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High frequency trading and the 2008 short-sale ban[☆]

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ABSTRACT

We examine the effects of high-frequency traders (HFTs) on liquidity using the September 2008 short sale-ban. To disentangle the separate impacts of short selling by HFTs and non-HFTs, we use an instrumental variables approach exploiting differences in the ban's cross-sectional impact on HFTs and non-HFTs. Non-HFTs' short selling improves liquidity, as measured by bid-ask spreads. HFTs' short selling has the opposite effect by adversely selecting limit orders, which can decrease liquidity supplier competition and reduce trading by non-HFTs. The results highlight that some HFTs' activities are harmful to liquidity during the extremely volatile short-sale ban period.

1. Introduction

High-frequency traders (HFTs) combine technology with short-horizon trading strategies and constitute a significant fraction of equity trading. Regulators, academics, and practitioners struggle to understand whether HFTs and high-speed automated markets improve the trading environment. With near-zero monitoring, updating, and order placement costs, HFTs could improve liquidity by reducing frictions in liquidity provision. Beyond simply having lower marginal costs, HFTs' efficiency could allow them to offer better prices to other investors by avoiding adverse selection through lower costs of revising and updating their quotes.

Despite their efficiency, HFTs are not necessarily beneficial to other investors. HFTs' ability to process and trade on public information before other investors are able to revise their orders can enable HFTs to adversely select other investors' orders. For example, when prices rise or fall in the index futures market, HFTs could buy or sell in the underlying stocks before other investors can cancel their orders or revise their orders' prices. In addition, the high fixed

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cost of investing in the fastest infrastructure and best programmers could raise barriers to entry and cause HFTs to reduce competition in liquidity provision. Empirical studies of HFTs primarily provide evidence on correlations between HFTs and liquidity. Showing causality is challenging. This paper provides causal evidence on short selling HFTs' impact during a volatile period when financial market liquidity significantly declined.

This paper examines the impact of HFTs' short selling on liquidity (the price of immediacy) using the 2008 short-selling ban as a source of significant variation in HFTs' trading. In addition, the differential impact of HFTs' and non-HFTs' short selling provides insight into heterogeneity among types of short sellers. Establishing the causal impact of HFTs' short selling on liquidity requires showing that short selling is an important component of HFTs' trading and disentangling the effect of HFTs' short selling from non-HFTs' short selling. To distinguish the effects of HFTs' and non-HFTs' short selling, we study differences in the cross section of banned stocks by constructing instruments based on the heterogeneity in the ban's cross-sectional impact on HFTs' and non-HFTs' short selling, e.g., prior to the ban, HFTs are more active in larger market capitalization stocks and non-HFTs are more active in higher book-to-market stocks. HFTs' short selling and trading falls more in large market capitalization stocks, while liquidity declines less in these stocks. Thus, we find that HFTs' short selling is detrimental to liquidity. This decline is driven by HFTs adversely selecting limit orders and by a decrease in competition for liquidity provision. HFTs' informational and noninformational impact could be linked if fewer liquidity suppliers can compete in the presence of HFTs. Consistent with HFTs raising the costs of trading for non-HFTs, we show that HFTs' short selling reduces trading by non-HFTs.

In September 2008, the US Securities and Exchange Commission (SEC) implemented a short-sale ban disallowing most short selling in financial stocks during a particularly volatile time for financial markets. Boehmer, Jones, and Zhang (2013) provide an in-depth analysis of this event and conclude that short selling fell and overall market quality deteriorated. Boehmer, Jones, and Zhang use a difference-in-differences approach by constructing a matched sample of non-banned stocks to examine the ban's impact. The short-sale ban is a natural instrument for examining short selling's impact as the ban targets only short selling. Unfortunately, the timing of the ban and the stocks selected for the ban are not random. This raises concerns that the ban could be correlated directly with changes in liquidity unrelated to short selling.

To mitigate concerns about being able to find a proper control group for the banned stocks, we allow the ban to directly affect liquidity and examine only the ban's interaction with cross-sectional variables known to be correlated with high frequency trading (HFT) and non-HFT trading volume. For example, HFTs are more active in larger market capitalization stocks before the ban and HFTs' trading declines more in these stocks during the ban. If HFTs affect liquidity, then the ban should impact the cross-section relation between market capitalization and liquidity. If HFTs improve liquidity, then liquidity in larger market capitalization stocks should deteriorate more during the ban than liquidity in smaller stocks. If HFTs harm liquidity, then liquidity in large stocks should improve relative to small stocks. Thus, the cross-sectional relation between market capitalization and liquidity should change during the ban and the direction of that change can identify HFTs' impact on liquidity.

We find that liquidity falls less in the large stocks and that the relation between market capitalization and liquidity becomes more positive during the ban. Both of these are consistent with HFTs harming liquidity. Our instrumental variable (IV) tests confirm this in a multivariate setting, allow for the inclusion of volatility controls, and enable the standard under- and overidentification tests. Similarly, cross-sectional variation during the ban in stocks' book-to-market ratios provides identification showing that non-HFTs' short selling improves liquidity.

Nasdaq provides our measures for short selling and HFTs. The HFT measure is the same as the one used in a number of other studies (Brogaard, Hendershott, and Riordan, 2014; Carrion, 2013; O'Hara, Yao, and Ye, 2014). During the ban HFTs' short selling falls from 6% of trading volume to less than 1%. non-HFTs' short selling declines from 15 percent of trading volume to six percent.[1] The first-stage IV regression shows differential declines in HFTs' and non-HFTs' short selling based on stocks' market capitalization, price-to-earnings ratio, book-to-market ratio, and price. During the ban, liquidity decreases, as measured by the quoted bid-ask spread and the effective spread. The decline is smaller in larger stocks and larger in stocks with higher book-to-market ratios.

We find that non-HFTs' short selling increases liquidity. In contrast, HFTs' short selling decreases liquidity. A 100% increase in relative HFT short selling causes a 10 basis point (bp) increase in spreads. A 1% increase in non-HFT short selling causes a 5 basis point decrease in spreads. We also estimate a log-linear specification to analyze the multiplicative increase in spreads. Here we find a 1% increase in relative HFT short selling causes a 3% increase in log spreads. Based on an average spread of roughly 50 basis points, this log-linear model estimates that a 1% increase HFTs' short-selling causes a 1 to 2 basis point increase in spreads. While the magnitude of the HFTs' impact on liquidity depends on the functional form of the relation between HFTs' trading and liquidity, the direction of the impact is the same.

The rest of the paper is organized as follows. Section 2 discusses our identification strategy and the related literature. Section 3 describes the data used and provides descriptive statistics. Section 4 discusses our specifications. Section 5 contains the main empirical results. Section 6 analyzes the mechanism and consequences. Section 7 concludes.

2. Identification and related literature

HFTs' short selling and liquidity are likely simultaneously determined in equilibrium leading to bi-directional

[1] HFTs' total trading activity, which includes both short selling and non–short selling, as a fraction of trading volume declines by almost 50%.

causality. If HFTs' trading increases and the bid-ask spread increases, it could be that HFTs cause the bid-ask spread to increase. Alternatively, HFTs could react to the higher bid-ask spread by increasing their participation. We use the cross-sectional impact of the ban on short selling to establish causal effects. As the ban can be directly correlated with liquidity, we use the ban as a control variable and base our instruments on the cross-sectional impact of the ban on short-selling activity.

For the ban to separately identify the effects of HFTs' and non-HFTs' short selling requires dimensions in which the ex-ante expectation is that the ban should differentially impact HFTs and non-HFTs. Brogaard, Hendershott, and Riordan (2014) show that HFTs are more concentrated in larger-market capitalization stocks. In contrast, from a period before HFTs were prevalent, Boehmer, Jones, and Zhang (2008) show that overall short selling is relatively constant across market capitalization. These cross-sectional differences likely arise from HFTs' short holding periods being easier to accomplish in larger, more liquid stocks. O'Hara, Saar, and Zhang (2013) find evidence that stock price levels impact HFTs' behavior due to the minimum price increment (tick size) being one cent for all stocks. One explanation for this is that various arbitrage strategies requiring immediate execution are more difficult in stocks in which the spread is constrained by a larger tick size relative to the stock price. For these reasons, we utilize the pre-ban (August 1) values of market capitalization and stock price interacted with a short sale-ban dummy variable as instruments.

Our instruments also capture cross-sectional differences in non-HFTs' relative short selling. Dechow, Hutton, Meulbroek, and Sloan (2001) show that, prior to the growth of HFTs, short sellers use the fundamental ratios of stocks' earning and book values to market values in their strategies. As with market capitalization and price, we interact the ban with the pre-ban price divided by the earnings per share and the ban interacted with the pre-ban book value of equity divided by the market value of equity.

For the heterogeneous cross-sectional effects to serve as valid instruments, they must satisfy the exclusion restriction. Changes in stocks' relative short selling must not be correlated with the error term in the liquidity equation. This does not require that the cross-sectional variation occurs randomly. The liquidity equation includes date and firm fixed effects and a set of control variables with the ban itself. The instruments remain valid even if the cross-sectional variation is related to these particular explanatory variables. For instance, if the stocks with lower prices tend to have higher liquidity, this would be picked up by the firm fixed effect and the exclusion restriction would still hold. The exclusion restriction is violated if the heterogeneous variation in short selling is somehow related to contemporaneous changes in firm-specific, idiosyncratic liquidity that are not due to changes in short selling.

The ban could be correlated with subsequent liquidity if there are sufficiently persistent but temporary shocks to liquidity. The September 2008 short-sale ban follows a volatile time in the financial markets. The events surrounding the ban and its institutional details are shown in Boehmer, Jones, and Zhang (2013). We control for possible overall temporary liquidity shocks by including the ban and day fixed effects as control variables in the second stage of the IV estimation. Another way the ban could impact future liquidity is if the short sale-ban is correlated with other temporary changes in the informational environment of the banned stocks during the ban. This seems plausible given the state of the financial system, the rushed introduction of the ban, and the various other measures introduced at the beginning of the ban, e.g., the Troubled Asset Relief Program (TARP). The exclusion restriction would be violated if these measures differentially affect stocks' informational environment in a way that is correlated with our cross-sectional instruments. To control for possible cross-sectional changes in stocks' informational environment, we use lagged stock volatility and contemporaneous financial sector volatility as measured by the Financial Select Sector SPDR fund (XLF ETF) volatility as controls.

The above discussion focuses on the econometric use of our cross-sectional instruments. HFTs can affect liquidity through a number of possible economic mechanisms. If HFTs are informed market participants, then their removal could result in spreads decreasing as other market participants adjust for the lower probability of trading with an informed trader. If HFTs are uninformed and trade as efficient market intermediaries, then their removal could cause spreads to increase. Consistent with HFTs harming liquidity, we show that HFTs profit from adversely selecting limit orders. Finally, we find that HFTs' trading causes non-HFTs to trade less.

Jones (2013) and Biais and Foucault (2014) offer reviews of the literature on HFTs. A number of papers examine how fast traders can adversely select slower traders. Foucault, Hombert, and Rosu (2016) and Rosu (2014) examine how some traders trading faster on public signals increase information asymmetry. Budish, Cramton, and Shim (2015) study how fast traders impose adverse selection on each other and decrease liquidity. Our results are consistent with these concerns.[2]

Empirically, technological changes have been used to examine how speed and fast trading impact markets. Hendershott, Jones, and Menkveld (2011) and Boehmer, Fong, and Wu (2012) show how algorithmic trading improves liquidity on the New York Stock Exchange and internationally, respectively. Riordan and Storkenmaier (2012) find that a trading system upgrade at Deutsche Börse improves liquidity. In contrast, Gai, Yao, and Ye (2014) find that technological improvements at Nasdaq are associated with decreasing depth. Menkveld and Zoican (2014) show that a new trading system introduced at Nasdaq OMX in 2010 increases spreads. Menkveld and Zoican are able to identify trading by different market participants and examine how HFTs demanding liquidity pick off HFTs supplying liquidity. Brogaard, Hagströmer, Nordén, and Riordan (2015) use a colocation upgrade at Nasdaq OMX Stockholm to find that HFTs' supplying liquidity are

[2] Budish, Cramton, and Shim (2015) and Biais, Foucault, and Moinas (2015) examine the social efficiency of investments in fast trading.

able to utilize the upgrade to improve liquidity.[3] The challenge with interpreting results from technological changes is that their introduction often impacts multiple investors and traders. Our cross-sectional IV approach shows how to identify causal impacts of HFTs, even in the presence of other investors being impacted.

Our results also contribute to the short-selling literature. We add insight on very short-horizon short sellers. Saffi and Sigurdsson (2011) find that short selling improves market efficiency, and Boehmer and Wu (2013) and Beber and Pagano (2013) find that short selling improves the price discovery process.[4] Certain types of short sellers are more informed than others. Boehmer, Jones, and Zhang (2008) find that institutional non-program short sales are the most informed. Engelberg, Reed, and Ringgenberg (2012) find that registered market maker short sellers are less informed than non–market makers. Kelley and Tetlock (2013) show that retail short sellers are informed. Theory predicts that when informed traders are removed, market liquidity improves due to lower adverse selection cost. The findings in our paper are consistent with HFT short sellers being informed.

3. Data and descriptive statistics

Nasdaq provides the HFT data used in this study to academics under a nondisclosure agreement. A number of other studies use the HFT measure (Brogaard, Hendershott, and Riordan, 2014; Carrion, 2013; O'Hara, Yao, and Ye, 2014). For every trade, the dataset includes an identifier for whether a trade involved an HFT firm and specifies whether or not the HFT firm supplied or demanded liquidity, or both. Firms are categorized as HFT based on Nasdaq's knowledge of their customers and analysis of firms' trading such as how often their net trading in a day crosses zero, their order duration, and their order-to-trade ratio. The HFT firms are the same as ones in Brogaard, Hendershott, and Riordan (2014), so the same limitations apply. The identifier capturers firms that are exclusively HFTs and not larger integrated firms such as Goldman Sachs. Nasdaq provides data between August 1, 2008 and October 31, 2008 for every symbol used in Boehmer, Jones, and Zhang (2013). This results in a sample of 758 banned stocks.

The data include trades executing against either displayed or hidden liquidity on the Nasdaq exchange, but not trades that execute on other markets including those that report on Nasdaq's trade reporting facility. Trades are time-stamped to the millisecond and identify the liquidity demander and supplier as an HFT or non-HFT.

When we use a matched sample, we use the same matches as Boehmer, Jones, and Zhang (2013). Nasdaq also provides the same data for these control stocks. As in Boehmer, Jones, and Zhang, we drop observations from the first day of the ban to avoid contaminating our results with the effects of the triple witching day and the TARP announcement. Triple witching day is the third Friday of the last month of each quarter when stock options, stock futures, and index options or futures expire simultaneously. We also drop the last day of the ban to avoid uncertainty regarding the end of the ban. Our results are robust to the inclusion of the first, the last, and both the first and the last day of the ban.

There are 64 trading days. To create a balanced panel, we use only stocks that trade every day of the sample period. Of the 379 stocks subject to the short-sale ban, 319 were part of the initial ban, and the rest were added later. The final sample has 379 banned stocks and 379 Boehmer, Jones, and Zhang matched control stocks.

The HFT data set is provided by Nasdaq and contains the following data fields: (1) symbol, (2) date, (3) time in milliseconds, (4) shares, (5) price, (6) buy–sell indicator, and (7) type (HH, HN, MH, NN). Symbol is the Nasdaq trading symbol for a stock. The buy–sell indicator captures whether the trade was buyer- or seller-initiated. The type flag captures the liquidity-demanding and liquidity-supplying participants in a transaction. The type variable can take one of four values: HH, HN, NH, or NN. HH indicates that an HFT demands liquidity and another HFT supplies liquidity in a trade; NN is similar with both parties in the trade being non-HFTs. HN trades indicate that an HFT demands liquidity and a non-HFT supplies liquidity; the reverse is true for NH trades. The remainder of the paper denotes HFT liquidity demand trades as HFT^D (HH plus HN) and HFT liquidity supply trades as HFT^S (NH plus HH). Total HFT trading activity ($HFT^D + HFT^S$) is labeled as HFT. The non-HFT trading volume variables are defined analogously.

Regulation SHO was introduced by the SEC in June 2004 to establish a new set of rules surrounding short selling.[5] After the introduction of Regulation SHO, exchanges implemented procedures to identify short sales at the transaction level. Nasdaq provides a second data set identifying short sales to supplement the HFT data set.

The Nasdaq HFT and Regulation SHO data set is supplemented with a National Best Bid and Offer (NBBO) from the Daily Trade and Quote (DTAQ) database. The NBBO measures the best prices prevailing across all markets and is calculated as outlined in Holden and Jacobsen (2014). DTAQ provides millisecond time stamps, the time resolution of the data from Nasdaq. Market capitalization data are retrieved from the Center for Research in Security Prices. We focus on continuous trading during normal trading hours by removing trading before 9:30 and after 16:00, as well as the opening and closing crosses, which aggregate orders into an auction. Matching the HFT data and

[3] Malinova, Park, and Riordan (2013) use the introduction of a message fee on the Toronto Stock Exchange to show that HFTs' liquidity supplying orders are positively related to liquidity. Menkveld (2013) show how the entry of one liquidity-supplying HFT improves liquidity in Dutch stocks.

[4] In contrast, using discontinuities in the regulation of short-selling eligibility on the Hong Kong Stock Exchange, Crane, Crotty, Michenaud, and Naranjo (2015) fail to find evidence of deleterious effects of short-selling restrictions. Our results showing differential impacts of different short sellers could explain why short-sale bans have different impacts in different markets depending on which types of short sellers are more active in each market.

[5] See http://www.sec.gov/investor/pubs/regsho.htm for more details on Regulation SHO.

Table 1
Descriptive statistics
This table reports descriptive statistics of the banned stocks and their non-banned (control) matches. The sample consists of 379 US stocks subject to the 2008 shorting ban and a matched control sample of stocks in which shorting was not banned. *Nasdaq Market share* is Nasdaq trading volume divided by the national trading volume. *Nasdaq Total Market Share* is the Nasdaq trading volume and Nasdaq trading volume in off-exchange trading (TRF). *Quoted Spread* is time-weighted. Effective Spread, five-minute *Realized Spread*, and five-minute *Price Impact* are trade-weighted and are proportional to the prevailing quote midpoint. *Std. Dev. of Returns* is the average one-second standard deviations of returns. Relative shorting and trading volume measures are based on Nasdaq trades during regular trading hours. High-frequency trader (HFT) liquidity demand trades are denoted as HFT^D; HFT liquidity supply trades, as HFT^s. Total HFT trading activity ($HFT^D + HFT^S$) is labeled as HFT. The non-HFT trading variables are defined analogously. We provide the relative trading volume by trader and trade type. The table reports the relative short selling by trader type and broken down by order type, identified by the prefix *RelSS*. The denominator for all of the relative short-selling statistics is Nasdaq volume on day t for stock i. bps = basis points.

	Banned			Control		
Variable	Pre-Ban	Ban	Post-Ban	Pre-Ban	Ban	Post-Ban
Number of Stocks	379	379	379	379	379	379
Nasdaq Volume (ten thousands of dollars)	268.69	204.37	244.50	203.80	222.67	223.62
Nasdaq Market Share (percent)	18.99	16.75	18.50	18.35	17.81	18.01
Nasdaq Total Market Share (percent)	40.72	38.98	40.03	40.15	39.78	39.36
Quoted Spread (bps)	30.37	73.66	74.11	28.44	44.08	59.20
Effective Spread (bps)	22.65	55.67	53.60	21.32	32.01	43.68
Realized Spread (bps)	4.48	22.10	15.48	6.06	11.00	11.40
Price Impact (bps)	18.17	33.55	38.12	15.26	21.01	32.28
Std. Dev. of Returns (bps)	5.55	15.17	12.74	5.38	9.32	10.87
Relative HFT (percent)	23.91	15.53	21.72	20.79	21.04	22.10
RelSS HFT (percent)	6.37	1.01	4.82	5.77	5.54	5.90
RelSS HFT^D (percent)	4.13	0.38	3.18	3.69	3.40	3.78
RelSS HFT^S (percent)	2.24	0.63	1.64	2.08	2.14	2.12
RelSS non-HFT (percent)	15.19	5.55	13.50	15.21	13.69	14.08
RelSS non-HFT^D (percent)	6.73	2.54	6.12	6.86	6.61	6.39
RelSS non-HFT^S (percent)	8.46	3.00	7.38	8.35	7.08	7.69

the Regulation SHO data is straightforward because both are from Nasdaq and contain the same time stamp.

Table 1 reports the descriptive statistics. All statistics are based on the daily time series average over the relevant interval and averaged across the cross section of stocks. Columns 1–3 report the descriptive statistics for banned stocks, and Columns 4–6 report for the control group of stocks. The statistics are broken down based on the pre-ban period, the ban period, and the post-ban period.

Nasdaq Volume is the average daily dollar volume per stock on Nasdaq. On average, a banned stock traded \$26.8 million before the ban, \$20.4 million during the ban, and \$24.4 million after the ban. The corresponding volumes for the matched sample are \$20.3 million, \$22.2 million, and \$22.3 million before, during, and after the ban, *Nasdaq Market Share* is Nasdaq volume divided by the national trading volume. *Nasdaq Market Share* falls slightly during the ban from 18.99% to 16.75% in banned stocks and from 18.35 to 17.81% in control stocks. *Nasdaq Total Market Share* is similar and includes Nasdaq's off-exchange trading volume. *Nasdaq's Total Market Share* falls negligibly during the ban from 40.72% to 38.98% in banned stocks and from 40.15% to 39.78% in control stocks.

The first measure of liquidity is the quoted spread. The quoted spread captures the costs of simultaneously buying and selling at the best quoted prices using marketable orders. This is the cost of immediacy. Lower costs of trading could be possible by placing limit orders, but those are more difficult to measure because many limit orders do not execute. The quoted spread is defined as

$$Quoted\ Spread_{i,t} = \frac{Ask\ Price_{i,t} - Bid\ Price_{i,t}}{M_{i,t}}, \tag{1}$$

where *Ask Price* is the lowest displayed price at which an investor will sell shares in stock i at time t and *Bid Price* is the highest displayed price at which an investor will buy shares in stock i at time t. M is the midpoint price prevailing at time t in stock i. *Quoted Spread* is the national quoted spread based on data from DTAQ from all exchanges. A higher value implies less liquidity. Quoted spreads measure only visible liquidity, so hidden orders can provide additional liquidity, possibly at better prices. For the banned stocks, the quoted spread increases from 30.37 basis points in the pre-ban period to 73.66 bps during the ban. The non-banned stocks have slightly lower quoted spreads prior to the ban.

The effective spread incorporates trading that occurs against non-displayed liquidity and is defined as

$$Effective\ Spread_{i,t} = \frac{|P_{i,t} - M_{i,t}|}{M_{i,t}}, \tag{2}$$

where P is the price at which the trade occurred. The *Effective Spread* measures trade prices relative to the NBBO midpoint price. The wider the effective spread, the less liquid is a stock. In Table 1, effective spreads are strictly lower than quoted spreads, showing that hidden liquidity is regularly available and that investors time their trading to coincide with high liquidity periods. The effective spreads before, during, and after the ban follow a similar pattern as the quoted spreads.

The realized spread captures profits to the liquidity supplier calculated in a mark-to-market manner relative to the quote midpoint five minutes after the trade and is defined for buyer-initiated trades as

$$Realized\ Spread_{i,t} = \frac{P_{i,t} - M_{i,t+5min}}{M_{i,t}}, \tag{3}$$

where $M_{i,t+5\ min}$ is the midpoint price prevailing five minutes after the stock i trade occurring at time t. The realized spread for seller-initiated trades multiplies Eq. (3) by minus one. The banned stocks experience a large increase in realized spread during and after the ban, from 4.48 bps in the pre-ban period to 22.1 bps during the ban and 15.48 bps following the ban. For the control group, the realized spread increases from the pre-ban level of 6.06 bps to 11.0 bps in the ban period and 11.4 bps in the post-ban period.

The difference between the effective and realized spread is the price impact, defined for buyer initiated trades as

$$Price\ Impact_{i,t} = \frac{M_{i,t+5min} - M_{i,t}}{M_{i,t}} \quad (4)$$

Eq. (4) is multiplied by minus one for seller initiated trades. This captures losses that liquidity suppliers suffer from liquidity demanders being able to forecast subsequent price movements. For banned stocks, a large increase is evident in the price impact from the pre-ban (18.17 bps) to the ban period (33.55 bps), and the price impact remains high in the post-ban period (38.12 bps). The price impact for the control group only moderately increases between the pre-ban (15.26 bps) and the ban (21.01 bps) period and rises further in the post-ban period (32.28 bps).

Table 1 provides information on the trading activity of the different market participants. Following Boehmer, Jones, and Zhang (2013) most of our analysis uses relative short sales (RelSS) for each trader type, which is the fraction of total trading volume for each trade type that is short sales. Table 1 reports the relative trading activity by different traders and order types, identified by the prefix *Rel*. Table 1 also reports the relative short selling performed by different segments of the population, identified by the prefix *RelSS*.

As expected, relative short sales fall for all trader types during the ban period. Before the ban, 21.5% of the dollar volume traded is a short sale. During the ban, the fraction drops to 6.56%. Overall, HFTs' RelSS declines from 6.37% pre-ban to 1.01% during the ban, and it recovers to 4.82% post-ban. Non-HFTs' RelSS decreases from 15.19% pre-ban to 5.55% during the ban and increases to 13.5% post-ban. RelSS for HFTs and non-HFTs exhibits little variation across time periods in the control stocks.

To more clearly examine the time series of the variables in Table 1, Fig. 1 plots overall relative high-frequency trades (Relative HFT) and RelSS for HFT and non-HFT. RelSS was fairly stable for both HFT and non-HFT before the ban, and the declines in RelSS appear immediately upon the ban's introduction and persist throughout the ban. The recovery in RelSS after the ban's removal is immediate and constant. Fig. 1 illustrates the ban's large and temporary impact on short selling.

HFTs could continue trading at the same level by holding long inventory to avoid shorting. Table 1 establishes that the ban significantly impacts HFTs, although HFTs are able to continue trading to a lesser extent due to the ban's market-making exemptions or by avoiding going short. This shows that while the ban produces economically large effects on HFTs, not all HFT activity is affected. The conclusion discusses this further in the context of interpreting the IV results.

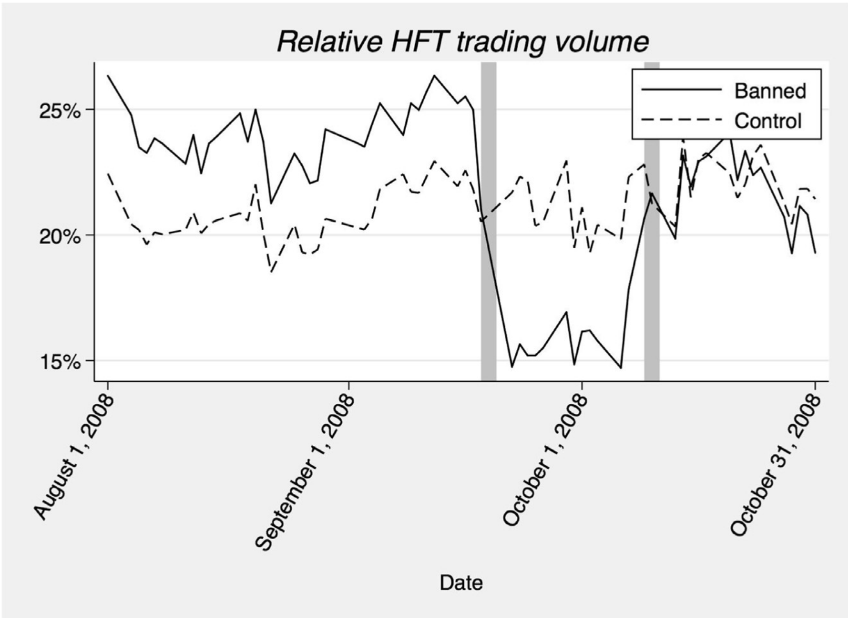
Fig. 2 plots the liquidity measures and shows that spreads are similar in banned and control stocks in the pre-period but do not fully converge in the post-period. Fig. 2 also shows that spreads increase immediately with the ban in the banned stocks, but not in the control stocks. Spreads drift upward during the ban in both the ban and control stocks, indicating the importance of controlling for other market-wide factors. The level of spreads in the ban and control stocks is similar before and after the ban. This suggests that the ban had a significant temporary impact on liquidity.

4. Specification details

The summary statistics and figures show a noticeable change in trading activity and in liquidity around the ban. This section formalizes our IV approach. Four instrumental variables in the first-stage regression identify the ban's cross-sectional shocks to relative short selling and relative short selling by different participants. These variables for stock i and day t are $Ban_{i,\ t} \times MCap_i$, the *ban* indicator interacted with the natural log of stock i's August 1 market capitalization; $Ban_{i,\ t} \times PE_i$, the *ban* indicator interacted with stock i's August 1 *Price* divided by the August 1 *Earnings per share;* $Ban_{i,\ t} \times BM_i$, the *ban* indicator interacted with stock i's August 1 *Book Value of Equity* divided by the August 1 *Market Value of Equity*; and $Ban_{i,\ t} \times Price_i$, the *ban* indicator interacted with the August 1 price of stock i.

Fig. 1 shows that shorting activity declines during the ban. *Ban* is a dummy variable for the short-sale ban itself that takes the value one for those days and stocks during which the ban applied and zero otherwise. The first instrumental variable is the *Ban* indicator interacted with the August 1 (pre-ban) log(market capitalization) as HFTs tend to trade in larger stocks (Brogaard, Hendershott, and Riordan, 2014). We include *Ban* interacted with the August 1 *Price*–to–*Earnings per Share*, and the *Ban* interacted with the August 1 *Book Value of Equity*–to–*Market Value of Equity*. Both the price-to-earnings ratio and book-to-market ratio are cross-sectional instruments for non-HFT short selling. Dechow, Hutton, Meulbroek, and Sloan (2001) show that, prior to the growth of HFTs, short sellers use the fundamental ratios of earning and book values to market values in their strategies. The final instrument is the short-sale ban dummy interacted with the August 1 stock price. O'Hara, Saar, and Zhang (2013) find evidence that, given the fixed tick size, stock price levels impact HFTs' behavior. Acemoglu and Angrist (2000) discuss the use of multiple correlated instruments for several possible treatment variables.

In addition to the instruments, the inclusion of time series variables related to the stocks' informational environment can improve the estimation and help isolate the ban's effect. These control variables also help to address the possibility that the ban's cross-sectional impact could be correlated with events or conditions unrelated to short selling. The control variables are *Ban*, which takes the value of one for banned stocks during the ban and a



b

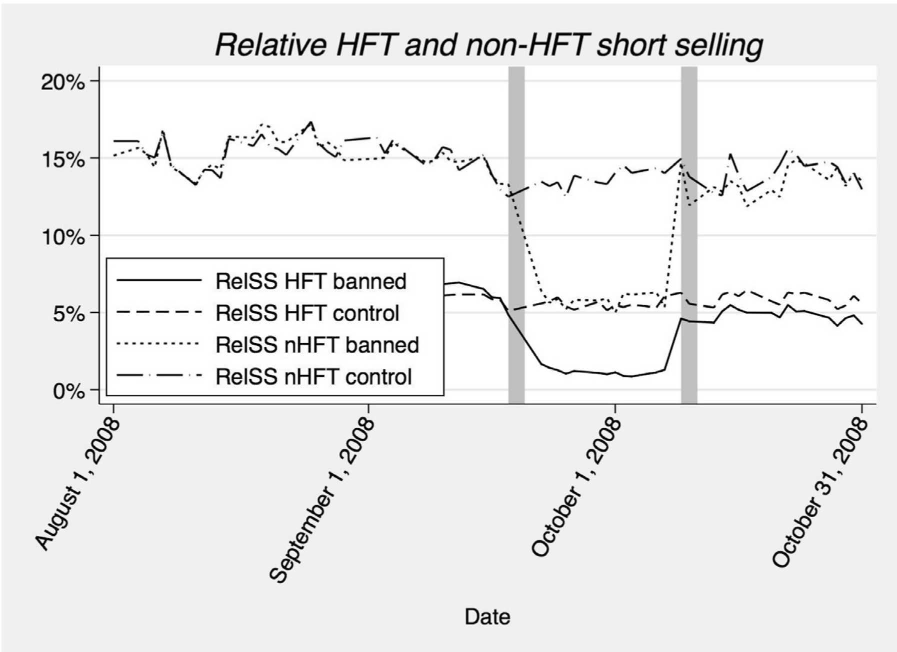
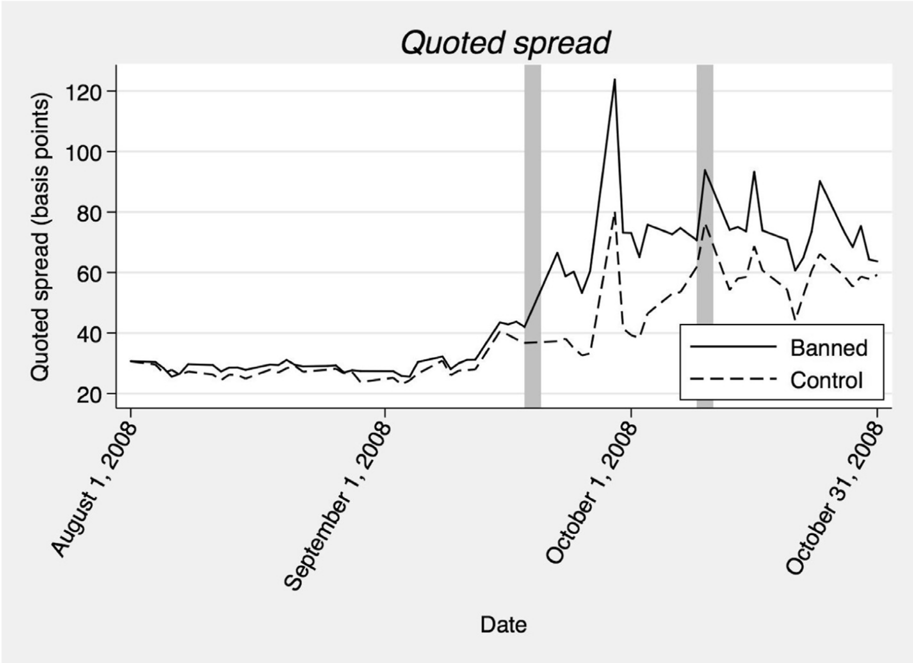


Fig. 1. Relative high-frequency trading (HFT) trading volume and relative HFT and non-HFT short selling. The graph reports the relative trading volume by HFT and relative short selling for HFT and non-HFT. Relative HFT trading volume is calculated as HFT dollar volume for each stock and day on Nasdaq divided by overall trading volume. Relative short trading volume is calculated as dollar volume for short sales for each stock and day on Nasdaq divided by overall trading volume. The sample consists of the common stocks that appear on the initial shorting ban list and their matched control firms that are not subject to the shorting ban from August 1, 2008 through October 31, 2008. The vertical lines correspond to the beginning and ending of the short sale ban. Relss = relative short sale.

zero otherwise; *Price*, the price of stock i on date t; *Rtn. Std. Dev.* $(t-1)$, the average one-second standard deviation of returns of stock i on the previous trading day; *XLF Rtn. Std. Dev.*, the average one-second standard deviation or returns of the Financial Select Sector exchange-traded fund on date t; *Banned * XLF Rtn. Std. Dev.*, the average one-second standard deviation returns of the Financial Select Sector exchange-traded fund for banned stocks and zero for control stocks; and *MCap*, the natural log of the market capitalization of stock i on date t.

a



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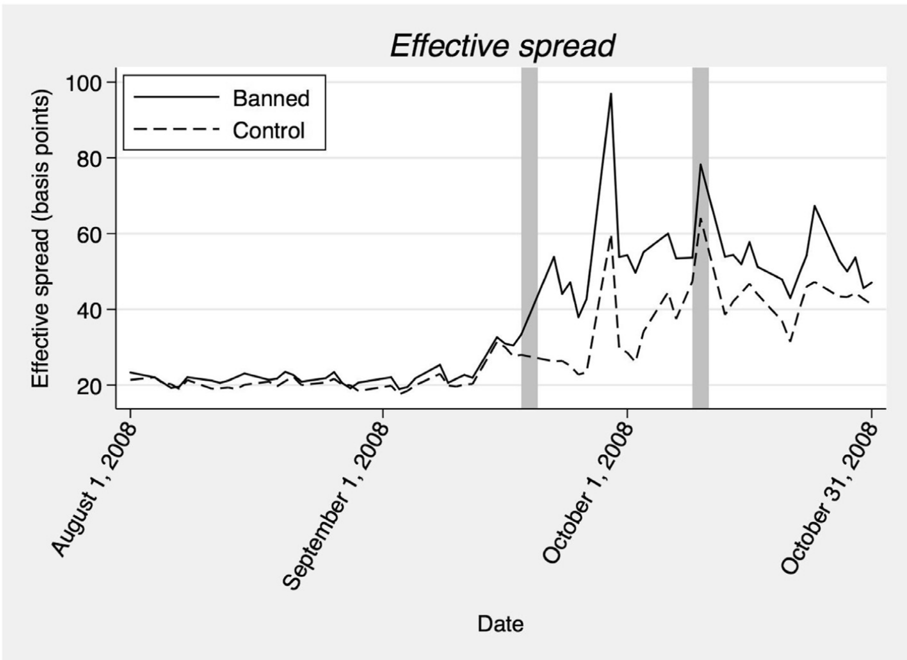

Fig. 2. Liquidity measures. Panel A reports the trade-weighted quoted spread for banned and control stocks. Panel B reports the trade-weighted effective spread. The sample consists of the common stocks that appear on the initial shorting ban list and their matched control firms that are not subject to the shorting ban from August 1, 2008 through October 31, 2008. We use the same matches as Boehmer, Jones, and Zhang (2011). The vertical lines correspond to the beginning and ending of the short sale ban.

Rtn. Std. Dev. $(t-1)$ captures potential time series variation in the information environment of a stock. We use the previous day's return standard deviation because contemporaneous measures of volatility and measures of liquidity are simultaneously determined. *XLF* is the ETF on the financial sector stocks. Under the assumption that liquidity in each individual stock does not cause volatility in *XLF*, then *XLF Rtn. Std. Dev.* controls for the contemporaneous

Table 2
Effect of short sale ban
This table reports liquidity regressions without instrumenting for relative short selling. It uses a daily panel of banned and, for some specifications, banned and matched stock pairs from August 1, 2008 to October 31, 2008. Each sample stock subject to the shorting ban is matched to a similar stock in which shorting was not banned. The pre-ban period is August 1, 2008 to September 18, 2008. We include the following independent variables: *Ban * MCap* is the *Ban* indicator interacted with the August 1 (pre-ban) log(market capitalization), *Ban * PE* is the *Ban* interacted with August 1 (*Price / Earnings per Share*) / 100; *Ban * BM* is *Ban* interacted with August 1 (*Book Value of Equity / Market Value of Equity*); *Ban * Price* is the *ban* indicator interacted with the August 1 stock price; *Ban* is an indicator variable taking the value one during the short-sale ban for stocks subject to the ban and zero otherwise; *XLF Rtn. Std. Dev.* is the one-second standard deviation of the Financial Select Sector SPDR Fund (ETF, XLF). *Banned * XLF Rtn. Std. Dev* is the previous variable for banned stocks only; Market Capitalization (*MCap)* and *Price; Rtn. Std. Dev. (t−1)* is the one-second standard deviation of stock *i* on the previous trading day. Firm fixed effects and date fixed effects are included. Dependent variables include time-weighted national quoted spreads, the natural logarithm of time-weighted national quoted spreads, trade-weighted effective spreads, and the natural logarithm of trade-weighted effective spreads. Standard errors are clustered by firm and date. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Panel A reports for quoted spreads, Panel B, for effective spreads.

Panel A: Quoted spreads

Variable	*Quoted Spread* (1)	*Quoted Spread* (2)	*Log(Quoted Spread)* (3)	*Log(Quoted Spread)* (4)
*Ban * MCap*	−12.60***	−12.60***	−0.03**	−0.03**
*Ban * PE*	−0.45	−0.48	−0.00	−0.00
*Ban * BM*	−5.82**	−5.91**	−0.08***	−0.08***
*Ban * Price*	0.04	0.01	0.00	0.00
Ban	28.19***	25.82***	0.42***	0.45***
Rtn. Std. Dev. (t−1)	0.50***	0.67***	0.00***	0.00***
Price	0.78***	1.08***	0.00	0.00*
MCap	−4.94	−14.18***	−0.30***	−0.30***
XLF Rtn. Std. Dev.	13.00***	8.84***	0.22***	0.17***
*Banned * XLF Rtn. Std. Dev.*	–	4.46**	–	0.06***
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes
N	23,877	47,754	23,877	47,754
Adj. R^2	0.59	0.63	0.93	0.94

Panel B: Effective spreads

Variable	*Effective Spread* (1)	*Effective Spread* (2)	*Log(Effective Spread)* (3)	*Log(Effective Spread)* (4)
*Ban * MCap*	−10.45***	−10.42***	−0.05***	−0.05***
*Ban * PE*	−0.74	−0.75	−0.01	−0.01
*Ban * BM*	−3.41*	−3.56*	−0.09***	−0.09***
*Ban * Price*	0.02	−0.01	0.00*	0.00
Ban	21.75***	21.19***	0.45***	0.50***
Rtn. Std. Dev. (t−1)	0.39***	0.56***	0.01***	0.01***
Price	0.67***	0.93***	−0.00	−0.00
MCap	−13.78***	−19.53***	−0.36***	−0.35***
XLF Rtn. Std. Dev.	11.09***	7.69***	0.26***	0.20***
*Banned * XLF Rtn. Std. Dev.*	–	3.31**	–	0.06***
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes
N	23,877	47,754	23,877	47,754
Adj. R^2	0.61	0.62	0.92	0.93

information environment for financial sector stocks. Given that the ban targets financial sector stocks, when using the matched sample the inclusion of XLF volatility for banned stocks allows only for a differential impact of XLF volatility on the banned and control stocks. Stock fixed effects capture any remaining time-invariant cross-sectional heterogeneity, and day fixed effects capture market-wide time series variation.

The final panel includes either 379 stocks or 379 × 2 = 758 stocks. Before using the IV approach to analyze the effect of HFT, we extend the main specification of Boehmer, Jones, and Zhang (2013) to include our ban cross-sectional interaction variables and our control variables. Table 2 reports the results of the regression

$$Y_{i,t} = \alpha_i + \gamma_t + \beta_1 \times Ban_{i,t} \times MCap_i + \beta_2 \times Ban_{i,t} \times PE_i + \beta_3 \times Ban_{i,t} \times BM_i + \beta_4 \times Ban_{i,t} \times Price_i + \beta_5 \times Ban_{i,t} + \theta X_{i,t} + \epsilon_{i,t}, \quad (5)$$

where $Y_{i,t}$ is either the quoted spread or the effective spread or the natural log of the two liquidity measures. The control variables capture time series variation in financial markets and any direct effects of the short sale ban that can influence the dependent variable. Columns 1 and 3 include only the banned stocks; Columns 2 and 4 also include the matched sample. The matched stock, firm fixed effects, and time fixed effects specifications results in a difference-in-differences methodology that aims to isolate the cross-sectional effects of the short-sale ban. Standard errors are clustered using the techniques of Petersen (2009) and Thompson (2011) to account for time series and cross-sectional correlation of the error term, as well as heteroskedasticity.

Given that spreads vary cross-sectionally and theory provides little guidance for the correct function form, we analyze a linear specification and a log-linear specification, which capture the possible multiplicative increase in spreads. Panel A reports the results for quoted spreads and the natural logarithm of quoted spreads. Panel B reports the results for effective spreads and the natural logarithm of effective spreads. Because the pre-period values for market capitalization, price-to-earnings, book-to-market, and price do not vary across observations, they are collinear with the stock fixed effects and not included separately from their interactions with the ban dummy variable. Because the ban is not effective for all banned stocks on the same day, the ban dummy variable is not collinear with the day fixed effects.

The coefficients on the ban variable in Panel A of Table 2 are consistent with Boehmer, Jones, and Zhang (2013) findings. For our sample stocks, quoted spreads increase by 28.19 basis points and effective spreads increase by 21.75 basis points. In relative terms (the log-linear model), the quoted spread increases by 42% to 45%, and effective spreads increase by 45% to 50%. In each specification, the ban coefficient is statistically significant at the 1% level. The ban interaction variables show a cross-sectional variation in the liquidity variables related to our instruments as the ban has a smaller effect on larger stocks. Quoted spreads on larger banned stocks increase less, with the −12.6 coefficient corresponding to a firm 2.7 times larger having spreads increase by 12.6 basis points less during the ban.[6] While the ban interacted with stock price and the ban interacted with the price-to-earnings ratio do not have statistically significant coefficients, they can be useful if they correlate differently with RelSS HFT and RelSS non-HFT. The control variables have the expected signs, e.g., the coefficients on volatility are positive. The results of effective spreads in Panel B of Table 2 are similar.

5. The effects of short selling and HFTs

To disentangle the effects of different types of short selling and trading, the first stage of our IV approach uses a specification similar to the one in Table 2 with the left-hand-side variable capturing different types of relative short selling and trading:

$$\begin{aligned} Trading_{i,t} = {} & \alpha_i + \gamma_t + \beta_1 \times Ban_{i,t} \times MCap_i + \beta_2 \times Ban_{i,t} \\ & \times PE_i + \beta_3 \times Ban_{i,t} \times BM_i + \beta_4 \times Ban_{i,t} \\ & \times Price_i + \beta_5 \times Ban_{i,t} + \theta X_{i,t} + \epsilon_{i,t}, \end{aligned} \tag{6}$$

where $Trading_{i,t}$ takes one of several different dependent trading variables. The unit of observation is stock i for day t. The regression includes $X_{i,\,t}$, which is a vector of the aforementioned control variables. Stock and date fixed effects are also included.[7] The results of the first stage are reported in Table 3.

Table 3 reports the regression for the different dependent variables with only banned stocks (Columns 1, 3, and 5) and with both banned and matched stocks (Columns 2, 4, and 6). The first column reports the results with the dependent variable being overall relative HFT short selling for banned stocks only, *RelSS HFT*. Consistent with Table 1, the coefficient on the ban dummy is negative, showing that HFT decreases during the ban relative to overall volume. In general, the results differ little between the banned only and the matched sample.

RelSS HFT falls more in large stocks than it does in smaller stocks. In contrast, the fall in *RelSS non-HFT* during the ban is not statistically different from zero across market capitalization. Non-HFT short sellers are positively related to both the ban interacted with the price-to-earnings ratio and with the ban interacted with the book-to-market ratio. *Ban * PE* and *Ban * BM* are not significantly related to relative short selling by HFT. The final instrument, *Ban * Price* is positively related to HFT short selling and unrelated to non-HFT short selling. The first-stage results for relative HFT are similar to those for relative HFT short selling. Table 3 shows that the ban differentially affects HFTs and non-HFTs in the cross section. These differences help to disentangle the effects of HFT and non-HFT on liquidity.

We calculate the first-stage *F*-statistic, the Sanderson and Windmeijer (2016) chi-squared test of underidentification, and the Sanderson and Windmeijer (2016) *F*-statistic test of weak identification using Newey and West (HAC) standard errors (based on five day lags). The *F*-statistic is the standard test of instrument relevance. The Angrist-Pischke (SW) first-stage chi-squared is a test of under-identification of the individual regressors. The SW first-stage *F*-statistic is the *F* form of the same test statistic, which tests whether an endogenous regressor is weakly identified. Our first-stage test statistics reject the null hypotheses of a weak or under-identified model at the 5% level. We also compute second-stage test statistics for under-identification (Kleibergen and Paap, 2006), weak identification (Cragg and Donald, 1993; Wald F-statistic) and overidentification (Hansen, 1982; and Sargan, 1958; *J*-statistic) using Newey and West (HAC) standard errors (based on five day lags) and find no evidence of misspecification.

Table 3 shows that HFTs' trading declines more in larger market capitalization stocks during the ban. Table 2 shows that spreads increase less during the ban in larger market capitalization stocks. These two facts suggest that HFTs are detrimental to liquidity. The opposite relation between liquidity and non-HFTs exists with respect to book-to-market ratios. Table 3 shows that non-HFTs' short selling declines less in higher book-to-market ratio stocks during the ban.

[6] To examine the time series change in the cross-sectional relations between spreads and market capitalization and book-to-market Appendix Figs. A1 and A2 graph the coefficient from daily Fama and MacBeth cross-sectional regressions of liquidity on market capitalization and book-to-market. The graphs show the relation changes when the ban is introduced and removed.

[7] Appendix Tables A1, A2, and A3 report results similar to Tables 3, 4, and 5 without the control variables.

Table 3
Short-sale ban and relative short selling and high-frequency trading (HFT)
This table reports the impact of the short-sale ban on short-selling activity. It uses a daily panel of banned and, for some specifications, banned and matched stock pairs from August 1, 2008 to October 31, 2008. Each sample stock subject to the shorting ban is matched to a similar stock in which shorting was not banned. We include the following independent variables: *Ban * MCap* is the *Ban* indicator interacted with the August 1 (pre-ban) log(market capitalization), *Ban * PE* is the *Ban* interacted with August 1 (*Price / Earnings per Share*) / 100; *Ban * BM* is *Ban* interacted with August 1 (*Book Value of Equity / Market Value of Equity*); *Ban * Price* is the Ban indicator interacted with the August 1 stock price; *Ban* is an indicator variable taking the value one during the short-sale ban for stocks subject to the ban and zero otherwise; *XLF Rtn. Std. Dev.* is the one-second standard deviation of the Financial Select Sector SPDR Fund, XLF. *Banned * XLF Rtn. Std. Dev* is the previous variable for banned stocks only; Market Capitalization (*MCap*) and Price; *Rtn. Std. Dev. (t−1)* is the one-second standard deviation of stock *i* on the previous trading day. Firm fixed effects and date fixed effects are included. We regress

$$Trading_{i,t} = \alpha_i + \gamma_t + \beta_1 \times Ban_{i,t} \times MCap_i + \beta_2 \times Ban_{i,t} \times PE_i + \beta_3 \times Ban_{i,t} \times BM_i + \beta_4 \times Ban_{i,t} \times Price_i + \beta_5 \times Ban_{i,t} + \theta X_{i,t} + \epsilon_{i,t},$$

where the dependent variables are different categories of relative trading: *RelSS HFT* is relative overall HFT short selling and *Relative HFT* is relative HFT. *RelSS non-HFT* is relative short selling by non-HFT. Standard errors are clustered by firm and date. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Variable	*RelSS HFT* (1)	*RelSS HFT* (2)	*RelSS non-HFT* (3)	*RelSS non-HFT* (4)	*Relative HFT* (5)	*Relative HFT* (6)
*Ban * MCap*	−1.42***	−1.42***	−0.18	−0.17	−1.65***	−1.67***
*Ban * PE*	0.09	0.09	0.53***	0.53***	0.01	−0.00
*Ban * BM*	−0.02	−0.03	0.77***	0.75***	0.20	0.22
*Ban * Price*	0.01***	0.01***	0.00	0.00	0.02*	0.02**
Ban	−3.34***	−4.49***	−6.76***	−9.01***	−5.53***	−7.67***
Rtn. Std. Dev. (t−1)	−0.01	−0.00	0.00	0.00	−0.02	−0.02*
Price	−0.01	−0.01*	−0.02	−0.03**	−0.02	−0.05***
MCap	0.30	0.46**	0.20	1.25**	3.86***	3.61***
XLF Rtn. Std. Dev.	−0.19***	0.28***	−0.82***	−0.34***	0.20**	1.09***
*Banned * XLF Rtn. Std. Dev.*	–	−0.64***	–	−0.28**	–	−1.35***
Stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes	No	Yes
N	23,877	47,754	23,877	47,754	23,877	47,754
Adj. R^2	0.75	0.73	0.39	0.34	0.83	0.83

Table 2 shows that spreads increase less during the ban in larger market capitalization stocks. These two facts suggest that non-HFTs are helpful to liquidity.

The IV approach examines whether the indications that HFTs harm liquidity from comparing Tables 2 and 3 hold in a formal statistical setting. The second-stage regression uses the estimates from the first-stage regression as measures of exogenous variation in different market participation types' trading to examine how these impact liquidity. Because all the instruments are fixed in the time series, the IV is similar to a multivariate difference-in-difference approach. The first specification considers how the decrease in relative HFT and non-HFT short selling affects liquidity:

$$Y_{i,t} = \alpha_i + \gamma_t + \beta_1 \times \widehat{RelSS\ HFT_{i,t}} + \beta_2 \times \widehat{RelSS\ non-HFT_{i,t}} + \beta_3 \times Ban_{i,t} + \theta X_{i,t} + \epsilon_{i,t}, \quad (7)$$

where $Y_{i,t}$ is either the quoted spread or the effective spread or the natural log of the two liquidity measures. The unit of observation is stock *i* for day *t*. The control variables are the same as in Eq. (5). $\widehat{RelSS\ HFT}$ and $\widehat{RelSS\ non-HFT}$ take the values estimated from Eq. (6), where the dependent variable is *RelSS HFT* and *RelSS non-HFT*, respectively. The results are reported in Table 4. Panel A reports the quoted spread results; Panel B, the effective spread results.

The units of *RelSS HFT* in Table 4 are in percent. Therefore, the quoted spread coefficient of 9.54, interpreted as a 1% increase in *RelSS HFT* causes the quoted spread to increase by 9.54 basis points. In the log-linear specification, we find that a 1% increase in *RelSS HFT* causes a 3% increase in quoted spreads. This translates into smaller but statistically significant 1–2 basis point increase (3% times the roughly 50 bps quoted spread during the pre-ban and post-ban sample periods).

RelSS non-HFT has negative and statistically significant coefficients for all the liquidity measures and specifications. A 1% increase in relative non-HFT short selling causes a 5.17 basis point decrease in the quoted spread and a 7% decrease in the log-linear specification. The evidence suggests that HFTs' short selling is detrimental to liquidity and that non-HFTs' short selling contributes to liquidity. The qualitative results of effective spreads are very similar. For example, a 1% increase in *RelSS HFT* causes a 4% increase in effective spreads.

To identify the impact of relative HFT and non-HFT short selling on liquidity, Table 5 extends the analysis in Table 4 by including all of HFTs' trading along with short selling by non-HFTs.

The regression in Eq. (8) uses the instrumented relative HFT and relative non-HFT short selling from the first-stage regression in Table 3:

$$Y_{i,t} = \alpha_i + \gamma_t + \beta_1 \times \widehat{Relative\ HFT_{i,t}} + \beta_2 \times \widehat{RelSS\ non-HFT_{i,t}} + \beta_3 \times Ban_{i,t} + \theta X_{i,t} + \epsilon_{i,t}. \quad (8)$$

Consistent with Table 4, Table 5 shows that relative HFT causes liquidity to decrease and that non-HFTs' short selling causes liquidity to improve. The regressions using the quoted spread, effective spread, and natural logarithm of both measures all provide similar inference. In the quoted spread (effective spread) regression, *Relative HFT* has a pos-

Table 4
Effect of relative high-frequency trading (HFT) short selling on liquidity
Using the first-stage estimates from Table 3 to instrument for variation in market activity, we estimate a second-stage regression to understand how market participation impacts liquidity. The regression is

$$Y_{i,t} = \alpha_i + \gamma_t + \beta_1 \times \widehat{RelSS\ HFT_{i,t}} + \beta_2 \times \widehat{RelSS\ non-HFT_{i,t}} + \beta_3 \times Ban_{i,t} + \theta X_{i,t} + \epsilon_{i,t},$$

where $Y_{i,t}$ takes one of several liquidity variables: time-weighted national quoted spreads, the natural logarithm of time-weighted national quoted spreads, trade-weighted effective spreads, and the natural logarithm of trade-weighted effective spreads. Control variables include *Ban*, Market Capitalization (*MCap*), and *Price*. *Rtn. Std. Dev. (t−1)* is the one-second standard deviation of stock *i* on the previous trading day; *XLF Rtn. Std. Dev.* is the one-second standard deviation of the Financial Select Sector SPDR Fund, XLF. *Banned * XLF Rtn. Std. Dev.* is the previous variable for banned stocks only. Date and firm fixed effects are included. The estimation uses a daily panel of banned and, for some specifications, banned and matched stock pairs from August 1, 2008 to October 31, 2008. Each sample stock subject to the shorting ban is matched to a similar stock in which shorting was not banned. Standard errors are clustered by firm and date. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Panel A reports for quoted spreads; and Panel B, for effective spreads.

Panel A: Quoted spreads

Variable	*Quoted Spread* (1)	*Quoted Spread* (2)	*Log(Quoted Spread)* (3)	*Log(Quoted Spread)* (4)
RelSS HFT	9.54***	9.61***	0.03**	0.02*
RelSS non-HFT	−5.17*	−5.20*	−0.07***	−0.07***
Ban	22.60	18.72	0.06	−0.03
Rtn. Std. Dev. (t−1)	0.57***	0.72***	0.00***	0.00***
Price	0.76***	1.03***	0.00	0.00
MCap	−6.73	−11.85*	−0.30***	−0.23***
XLF Rtn. Std. Dev.	−5.56	−9.75***	0.14	0.12***
*Banned * XLF Rtn. Std. Dev.*	–	9.18***	–	0.05***
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes

Panel B: Effective spreads

Variable	*Effective Spread* (1)	*Effective Spread* (2)	*Log(Effective Spread)* (3)	*Log(Effective Spread)* (4)
RelSS HFT	7.96***	8.03***	0.04***	0.04***
RelSS non-HFT	−3.42*	−3.51*	−0.08***	−0.08***
Ban	23.20*	22.77	0.07	−0.02
Rtn. Std. Dev. (t−1)	0.45***	0.61***	0.01***	0.01***
Price	0.65***	0.91***	−0.00	−0.00
MCap	−15.05***	−18.38***	−0.37***	−0.28***
XLF Rtn. Std. Dev.	−11.54	−9.66***	0.01	0.03**
*Banned * XLF Rtn. Std. Dev.*	–	7.48***	–	0.07***
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes

itive coefficient of 8.03 bps (6.75 bps), and non-HFT has a negative coefficient of −5.61 bps (−3.86 bps).

6. Mechanism and consequences

Many theoretical papers assume HFTs adversely select slower non-HFTs, e.g., Biais, Foucault, and Moinas (2015), Foucault, Hombert, and Rosu (2016), and Hoffmann (2014). Several empirical papers provide results consistent with this assumption, e.g., Brogaard, Hendershott, and Riordan (2014) and Carrion (2013). To directly examine whether HFTs profit from adversely selecting other traders in our sample, we calculate effective spreads and realized spreads by whether HFTs or non-HFTs are supplying or demanding liquidity in each trade.

Table 6 reports average pre-ban effective spreads and realized spreads so as to avoid any contamination due to the ban. The reported measures are an equal-weighted average across stock days, participants and order types. Because HFTs are identified only for Nasdaq trades, we match the Nasdaq HFT trades with trades reported on Nasdaq in the DTAQ data set. Outside of Table 6, the liquidity measures use all trades and are calculated using only DTAQ. The two data sets do not include identical time stamps, making matching less straightforward than for the HFT and Regulation SHO data. We use a one hundred-millisecond window between the Nasdaq HFT and the slower DTAQ data set. In some cases, one Nasdaq HFT trade matches multiple DTAQ trades. For this case, we assume that the first trade that matches is correct. Ninety-nine percent of all trades are matched within the first five milliseconds. In addition to the five-minute realized spread reported in Table 1, realized spreads are reported for ten-second and one-minute horizons. Fig. 1 shows the effective spreads and realized spreads increase in September. Table 6 uses data for August, so the spreads are smaller than those in Table 1.[8]

[8] The realized spreads do not incorporate the liquidity rebates limit orders receive from Nasdaq. See Brogaard, Hendershott, and Riordan (2014) for how rebates affect the profitability of liquidity supply by HFTs and non-HFTs.

Table 5
Effect of relative high-frequency trading (HFT) on liquidity

Using the first-stage estimates from Table 3 to instrument for variation in market activity, we estimate a second-stage regression to understand how market participation impacts liquidity. The regression is

$$Y_{i,t} = \alpha_i + \gamma_t + \beta_1 \times \widehat{Relative\ HFT_{i,t}} + \beta_2 \times \widehat{RelSS\ non - HFT_{i,t}} + \beta_3 \times Ban_{i,t} + \theta X_{i,t} + \epsilon_{i,t},$$

where $Y_{i,t}$ takes one of several liquidity variables: time-weighted national quoted spreads, the natural logarithm of time-weighted national quoted spreads, trade-weighted effective spreads, and the natural logarithm of trade-weighted effective spreads. Control variables include *Ban*, Market Capitalization (*MCap*), and *Price. Rtn. Std. Dev. (t−1)* is the one-second standard deviation of stock *i* on the previous trading day; *XLF Rtn. Std. Dev.* is the one-second standard deviation of the Financial Select Sector SPDR Fund, XLF. *Banned * XLF Rtn. Std. Dev.* is the previous variable for banned stocks only. Date and firm fixed effects are included. The estimation uses a daily panel of banned and, in some specifications, banned and matched stock pairs from August 1, 2008 to October 31, 2008. Each sample stock subject to the shorting ban is matched to a similar stock in which shorting was not banned. Standard errors are clustered by firm and date. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Panel A reports for quoted spreads; Panel B, for effective spreads.

Panel A: Quoted spreads

Variable	*Quoted Spread* (1)	*Quoted Spread* (2)	*Log(Quoted Spread)* (3)	*Log(Quoted Spread)* (4)
Relative HFT	8.03***	7.99***	0.02*	0.02*
RelSS non-HFT	−5.61	−5.57	−0.06***	−0.06***
Ban	30.02	30.55	0.08	0.00
Rtn. Std. Dev. (t−1)	0.63***	0.84***	0.00***	0.01***
Price	0.82***	1.28***	0.00	0.00
MCap	−34.73***	−35.68***	−0.37***	−0.29***
XLF Rtn. Std. Dev.	−24.77	−20.03***	0.09	0.10***
*Banned * XLF Rtn. Std. Dev.*	–	13.74***	–	0.07***
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes

Panel B: Effective spreads

Variable	*Effective Spread* (1)	*Effective Spread* (2)	*Log(Effective Spread)* (3)	*Log(Effective Spread)* (4)
Relative HFT	6.75***	6.72***	0.03**	0.03**
RelSS non-HFT	−3.86	−3.88	−0.08***	−0.08***
Ban	29.24*	32.49*	0.10	0.02
Rtn. Std. Dev. (t−1)	0.50***	0.71***	0.01***	0.01***
Price	0.70***	1.11***	−0.00	−0.00
MCap	−38.61***	−38.37***	−0.47***	−0.37***
XLF Rtn. Std. Dev.	−27.69	−18.26***	−0.06	−0.01
*Banned * XLF Rtn. Std. Dev.*	–	11.33***	–	0.08***
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes

Table 6
Spreads by type

This table breaks down liquidity variables by liquidity demander and supplier, for all trading and for short sales only, and by trading horizon for Nasdaq trades only. It uses a daily panel of banned and matched stock pairs from August 2008. Each sample stock subject to the shorting ban is matched to a similar stock in which shorting was not banned. We report the effective spread and the realized spread at ten-second, one-minute, and five-minute horizons.

Variable	Effective spread	Ten-second realized spread	One-minute realized spread	Ten-minutes realized spread
HFT^D	8.29	−3.24	−5.47	−6.24
HFT^S	13.39	1.83	−1.84	−3.48
Non-HFT^D	12.90	2.68	−1.03	−2.66
Non-HFT^S	9.57	−1.13	−3.90	−4.82
Short HFT^D	7.37	−3.50	−5.50	−4.52
Short HFT^S	15.27	2.87	−0.42	−2.88
Short non-HFT^D	11.97	0.97	−2.95	−2.85
Short non-HFT^S	10.56	−0.36	−3.38	−5.78

Table 6 shows that HFTs demand liquidity when spreads are narrower: 8.29 bps versus 12.90 bps for non-HFTs. In addition, the realized spread, or the profit earned by a liquidity supplier, is negative for all horizons when HFTs demand liquidity (−3.24, −5.47, and −6.24 bps for ten-second, one-minute, and five-minute horizons, respectively). Conversely, HFTs earn a positive realized spread when supplying liquidity if they can off-load their inventory at the midquote within at least ten seconds. When non-HFTs demand liquidity, their initial price impact is smaller than the spread so the realized spread is positive at a ten-second horizon. At the five-minute horizon, non-HFTs' price impact is greater than the spread. When non-HFTs supply liquidity, their realized spread is negative at all horizons, ranging between −1.13 and −4.82 basis points. The average HFTs' liquidity demanding trade causes liquidity suppliers to lose 6.24 bps, at the five-minute horizon, and the average non-HFT trade causes liquidity suppliers to lose only 2.66 bps. This suggests that HFTs are disproportionately responsible for non-HFTs 4.82 bps loss.

The results show that HFTs' advantage comes primarily from executing their trades at narrow spreads, leading to liquidity suppliers having negative realized spreads at all horizons. When demanding liquidity, HFTs' ability to predict future price changes is larger than the spread at the time of their trades causing the liquidity supplier to lose money almost immediately. This reduces liquidity suppliers' incentives to narrow the spread. This confirms the adverse selection channel behind the IV results that HFTs reduce liquidity. Little difference exists between HFTs' short selling and overall HFT. For liquidity-demanding trades, the differences in realized spread at ten seconds between HFTs and non-HFTs is almost 6 basis points and the point estimate for relative HFT's impact on spreads in Panel A in Table 4 is 2 to 9 basis points depending on the specification.

The results in Table 6 suggest that the liquidity findings can also operate through the noninformation liquidity channel. Theoretical models of HFTs focus primarily on their impact on adverse selection (Biais, Foucault, and Moinas, 2015; Foucault, Hombert, and Rosu, 2016; Hoffmann, 2014). The realized spread results suggest that modeling and studying HFTs' role in competition in liquidity supply is also important (Brogaard and Garriott, 2014). HFTs' informational and noninformational impact could be linked if fewer liquidity suppliers can compete in the presence of HFTs.

Tables 3–5 show that HFTs' short selling, and HFTs' trading more generally, harms liquidity. In contrast, non-HFTs' short selling improves liquidity. Table 6 suggests that the HFTs' ability to time liquidity could be driving the HFT-induced spread increases. Table 7 reports the second-stage IV regression similar to Tables 3–5 for the realized spread and price impact. If the primary channel is via HFTs' liquidity timing ability, the coefficient on the realized spread should be larger than the coefficient on price impact, thereby confirming the intuition of Table 6.

The results in Table 7 are consistent with the findings in Table 6. The positive relation between *RelSS HFT* and the effective spread appears to be primarily driven by the realized spread and less so by their relation with adverse selection (price impact). A 1% increase in RelSS HFT causes a 6.07 basis point increase in the realized spread and only a 1.89 basis point increase in the price impact. While the RelSS HFT coefficients are statistically significant for both realized spread and price impact, the magnitudes are three times larger for the realized spread.

The results in Table 6 also suggest that HFTs' liquidity-demanding trades play an important role in the effect HFTs have on liquidity. Table 8 decomposes relative HFT short selling into its liquidity-demanding and -supplying components and performs the IV analysis as in Tables 3–5. Panel A reports the first stage, and Panel B reports the second stage.

The approach in Table 8 effectively assumes that HFTs' short-selling liquidity supply and liquidity demand are different strategies. Hagströmer and Nordén (2013) find support for some HFTs specializing in either liquidity demand or liquidity supply. But, without data identifying individual HFTs, we cannot test this assumption directly in our data.

Table 8 shows that relative HFT liquidity demand increases the quoted and effective spreads. The average of the coefficients on relative HFT short-selling liquidity demand and supply are similar in magnitude to those reported in Tables 4 and 5 for relative HFT short selling. However, the *RelSS HFT*^{S} coefficient is not statistically significant in any of the specifications.[9] Relative HFT liquidity demand short-selling trades harming liquidity is consistent with Table 6, which shows that the *HFT*^{D} *Realized Spread* is negative at the 10-second, one-minute, and five-minute horizon.

Tables 6, 7, and 8 provide evidence on how HFTs and short selling impact liquidity. In models in which all trades

Table 7
Effect of relative high-frequency trading (HFT) short selling on liquidity decomposition

Using the first-stage estimates from Table 3 to instrument for variation in market activity, we estimate a second-stage regression to understand how market participation impacts liquidity. The regression is

$$Y_{i,t} = \alpha_i + \gamma_t + \beta_1 \times \widehat{RelSS\ HFT_{i,t}} + \beta_2 \times \widehat{RelSS\ non-HFT_{i,t}} + \beta_3 \times Ban_{i,t} + \theta X_{i,t} + \epsilon_{i,t},$$

where $Y_{i,t}$ takes one of several liquidity variables: trade-weighted realized spreads and the price impact. Control variables include *Ban*, Market Capitalization (*MCap*), and *Price*. *Rtn. Std. Dev. (t−1)* is the one-second standard deviation of stock *i* on the previous trading day; *XLF Rtn. Std. Dev.* is the one-second standard deviation of the Financial Select Sector SPDR Fund, XLF. *Banned * XLF Rtn. Std. Dev.* is the previous variable for banned stocks only. Date and firm fixed effects are included. The estimation uses a daily panel of banned and, for some specifications, banned and matched stock pairs from August 1, 2008 to October 31, 2008. Each sample stock subject to the shorting ban is matched to a similar stock in which shorting was not banned. Standard errors are clustered by firm and date. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Panel A reports for *RelSS HFT* and *RelSS non-HFT*; Panel B, for *Relative HFT* and *RelSS non-HFT.*

Variable	Realized spread (1)	Realized spread (2)	Price impact (3)	Price impact (4)
Panel A: RelSS HFT and RelSS non-HFT				
RelSS HFT	6.07***	6.11***	1.89**	1.91**
RelSS non-HFT	−2.35	−2.40	−1.08	−1.12
Ban	17.58*	16.85	5.55	5.81
Rtn. Std. Dev. (t−1)	0.04	0.15**	0.41***	0.45***
Price	0.22**	0.29***	0.43***	0.62***
MCap	−1.30	−1.96	−13.74***	−16.40***
XLF Rtn. Std. Dev.	−0.96	−2.19	−10.56	−7.44***
*Banned * XLF Rtn. Std. Dev.*	–	5.01***	–	2.45**
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes
Panel B: Relative HFT and RelSS non-HFT				
Relative HFT	5.17***	5.14***	1.58**	1.57**
RelSS non-HFT	−2.71	−2.73	−1.16	−1.16
Ban	22.13*	24.11	7.03	8.26
Rtn. Std. Dev. (t−1)	0.08	0.23***	0.42***	0.47***
Price	0.26*	0.45***	0.44***	0.66***
MCap	−19.33**	−17.23***	−19.26***	−21.10***
XLF Rtn. Std. Dev.	−13.30***	−8.74***	−14.35	−9.48***
*Banned * XLF Rtn. Std. Dev.*	–	7.98***	–	3.34**
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes

[9] Rock (1990) shows how a liquidity supplier with a last mover advantage can impose adverse selection on other traders by strategically trading only with less informed traders. This is a channel by which HFTs' liquidity supply could decrease liquidity.

Table 8
Relative high-frequency trading (HFT) liquidity demand and supply short selling, the short-sale ban and liquidity
This table shows the first and second-stage regression for different market participation types and how it impacts liquidity. The regressions are

$$Trading_{i,t} = \alpha_i + \gamma_t + \beta_1 \times Ban_{i,t} \times MCap_i + \beta_2 \times Ban_{i,t} \times PE_i + \beta_3 \times Ban_{i,t} \times BM_i + \beta_4 \times Ban_{i,t} \times Price_i + \beta_5 \times Ban_{i,t} + \theta X_{i,t} + \in_{i,t}$$

and

$$Y_{i,t} = \alpha_i + \gamma_t + \beta_1 \times \widehat{RelSS\ HFT^D}_{i,t} + \beta_2 \times \widehat{RelSS\ HFT^S}_{i,t} + \beta_3 \times \widehat{RelSS\ non-HFT}_{i,t} + \beta_4 \times Ban_{i,t} + \theta X_{i,t} + \epsilon_{i,t},$$

where *Trading* is different categories of relative trading: *RelSS* HFT^D is relative short selling by HFT liquidity demanders *and RelSS* HFT^s is relative short selling by HFT liquidity suppliers. *RelSS non-*HFT^A is relative short selling by non-HFT. $Y_{i,t}$ takes one of several liquidity variables: time-weighted national quoted spreads, the natural logarithm of time-weighted national quoted spreads, trade-weighted effective spreads, and the natural logarithm of trade-weighted effective spreads. Control variables include *Ban*, Market Capitalization (*MCap)*, and *Price; Rtn. Std. Dev. (t*−1*)* is the one-second standard deviation of stock *i* on the previous trading day; *XLF Rtn. Std. Dev.* is the one-second standard deviation of the Financial Select Sector SPDR Fund, XLF. *Banned * XLF Rtn. Std. Dev.* is the previous variable for banned stocks only. Date and firm fixed effects are included. The estimation uses a daily panel of banned and, for some specifications, banned and matched stock pairs from August 1, 2008 to October 31, 2008. Each sample stock subject to the shorting ban is matched to a similar stock in which shorting was not banned. Standard errors are clustered by firm and date. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Panel A reports the first stage. Panel B reports the second stage for quoted and effective spread. Panel C reports the second stage for realized spread and price impact.

Panel A: first stage

Variable	*RelSS* HFT^D (1)	*RelSS* HFT^D (2)	*RelSS* HFT^s (3)	*RelSS* HFT^s (4)
*Ban * MCap*	−0.82***	−0.81***	−0.60***	−0.61***
*Ban * PE*	0.12**	0.12**	−0.04	−0.04
*Ban * BM*	0.23***	0.22***	−0.26***	−0.25***
*Ban * Price*	−0.00	−0.00	0.01***	0.01***
Ban	−2.45***	−3.21***	−0.89***	−1.27***
Rtn. Std. Dev. (t−1)	−0.00	−0.00	−0.00	−0.00
Price	0.00	0.00	−0.01***	−0.01***
MCap	−0.10	0.32*	0.40**	0.14
XLF Rtn. Std. Dev.	−0.02	0.30***	−0.17***	−0.02
*Banned * XLF Rtn. Std. Dev.*	–	−0.43***	–	−0.21***
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes
N	23,877	47,754	23,877	47,754
Adj. R^2	0.67	0.66	0.68	0.67

Panel B: Second stage: RelSS HFT demand, RelSS HFT supply, and RelSS non-HFT

Variable	*Quoted spread* (1)	*Quoted spread* (2)	*Effective spread* (3)	*Effective spread* (4)
RelSS HFT^D	9.38*	10.80**	9.91**	11.04**
RelSS HFT^s	9.76	7.97	5.31	3.90
RelSS non-HFT	−5.07	−5.94	−4.65	−5.37
Ban	23.03	14.33	18.03	11.69
Rtn. Std. Dev. (t−1)	0.57***	0.72***	0.45***	0.61***
Price	0.76**	1.00***	0.61***	0.81***
MCap	−6.82	−11.24	−13.92**	−16.82***
XLF Rtn. Std. Dev.	−5.56	−8.64*	−11.57	−6.85
*Banned * XLF Rtn. Std. Dev.*	–	9.15***	–	7.41***
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes

Panel C: Second stage: RelSS HFT demand, RelSS HFT supply, and RelSS non-HFT

Variable	Realized spread (1)	Realized spread (2)	Price impact (3)	Price impact (4)
RelSS HFT^D	6.96*	7.16*	2.96**	3.88**
RelSS HFT^s	4.86	4.66	0.42	−0.78
RelSS non-HFT	−2.91	−3.05	−1.76	−2.33*
Ban	15.23	12.98	2.69	−1.43
Rtn. Std. Dev. (t−1)	0.04	0.15**	0.41***	0.45***
Price	0.20*	0.26**	0.41***	0.56***
MCap	−0.78	−1.41	−13.11***	−15.38***
XLF Rtn. Std. Dev.	−0.97	−1.20	−10.58	−5.60**
*Banned * XLF Rtn. Std. Dev.*	–	4.99***	–	2.40**
Stock Fixed Effects	Yes	Yes	Yes	Yes
Date Fixed Effects	Yes	Yes	Yes	Yes
Matched Sample	No	Yes	No	Yes

Table 9
The effect of relative high-frequency trading (HFT) short selling on non-HFT long trading volume

Using the first-stage estimates from Table 3 to instrument for variation in market activity we estimate a second-stage regression to understand how market participation impacts non-HFT trading measured as the log of non-HFT non-short selling volume. The regressions are

$$Trading_{i,t} = \alpha_i + \gamma_t + \beta_1 \times Ban_{i,t} \times MCap_i + \beta_2 \times Ban_{i,t} \times PE_i + \beta_3 \times Ban_{i,t} \times BM_i + \beta_4 \times Ban_{i,t} \times Price_i + \beta_5 \times Ban_{i,t} + \theta X_{i,t} + \epsilon_{i,t}$$

and

$$\text{Log}(non-HFT_{i,t}) = \alpha_i + \gamma_t + \beta_1 \times \widehat{RelSS\ HFT_{i,t}} + \beta_2 \times \widehat{RelSS\ non-HFT_{i,t}} + \beta_5 \times Ban_{i,t} + \theta X_{i,t} + \epsilon_{i,t},$$

where *Trading* is different categories of relative trading: *RelSS HFT* is relative short selling by HFT. *RelSS non-HFT* is relative short selling by non-HFT. Control variables include *Ban*, Market Capitalization (*MCap*), and *Price; Rtn. Std. Dev. (t-1)* is the one-second standard deviation of stock *i* on the previous trading day; *XLF Rtn. Std. Dev.* is the one-second standard deviation of the Financial Select Sector SPDR Fund, XLF. *Banned * XLF Rtn. Std. Dev* is the previous variable for banned stocks only. Date and firm fixed effects are included. The estimation uses a daily panel of banned and, for some specifications, banned and matched stock pairs from August 1, 2008 to October 31, 2008. Each sample stock subject to the shorting ban is matched to a similar stock in which shorting was not banned. Standard errors are clustered by firm and date. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Variable	Log(non-HFT)	Log(non-HFT) (2)
RelSS HFT	−0.04*	−0.04**
RelSS non-HFT	0.04	0.04
Ban	−0.06	−0.12
Rtn. Std. Dev. (t−1)	0.01***	0.01***
Price	−0.01***	−0.01***
MCap	0.77***	0.89***
XLF Rtn. Std. Dev.	−0.91	−0.69***
*Banned * XLF Rtn. Std. Dev.*	–	−0.05**
Stock fixed effects	Yes	Yes
Date fixed effects	Yes	Yes
Matched sample	No	Yes

are intermediated, higher spreads lead to lower welfare due to fewer gains from trade being captured. If the bid-ask spread is merely a transfer from impatient to patient traders, then the welfare costs of lower liquidity are less clear. If wider spreads lead to less trading due to opportunity costs for patient traders or impatient traders' unwillingness to pay the spread, then welfare can fall. To directly test whether more relative short selling by HFTs reduces trading, we use the IV approach from Tables 3, 4, and 5 with non-HFT non–short selling trading volume as the dependent variable.[10] Non-HFT non-short trading is measured as the natural logarithm of non-HFT non–short selling volume. Table 9 relates this trading volume to *RelSS HFT* using the same instruments as in Tables 3, 4, and 5.

Table 9 shows that a 1% increase in *RelSS HFT* causes a 4% decrease in non-HFT non–short trading. Any gains from trade associated with these trades are lost. If demand curves are downward-sloping, then the decrease in the gains from trade is lower than the decrease in volume. This suggests that increases in HFT lead to decreases in non-HFT welfare, but it is difficult to quantify.

7. Conclusion

This paper uses the 2008 short-sale ban to study the effect of HFTs in financial markets. We use the short-sale ban's differential cross-sectional impact as instrumental variables to make causal statements about how HFTs affects liquidity. Overall, HFTs' trading and HFTs' short selling decreases liquidity by adversely selecting liquidity suppliers. Non-HFTs' short-selling activity improves liquidity.

The IV approach captures the local average treatment effect. The ban largely eliminates HFTs' shorting activity, but it has a smaller impact on overall HFT activity. Therefore, the ban captures a large amount of trading activity, but it is difficult to know how representative it is of overall HFT activity. HFT firms or strategies that rely on short selling could be significantly different from strategies that do not use short selling. HFTs' liquidity demand from strategies not using short selling could be more benign or even beneficial to liquidity. In addition, the short-sale ban occurred during some of the most stressful times for financial markets. The adverse selection imposed by HFTs could have been unusually high under these conditions. Hence, a conservative interpretation of the results is that a component of HFTs' activity can be harmful during times of extreme market stress. Further research on HFTs' impact during more normal market conditions is important.

Consistent with a number of theoretical papers, the results suggest that a policy response to HFTs could include restrictions on HFTs. The possible positive benefits of HFTs' liquidity demanding trades are their causing more information to be impounded into prices. Whether such short-lived information is socially valuable is discussed in Brogaard, Hendershott, and Riordan (2014). However, in considering restrictions on HFTs' liquidity demand an important consideration is the ability of HFTs to supply liquidity with less ability to demand liquidity. For example, limiting the ability of HFTs to demand liquidity could impair their ability to manage risk and thereby supply liquidity.

Limiting the ability of those closest to the markets to demand liquidity has some precedence. In the past, market makers were limited in their use of liquidity demanding trades. The market makers, or specialists, were also guaranteed access to incoming order flow, providing them with opportunities to better manage their inventory. Without these types of benefits, limiting HFTs' ability to demand liquidity might not improve overall liquidity. Finally, defining who is an HFT is challenging, contentious, and difficult to enforce. A simpler approach could place limits on liquidity demand by all collocated traders.

Appendix

[10] Similar to Fig. 1, the Appendix includes Fig. A3 that plots the non-HFT non–short trading volume in banned and control stocks. The figure shows a decrease in non–HFT trading volume in banned stocks relative to control stocks. Table A4 relates performs an IV analysis of liquidity's impact on non–HFT trading volume similar to Table 9. All of these analyses support the conclusion that HFTs increase spreads, leading to a reduction in non-HFT trading.

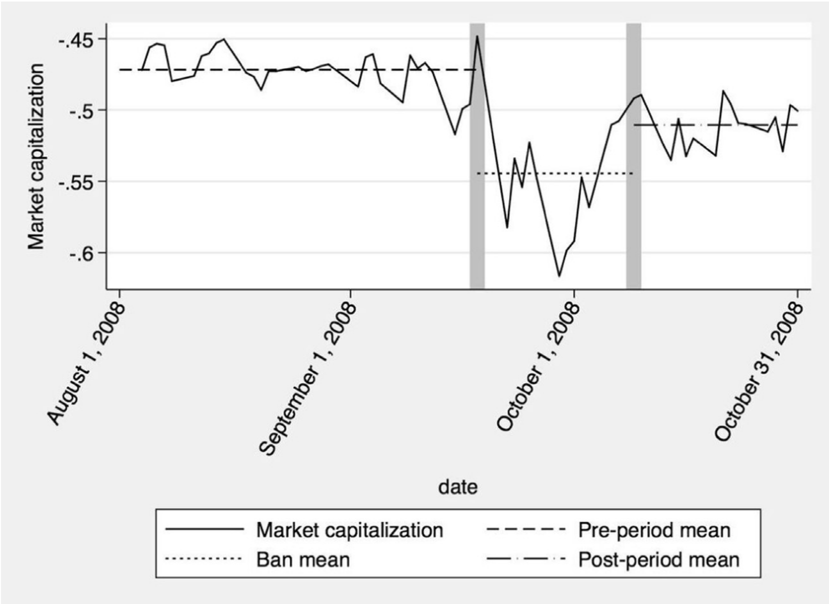

Fig. A1. Coefficient on Pre-Period Market Capitalization. The figure plots the coefficient of the log(quoted spread) on pre-period market capitalization. We run a regression for each day of the sample period and plot the coefficient on pre-period market capitalization (β_1):
$Log(Quoted\ Spread)_{i,t} = \alpha + \beta_1 \times MCap_i + \beta_2 \times Ban_{i,t} \times PE_i + \beta_3 \times Ban_{i,t} \times BM_i + \beta_4 \times Ban_{i,t} \times Price_i + \beta_5 \times Ban_{i,t} + \theta X_{i,t} + \in_i$.
MCap is the pre-period (August 1) market capitalization. We include the following independent variables: *Ban * PE* is *Ban* interacted with August 1 (Price / Earnings per Share); *Ban * BM* is *Ban* interacted with August 1 (*Book Value of Equity / Market Value of Equity*); *Ban * Price* is the Ban indicator interacted with the August 1 stock price; *Ban* is an indicator variable taking the value one during the short-sale ban for stocks subject to the ban and zero otherwise; *X* represents the same controls variables used in the main text. The sample consists of the common stocks that appear on the initial shorting ban list from August 1, 2008 through October 31, 2008. The vertical lines correspond to the beginning and ending of the short sale ban.

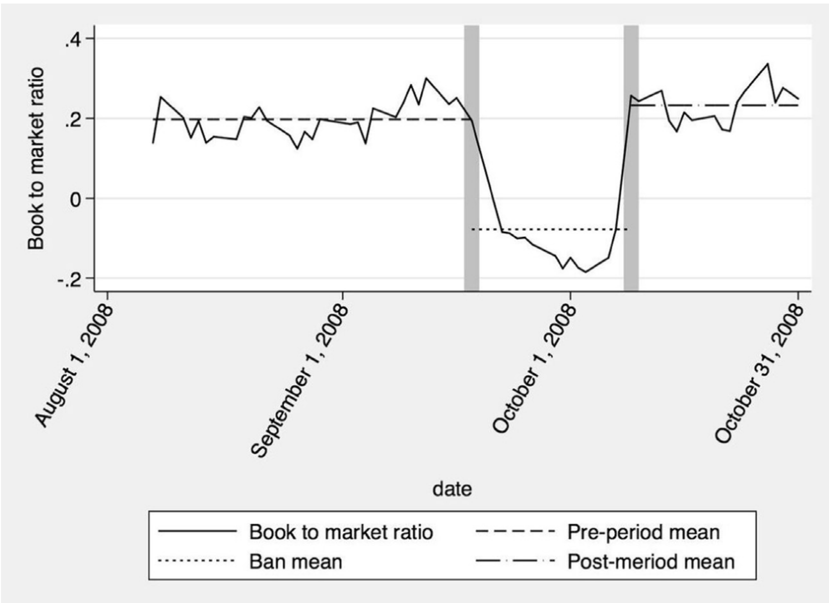

Fig. A2. Coefficient on pre-period book-to-market ratio. The figure plots the coefficient of the log(quoted spread) on pre-period book-to-market ratio. We run a regression for each day of the sample and plot the coefficient on pre-period book to market ratio (β_1):
$Log(Quoted\ Spread)_{i,t} = \alpha + \beta_1 \times BM_i + \beta_2 \times Ban_{i,t} \times PE_i + \beta_3 \times Ban_{i,t} \times MCap_i + \beta_4 \times Ban_{i,t} \times Price_i + \beta_5 \times Ban_{i,t} + \theta X_{i,t} + \in_i$
BM is the pre-period (August 1) book-to-market ratio. We include the following independent variables: *Ban * MCap* is the *Ban* indicator interacted with the average pre-ban log(market capitalization), *Ban * PE* is *Ban* interacted with August 1 (*Price / Eearnings per Share*); *Ban * BM* is Ban interacted with August 1 (*Book Value of Equity / Market Value of Equity*); *Ban * Price* is the *Ban* indicator interacted with the August 1 stock price; *Ban* is an indicator variable taking the value one during the short-sale ban for stocks subject to the ban and zero otherwise; *X* represents the same controls variables used in the main text. The sample consists of the common stocks that appear on the initial shorting ban list from August 1, 2008 through October 31, 2008. The vertical lines correspond to the beginning and ending of the short sale ban.

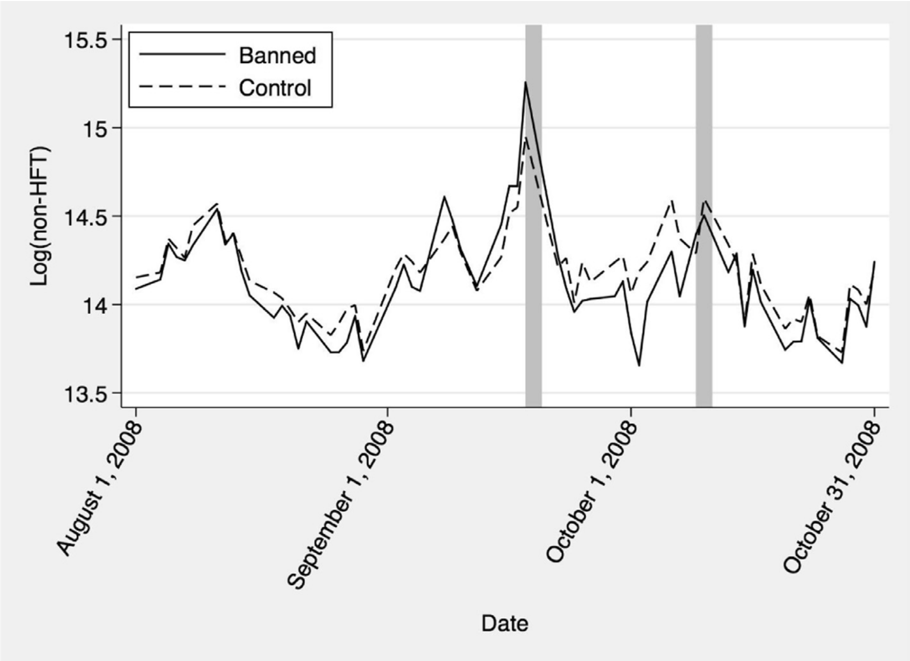

Fig. A3. Non-high-frequency trading (HFT) non-short trading volume. The figure plots the non-HFT non-short trading volume. Non-HFT non-short relative trading volume is calculated as the natural logarithm of non–HFT trading volume minus non-HFT short selling dollar volume for each stock and day. The sample consists of the common stocks that appear on the initial shorting ban list and their matched control firms that are not subject to the shorting ban from August 1, 2008 through October 31, 2008. The vertical lines correspond to the beginning and ending of the short sale ban.

Table A1

The short sale ban and relative short selling and high-frequency trading (HFT) trading using only *Ban* as a control

This table reports the impact of the short-sale ban on short selling. It uses a daily panel of banned and, for some specifications, banned and matched stock pairs from August 1, 2008 to October 31, 2008. Each sample stock subject to the shorting ban is matched to a similar stock in which shorting was not banned. We include the following independent variables: *Ban * MCap* is the *Ban* indicator interacted with the average pre-ban (August 1) log(market capitalization), *Ban * PE* is *Ban* interacted with August 1 (*Price* / Earnings per Share) / 10,000; *Ban * BM* is *Ban* interacted with August 1 (*Book Value of Equity* / *Market Value of Equity*); *Ban * Price* is the *Ban* indicator interacted with the August 1 stock price; *Ban* is an indicator variable taking the value one during the short sale ban for stocks subject to the ban and zero otherwise. Date and firm fixed-effects are included. We regress:

$$Trading_{i,t} = \alpha_i + \gamma_t + \beta_1 \times Ban_{i,t} \times MCap + \beta_2 \times Ban_{i,t} \times PE_i + \beta_3 \times Ban_{i,t} \times BM_i + \beta_4 \times Ban_{i,t} \times \ Price_i + \beta_5 \times Ban_{i,t} + \epsilon_{i,t},$$

where the dependent variables are different categories of relative trading: *RelSS HFT* is relative overall HFT short selling and *RelativeHFT* is relative HFT. *RelSS non-HFT* is relative short selling by non-HFT. Standard errors are clustered by firm and date. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Variable	*RelSS HFT* (1)	*RelSS HFT* (2)	*RelSS non-HFT* (3)	*RelSS non-HFT* (4)	*Relative HFT* (5)	*Relative HFT* (6)
*Ban * MCap*	−1.42***	−1.42***	−0.16	−0.16	−1.72***	−1.71***
*Ban * PE*	0.00	0.00	0.01***	0.01***	−0.00	−0.00
*Ban * BM*	−0.02	−0.02	0.78***	0.78***	0.26	0.26
*Ban * Price*	0.01***	0.01***	0.00	0.00	0.01*	0.01*
Ban	−3.35***	−4.65***	−6.70***	−9.00***	−5.70***	−7.93***
Stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes	No	Yes
N	23,877	47,754	23,877	47,754	23,877	47,754
Adj. R^2	0.75	0.73	0.39	0.34	0.83	0.83

Table A2
Effect of relative high-frequency trading (HFT) short selling on liquidity using only *Ban* as a control

Using the first-stage estimates from Table A1 to instrument for variation in market activity, we estimate a second-stage regression to understand how market participation impacts liquidity. The regression is

$$Y_{i,t} = \alpha_i + \gamma_t + \beta_1 \widehat{RelSS\ HFT_{i,t}} + \beta_2 \widehat{RelSS\ non-HFT_{i,t}} + \beta_3 \times Ban_{i,t} + \epsilon_{i,t}$$

where $Y_{i,t}$ takes one of several liquidity variables: time-weighted national quoted spreads, the natural logarithm of time-weighted national quoted spreads, trade-weighted effective spreads, and the natural logarithm of trade-weighted effective spreads. Date and firm fixed effects are included. The estimation uses a daily panel of banned and, for some specifications, banned and matched stock pairs from August 1, 2008 to October 31, 2008. Each sample stock subject to the shorting ban is matched to a similar stock in which shorting was not banned. Standard errors are clustered by firm and date. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Panel A reports for quoted spreads; Panel B, for effective spreads.

Panel A: Quoted spreads

Variable	*Quoted Spread* (1)	*Quoted Spread* (2)	*Log(Quoted* (3)	*Log(Quoted Spread)* (4)
RelSS HFT	9.44***	9.45***	0.02	0.02
RelSS non-HFT	−5.01**	−5.01**	−0.07***	−0.07***
Ban	25.99*	25.52	0.05	−0.06
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes

Panel B: Effective spreads

Variable	*Effective Spread* (1)	*Effective Spread* (2)	*Log(Effective Spread)* (3)	*Log(Effective Spread)* (4)
RelSS HFT	7.77***	7.78***	0.03**	0.03**
RelSS non-HFT	−3.35*	−3.34*	−0.08***	−0.08***
Ban	25.29**	27.46*	0.05	−0.06
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes

Table A3
Effect of relative high-frequency trading (HFT) on liquidity using only *Ban* as a control

Using the first-stage estimates from Table A1 to instrument for variation in market activity we estimate a second-stage regression to understand how market participation impacts liquidity. The regression is

$$Y_{i,t} = \alpha_i + \gamma_t + \beta_1 \widehat{Relative\ HFT_{i,t}} + \beta_2 \widehat{RelSS\ non\text{-}HFT_{i,t}} + \beta_3 \times Ban_{i,t} + \epsilon_{i,t},$$

where $Y_{i,t}$ takes one of several liquidity variables: time-weighted national quoted spreads, the natural logarithm of time-weighted national quoted spreads, trade-weighted effective spreads, and the natural logarithm of trade-weighted effective spreads. Date and firm fixed effects are included. The estimation uses a daily panel of banned and, for some specifications, banned and matched stock pairs from August 1, 2008 to October 31, 2008. Each sample stock subject to the shorting ban is matched to a similar stock in which shorting was not banned. Standard errors are clustered by firm and date. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Panel A reports for uoted preads; Panel B, for effective spreads.

Panel A: Quoted spreads

Variable	*Quoted Spread* (1)	*Quoted Spread* (2)	*Log(Quoted Spread)* (3)	*Log(Quoted Spread)* (4)
Relative HFT	7.66***	7.68***	0.02	0.02
RelSS non-HFT	−5.67	−5.61	−0.07***	−0.07***
Ban	31.34	34.81	0.07	−0.03
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes

Panel B: Effective spreads

Variable	*Effective Spread* (1)	*Effective Spread* (2)	*Log(Effective Spread)* (3)	*Log(Effective Spread)* (4)
Relative HFT	6.34***	6.37***	0.02**	0.02**
RelSS non-HFT	−3.96	−3.90	−0.08***	−0.08***
Ban	29.54*	34.91*	0.07	−0.02
Stock fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes	Yes	Yes	Yes
Matched sample	No	Yes	No	Yes

Table A4
Effect of liquidity on non-high-frequency trading (HFT) non-short trading volume

Using the first-stage estimates from Table 2 to instrument for variation in liquidity, we estimate a second-stage regression to understand how liquidity impacts liquidity non-HFT trading measured as the natural logarithm of non–HFT non-short trading volume. The regressions are

$$Liquidity_{i,t} = \alpha_i + \gamma_t + \beta_1 \times Ban_{i,t} \times MCap + \beta_2 \times Ban_{i,t} \times PE_i + \beta_3 \times Ban_{i,t} \times BM_i + \beta_4 \times Ban_{i,t} \times Price_i + \beta_5 \times Ban_{i,t} + \theta X_{i,t} + \epsilon_{i,t}.$$

and

$$\text{Log}(non - HFT_{i,t}) = \alpha_i + \gamma_t + \beta_1\ \widehat{Liquidity_{i,t}} + \beta_2 \times Ban_{i,t} + \theta X_{i,t} + \epsilon_{i,t}.$$

where $Liquidity_{i,t}$ is one of two liquidity variables: time-weighted national quoted spreads and trade-weighted effective spreads in percent. Control variables include *Ban, MCap,* and *Price; Rtn. Std. Dev. (t−1)* is the one-second standard deviation of stock *i* on the previous trading day; *XLF Rtn. Std. Dev.* is the one-second standard deviation of the Financial Select Sector SPDR Fund (ETF, XLF). *Banned * XLF Rtn. Std. Dev.* is the previous variable for banned stocks only. Date and firm fixed effects are included. The estimation uses a daily panel of banned and, for some specifications, banned and matched stock pairs from August 1, 2008 to October 31, 2008. Each sample stock subject to the shorting ban is matched to a similar stock in which shorting was not banned. Standard errors are clustered by firm and date. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Variable	Log(non-HFT) (1)	Log(non-HFT) (2)	Log(non-HFT) (3)	Log(non-HFT) (4)
Quoted Spread	−0.37**	−0.36**	–	–
Effective Spread	–	–	−0.42**	−0.41*
Ban	−0.17***	−0.07	−0.17***	−0.08
XLF Rtn. Std. Dev.	−0.69	−0.90	−0.69	−0.93
*Banned * XLF Rtn. Std. Dev.*	−0.02		−0.02	
MCap	0.87***	0.76***	0.84***	0.72***
Price	−0.01***	−0.01***	−0.01***	−0.01***
Rtn. Std. Dev. (t−1)	0.02***	0.01***	0.02***	0.01***
Stock Fixed Effects	Yes	Yes	Yes	Yes
Date Fixed Effects	Yes	Yes	Yes	Yes
Matched Sample	No	Yes	No	Yes

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