + 0 Days		+ 5 Days		+ 10 Days	
Execution Quality	**N**	Profit	*t*-Statistics	Profit	*t*-Statistics	Profit	*t*-Statistics
Panel A: Full Sample							
ESP							
Execution Quality = Low	913	0.44%	(1.31)	2.48%	(6.08)	4.13%	(8.21)
Execution Quality = High	916	0.27%	(0.96)	1.55%	(4.77)	2.95%	(7.50)
High – Low		-0.17%	(-0.39)	-0.93%	(-1.78)	-1.19%	(-1.86)
AIPV (ESP)							
Execution Quality = Low	906	0.23%	(0.71)	2.26%	(5.61)	4.07%	(8.09)
Execution Quality = High	923	0.47%	(1.62)	1.78%	(5.33)	3.02%	(7.63)
High – Low		0.23%	(0.54)	-0.48%	(-0.92)	-1.05%	(-1.65)
Panel B: Non-Hedge Funds							
ESP							
Execution Quality = Low	674	0.40%	(1.01)	2.76%	(5.70)	4.67%	(7.88)
Execution Quality = High	674	0.04%	(0.13)	1.09%	(3.09)	2.20%	(4.98)
High – Low		-0.36%	(-0.74)	-1.67%	(-2.79)	-2.47%	(-3.34)
AIPV (ESP)							
Execution Quality = Low	674	0.16%	(0.42)	2.48%	(5.20)	4.48%	(7.61)
Execution Quality = High	674	0.28%	(0.91)	1.37%	(3.78)	2.40%	(5.34)
High – Low		0.12%	(0.23)	-1.10%	(-1.84)	-2.08%	(-2.81)
Panel C: Hedge Funds							
ESP							
Execution Quality = Low	239	0.56%	(0.89)	1.67%	(2.23)	2.92%	(3.04)
Execution Quality = High	242	0.89%	(1.32)	2.83%	(3.85)	4.63%	(5.77)
High – Low		0.34%	(0.36)	1.16%	(1.10)	1.70%	(1.36)
AIPV (ESP)							
Execution Quality = Low	240	0.83%	(1.28)	1.94%	(2.50)	3.16%	(3.25)
Execution Quality = High	241	0.63%	(0.95)	2.57%	(3.60)	4.39%	(5.56)
High – Low		-0.20%	(-0.22)	0.63%	(0.60)	1.23%	(0.98)

Table 10
Profitability of 13D Trades by Execution Quality and Trade Type Across Time Intervals

This table summarizes the profitability of 13D trades categorized by execution quality groups (high vs. low) and trade types (patient vs. impatient) across three time intervals: Day 0 (filing day), Day +5, and Day +10. Patient trades are defined as those executed 20 days or more before the filing date, while impatient trades refer to those executed closer to the filing date, following the methodology outlined by Bogousslavsky et al. (2024). Execution quality is evaluated using two measures: ESP, which directly reflects the effective spread of execution, and $AIPV(ESP)$, which adjusts for economic determinants of trading costs to capture execution-related cost reductions. Both measures follow the same methodology and calculation as in Table 9. Schedule 13D filers are divided into top and bottom halves based on these execution quality measures, and their VWAAR is calculated and compared across groups. Panel A reports results for impatient trades, showing the differences in profitability between high- and low-execution-quality groups (High - Low). Panel B presents results for patient trades, using the same structure. Profitability is winsorized at the 1% and 99% levels. Returns are expressed in percentage terms.

		Filing Date + 0 Days		+ 5 Days		+ 10 Days	
Execution Quality	**N**	Profit	t-Statistics	Profit	t-Statistics	Profit	t-Statistics
Panel A: Impatient Trades							
ESP							
Execution Quality = Low	529	0.51%	(1.46)	3.10%	(6.47)	5.08%	(8.22)
Execution Quality = High	563	-0.32%	(-1.66)	0.60%	(2.37)	1.31%	(4.19)
High – Low		-0.83%	(-2.08)	-2.50%	(-4.61)	-3.77%	(-5.45)
AIPV (ESP)							
Execution Quality = Low	503	0.45%	(1.24)	2.97%	(6.09)	4.70%	(7.70)
Execution Quality = High	589	-0.23%	(-1.18)	0.84%	(3.09)	1.85%	(4.98)
High – Low		-0.67%	(-1.65)	-2.13%	(-3.82)	-2.85%	(-3.99)
Panel B: Patient Trades							
ESP							
Execution Quality = Low	509	1.09%	(1.78)	2.94%	(4.28)	4.37%	(5.91)
Execution Quality = High	509	0.50%	(0.97)	1.69%	(2.90)	2.64%	(4.04)
High – Low		-0.59%	(-0.74)	-1.25%	(-1.39)	-1.73%	(-1.75)
AIPV (ESP)							
Execution Quality = Low	506	1.05%	(1.71)	2.93%	(4.30)	4.31%	(5.90)
Execution Quality = High	512	0.55%	(1.06)	1.71%	(2.89)	2.72%	(4.07)
High – Low		-0.50%	(-0.63)	-1.22%	(-1.36)	-1.59%	(-1.60)

Appendix A.1. Robustness Tests

Table A.1
Panel Regression of Illiquidity on 13D Trades: Stock-Day-Time Fixed Effects using TAQ Simple Match

This table reports the results from stock-day-time fixed-effect panel regressions to estimate the impact of 13D-reported trades on various Illiquidity measures The regression model is specified as: $IlliquidityMeasure_{i,s,d,t} = \beta_{13D} 13D_{i,s,d,t} + FE + \epsilon_{i,s,d,t}$ where $IlliquidityMeasure_{i,s,d,t}$ represents one of the illiquidity measures for stock s on day d, calculated over 1-minute intraday intervals t with i indicating whether the 1-minute interval includes 13D trades (1) or not (0). The illiquidity measures include quoted spread (QSP), effective spread (ESP), price improvement ($PIMPRV$), realized spread (RSP), price impact ($PIMPCT$), size ($SIZE$), and dollar volume ($DVOL$). The indicator variable $13D$ equals 1 for trades reported under 13D filings and 0 otherwise. The model includes fixed effects for intraday 30-minute intervals, dates, stocks, and stock-by-date interactions to control for unobserved heterogeneity at various levels. Standard errors are clustered by stock and date to account for serial correlation and heteroskedasticity. The sample spans January 1994, to December 2021, with data aggregated at 1-minute intervals. The data used in this analysis is sourced from TAQ simple match. Coefficients for $13D$ measure its marginal impact on illiquidity measures relative to the baseline and are scaled by 10,000 to improve interpretability, except for SIZE and DVOL, which retain their original units. Panel A compares 13D trades to all trades within the same stock-day, while Panel B focuses on trades occurring in the same 1-minute intervals. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and * respectively. t-statistics are in parentheses below the coefficients.

Variable	QSP	ESP	PIMPRV	RSP	PIMPCT	SIZE	DVOL
Panel A: 13D vs trades throughout the day							
$13D$	-0.63***	0.11	-0.74***	-1.50***	1.61***	152.73**	52.86
	(-2.58)	(0.39)	(-3.86)	(-4.07)	(6.64)	(2.32)	(0.04)
Adjusted-R2	0.54	-0.00	-0.01	-0.01	0.13	0.03	0.05
N	1,428,920	1,428,920	1,428,920	1,428,920	1,428,920	1,428,920	1,428,920
StockDayTimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: 13D vs trades within the same interval							
$13D$	-0.43***	-0.29*	-0.14	-0.70**	0.41*	-41.91	-3297.43***
	(-4.76)	(-1.82)	(-0.94)	(-2.32)	(1.87)	(-0.78)	(-2.62)
Adjusted-R2	0.80	0.67	0.32	0.32	0.28	0.31	0.12
N	232,710	232,710	232,710	232,710	232,710	232,710	232,710
StockDayTimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.2
Panel Regression of Illiquidity on 13D Trades: Stock-Day-Time Fixed Effects using TAQ Unique Match

This table reports the results from stock-day-time fixed-effect panel regressions to estimate the impact of 13D-reported trades on various illiquidity measures The regression model is specified as: $IlliquidityMeasure_{i,s,d,t} = \beta_{13D}13D_{i,s,d,t} + FE + \epsilon_{i,s,d,t}$ where $IlliquidityMeasure_{i,s,d,t}$ denotes one of the illiquidity measures, estimated as the average of all trades for stock s on day d within the 1-minute interval t, calculated separately for 13D trades ($i = 1$) and non-13D trades ($i = 0$). The illiquidity measures include quoted spread (QSP), effective spread (ESP), price improvement ($PIMPRV$), realized spread (RSP), price impact ($PIMPCT$), size ($SIZE$), and dollar volume ($DVOL$). The indicator variable $13D$ equals 1 for trades reported under 13D filings and 0 otherwise. The model includes fixed effects for intraday 30-minute intervals, dates, stocks, and stock-by-date interactions to control for unobserved heterogeneity at various levels. Standard errors are clustered by stock and date to account for serial correlation and heteroskedasticity. The sample spans January 1994, to December 2021, with data aggregated at 1-minute intervals. The data used in this analysis is sourced from TAQ unique match. Coefficients for $13D$ measure its marginal impact on illiquidity measures relative to the baseline and are scaled by 10,000 to improve interpretability, except for SIZE and DVOL, which retain their original units. Panel A compares 13D trades to all trades within the same stock-day, while Panel B focuses on trades occurring in the same 1-minute intervals. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and * respectively. t-statistics are in parentheses below the coefficients.

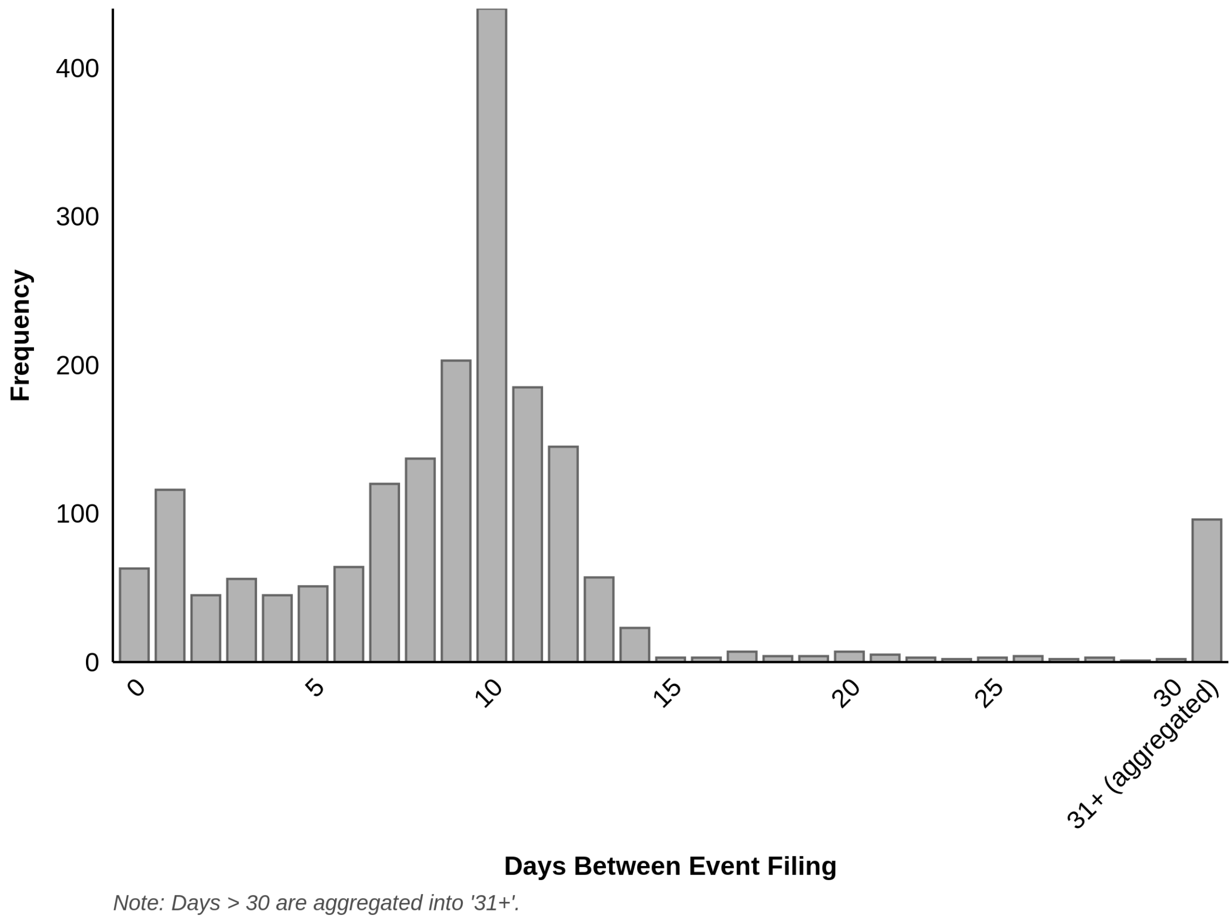
Variable	QSP	ESP	PIMPRV	RSP	PIMPCT	SIZE	DVOL
Panel A: 13D vs trades throughout the day							
$13D$	-0.20	1.94**	-2.14***	-3.74***	5.68***	1142.21***	15109.09***
	(-0.51)	(2.55)	(-3.31)	(-4.07)	(8.06)	(5.53)	(5.01)
Adjusted-R2	0.53	-0.00	-0.01	-0.01	0.13	0.03	0.05
N	1,307,053	1,307,053	1,307,053	1,307,053	1,307,053	1,307,053	1,307,053
StockDayTimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: 13D vs trades within the same interval							
$13D$	-1.35***	-0.33	-1.02**	-1.55*	1.22*	783.30***	10526.93***
	(-4.51)	(-0.68)	(-2.23)	(-1.79)	(1.92)	(4.18)	(3.66)
Adjusted-R2	0.80	0.72	0.40	0.29	0.26	0.35	0.24
N	51,768	51,768	51,768	51,768	51,768	51,768	51,768
StockDayTimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.3
Panel Regression of Liquidity Measures on 13D Indicator, Buy Indicator, and Limit Order Indicator with Stock-Day-Time Fixed Effects

This table integrates the analyses from Tables 3 and 4 by differentiating not only between buy and sell orders for 13D-reported trades but also between market and limit orders. The model incorporates three key indicator variables - $13D$ indicator, Buy indicator, and $LimitOrder$ - along with their interaction terms. The regression model is specified as: $LiquidityMeasure_{i,s,d,t,b} = \beta_{13D}13D_{i,s,d,t,b} + \beta_{Buy}Buy_{i,s,d,t,b} + \beta_{LimitOrder}LimitOrder_{i,s,d,t,b} + \beta_{13D\times Buy}(13D \times Buy)_{i,s,d,t,b} + \beta_{Buy\times LimitOrder}(Buy\times LimitOrder)_{i,s,d,t,b} + FE + \epsilon_{i,s,d,t,b}$ where $LiquidityMeasure_{i,s,d,t,b}$ represents one of the liquidity measures for stock s on day d, calculated over 1-minute intraday intervals t with b indicating whether the interval aggregates buy trades or sell trades, and i indicating whether the interval includes 13D trades (1) or not (0). Order directions and limit orders are identified following the methodology described in Table 3 and 4. Since all limit orders are 13D trades, the $13D$ and $LimitOrder$indicators are perfectly collinear their interaction terms are excluded to avoid multicollinearity. All coefficients are scaled by 10,000, except those associated with SIZE and DVOL. The sample spans January 1994 to December 2021, with data aggregated at 1-minute intervals and sourced from TAQ simple match. Panel A compares 13D trades (buy and sell orders) to all trades within the same stock-day, while Panel B focuses on trades occurring in the same 1-minute intervals. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and * respectively. t-statistics are in parentheses below the coefficients.

Variable	QSP	ESP	PIMPRV	RSP	PIMPCT	SIZE	DVOL
Panel A: 13D vs trades throughout the day							
$13D$	-0.67**	-0.73	0.06	-2.45**	1.72***	-23.46	-2503.22**
	(-2.52)	(-0.75)	(0.06)	(-2.41)	(5.90)	(-0.45)	(-2.20)
Buy	0.11	-1.00	1.11	-0.59	-0.41***	-43.35***	-879.73**
	(1.31)	(-0.52)	(0.58)	(-0.31)	(-2.96)	(-3.35)	(-2.04)
Limit	0.08	0.42**	-0.34*	0.43	-0.01	48.83	-131.81
	(0.57)	(2.11)	(-1.88)	(1.48)	(-0.05)	(1.38)	(-0.25)
$13D \times Buy$	-0.27	1.12	-1.39	0.32	0.80***	230.81***	5188.54***
	(-1.56)	(0.58)	(-0.72)	(0.17)	(3.50)	(5.00)	(4.70)
$Buy \times Limit$	0.23*	0.25	-0.02	0.48	-0.23	29.30	-788.58
	(1.70)	(1.38)	(-0.10)	(1.64)	(-1.07)	(0.83)	(-1.43)
Adjusted-R2	0.51	-0.00	-0.00	-0.00	0.06	0.02	0.03
N	2,128,279	2,128,279	2,128,279	2,128,279	2,128,279	2,128,279	2,128,279
StockDayTimeFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: 13D vs trades within the same interval							
$13D$	-0.38***	-1.26***	0.88***	-2.85***	1.59***	-167.30***	-5809.32***
	(-2.67)	(-4.91)	(5.22)	(-5.85)	(4.08)	(-3.53)	(-4.31)
Buy	-0.06	-2.16***	2.11***	-2.24***	0.08	-12.49	-1364.56*
	(-0.46)	(-7.44)	(8.22)	(-4.18)	(0.17)	(-0.43)	(-1.69)
Limit	-0.05	0.29*	-0.35**	0.10	0.19	48.87*	266.86
	(-0.50)	(1.79)	(-2.34)	(0.37)	(0.74)	(1.78)	(0.56)
$13D \times Buy$	-0.07	2.19***	-2.26***	2.15***	0.04	88.97**	3795.55***
	(-0.57)	(7.85)	(-8.52)	(4.05)	(0.08)	(2.08)	(3.31)
$Buy \times Limit$	0.03	0.07	-0.04	0.04	0.03	31.82	-389.80
	(0.34)	(0.45)	(-0.30)	(0.16)	(0.14)	(1.17)	(-0.81)
Adjusted-R2	0.68	0.60	0.20	0.15	0.09	0.15	0.07

Figure A.1. Distribution of Days Between Event Date and Filing Date



This figure illustrates the time it took filers to submit 13D filings after crossing the 5% ownership threshold (the event date). While 13D filers are required to file within 10 days of this threshold, many failed to meet the deadline. A significant number of filings were submitted within the required 10-day window, as indicated by the peak near day 10. However, the distribution also reveals a considerable number of late filings, ranging from slight delays to extreme cases. These delays include a long tail of submissions aggregated into the "31+ (aggregated)" category. Remarkably, the maximum delay between the event date and the filing date in our sample extends to an extraordinary 1989 days.

Table A.4
Value Weighted Holding Period Returns of 13D Filers: Post-Filing Comparison Across Full Sample, Hedge Funds, and Non-Hedge Funds

The table summarizes the value-weighted holding period return (VWHPR) for 13D filers, calculated for each day after the filing date. The VWHPR for each filing is the value-weighted average of holding period returns from individual 13D trades. Daily average VWHPR are computed by averaging across all 13D filings, with returns expressed as arithmetic values. The analysis uses a sample of 13D trades classified as common stock, priced between $1 and $1,000. It is restricted to trades executed on stock-days with multiple TAQ matches, excluding sell trades, option-like trades (executed outside the bid-ask spread), and stocks delisted within 20 days of the filing date. Results are further grouped into three categories: all 13D filers, hedge fund filers, and non-hedge fund filers.

Days After Filing	Full Sample			Hedge Fund			Non HF		
	Ret	Median	n_obs	Ret	Median	n_obs	Ret	Median	n_obs
0	0.0121	0.0025	1638	0.0198	0.0063	466	0.0090	0.0016	1172
1	0.0158	0.0034	1638	0.0228	0.0062	466	0.0131	0.0027	1172
2	0.0197	0.0064	1638	0.0256	0.0114	466	0.0173	0.0051	1172
3	0.0244	0.0077	1638	0.0303	0.0122	466	0.0221	0.0069	1172
4	0.0268	0.0094	1638	0.0336	0.0161	466	0.0241	0.0076	1172
5	0.0309	0.0139	1638	0.0373	0.0201	466	0.0283	0.0110	1172
6	0.0334	0.0152	1638	0.0406	0.0215	466	0.0305	0.0110	1172
7	0.0372	0.0175	1638	0.0425	0.0242	466	0.0350	0.0150	1172
8	0.0431	0.0231	1638	0.0484	0.0311	466	0.0410	0.0198	1172
9	0.0472	0.0248	1638	0.0537	0.0330	466	0.0446	0.0216	1172
10	0.0487	0.0282	1638	0.0556	0.0328	466	0.0460	0.0240	1172
11	0.0494	0.0261	1638	0.0562	0.0371	466	0.0467	0.0223	1172
12	0.0524	0.0266	1638	0.0597	0.0369	466	0.0495	0.0228	1172
13	0.0523	0.0265	1638	0.0587	0.0329	466	0.0498	0.0231	1172
14	0.0521	0.0285	1638	0.0567	0.0365	466	0.0503	0.0246	1172
15	0.0517	0.0281	1638	0.0557	0.0310	466	0.0502	0.0262	1172
16	0.0533	0.0281	1638	0.0559	0.0311	466	0.0522	0.0267	1172
17	0.0528	0.0235	1638	0.0552	0.0297	466	0.0519	0.0216	1172
18	0.0536	0.0250	1638	0.0565	0.0291	466	0.0524	0.0236	1172
19	0.0550	0.0250	1638	0.0596	0.0326	466	0.0532	0.0231	1172
20	0.0558	0.0274	1638	0.0590	0.0303	466	0.0545	0.0265	1172

Table A.5
Value-Weighted Average Abnormal Returns (VWAAR) for 13D Filers: Post-Filing Comparison Between On-Time and Late Filers

This table builds on the analysis presented in Table 9 and summarizes the value-weighted average abnormal return (VWAAR) for 13D filers, calculated for each day following the filing date. The data is categorized into two groups: on-time filers and late filers. While 13D filers are required to file within 10 days after the event date, a significant number failed to comply with this deadline (detailed in Figure 2). On-time filers are defined as those who filed within the 10-day window, while late filers include those who filed after the 10-day window. This categorization prompted an analysis to explore potential differences in profitability between the two groups. For each group, the table reports the average VWAAR (*Ret*), the median VWAAR (*Median*), and the number of observations (*n_obs*). Additionally, it includes the difference in returns between on-time and late filers. The methodology and calculation of the VWAAR are consistent with those in Table 9. While not all trades in the calculation are matched to TAQ, the analysis is restricted to trades executed on stock-days identified by multiple TAQ matches. The sample further omits sell trades, option-like trades (executed outside the bid-ask spread), and stocks delisted within 20 days after the filing date. All returns are expressed as arithmetic returns.

Days After Filing	On-Time Filers			Late Filers			On-Time Minus Late
	Ret	Median	n_obs	Ret	Median	n_obs	Difference
0	0.0063	-0.0042	1150	0.0019	-0.0067	488	0.0044
1	0.0101	-0.0010	1150	0.0041	-0.0005	488	0.0059
2	0.0147	0.0046	1150	0.0055	-0.0039	488	0.0092
3	0.0199	0.0059	1150	0.0074	-0.0018	488	0.0125
4	0.0232	0.0080	1150	0.0059	-0.0016	488	0.0173
5	0.0270	0.0102	1150	0.0088	0.0023	488	0.0182
6	0.0283	0.0115	1150	0.0108	0.0021	488	0.0175
7	0.0320	0.0146	1150	0.0146	0.0048	488	0.0174
8	0.0368	0.0170	1150	0.0237	0.0112	488	0.0131
9	0.0391	0.0185	1150	0.0286	0.0174	488	0.0104
10	0.0387	0.0191	1150	0.0332	0.0213	488	0.0056
11	0.0385	0.0209	1150	0.0344	0.0204	488	0.0042
12	0.0398	0.0219	1150	0.0398	0.0238	488	-0.0000
13	0.0384	0.0192	1150	0.0408	0.0195	488	-0.0024
14	0.0375	0.0188	1150	0.0417	0.0199	488	-0.0042
15	0.0373	0.0207	1150	0.0397	0.0182	488	-0.0025
16	0.0401	0.0214	1150	0.0366	0.0196	488	0.0036
17	0.0384	0.0153	1150	0.0382	0.0192	488	0.0002
18	0.0382	0.0174	1150	0.0391	0.0151	488	-0.0009
19	0.0398	0.0180	1150	0.0396	0.0212	488	0.0002
20	0.0415	0.0197	1150	0.0384	0.0161	488	0.0031

Table A.6
Value-Weighted Average Abnormal Returns (VWAAR) for 13D Filers: Post-Filing Comparison Between Patient and Impatient Trades

This table builds on the analysis presented in Table 9 and summarizes the value-weighted average abnormal return (VWAAR) for 13D filers, calculated for each day following the filing date. The data is categorized into two groups: patient trades and impatient trades. Patient trades are defined as trades executed 20 days or more before the filing date, while impatient trades refer to those executed closer to the filing date, within a shorter time window, following the methodology outlined by Bogousslavsky, Fos, and Muravyev (2024). This categorization prompted an analysis to investigate potential differences in profitability between the two groups, given the differing time horizons before the information is disclosed to the public. For each group, the table reports the average VWAAR (*Ret*), the median VWAAR (*Median*), and the number of observations (*n_obs*). Additionally, it includes the difference in returns between patient and impatient trades. The methodology and calculation of the VWAAR are consistent with those in Table 9. While not all trades in the calculation are matched to TAQ, the analysis is restricted to trades executed on stock-days identified by multiple TAQ matches. The sample further omits sell trades, option-like trades (executed outside the bid-ask spread), and stocks delisted within 20 days after the filing date. All returns are expressed as arithmetic returns.

Days After Filing	Patient Trades			Impatient Trades			Patient Minus Impatient
	Ret	Median	n_obs	Ret	Median	n_obs	Difference
0	0.0116	-0.0069	1147	0.0023	-0.0030	1214	0.0093
1	0.0131	-0.0032	1147	0.0074	0.0003	1214	0.0057
2	0.0175	-0.0002	1147	0.0109	0.0029	1214	0.0066
3	0.0218	0.0033	1147	0.0145	0.0027	1214	0.0073
4	0.0229	0.0053	1147	0.0170	0.0051	1214	0.0059
5	0.0260	0.0083	1147	0.0210	0.0069	1214	0.0051
6	0.0272	0.0077	1147	0.0223	0.0086	1214	0.0049
7	0.0311	0.0117	1147	0.0261	0.0106	1214	0.0050
8	0.0368	0.0184	1147	0.0318	0.0124	1214	0.0050
9	0.0402	0.0187	1147	0.0347	0.0143	1214	0.0054
10	0.0403	0.0195	1147	0.0357	0.0164	1214	0.0046
11	0.0409	0.0192	1147	0.0350	0.0170	1214	0.0059
12	0.0435	0.0218	1147	0.0378	0.0157	1214	0.0058
13	0.0416	0.0190	1147	0.0367	0.0158	1214	0.0049
14	0.0420	0.0205	1147	0.0358	0.0151	1214	0.0062
15	0.0413	0.0201	1147	0.0349	0.0156	1214	0.0064
16	0.0431	0.0208	1147	0.0365	0.0145	1214	0.0066
17	0.0423	0.0154	1147	0.0363	0.0165	1214	0.0059
18	0.0421	0.0167	1147	0.0364	0.0150	1214	0.0058
19	0.0434	0.0156	1147	0.0375	0.0173	1214	0.0058
20	0.0433	0.0166	1147	0.0381	0.0172	1214	0.0052

References

Admati, A. R. and P. Pfleiderer (1988). A Theory of Intraday Patterns: Volume and Price Variability. *Review of Financial Studies 1*, 3–40.

Agarwal, V., W. Jiang, Y. Tang, and B. Yang (2013). Uncovering Hedge Fund Skill from the Portfolio Holdings They Hide. The Journal of Finance. *The Journal of Finance 68*, 739–783.

Akey, P., V. Grégoire, and C. Martineau (2022). Price revelation from insider trading: Evidence from hacked earnings news. *Journal of Financial Economics 143*(3), 1162–1184.

Amihud, Y. and H. Mendelson (1986). Asset pricing and the bid-ask spread. *Journal of Financial Economics 17*(2), 223–249.

Anand, A., P. Irvine, A. Puckett, and K. Venkataraman (2012). Performance of Institutional Trading Desks: An Analysis of Persistence in Trading Costs. *Review of Financial Studies 25*(2), 557–598.

Arellano, M. and S. Bond (1991). ome tests of specification for panel data: Monte carlo evidence and an application to employment equations. *The Review of Economic Studies 58*(2), 277–297.

Barclay, M. J. and J. B. Warner (1993). Stealth trading and volatility: Which trades move prices? *Journal of Financial Economics 34*(3), 281–305.

Barth, D., J. Joenväärä, M. Kauppila, and R. Wermers (2023). A bias-free assessment of the hedge fund industry: A new evaluation of total assets, alphas, and the flow-performance relation. *Working Paper, Board of Governors of the Federal Reserve System*.

Bartlett, R. P., J. McCrary, and M. O'Hara (2025). The market inside the market: Odd-lot quotes. *The Review of Financial Studies 38*(3), 661–711.

Bogousslavsky, V., V. Fos, and D. Muravyev (2024). Informed trading intensity. *Journal of Finance 79*(2), 903–948.

Bolandnazar, M., R. J. J. Jackson, W. Jiang, and J. Mitts (2020). Trading against the random expiration of private information: A natural experiment. *The Journal of Finance 75*, 5–44.

Bordalo, P., N. Gennaioli, R. L. Porta, and A. Shleifer (2024). Belief overreaction and stock market puzzles. *Journal of Political Economy 132*(5), 1450–1484.

Brav, A., W. Jiang, F. Partnoy, and R. Thomas (2008a). Hedge Fund Activism, Corporate Governance, and Firm Performance. *The Journal of Finance 63*, 1729–1775.

Brav, A., W. Jiang, F. Partnoy, and R. S. Thomas (2008b). The returns to hedge fund activism. *Financial Analysts Journal 64*(6), 45–61.

Brogaard, J., T. Hendershott, and R. Riordan (2019). Price discovery without trading: Evidence from limit orders. *Journal of Finance 74*(4), 1621–1658.

Chabakauri, G., V. Fos, and W. Jiang (2024). *Trading Ahead of Barbarians' Arrival at the Gate: Insider Trading on Non-Inside Information.* LSE Financial Markets Group.

Chan, L. K. C. and J. Lakonishok (1995). The behavior of stock prices around institutional trades. *Journal of Finance 50*(4), 1147–1174.

Chen, Y., B. Kelly, and W. Wu (2020). Sophisticated investors and market efficiency: Evidence from a natural experiment. *Journal of Financial Economics 138*(2), 316–341.

Collin-Dufresne, P. and V. Fos (2015). Do prices reveal the presence of informed trading? *Journal of Finance 70*(4), 1555–1582.

Collin-Dufresne, P. and V. Fos (2016). Insider trading, stochastic liquidity and equilibrium prices. *Econometrica 84*(4), 1441–1475.

Conrad, J. and S. Wahal (2020). The term structure of liquidity provision. *Journal of Financial Economics 136*(1), 239–259.

Glosten, L. and P. Milgrom (1985). Bid, Ask and Transaction Prices in a Specialist Market with Heterogeneously Informed Traders. *Journal of Financial Economics 14*, 71–100.

Glosten, L. R. and L. E. Harris (1988). Estimating the Components of the Bid/Ask Spread. *Journal of Financial Economics 21*, 123–142.

Goldstein, I., C. S. Spatt, and M. Ye (2024, 12). The next chapter of big data in finance. *Review of Financial Studies*, hhae083.

Goyal, A., A. V. Reed, E. Smajlbegovic, and A. Soebhag (2025). Stealthy Shorts: Informed Liquidity Supply. *working paper, University of Lausanne*.

Grinblatt, M., G. Jostova, L. Petrasek, and A. Philipov (2020). Style and skill: Hedge funds, mutual funds, and momentum. *Management Science 66*(12), 5505–5531.

Grossman, S. J. and J. E. Stiglitz (1980). On the Impossibility of Informationally Efficient Markets. *American Economic Review 70*, 393–408.

Ha, J., J. Hu, and Y. Tang (2024). Informed trading under the microscope: Evidence from 30 years of daily hedge fund trades. *Working Paper*.

Henry, T. R. and J. L. Koski (2017). Ex-dividend profitability and institutional trading skill. *Journal of Finance 72*(1), 461–494.

Holden, C. W. and S. Jacobsen (2014). Liquidity measurement problems in fast, competitive markets: Expensive and cheap solutions. *The Journal of Finance 69*(4), 1747–1785.

Huang, A. G., H. Tan, and R. Wermers (2020). Institutional trading around corporate news: Evidence from textual analysis. *The Review of Financial Studies 33*(10), 4627–4675.

Huang, R. D. and H. R. Stoll (1996). Dealer versus auction markets: A paired comparison of execution costs on nasdaq and the nyse. *Journal of Financial Economics 41*(3), 313–357.

Inci, A. C., B. Lu, and H. N. Seyhun (2010). Intraday behavior of stock prices and trades around insider trading. *Financial Management 39*(1), 323–363.

Jagannathan, R., A. Malakhov, and D. Novikov (2010). Do hot hands exist among hedge fund managers? an empirical evaluation. *The Journal of Finance 65*(1), 217–255.

Jame, R. (2018). Liquidity provision and the cross section of hedge fund returns. *Management Science 64*(7), 3288–3312.

Joenväärä, J., M. Kauppila, R. Kosowski, and P. Tolonen (2021). Hedge fund performance: Are stylized facts sensitive to which database one uses? *Critical Finance Review 10*(2), 271–327.

Judson, R. and A. Owen (1999). Estimating dynamic panel data models: a guide for macroeconomists. *Economics Letters 65*(1), 9–15.

Kacperczyk, M. and E. S. Pagnotta (2019). Chasing private information. *Review of Financial Studies 32*(12), 4997–5047.

Kacperczyk, M. and E. S. Pagnotta (2024). Legal risk and insider trading. *Journal of Finance 79*(1), 305–355.

Kaniel, R. and H. Liu (2006). So what orders do informed traders use? *The Journal of Business 79*(4), 1867–1913.

Kosowski, R., N. Y. Naik, and M. Teo (2007). Do hedge funds deliver alpha? a bayesian and bootstrap analysis. *Journal of Financial Economics 84*(1), 229–264.

Kwan, A., R. Philip, and A. Shkilko (2024). The conduits of price discovery: A machine learning approach. *working paper, University of New South Wales*.

Kyle, A. S. (1985). Continuous Auctions and Insider Trading. *Econometrica 53*, 1315–1335.

Lee, C. M. and M. J. Ready (1991). Inferring Investor Behavior from Intraday Data. *Journal of Finance 46*, 733–746.

Ljungqvist, A. and W. Qian (2016). How Constraining Are Limits to Arbitrage? *The Review of Financial Studies 29*, 1975–2028.

Roşu, I. (2020). Liquidity and information in limit order markets. *Journal of Financial and Quantitative Analysis 55*(6), 1792–1839.

Rösch, D. (2021). The impact of arbitrage on market liquidity. *Journal of Financial Economics 142*(1), 195–213.

Shkilko, A. (2020). Insider trading under the microscope. *working paper*.

Stein, J. C. (2009). Presidential address: Sophisticated investors and market efficiency. *The Journal of Finance 64*(4), 1517–1548.

Wu, S.-Y. J. and K. H. Chung (2022). Hedge fund activism and corporate ma decisions. *Management Science 68*(2), 1378–1403.

Wurgler, J. (2000). Financial markets and the allocation of capital. *Journal of Financial Economics 58*(1), 187–214.