Speed of market access and market quality: Evidence from the SEC naked access ban

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Abstract

We examine the notion that in modern securities markets, fast traders with small speed advantages adversely select liquidity providers, thereby leading to higher trading costs for all market participants. Using the SEC's naked access ban, we show that when regulation slows down some fast traders, trading costs decline across the board driven by reduced adverse selection. The benefit of lower trading costs accrues entirely to the liquidity demanders, consistent with a competitive market for liquidity provision. Although price efficiency declines, the decline is small compared to the reduction in trading costs, corroborating the view that the cost of modern price efficiency may outweigh its benefit.

Keywords: Speed of market access; Market quality; Naked access ban; Fast trading

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I. Introduction

Recent technological advances have facilitated the proliferation of fast automated trading that occurs at speeds largely undetectable by the human senses. Competing for fleeting profit opportunities, trading firms that use modern technology work vigorously to implement better, faster, and sometimes even military-grade equipment in a race to outpace each other's orders.[1]

Does the speed race benefit markets? The literature does not yet provide a consensus answer. On the one hand, some studies find that fast trading improves liquidity and price efficiency (Hasbrouck and Saar, 2013; Brogaard, Hendershott, and Riordan, 2014; Chaboud, Chiquoine, Hjalmarsson, and Vega, 2014; Conrad, Wahal, and Xiang, 2015). On the other hand, a number of recent theory models express concerns that the arms race for fast technology has reached a point where the fastest traders adversely select liquidity providers, leading to higher transaction costs (Budish, Cramton, and Shim, 2015; Biais, Foucault, and Moinas, 2015; Foucault, Kozhan, and Tham, 2015; and Menkveld and Zoican, 2015).[2]

This concern is shared by both academics and practitioners. Harris (2013) suggests that regulators should consider randomly slowing down incoming orders to reduce the adverse selection pressure on liquidity providers. In a letter to the Securities and Exchange Commission (SEC), Interactive Brokers Group floats a similar proposal.[3] Goldman Sachs recommends that its equities unit routes orders to the IEX, a trading venue that operates a speed bump.[4]

[1] Activity of such firms is often referred to as algorithmic trading (AT) or high-frequency trading (HFT). Although the terms are used widely, there is no universally accepted definition of either activity. HFT is generally considered a subset of AT. In AT, once a trading decision is made by a trader, computers are used to implement the execution strategy. In HFT, trading opportunities are identified by computers that then decide how to trade (Jones, 2013). Distinguishing between AT and HFT is not essential for our purposes, and we use the term *fast* to refer to traders, who use modern order submission technology.

[2] The logic in these models is akin to Glosten and Milgrom (1985) and Kyle (1985), who show that increased adverse selection leads market makers to post relatively high ask prices and relatively low bid prices.

[3] https://www.interactivebrokers.com/download/SEC_proposal_high_frequency_trading.pdf

[4] Sheer, D., "Goldman Sachs Considers Closing Sigma X Dark Pool," Traders Magazine, April 9, 2014 (http://goo.gl/PH7Zah).

Despite the abovementioned concerns, the empirical literature does not contain much evidence of the negative effects of fast trading. Our study provides such evidence. The results are consistent with the notion that fast traders adversely select liquidity providers while competing for fleeting trading opportunities. The liquidity providers in turn shift the added adverse selection cost to the liquidity demanders by widening spreads.

To establish this result, we use a previously unexplored regulatory event that significantly restricted some traders' speed. In November 2011, the SEC began enforcing Rule 15c3-5 – the ban on unfiltered market access (also known as naked access) – a practice that had allowed traders to bypass brokerage controls and directly connect to exchange servers.[5] Unfiltered access accounted for as much as 38% of trading volume in U.S. equities prior to the ban and allowed for significant increases in order submission speeds.[6] The ban resulted in substantial declines in adverse selection and trading costs. Notably, the benefits of lower adverse selection accrued entirely to liquidity demanders, pointing to the highly competitive market for liquidity provision.

Although the extant literature disagrees on the effects of fast trading on the cost of liquidity, there is greater consensus on the price efficiency effects, with both theoretical and empirical studies showing that the speeds of price efficiency and price discovery benefit from the presence of fast traders.[7] This said, some authors ask if the benefits of ultra-fast price discovery and improved price efficiency justify the social cost of achieving them (Harris, 2013; Stiglitz, 2014). Our setting allows us to shed some light on this issue by comparing the magnitude of the post-ban price efficiency changes to the magnitude of trading cost changes.

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[5] See http://www.sec.gov/rules/final/2011/34-64748fr.pdf

[6] Patterson, S., "Big Slice of the Market is Going 'Naked,'" Wall Street Journal, December 14, 2009.

[7] The empirical studies by Brogaard, Hendershott, and Riordan (2014), Chaboud, Chiquoine, Hjalmarsson, and Vega (2014), and Conrad, Wahal, and Xiang (2015) are supplemented by theory models by Martinez and Roşu (2013), Dugast and Foucault (2014), and Roşu (2014).

Consistent with expectations derived from prior literature, we show that pricing errors increase after the ban. To evaluate the tradeoff between price efficiency and trading costs, we use the Beveridge and Nelson (1981) decomposition, also used by Hasbrouck (1993), to express pricing errors and trading costs on the same scale. We find that the trading cost reduction was substantially larger than the costs potentially arising from the decline in price efficiency. Put differently, only those traders who place an exceptionally high value on trading at efficient prices would view the results of the ban negatively. We note that traders who fall into such a category are likely rare. Those who trade frequently are not likely to be concerned about the zero-mean pricing error. Those who only trade occasionally are mainly interested in long-term returns, whose magnitude dwarfs the pricing error. As such, the concerns regarding the high cost of marginally improved price efficiency appear justified in our setting. We however note that our comparison between trading costs and price efficiency is a novel approach that has not been theoretically modelled. We suggest that the literature will benefit from a model of this tradeoff.

When the SEC first proposed Rule 15c3-5, the Commission did not intend to slow down fast traders. Rather, the SEC was concerned with the possibility of erroneous order submissions that stemmed from the ability of some algorithms to access trading platforms without compliance checks (SEC, 2010b). The commission's intent was generally welcomed by the financial industry; however, investors also raised concerns with possible unintended consequences of the ban. One such consequence was a substantial reduction in speed and profitability of some firms involved in fast trading.[8]

[8] See letters from Lek Securities, Fortis Clearing, ITG, Jane Street Holding, and the American Bar Association. See also: Grant, J., "Is the SEC's 'Naked' Access Ban Anti-Small Business?" Wall Street & Technology, November 17, 2010.

In the post-ban environment, trading firms that had not been registered as broker-dealers had two choices: (i) switch from routing directly to the exchanges to routing through a broker-dealer or (ii) register as broker-dealers. Either alternative was costly. The former put the firms at a speed disadvantage compared to the registered competitors, as they suffered from increased latency associated with the broker-dealer's hardware and software. The latter would lead to significant declines in liquidity rebates. Before the ban, small trading firms would often pool orders through the same broker to qualify for higher volume rebate tiers. Going it alone would make these firms ineligible for rebate levels that their bottom lines depended on.[9] Furthermore, combined technology and compliance costs of the ban were estimated at around $2-3M/year per firm.[10]

Meanwhile, at the time of ban implementation some electronic trading firms (e.g., Getco (now KCG), Tradebot, Virtu) had already been registered as broker-dealers. These firms commonly act as designated market makers (DMMs) or supplemental liquidity providers (SLPs).[11] In the final release of Rule 15c3-5, the SEC stated that the ban should have had minimal to no impact on the order submission and cancellation speeds of these registered firms. Meanwhile, the commission's release acknowledged that latency was likely to increase for non-registered firms that relied on naked access.

As such, the period surrounding ban implementation gives us a unique opportunity to test the premise behind studies that model interactions between fast traders, some of whom may 'snipe' liquidity providers' quotes even if the liquidity providers are also fast. For instance, Budish, Cramton, and Shim (2015) note that because exchange engines execute trades sequentially, the smallest differences in the order submission speed may result in adverse selection of liquidity

[9] Lime Brokerage and Wedbush Securities cited declines in rebate revenue on the scale of 32%.
[10] See letters from BNY ConvergEx Group, Lek Securities, and Wedbush Securities.
[11] See for instance http://www.virtu.com, https://www.kcg.com, https://www.nyse.com/market-model/overview.

providers' quotes.[12] Consequently, slowing down a large group of traders should result in lower adverse selection. If liquidity provision is sufficiently competitive, lower adverse selection should lead to lower trading costs.

We find that the effects of the ban on trading activity are substantial. Quote revisions fall by more than 33%, and trading volume falls by more than 21% (Figure 1). The decline in order submissions is not limited to the top of the book; order activity throughout the book declines by 28%. Notably, displayed liquidity improves after the ban, as quoted spreads decrease by 11.2% and depths improve by 11.3%. Liquidity moves from the outer layers of the book to the inner layers, closer to the inside quotes.

Corroborating the view that the ban lessens the pressure on liquidity providers by reducing the speed with which their orders are picked off, we find that the time between posting of a liquidity providing order and it being hit increases by 27.6%. Consequently, trading costs decline by 8.5% across the board. The decline in trading costs is entirely due to the reduction in the adverse selection component; the realized spread component does not change. Reduced price impacts coupled with increased liquidity are together consistent with the removal of traders relying on a speed advantage to exploit short-lived profit opportunities. Upon their removal, liquidity suppliers are able to narrow quotes, yet prices become less efficient. Specifically, we find that pricing errors increased by 16.1%.

Notably, the magnitude of changes in pricing errors is much smaller than the change in effective spreads. When we juxtapose the two effects, the reduction in trading costs dominates the price efficiency reduction; the effective spreads adjusted for price efficiency decline by 7.4% after

[12] Market making obligations imposed on liquidity providers by some venues further reduce their ability to avoid being adversely selected. For instance, the SLPs on the NYSE must match the best quotes at least 10% of the time. The requirements for the DMMs are even stricter: https://www.nyse.com/markets/nyse/membership-types.

the ban. Our results are robust to controlling for changes in turnover, stock volatility, and VIX. We also rule out a time trend and seasonal effects as causal factors. The results remain robust in a difference-in-differences setting, where we use a matched control sample of Canadian stocks that were not subject to the ban.

The remainder of this paper is organized as follows. Section II discusses recent developments in securities markets and the literature that examines the effects of these developments. The section also provides background on unfiltered market access. Section III presents the data sources, sample selection, and variable construction. The empirical results are in Section IV. Robustness tests are in Section V. The paper concludes with Section VI.

II. Recent developments in securities markets

A. Proliferation of algorithmic and high-frequency trading

Algorithmic trading, the trading practice that heavily relies on computer technology, dominates modern securities markets. Among algorithmic traders, HFTs play an important role. An estimated 40% to 60% of all trades in stocks, derivatives, and foreign currencies is often attributed to them. The growth of HFT has been accompanied by increased regulatory attention, media scrutiny, and academic research. The 2010 SEC concept release on equity market structure (SEC, 2010a), the Foresight Project on the Future of Computer Trading in Financial Markets (BIS, 2011), and the MiFID II proposals are all regulatory efforts to understand the impact of AT and HFT on market quality. In a pioneering academic study, Hendershott, Jones, and Menkveld (2011) find that AT improves several measures of market quality as it reduces spreads and enhances the informativeness of quotes. Hasbrouck and Saar (2013) find similar evidence when they examine low-latency (high-frequency) trading. Namely, they show that such trading is associated with reduced spreads, larger depths and lower short-term volatility. Brogaard, Hendershott, and Riordan (2014) and Chaboud, Chiquoine, Hjalmarsson, and Vega (2014) find that AT and HFT facilitate price discovery and improve price efficiency.

Against the backdrop of the studies that document the positive effects of AT and HFT, other research discusses negative effects. Theory models by Biais, Foucault, and Moinas (2015), Budish, Cramton, and Shim (2015), and Foucault, Kozhan, and Tham (2015) propose that when traders compete on speed, markets may become less liquid as the fastest traders arbitrage stale quotes. Harris (2013) and Stiglitz (2014) question if the ultra-fast price discovery made possible by HFT is socially valuable. Emerging empirical evidence also documents certain negative aspects of fast trading. Boehmer, Fong, and Wu (2014) find that while algorithms improve price efficiency, they also increase volatility, and negatively affect market quality in small stocks. Baron, Brogaard, and Kirilenko (2012) find that HFT firms do not always provide liquidity, and that the most profitable HFT firms are the ones that most aggressively take liquidity. Brogaard, Hendershott, and Riordan (2015) find negative effects from HFT short selling. Examining the Flash Crash of May 2010, Kirilenko, Kyle, Samadi, and Tuzun (2014) show that although fast traders did not trigger the crash, their responses to the event exacerbated volatility. Raman, Robe, and Yadav (2014) and Korajczyk and Murphy (2015) find that modern electronic liquidity providers reduce market participation during stressful times and when dealing with large potentially informed traders. Yet Brogaard et al. (2015) show that HFT provide liquidity during extreme price movements. As such, despite a significant amount of research, the net effect of fast trading on market quality remains an open issue.

Our study is not the only attempt to understand modern fast trading by exploiting changes induced by a regulatory shock. Malinova, Park, and Riordan (2014) examine a regulatory initiative in Canada that resulted in an increase in fees levied on high-frequency traders. As a result, order activity fell and market quality declined. At first glance, these results may appear to be in conflict with ours. However, they are easy to reconcile if one considers the differential impact of the fee change on market makers versus other traders. By effectively taxing traders who submitted the most orders, the Canadian event affected those firms that posted a lot of quotes, a common feature of modern market makers.[13] In contrast, the U.S. ban reduced market access speeds for only some HFT firms. Many unaffected firms are commonly known as liquidity providers. In light of the different trader categories impacted by the Canadian fee versus the ban, our results and those obtained in the Canadian market are complementary.[14]

B. Fast trading and unfiltered market access

Although the intense interest in electronic trading practices is a relatively recent phenomenon, the growth of these practices can be traced back to the market structure developments in the late 1990s. The 1997 changes to order handling rules facilitated the rise of Electronic Communications Networks (ECNs) and ushered in increased competition among trading venues. In this multimarket environment, one dimension along which trading firms now competed was latency. Firms like Automated Trading Desk, Getco, and Tradebot Systems began actively using computer algorithms to trade stocks, ETFs, options, and futures. In the meantime, the new ECNs like Better Alternative Trading Systems (BATS) started catering to high-speed traders by offering dissemination of low-latency data feeds and offering co-location services.

[13] In models by Aït-Sahalia and Saglam (2013), Baruch and Glosten (2013), Xu (2013), and Yueshen (2014) liquidity providers in the modern high-frequency setting routinely submit and cancel large numbers of orders in the course of liquidity provision. Hagströmer and Nordén (2013) find empirical support for this notion.

[14] Our results are also consistent with Easley, Hendershott, and Ramadorai (2014), who show that the 1980-1981 NYSE system upgrade that leveled the playing field between different groups of liquidity providers led to improvements in liquidity.

The rise in computer-driven trading was further facilitated by changes in the order handling rules. The change allowed any broker-dealer, and not just the registered market makers, to enter orders into the NASDAQ SuperMontage.[15] Once this rule went into effect, broker-dealers could create mechanisms for direct (and often unfiltered) market access, by allowing their clients to route orders directly to the exchange server. This "pass-through" use of brokers' licenses allowed for faster order routing and execution, and has bolstered the evolution of the latency competition that characterizes the current trading landscape. In addition, the pass-through system helped trading firms maintain confidentiality of their proprietary trading strategies by allowing for anonymity.

C. The ban on unfiltered (naked) access

On November 30, 2011, the SEC finalized implementation of the market access rule (Rule 15c3-5), also known as *the ban on naked access*, aimed at ending the practice whereby brokers gave some of their customers – mainly the firms using HFT technology – a direct line to exchanges without much pre-trade supervision. The rule mandated that brokers with market access put in place risk management and supervisory systems to help prevent erroneous orders, ensure compliance with regulatory requirements, and enforce credit or capital thresholds. Because these systems introduced additional latency to the flow of orders, they slowed down fast traders.

The term *naked access* refers to the practice of market makers and broker-dealers allowing their clients to directly connect to exchange servers through their order routing platform. Through naked access, traders could potentially bypass some or all of the broker's pre-trade checks, reducing the time from order origination to placement/cancellation. This reduction in latency was especially attractive to high-frequency traders, who make time-sensitive trading decisions.

[15] See NASD amendment to rule 4701 at http://www.gpo.gov/fdsys/pkg/FR-2003-02-19/html/03-3944.htm.

In the U.S., the naked access ban was the first significant cutback to connectivity and speed of fast traders. Aside from the ban, the U.S. has stayed relatively clear of harsh HFT regulation or taxation. Still the issue is on the forefront of regulatory agendas. The Commodities Futures Trading Commission (CFTC) has considered a proposal to limit the maximum number of orders a firm can place in a given amount of time (CFTC, 2013). In Europe, a number of countries have adopted or proposed rules that seek to curb HFT activity. France, for instance, implemented an HFT tax and a transaction tax in August 2012 (Colliard and Hoffmann, 2013). Italy followed suit with an HFT tax in September 2013. There is a broader financial transaction tax being considered by eleven Eurozone countries,[16] along with the pending MiFID II that proposes a raft of measures to slow HFT via Eurozone-wide legislation. These measures include minimum resting times for orders, limits on order-to-trade ratios, and order cancellation charges.[17] Although we do not advocate for any of these initiatives, our results shed new light on the effect of fast trading on market quality.

III. Data, sample, and market quality metrics

A. Sample construction

Our four-month sample period ranges from October 3, 2011 to January 31, 2012, straddling the November 30 implementation of the naked access ban. We exclude half-days adjacent to holiday closures. Speed traders are more active in frequently traded stocks, therefore the effects of the ban on these stocks may be different from the small, less frequently traded ones. To account for this possibility, we select three groups of stocks using the following three-step procedure. First, we double-sort all NYSE and NASDAQ common equity securities (CRSP share codes 10 and 11,

[16] Thomas, L., "EU's Moscovici sees financial transaction tax in place early 2017," Reuters, July 8, 2015 (http://goo.gl/w5F7O5).

[17] See http://marketsmedia.com/hft-makes-last-stand-in-europe-as-ftt-and-mifid-ii-edge-closer/ for a discussion of the proposed regulations.

exchange codes 1 and 3) by market capitalization and trading volume in November 2011.[18] Second, we assign a market cap rank and a volume rank to each stock and sort the stocks by the sum of the ranks. Finally, we assign 50 stocks with the highest combined ranks to group 1 (largest and most active), stocks with combined ranks from 500 to 550 to group 2, and stocks with combined ranks from 1,000 to 1,050 into group 3 (small and least active).

We obtain trade and quote data from the TAQ database and remove trades that occur outside of regular trading hours and trade records with zero prices or zero volumes. We further eliminate quotes with bids that are greater than offers. We also use the filtering techniques suggested by Holden and Jacobsen (2014). These include (i) interpolating the times of trades and the times of NBBO quotes within a second, (ii) adjusting for withdrawn quotes, and (iii) deleting locked and crossed NBBO quotes as well as trades reported while the NBBO is locked or crossed.

We also obtain order submission, execution, and cancellation data from NASDAQ's TotalView-ITCH. These data allow us to examine changes throughout the limit order book.

B. Market quality metrics

We measure liquidity using conventional metrics such as quoted spreads/depths and effective/realized spreads. We define the percent quoted spread at time t, QSP_t, as the difference between the NBBO quotes divided by the prevailing quote midpoint:

$$QSP_t = (NBBO\ Ask_t - NBBO\ Bid_t)/mid_t, \tag{1}$$

where

$$mid_t = \frac{NBBO\ Ask_t + NBBO\ Bid_t}{2}. \tag{2}$$

[18] We drop class A shares of Berkshire Hathaway that trades around $115,000 in November 2011 to prevent them from skewing the distribution of prices.

Quoted depth, $DEPTH_t$, is the average number of shares offered at the best bid and ask. We time-weight quoted spread and depth measures.

Effective spread, ESP_t, a measure that captures the cost of a liquidity-demanding transaction executed at time t, is computed as twice the difference between the NBBO midpoint prevailing at the time of the trade and the trade price, p_t, multiplied by an indicator variable q_t that equals one for buyer-initiated trades and negative one for seller-initiated trades. We use the Lee and Ready (1991) algorithm without lagging quotes to sign trades.[19] Effective spread measures are scaled by the quote midpoint (transforming them into percent measures) as follows:

$$ESP_t = 2q_t(p_t - mid_t)/mid_t \tag{3}$$

We decompose ESP_t into the adverse selection component measured by price impact, $PRIMP_t$, and the realized spread component, RSP_t. The former is computed as twice the signed difference between a future quote midpoint, mid_{t+n}, and the quote midpoint at the time of the trade, scaled by the prevailing quote midpoint. The adverse selection component represents the portion of the spread that liquidity suppliers potentially lose to the informed traders.

$$PRIMP_t = 2q_t(mid_{t+n} - mid_t)/mid_t \tag{4}$$

The realized spread component, RSP_t, is computed for each trade as the difference between the effective spread and the corresponding price impact. This component represents liquidity providers' income net of adverse selection costs. We volume-weight the ESP_t and its components.

To gauge price efficiency, we calculate pricing errors as in Hasbrouck (1993). We start with an assumption that the observed log-prices follow a random walk with two components:

$$p_t = m_t + s_t, \tag{5}$$

[19] For a sample of trades in 2011, Chakrabarty, Pascual, and Shkilko (2015) show that the Lee-Ready algorithm performs well when the quotes are not lagged.

where m_t is the efficient price (the expectation of price conditioned on all available information at time t), and s_t is a deviation of the transaction price from the efficient price. Next, we estimate the following vector autoregression (VAR) system with five lags:

$$r_t = a_1 r_{t-1} + a_2 r_{t-2} + \cdots + b_1 x_{t-1} + b_2 x_{t-2} + \cdots \ v_{r,t}$$

$$x_t = c_1 r_{t-1} + c_2 r_{t-2} + \cdots + d_1 x_{t-1} + d_2 x_{t-2} + \cdots \ v_{x,t}, \tag{6}$$

where r_t is the difference in log-prices; x_t is the column vector of three signed trade-related variables: a signed trade indicator, signed trading volume, and signed square root of trading volume that allows for a nonlinear relation between returns and trades, and $v_{r,t}$ and $v_{x,t}$ are zero-mean serially uncorrelated disturbances. The results are robust to using ten lags. Next, the VAR system is inverted to obtain the vector moving average (VMA) representation:

$$r_t = a_0^* v_{r,t} + a_1^* v_{r,t-1} + \ldots + b_0^* v_{x,t} + b_1^* v_{x,t-1} + \cdots$$

$$x_t = c_0^* v_{r,t} + c_1^* v_{r,t-1} + \ldots + d_0^* v_{x,t} + d_1^* v_{x,t-1} + \cdots. \tag{7}$$

Using the return equation from the VMA model, we then obtain an expanded representation of the pricing error:

$$s_t = \alpha_0 v_{r,t} + \alpha_1 v_{r,t-1} + \ldots + \beta_0 v_{x,t} + \beta_1 v_{x,t-1} + \cdots, \tag{8}$$

where $\alpha_j = -\sum_{k=j+1}^{\infty} a_k^*$ and $\beta_j = -\sum_{k=j+1}^{\infty} b_k^*$. The pricing error variance is then computed as:

$$\sigma_s^2 = \sum_{j=0}^{\infty} \left[\alpha_j \ \beta_j\right] Cov(v) \begin{bmatrix} \alpha_j \\ \beta_j \end{bmatrix}. \tag{9}$$

As in Hasbrouck (1993), we exclude overnight returns. We also bunch consecutive trades executed at the same price. Following Boehmer and Wu (2012), we scale pricing error variances to control for cross-sectional differences in return variance. Specifically, for each stock-day, we compute the ratio of the standard deviation of s_t to the standard deviation of the log-price, σ_s/σ_p, and use this ratio to proxy for the pricing error.

In addition to liquidity and trading cost metrics, we compute daily share turnover, $TURN$, as the ratio of daily share volume scaled by the number of shares outstanding. We estimate daily stock volatility, $VOLAT$, as the daily high minus low price, scaled by the high price, as defined in Hendershott, Jones, and Menkveld (2011).

IV. Results

A. Market activity and displayed liquidity

In Table 1, we report summary statistics and ban-related changes in market activity and displayed liquidity variables. For the entire sample, TAQ quotes average 602,980 per stock-day (column 1). This translates into nearly 26 quote updates every second. We note that the $QUOTES$ statistic is rather skewed, and its median value is 239,410 (column 3). In the remainder of this discussion, we focus on the medians. In the pre-ban period, the median number of daily quotes is 282,420 (column 5) falling to 178,900 after the ban (column 6). In percentage terms, this drop is 36.7% and is statistically significant (column 8).

[Table 1]

The number of trades also drops; from the median of 9,170 in the pre-ban period to 8,080 post-ban, an 11.9% decline. As we mention earlier, reports suggest that naked access was associated with 38% of trading activity in the pre-ban period. Our findings imply that nearly one third of this activity vanished after the ban.

Hendershott, Jones, and Menkveld (2011) use order submissions normalized by trades as a proxy for electronic trading. Following their reasoning, we ask whether the ratio of order submissions to trades changes post-ban. In this section, we use TAQ to examine the orders that establish the top of the book at each of the 13 market centers that submit quotations to the National

Market System.[20,21] In a subsequent section, we examine the deeper layers of the order book using ITCH data. We compute the quote-to-trade ratio, QTR, as the ratio of the top of the book quotes and trades. The QTR statistics reported in column 1 of Table 1 indicate that on average there are 28.61 quotes per trade in the full sample, with the ratio declining from a median of 29.89 before the ban to 23.90 after the ban, a 20.0% decline.

Having shown that quote and trade activity undergoes significant changes in the wake of the ban, we ask whether displayed liquidity, measured by quoted spread and quoted depth, is also affected. We find that the median percentage quoted spreads decline by 11.3%, from 18.84 bps in the pre-ban period to 16.71 bps post-ban. Meanwhile, median depth increases by 4.7%. In addition, intra-day price volatility declines by 24.2%.

Post-ban changes discussed so far are derived from univariate tests. To check robustness of these results, in columns 9 and 10, we report coefficient estimates $\beta_1$and β_2 from the following regressions:

$$DEPVAR_{it} = \alpha_i + \gamma_t + \beta_1 BAN_t + \delta_1 IPRICE_{it} + \delta_2 MCAP_{it} + \varepsilon_{it} \tag{10}$$

and

$$\begin{aligned} DEPVAR_{it} = \alpha_i + \gamma_t + \beta_2 BAN_t + \delta_1 IPRICE_{it} + \delta_2 MCAP_{it} \\ + \delta_3 VOLAT_{it} + \delta_4 TURN_{it} + \varepsilon_{it}, \end{aligned} \tag{11}$$

where $DEPVAR$ is one of the following variables: $QUOTES$, $TRADES$, QTR, QSP, $DEPTH$, and $VOLAT$; BAN is an indicator with the value of 0 in the pre-ban period and 1 in the post-ban period,

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[20] These market centers include BATS, BATS Y, CBOE, Chicago Stock Exchange, EDGA, EDGX, NASDAQ, NASDAQ OMX BX, NASDAQ OMX PSX, National Stock Exchange, the NYSE, the NYSE AMEX, and the NYSE Arca. An additional market, FINRA's ADF does not display quotes but reports trades during our sample period.

[21] At the time of our study, TAQ does not report odd-lot trades. In addition, odd-lot orders that improve existing quotes are not included in the quote montage, whereas odd-lot orders that add depth at the existing displayed quote may be included (O'Hara, Yao, and Ye, 2014). As such, both of our $QUOTES$ and $TRADES$ statistics are downward-biased. Nonetheless, the conclusions in this section are similar to those obtained using the full limit order book from ITCH as reported in Section IV.F.

$IPRICE$ is the inverse of the stock price, $MCAP$ is market capitalization, $VOLAT$ is volatility, and $TURN$ is turnover. Because of turnover's correlation with trading volume, we exclude $TURN$ from regressions that have $TRADES$ and QTR as dependent variables. We do not include $VOLAT$ in the regression with $VOLAT$ as the dependent variable. We log-transform all non-dummy variables (dependent and independent) for two reasons. First, the transformation brings the distributions of the variables closer to normal. Second, the coefficient estimates in log-transformed models can be interpreted as percentage changes allowing for an easy comparison with the statistics in column 8. The regressions are estimated with stock fixed effects and a trend variable γ_t. Standard errors are robust to cross-section and time-series heteroskedasticity and autocorrelation. The results are also robust to clustering standard errors by firm and day.

Generally, the list of regressors in our models is similar to that in Hendershott, Jones, and Menkveld (2011), who use the same controls, including a trend variable. Controlling for the trend allows us to account for the possibility that the variables of interest change continuously over time rather than as a result of the ban. This control is important given the global economic environment at the time of our study, particularly the crisis in Europe. Although the Eurozone approved a second Greek bailout in July 2011, the nervousness about the sovereign debt crisis continued until October when European leaders reached a final agreement on the crisis action plan and agreed to a 50% haircut on Greek debt. The following months of November and December were relatively devoid of major global economic events, with signs of trouble arising again in January 2012 when Standard & Poor's downgraded France and the European Financial Stability Facility.

The absence of major economic news in November and December 2011 supports the notion that the BAN dummy captures the effect of the ban rather than confounding macro events. Further, controlling for the trend allows us to account for the possibility that economic conditions change gradually during the sample period, and this change (and not the ban) causes the effects observed herein. If this were the case, the trend variable should absorb the effect, leaving the BAN variable insignificant. Moreover, had economic forces been responsible for most of our results, we would have observed a reversal of market quality metrics in January 2012, when the macroeconomic conditions worsened again. Additionally, the results hold when we control for VIX in eq. 11. Finally, to further assuage concerns about the event effect measurement, we examine market quality in a matched sample of Canadian stocks at the time of the ban implementation. The results discussed in Section V B. confirm our main findings.[22]

Regression results support our earlier findings. The coefficient estimates from eq. 10 reported in column 9 of Table 1 show that quote submissions decline by 33.1% post-ban. We note that our univariate results suggest that the change in quote intensity is accompanied by changes in other market activity variables such as trade intensity and volatility. The simultaneous nature of these changes makes separating the causal effects difficult, but the regression framework allows us to account for the possibility that some of the change in quoting activity may be driven by changes in trading activity and volatility. To do so, eq. 11 adds controls for these two variables. The BAN coefficient in the $QUOTES$ regression retains its negative sign, and its magnitude decreases from 33.1% (column 9) to 25.6% (column 10). As such, the intensity of BBO quote changes drops after the ban even after controlling for trading volume and volatility.

Our univariate results are confirmed in the multivariate setting for all variables of interest. The coefficient estimates in column 10 indicate that, after the ban, trading activity declines by 11.6%, QTR declines by 15.2%, quoted spread declines by 9.6%, and depth increases by 11.3%.[23]

[22] Although the Canadian data allow us to carry out a difference-in-difference analysis, these data are daily. As such, we do not use them in the main sample analysis that relies on intraday data.
[23] For several variables ($TRADES$, QTR, and $DEPTH$), coefficient magnitude in columns 9 or 10 does not always match the magnitude of the univariate statistics in column 8. This difference arises for two reasons: the effect of

One of our goals is to examine changes in adverse selection resulting from the ban. Past literature often measures adverse selection as the change in the quote midpoint five minutes after the trade – a five-minute price impact, $PRIMP$. Five minutes is however an eternity in modern markets, with Brogaard, Hendershott, and Riordan (2014) and Conrad, Wahal, and Xiang (2015) showing that prices adjust to new information in a matter of seconds. The level of granularity of TAQ data allows us to compute one-second $PRIMP$s. To check that the number of quote updates is sufficient for such computation in all stock groups, we compute the quotes-per-second, QPS, statistics (Table 2). In all stock groups, there are several quote updates per second, ranging from the mean of 61.43 in the large stocks to 4.20 in the small ones (column 1). In all groups, QPS declines after the ban, yet continues to be sufficient for a one-second $PRIMP$ computation.

[Table 2]

While $PRIMP$ statistics are based on quote updates, the concept of the realized spread, RSP, relies on turning around liquidity providers' postitions. As such, trade frequency is more relevant for RSP calculations than quote frequency. In Table 2, we report that large stocks have a mean of 3.33 trades per second, TPS, making one-second RSP calculation plausible. In the meantime, medium and small stocks have TPS of 0.42 and 0.16; therefore, in the subsequent analysis we report 3-second RSPs for medium stocks, and 7-second RSPs for small stocks.

B. Trading cost and its components

Elimination of (at least some of) the fast traders after the ban may lead to a decline in adverse selection. Lower adverse selection should in turn result in lower trading costs. Table 3

controls, and because column 8 reports percentage changes in medians, whereas regression coefficients capture changes in means.

presents results consistent with these expectations. In the univariate setting, we observe that effective spreads fell from 7.82 bps to 7.11 bps, a 9.1% decline (column 8). Price impacts declined from 1.88 bps to 1.54 bps, an 18.1% decline. Supportive of these estimates, the regression results in column 9, indicate that effective spreads fell by 8.5%, and price impacts declined by 20.2%. Upon adjustment for turnover and volatility (column 10), the decline in effective spreads is 4.7%, and the decline in price impacts is 16.9%. The declines are observed in all three stock groups.

[Table 3]

While the ban is accompanied by significant changes in trading costs and price impacts, changes in realized spreads are statistically indistinguishable from zero in all stock groups (Table 4).[24] As such, it appears that liquidity providers reduce the cost of liquidity commensurately to the reduction in adverse selection. This result is expected in a competitive market, where competition between liquidity providers leads to a full transfer of cost reductions to the liquidity demanders.[25]

[Table 4]

C. Price efficiency

Martinez and Roşu (2013), Dugast and Foucault (2014), and Roşu (2014) model markets in which fast traders promote price efficiency. In turn, Brogaard, Hendershott, and Riordan (2014) and Chaboud, Chiquoine, Hjalmarsson, and Vega (2014) show that fast trading facilitates price discovery. Given these findings and the fact that the ban results in smaller price impacts, we suggest that information may not be incorporated into prices as efficiently as before. To examine

[24] As discussed earlier, we compute realized spreads separately for each stock group to account for different trading frequency. As such, Table 4 does not contain a statistic for the entire sample.
[25] We note that an additional effect of the ban may have stemmed from a reduction in counterparty risk. Yet if this reduction were to significantly influence trading costs, we should have observed effective spreads decline by the amount larger than the price impact.

this possibility, we compute Hasbrouck's (1993) pricing errors that measure the deviation of transaction prices from the efficient prices.

The results are reported in Table 5. In the univariate setting, we find that pricing errors, σ_s/σ_p, increase from 0.043 in the pre-ban period (column 5) to 0.051 in the post-ban period (column 6), implying an 18.6% decline in price efficiency. This result is consistent with the notion that the ban negatively affects the price discovery process. The coefficient estimates in columns 9 and 10 confirm these results, suggesting that the pricing error increases by 16.1% and 8.6%, respectively. The results are similar for all three stock groups.

[Table 5]

D. Adjusted effective spreads

If one compares our results for the percentage change in pricing errors to those for the percentage change in trading costs, it may be tempting to conclude that the latter has a less important effect. Indeed, regression results in Tables 3 and 5 seem to indicate that pricing errors increase nearly twice as much as trading costs decrease (respectively, 16.1% v. 8.5% in columns 9). We caution against comparing the magnitude of changes using these percentage figures. First, pricing errors have a much lower starting value than the effective spreads, therefore a change of a certain magnitude in the former will appear larger in percentage form. Second, pricing errors are expressed as a percentage of price volatility, whereas trading costs are a percentage of the midpoint. To allow for proper comparison, in this section we follow the approach of Beveridge and Nelson (1981), also used by Hasbrouck (1993), to extract s_t from the price equation (eq. 5). We then rescale s_t by the contemporaneous midpoint, making it comparable to the effective spread metric. Finally, we augment the effective spread by twice the absolute value of the rescaled s_t.

Using the resulting adjusted effective spread metric, $AESP$, we ask if an investor who equally values low trading costs and efficient prices benefits from the ban's effects. We note that a direct comparison between trading costs and price efficiency has not been modeled theoretically, and therefore $AESP$ should be interpreted as an empirical estimate only. More specifically, it is not clear if investors assign the same value to one unit of price efficiency and one unit of trading costs. Furthermore, some traders may have very limited interest in price efficiency. For instance, since s_t has a mean of zero, its effect will likely cancel out over time for investors who trade often, such as market makers and high-frequency arbitrageurs. As such, $AESP$ may be more of interest to investors who only trade occasionally. Yet for such investors the magnitude of the pricing error may be trivial compared to the expected long-term return. With this in mind, we show that $AESP$ declines by 7.4% after the ban (column 9 of Table 5), a magnitude that is comparable to the 8.5% change in the unadjusted ESP reported earlier. As such, the economic effect of higher post-ban s_t is small, and an investor must value price efficiency exceptionally high to view the outcome of the ban negatively.[26]

E. Trade size categories

Next, we examine changes in trade sizes after the ban. We are interested in this issue for two reasons. First, small trades often (although not always) result from orders submitted by high-frequency traders (O'Hara, Yao, and Ye, 2014). As such, a disproportionate post-ban decline in small trades may be consistent with the decline in HFT. Second, with inside depth increasing post-ban, we may observe a shift to larger trades. Such a shift may be indicative of improved trading opportunities for large investors, who are naturally interested in trading large volumes.

[26] We note that $AESP$ does not allow us to gauge the ban's effect on traders who had relied on naked access, because the metric does not quantify the cost of the lost speed advantage.

In Table 6, we split trades into eight size categories: [100; 200), [200; 300), [300; 400), [400; 500), [500; 2,000), [2,000; 5,000), [5,000; 10,000), and 10,000+ shares. The majority of trades, 66.4%, are small and fall into the [100; 200)-share range (Panel A). Expectedly, the proportion of small trades is highest in small stocks. The next size category, [200-300)-share trades, captures an additional 14.3% of trades in the full sample. The remaining six trade size categories capture less than 20% of trades across all firms.

[Table 6]

The two smallest trade size categories decline the most after the ban, over 24% for large and medium stocks and over 17% for small stocks (Panel B). In the meantime, large trades (greater than 2,000 shares) become somewhat more common. Finally, in Panel C, we examine trade-size-related changes in execution costs. The reduction in trading costs is concentrated in small and medium-sized trades and is observed across all three firm size categories. Large trades experience reduction in trading costs only in large stocks.

F. INET order book analysis

In the previous sections, we show that the number of quotes declines after the ban. We note that quotes obtained from TAQ are limited to the top-of-the book orders aggregated across 13 sample markets. In this section, we extend our analysis to assess the effect of the ban on orders located deeper in the book. With high speeds of quote cancellations in modern markets, understanding the depth beyond the best quotes is necessary to gauge stock market liquidity (Jain, Jain, and McInish, 2014). Depth beyond the best quotes is particularly important for large investors, whose desired order quantities often exceed the depth at the best bid and offer.

To shed light on this issue, we use order book data from NASDAQ's TotalView-ITCH (ITCH) to compute the number of orders and trades. Table 7 shows that ITCH orders and trades decline after the ban. The decline is commensurate with the dynamics that we report in the earlier tables. More specifically, post-ban changes reported in column 8 suggest that in the full sample the number of ITCH orders declines by 32.2%, and the number of trades declines by 24.0%. We obtain similar results for all stock groups.

[Table 7]

The results in Table 7 show that the ban affected order submissions throughout the book and not just its top layer. Next, we enhance this analysis and ask if (and how) the ban affected the deeper layers of the book. To do so, we build the limit order book for each stock each day. When building the book, we record orders according to price-time priority. We then allow executions and cancellations, full or partial, to remove shares from the book. We build order books for each stock-day and record the incremental share depth at the following distances away from the inside quotes: 0.119%, 0.166%, 0.6%, 0.833%, 2.38%, 3.333%, 11.9%, 16.67%, 25%, 50%, 60%, 70%, 80%, 90%, and 100%.[27] These distances represent price percentages away from the inside quotes and are similar in nature to those used by Boehmer, Saar, and Yu (2005). For instance, a 0.119% cutoff for a $42 stock (about the average price of our sample stocks) captures depth up to $0.05 away from the best quote.

Table 8 reports ban-induced differences in incremental depth (that is additional depth available by moving from one book layer to another). Given that the first and the last half hours may be affected by the exchange opening and closing procedures, we aggregate the book snapshots

[27] For sell limit orders, 100% away from the best offer is not the highest possible price. On rare occasions, we observe sell limit order prices that are more than twice the prevailing best offer. Such occasions are exceptionally rare, and we do not report them.

in two ways: (i) for the 10:00 a.m. – 3:30 p.m. interval (columns 1-3) and (ii) for the full day (columns 4-6). Overall, after the ban, the inner layers of the book thicken, and the outer layers become thinner. For instance, the layer between the inside and 0.119% away from the inside thickens by 28.8% (column 3), whereas the layer between 50% and 60% away from the inside becomes thinner by 19.6%. As such, the ban appears to have refocused liquidity supply to price points closer to the midpoint. In the Appendix Table A1, we report book depths for the three size-volume stock groups. These results corroborate the full sample findings reported here.

[Table 8]

Our argument relies on the premise that by reducing traders' speed of market access, the ban conceivably changed fast traders' reaction times, thereby lowering liquidity providers' adverse selection costs. ITCH data allow us to examine this premise. In Table 9, we report the time to a trade execution after (i) an order posts on the side of the book that provides liquidity for the execution (e.g., for a seller-initiated trade, we look at the bid side of the book), and (ii) any book event (i.e., submission, cancellation, modification on either side of the book). We reason that an order posted on the liquidity providing side may either become a target of fast traders or may provide them with new information. Similarly, traders may derive information from any change in the book – the premise for the second (broader) measure. Our results indicate that trader reactions slow down after the ban. Specifically, reaction time to the liquidity providing orders increases from 520 milliseconds (ms) to 842 ms, and reaction time to a book event increases from 221 ms to 344 ms. Controlling for the overall change in turnover and volatility in column 5, we show that reaction times to the liquidity providing orders increase by 27.6% and reactions to book events lengthen by 27.0%.[28] The results are similar for all three size-volume groups.

[28] In untabulated results, we control for the number of ITCH order submissions instead of turnover. Our results are similar.

[Table 9]

V. Robustness

A. Seasonal effects

Although we control for the time trend in regressions, our results may be affected by seasonality due to the year-end effects. To examine this possibility, we use the difference-in-differences (DID) approach to compare our 2011-2012 sample period to an earlier period that includes the same calendar months, October 2010 through January 2011.

For the DID analysis, we construct a matched sample of stocks. We begin by filtering the CRSP universe for October 2010 to January 2011 in the same way as the main sample is filtered. We then follow Chakrabarty, Moulton, and Shkilko (2012) and construct a matched sample based on market capitalization, price, and volume. We calculate the following matching error for each 2011-2012 stock i and each 2010-2011 stock j:

$$matching\ error = \left|\frac{MCAP_i}{MCAP_j} - 1\right| + \left|\frac{PRC_i}{PRC_j} - 1\right| + \left|\frac{VOL_i}{VOL_j} - 1\right|, \tag{12}$$

where $MCAP$ is the stock's average daily market capitalization, PRC is the stock's average daily closing price, and VOL is the stock's average daily share volume. For each stock from our 2011-2012 sample, we select a stock from the 2010-2011 sample that has the lowest matching error and subsequently remove the selected 2010-2011 stock from the list of potential matches. We allow stocks to match themselves, which happens when a given stock's characteristic does not change significantly between the two periods. Our matching procedure is successful; all three matching variables are statistically indistinguishable between the 2010-2011 and 2011-2012 samples. The matching scores are available upon request.

Next, we compute all of the market quality metrics (e.g., effective spreads, price impacts, pricing errors, adjusted effective spreads) for our matched sample in the October 2010 through January 2011 period. For this matched sample, we also compute all of the independent variables used in eq. 11 and re-estimate the equation upon differencing the variables. The results (available upon request) corroborate our earlier conclusions. The estimated coefficients on BAN indicate that the implementation of the ban in November 2011 served to reduce execution costs (both unadjusted and adjusted for pricing errors) via a reduction in adverse selection, and that this result is not driven by seasonality.

B. Global economic effects

Our discussion in Section IV.A suggests that the November 30th ban does not coincide with any macroeconomic events that could have had a similar effect on market quality. To avoid relying solely on this discussion, we report that our results are robust to controlling for VIX – the variable that is likely to capture economy-wide processes. In this section, we further enhance this analysis by comparing pre- and post-ban market characteristics in the U.S. to those in Canada. Canadian market structure during our sample period is rather similar to that in the U.S., with high levels of high-frequency trading. Yet Canadian securities trading was not subject to the naked access ban. If our results are attributable to the general improvement in global economic conditions or a global event that resulted in significant market quality improvements, both U.S. and Canadian stocks should be affected. If so, compared to their Canadian counterparts in the DID framework, the U.S. stocks should not exhibit notable changes around the ban. In contrast, statistically significant DID estimates would indicate that the change in market quality is not attributable to changes in global economic conditions.

In Table 10, we report the results from the DID analysis of a matched sample of non-cross-listed Canadian stocks. To identify matches, we obtain a list of stocks from the Canadian Financial Markets Research Centre (CFMRC) database.[29] Next, we collect a list of cross-listed firms from the September 2011-January 2012 eReviews published by the Toronto Stock Exchange. Cross-listed stocks trade both in Canada and the U.S. (68% of volume in an average cross-listed stock executes in the U.S. markets), and their Canadian trading may therefore be affected by the ban. We exclude such stocks. Following Skjeltorp, Sojli, and Tham (2014), we match the remaining Canadian stocks to the U.S. sample stocks by market capitalization, price (adjusted for the exchange rate), and the percent quoted spread in September 2011.[30] Canadian stocks are generally smaller than the U.S. stocks. As such, capitalizations match well for the medium and small U.S. stocks, but not for the large stocks. Prices and spreads match well for all three stock size groups.

Given that our Canadian equities data are at the stock-day level, we follow Skjeltorp, Sojli, and Tham (2014) who use daily metrics in a setting similar to ours. From CRSP, we derive the following daily metrics for the sample of U.S. stocks: the number of shares traded ($VOLUME$); the end-of-day quoted spread, QSP, computed as the difference between the closing ask and bid prices divided by the midpoint of these prices; the Amihud's (2002) illiquidity measure, $ILLIQ$; and the spread estimator of Corwin and Schultz (2012), $CSSP$. To estimate Canadian equivalents of the abovementioned metrics, we use the CFMRC database. Table 10 reports coefficient estimates on the BAN dummy from the fixed effects regressions of the differences in the abovementioned metrics between the sample stocks (the treatment sample) and their Canadian matches (the control sample). Among the dependent variables are the differences in previously defined regressors: $IPRICE$, $MCAP$, $VOLAT$, and $TURN$.

[29] http://clouddc.chass.utoronto.ca/ds/cfmrc.

[30] The results are robust to matching on market capitalization, price, and volume.

[Table 10]

The results corroborate our earlier findings. Trading activity declines and liquidity improves for the U.S. sample after the naked access ban relative to the Canadian control sample. These results are robust to volatility and volume controls and are observed for firms of all sizes. One exception is the $ILLIQ$ metric in regressions with volatility and turnover controls in medium and small stocks. Overall, the results support the notion that liquidity improvements associated with the naked access ban are not driven by global macroeconomic conditions.

VI. Conclusions

After several years of spectacular growth, some types of fast trading in the U.S. appear to have hit a speed bump in the form of the SEC ban on naked market access. Prior to the ban, broker-dealers allowed some of their clients, a number of whom were HFT firms, to directly route orders to exchanges using the broker's MPID. Naked access was particularly attractive to traders generating, who rely on short-lived informational advantage. According to theory models, presence of such traders may increase adverse selection costs for liquidity suppliers.

We contrast various measures of market quality before and after the ban and document a significant decline in order submissions and cancellations. The number of trades also declines, and trader reactions to new liquidity providing orders slow down. In spite of the reduction in market activity, there is no adverse effect on liquidity. In fact, liquidity improves, with quoted spreads declining, and liquidity moving from the deeper layers of the book closer to the inside quotes. More importantly, the post-ban period shows a significant reduction in trading costs. The effects are robust across firms of different sizes and after controlling for volatility, a time trend, and seasonality. Using a matched sample of Canadian stocks, we confirm these findings in a difference-in-differences setting.

When we decompose trading costs into the adverse selection component and the net benefit to liquidity providers, we find that the reduction in trading costs was driven entirely by the reduction in adverse selection. Meanwhile, removal of naked access for traders, whose order flow was based on short-lived information is associated with lower price efficiency. When we juxtapose the decline in trading costs and price efficiency, the former effect dominates. As such, a liquidity-demanding trader who prefers to trade at the lowest trading cost, at the most efficient price, and does not rely on ultra-fast market access appears to have benefited from the effects of the ban. These findings corroborate concerns expressed by Harris (2013) and Stiglitz (2014), who suggest that modern levels of price efficiency may not be justifiable given their cost.

Our results bring new evidence to the ongoing debate on restrictions to fast trading. The effect of this activity is of current interest to financial regulators around the globe. Several countries – including Canada, France, Germany, Italy, and the U.K. – have implemented or are considering regulation that curbs some aspects of HFT. In the U.S., the CFTC (2013) has also considered instituting curbs on fast trading. We believe that our results may be of relevance to such regulatory initiatives.

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Table 1. Quote, trade, and displayed liquidity dynamics

Panel A of the table reports summary statistics for the 150 sample stocks from October 2011 through January 2012, spanning the naked access ban implementation. The pre- and post-implementation statistics are reported in Panel B. Event coefficients from the multivariate regression models discussed below are reported in Panel C. $QUOTES$ and $TRADES$ are the number of daily quotes and trades, in thousands. QTR is the quote-to-trade ratio. QSP is the quoted spread computed as the difference between the NBBO offer and bid quotes scaled by the prevailing NBBO midpoint. $DEPTH$, is the average number of shares offered at the best bid and ask, in round lots. We time-weight quoted spread and depth measures. All of the abovementioned metrics are computed on the intraday basis and then converted into daily aggregates. $VOLAT$ is the daily high minus low stock price, scaled by the high price. In columns 1 through 6, we report cross-sectional summary statistics for the full sample and the medians for the pre-ban and post-ban periods. In columns 7 and 8, we report the differences between the post- and pre-ban statistics, respectively, in original units and as proportion of the pre-ban value. The asterisks *** and * in column 8 denote 1% and 10% statistical significance of the difference in medians between the pre- and post-ban periods according to the Wilcoxon rank sum test. Columns 9 and 10 contain coefficient estimates β_1 and β_2 from the following regression models:

$$DEPVAR_{it} = \alpha_i + \gamma_t + \beta_1 BAN_t + \delta_1 IPRICE_{it} + \delta_2 MCAP_{it} + \varepsilon_{it}$$

and

$$DEPVAR_{it} = \alpha_i + \gamma_t + \beta_2 BAN_t + \delta_1 IPRICE_{it} + \delta_2 MCAP_{it} + \delta_3 VOLAT_{it} + \delta_4 TURN_{it} + \varepsilon_{it},$$

where $DEPVAR$ is one of the quote and trade variables identified above, BAN is the indicator variable with the value of 0 in the pre-ban period and the value of 1 in the post-ban period, $IPRICE$ is the inverse of the stock price, $MCAP$ is market capitalization, $VOLAT$ is volatility, and $TURN$ is turnover. We exclude the $TURN$ regressor from regressions with $TRADES$ and QTR as dependent variables. We exclude the $VOLAT$ regressor from the regression with $VOLAT$ as the dependent variable. All non-dummy variables are log-transformed so that the coefficients may be interpreted as percentage changes in the dependent variable. The regressions are estimated with stock fixed effects and a trend variable, γ_t. Standard errors are robust to heteroskedasticity and autocorrelation. The results are robust to using standard errors that are double-clustered by stock and day. The asterisks *** in columns 9 and 10 denote statistical significance of β_1 and β_2 coefficient estimates at 1%.

	Panel A: summary statistics				Panel B: median changes around the ban				Panel C: regression	
	mean	25%	median	75%	pre	post	post-pre	%	β_1	β_2
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
QUOTES, '000	602.98	113.97	239.41	1,024.95	282.42	178.90	-103.52	-0.367***	-0.331***	-0.256***
TRADES, '000	30.53	4.25	9.12	49.58	9.17	8.08	-1.09	-0.119***	-0.219***	-0.116***
QTR	28.61	21.02	27.01	35.25	29.89	23.90	-5.99	-0.200***	-0.118***	-0.152***
QSP, bps.	20.16	6.35	17.68	31.90	18.84	16.71	-2.13	-0.113***	-0.112***	-0.096***
DEPTH, 100 sh.	18.70	2.72	3.64	10.76	3.62	3.79	0.17	0.047*	0.113***	0.113***
VOLAT, %	2.60	2.01	2.55	3.47	2.93	2.22	-0.71	-0.242***	-0.193***	-0.111***

Table 2. Market activity by firm size

Panel A reports summary statistics for three firm size groups: large, medium, and small. The pre- and post-ban statistics are in Panel B. Event coefficients from the regression models discussed below are reported in Panel C. QPS and TPS are the number of quotes and trades per second. All metrics are computed on the intraday basis and then converted into daily aggregates. In columns 1 through 6, we report cross-sectional summary statistics and the medians for the pre-ban and post-ban periods. In columns 7 and 8, we report differences between the post- and pre-ban statistics, respectively, in original units and as proportion of the pre-ban value. The asterisks *** in column 8 denote 1% statistical significance of the difference in medians between the pre- and post-ban periods according to the Wilcoxon rank sum test. Columns 9 and 10 contain coefficient estimates β_1 and β_2 from the following regression models:

$$DEPVAR_{it} = \alpha_i + \gamma_t + \beta_1 BAN_t + \delta_1 IPRICE_{it} + \delta_2 MCAP_{it} + \varepsilon_{it}$$

and

$$DEPVAR_{it} = \alpha_i + \gamma_t + \beta_2 BAN_t + \delta_1 IPRICE_{it} + \delta_2 MCAP_{it} + \delta_3 VOLAT_{it} + \delta_4 TURN_{it} + \varepsilon_{it},$$

where $DEPVAR$ is either QPS or TPS, BAN is the indicator variable with the value of 0 in the pre-ban period and the value of 1 in the post-ban period, $IPRICE$ is the inverse of the stock price, $MCAP$ is market capitalization, $VOLAT$ is volatility, and $TURN$ is turnover. We exclude the $TURN$ regressor from the TPS regressions. All non-dummy variables are log-transformed so that the coefficients may be interpreted as percentage changes in the dependent variable. The regressions are estimated with stock fixed effects and a trend variable, γ_t. Standard errors are robust to heteroskedasticity and autocorrelation. The results are robust to using standard errors that are double-clustered by stock and day. The asterisks *** in columns 9 and 10 denote statistical significance of β_1 and β_2 coefficient estimates at 1%.

	Panel A: summary statistics				Panel B: median changes around the ban				Panel C: regression	
	mean	25%	median	75%	pre	post	post-pre	%	β_1	β_2
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Large QPS	61.43	43.80	56.08	71.20	68.59	44.02	-24.57	-0.358***	-0.323***	-0.242***
Medium QPS	11.68	7.83	10.23	14.36	12.07	7.65	-4.42	-0.367***	-0.322***	-0.243***
Small QPS	4.20	3.33	3.87	4.87	4.95	3.04	-1.91	-0.386***	-0.336***	-0.266***
Large TPS	3.33	2.12	2.72	4.26	3.11	2.28	-0.84	-0.269***	-0.245***	-0.155***
Medium TPS	0.42	0.27	0.36	0.54	0.39	0.31	-0.07	-0.188***	-0.214***	-0.105***
Small TPS	0.16	0.12	0.14	0.18	0.15	0.12	-0.03	-0.170***	-0.176***	-0.081***

Table 3. Effective spreads and price impacts

Panel A reports summary statistics for the full sample period: October 2011 through January 2012. The pre- and post-ban statistics are reported in Panel B. Event coefficients from the multivariate regression models are reported in Panel C. Percentage effective spread, ESP, is twice the signed difference between the midpoint of the bid and ask quotes and the actual trade price, scaled by the midpoint. We sign trades using the Lee-Ready algorithm. We adjust TAQ as proposed by Holden and Jacobsen (2014). Percentage price impact, $PRIMP$, is the signed difference between a quote midpoint one second in the future and the quote midpoint at the time of the trade, scaled by the latter. All metrics are volume-weighted; they are computed on the intraday basis and then converted into daily aggregates. In columns 1-6, we report cross-sectional summary statistics for the full sample and the means for the pre-ban and post-ban periods. In columns 7 and 8, we report the differences between the post- and pre-ban statistics, in original units and as proportion of the pre-ban value. Columns 9 and 10 contain coefficient estimates β_1 and β_2 from the following regression models:

$$DEPVAR_{it} = \alpha_i + \gamma_t + \beta_1 BAN_t + \delta_1 IPRICE_{it} + \delta_2 MCAP_{it} + \varepsilon_{it}$$

and

$$DEPVAR_{it} = \alpha_i + \gamma_t + \beta_2 BAN_t + \delta_1 IPRICE_{it} + \delta_2 MCAP_{it} + \delta_3 VOLAT_{it} + \delta_4 TURN_{it} + \varepsilon_{it},$$

where $DEPVAR$ is either ESP or $PRIMP$, BAN is the indicator variable with the value of 0 in the pre-ban period and the value of 1 in the post-ban period, $IPRICE$ is the inverse of the stock price, $MCAP$ is market capitalization, $VOLAT$ is volatility, and $TURN$ is turnover. All non-dummy variables are log-transformed so that the coefficients may be interpreted as percentage changes. The regressions are estimated with stock fixed effects. Standard errors are robust to heteroskedasticity and autocorrelation. The results are robust to using standard errors that are double-clustered by stock and day. The asterisks *** denote statistical significance at the 1% level.

	Panel A: summary statistics				Panel B: mean changes around the ban				Panel C: regression	
	mean	25%	median	75%	pre	post	post-pre	%	β_1	β_2
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
All ESP	7.47	4.03	6.25	8.80	7.82	7.11	-0.71	-0.091***	-0.085***	-0.047***
All PRIMP	1.72	1.01	1.43	2.05	1.88	1.54	-0.34	-0.181***	-0.202***	-0.169***
Large ESP	4.60	2.95	4.03	5.59	4.81	4.36	-0.45	-0.094***	-0.074***	-0.030***
Large PRIMP	1.14	0.77	1.03	1.43	1.28	1.00	-0.28	-0.219***	-0.164***	-0.142***
Medium ESP	7.07	4.15	5.65	8.80	7.54	6.58	-0.96	-0.127***	-0.104***	-0.065***
Medium PRIMP	1.76	1.05	1.44	2.38	2.00	1.52	-0.48	-0.240***	-0.200***	-0.168***
Small ESP	10.75	7.21	8.32	10.52	11.10	10.39	-0.71	-0.064***	-0.077***	-0.040***
Small PRIMP	2.24	1.39	1.80	2.41	2.37	2.11	-0.26	-0.110***	-0.239***	-0.192***

Table 4. Realized spreads

Panel A reports summary statistics for the full sample period: October 2011 through January 2012. The pre- and post-ban statistics are reported in Panel B. Event coefficients from the multivariate regression models are reported in Panel C. Realized spread, RSP, is computed for each trade as the difference between the effective spread and the price impact, scaled by the contemporaneous midpoint. We estimate RSPs based on the price impacts that occur during the minimum time required by a liquidity provider to turn around a position; 1 second for large, 3 seconds for medium, and 8 seconds for small stocks. All metrics are volume-weighted; they are computed on the intraday basis and then converted into daily aggregates. In columns 1-6, we report cross-sectional summary statistics for the full sample and the means for the pre-ban and post-ban periods. In columns 7 and 8, we report the differences between the post- and pre-ban statistics, respectively, in original units and as proportion of the pre-ban value. Columns 9 and 10 contain coefficient estimates β_1 and β_2 from the following regression models:

$$RSP_{it} = \alpha_i + \gamma_t + \beta_1 BAN_t + \delta_1 IPRICE_{it} + \delta_2 MCAP_{it} + \varepsilon_{it}$$

and

$$RSP_{it} = \alpha_i + \gamma_t + \beta_2 BAN_t + \delta_1 IPRICE_{it} + \delta_2 MCAP_{it} + \delta_3 VOLAT_{it} + \delta_4 TURN_{it} + \varepsilon_{it},$$

where BAN is the indicator variable with the value of 0 in the pre-ban period and the value of 1 in the post-ban period, $IPRICE$ is the inverse of the stock price, $MCAP$ is market capitalization, $VOLAT$ is volatility, and $TURN$ is turnover. All non-dummy variables are log-transformed so that the coefficients may be interpreted as percentage changes. The regressions are estimated with stock fixed effects. Standard errors are robust to heteroskedasticity and autocorrelation. The results are robust to using standard errors that are double-clustered by stock and day. None of the estimated regression coefficients β_1 and β_2 are statistically significant.

	Panel A: summary statistics				Panel B: mean changes around the ban				Panel C: regression	
	mean	25%	median	75%	pre	post	post-pre	%	β_1	β_2
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Large RSP	3.45	2.20	2.98	3.98	3.53	3.36	-0.17	-0.048	-0.021	0.010
Medium RSP	4.56	2.63	3.72	5.54	4.49	4.69	0.20	0.045	-0.031	0.009
Small RSP	6.65	4.23	5.15	6.96	6.62	6.67	0.05	0.008	-0.014	0.027

Table 5. Price efficiency and adjusted effective spreads

Panel A reports the summary statistics. The pre- and post-implementation statistics are in Panel B. Event coefficients from the multivariate regression models are in Panel C. To gauge price efficiency, we follow Hasbrouck (1993) and Boehmer and Wu (2012) and calculate the pricing error, σ_s/σ_p. Further, we compute $AESP$ as the effective spread augmented by twice the absolute value of s_t from $p_t = m_t + s_t$. All of the abovementioned metrics are computed on the intraday basis and converted into daily aggregates. In columns 1-6, we report cross-sectional summary statistics for the full sample and for the pre- and post-ban periods. In columns 7 and 8, we report the differences between the post- and pre-ban statistics in original units and as proportion of the pre-ban value. Columns 9 and 10 contain coefficient estimates β_1 and β_2 from the following regression models:

$$DEPVAR_{it} = \alpha_i + \gamma_t + \beta_1 BAN_t + \delta_1 IPRICE_{it} + \delta_2 MCAP_{it} + \varepsilon_{it}$$

and

$$DEPVAR_{it} = \alpha_i + \gamma_t + \beta_2 BAN_t + \delta_1 IPRICE_{it} + \delta_2 MCAP_{it} + \delta_3 VOLAT_{it} + \delta_4 TURN_{it} + \varepsilon_{it},$$

where $DEPVAR$ is either the pricing error or $AESP$, BAN is the indicator variable with the value of 0 in the pre-ban period and the value of 1 post-ban, $IPRICE$ is the inverse of the stock price, $MCAP$ is market capitalization, $VOLAT$ is volatility (used only in the $AESP$ specifications), and $TURN$ is turnover. All non-dummy variables are log-transformed so that the coefficients may be interpreted as percentage changes. The regressions are estimated with stock fixed effects. Standard errors are robust to heteroskedasticity and autocorrelation. The results are robust to using standard errors that are double-clustered by stock and day. The asterisks *** denote statistical significance at the 1% level.

	Panel A: summary statistics				Panel B: mean changes around ban				Panel C: regression	
	mean	25%	median	75%	pre	post	post-pre	%	β_1	β_2
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
$All\ \sigma_s/\sigma_p$	0.047	0.034	0.042	0.055	0.043	0.051	0.008	0.186***	0.161***	0.086***
$Large\ \sigma_s/\sigma_p$	0.038	0.028	0.037	0.048	0.035	0.042	0.007	0.200***	0.156***	0.074***
$Medium\ \sigma_s/\sigma_p$	0.042	0.031	0.039	0.048	0.039	0.046	0.007	0.179***	0.159***	0.076***
$Small\ \sigma_s/\sigma_p$	0.060	0.044	0.055	0.063	0.055	0.065	0.010	0.182***	0.166***	0.104***
$All\ AESP$	11.51	6.39	8.97	12.40	11.95	11.06	-0.89	-0.074***	-0.074***	-0.045***
$Large\ AESP$	7.27	4.95	6.51	8.24	7.62	6.90	-0.72	-0.094***	-0.072***	-0.037***
$Medium\ AESP$	10.95	6.65	8.32	14.00	11.53	10.35	-1.18	-0.102***	-0.088***	-0.058***
$Smll\ AES$	16.33	10.56	11.96	16.67	16.70	15.93	-0.77	-0.046***	-0.063***	-0.035***

Table 6. Trading costs around the ban (by firm and trade size)

In this table, we separate trades into eight groups by size. Group 1 (small) contains trades in the [100; 200)-share range, Group 2 contains trades in the [200; 300)-share range, Group 3 contains trades in the [300; 400)-share range, Group 4 contains trades in the [400; 500)-share range, Group 5 contains trades in the [500; 2,000)-share range, Group 6 contains trades in the [2,000; 5,000)-share range, Group 7 contains trades in the [5,000; 10,000)-share range, Group 8 contains trades with sizes greater than or equal to 10,000 shares. We also consider large, medium, and small firms separately. In Panel A, we report the percentage shares of trades in eight above-mentioned size groups. In Panel B, we report the change in the number of trades in a particular group after the ban. In Panel C, we report the estimated β_2 coefficients from the following model of trading cost:

$$ESP_{it} = \alpha_i + \gamma_t + \beta_2 BAN_t + \delta_1 IPRICE_{it} + \delta_2 MCAP_{it} + \delta_3 VOLAT_{it} + \delta_4 TURN_{it} + \varepsilon_{it},$$

where all variables are as previously defined. The asterisks ***, **, and * denote statistical significance at, respectively, 1%, 5%, and 10%.

trade	Panel A: % in all trades				Panel B: pre-post				Panel C: % change in ESP (β_2)			
size	all	firm size			all	firm size			all	firm size		
cat.	firms	large	med.	small	firms	large	med.	small	firms	large	med.	small
1	66.4	63.3	82.0	83.9	-0.225***	-0.255***	-0.244***	-0.176***	-0.068***	-0.063***	-0.095***	-0.069***
2	14.3	15.2	9.5	8.8	-0.201***	-0.207***	-0.206***	-0.192***	-0.066***	-0.059***	-0.080***	-0.071***
3	5.8	6.3	3.3	2.9	-0.106***	-0.134***	-0.129***	-0.056***	-0.084***	-0.044***	-0.093***	-0.110***
4	3.4	3.8	1.7	1.4	-0.098***	-0.144***	-0.114***	-0.034	-0.073***	-0.034***	-0.024	-0.100***
5	8.6	9.6	3.3	2.7	-0.041***	-0.100***	-0.056***	0.035	-0.073***	-0.037***	-0.092***	-0.075***
6	1.1	1.3	0.3	0.2	0.037***	-0.030**	0.044*	0.109***	-0.046	-0.029***	-0.024	-0.091
7	0.3	0.3	0.1	0.1	0.044***	0.014	0.063***	0.074***	-0.016*	-0.029*	-0.015	-0.394
8	0.2	0.2	0.0	0.0	0.051***	0.037**	0.046	0.090***	-0.044	-0.131***	-0.014	0.04

Table 7. ITCH orders and trades

The table reports order and trade statistics derived from NASDAQ's TotalView-ITCH database. In Panel A, we report summary statistics for the full sample, for all stocks and for the three stock size groups. In Panel B, we compare pre- and post-ban univariate medians (columns 5 through 8). We report regression coefficients on the BAN dummy from models in eqs. 10 and 11 in, respectively, columns 9 and 10 (Panel C). Asterisks *** indicate statistical significance at the 1% level.

	Panel A: summary statistics				Panel B: median changes around ban				Panel C: regression	
	mean	25%	median	75%	pre	post	post-pre	diff	β_1	β_2
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
All ORDERS, '000	131.97	39.45	71.85	201.06	85.15	57.73	-27.42	-0.322***	-0.281***	-0.218***
All TRADES, '000	7.87	1.29	2.99	13.10	3.38	2.57	-0.81	-0.240***	-0.219***	-0.118***
Large ORDERS, '000	284.40	201.06	259.22	351.92	295.32	205.10	-90.220	-0.305***	-0.294***	-0.218***
Large TRADES, '000	19.29	13.10	16.75	22.85	19.43	13.55	-5.880	-0.303***	-0.317***	-0.222***
Medium ORDERS, '000	78.93	54.64	71.85	99.35	84.58	57.73	-26.850	-0.317***	-0.271***	-0.211***
Medium TRADES, '000	3.18	1.97	2.94	4.15	3.36	2.46	-0.900	-0.268***	-0.212***	-0.102***
Small ORDERS, '000	32.57	24.60	30.66	40.39	37.93	24.46	-13.470	-0.355***	-0.266***	-0.210***
Small TRADES, '000	1.13	0.68	0.88	1.40	1.00	0.78	-0.220	-0.220***	-0.117***	-0.023

Table 8. ITCH limit order book depth

The table reports changes in incremental average depth of ITCH limit order book around the ban. Incremental average depth is additional depth available by moving from one book layer to another averaged between the two sides of the book. To build the book, we record orders according to price-time priority. We then allow executions (full or partial) and cancellations (full or partial) to remove shares from the book. We build order books for each stock-day for all days in our sample period, and record the incremental share depth at the following distances away from ITCH best bid and ask: 0.119%, 0.166%, 0.6%, 0.833%, 2.38%, 3.333%, 11.9%, 16.67%, 25%, 50%, 60%, 70%, 80%, 90%, and 100%. A 0.119% cutoff for a \$42 stock (about the average price of our sample stocks) captures depth up to \$0.05 away from the best quote. We then take snapshots of the state of the order book at every half hour interval beginning at 9:30 a.m. and ending at 4:00 p.m. Given that the first and the last half hours may be affected by the opening and closing procedures, we report the results twice: for the 10:00 a.m. – 3:30 p.m. interval (columns 1-3) and for the full day (columns 4-6). Columns 1 and 4 report percentage differences between the post and pre periods. Columns 2 and 3 as well as 5 and 6 report regression coefficients on the BAN dummy from models in eqs. 10 and 11. Asterisks ***, **, and * indicate statistical significance at, respectively, 1%, 5%, and 10% levels.

% from ins. quote	10:00 a.m. – 3:30 p.m.			9:30 a.m. – 4:00 p.m.		
	post-pre	β_1	β_2	post-pre	β_1	β_2
	[1]	[2]	[3]	[4]	[5]	[6]
0.119	0.394***	0.323***	0.288***	0.294***	0.259***	0.210***
0.166	0.152***	0.157***	0.139***	0.131***	0.132***	0.106***
0.6	0.169***	0.070***	0.061***	0.162***	0.063***	0.052***
0.833	0.340***	0.020	0.027**	0.281***	0.048***	0.057***
2.38	0.362***	0.023***	0.021**	0.320***	-0.002	0.002
3.333	0.167**	0.052***	0.090***	0.169**	0.051***	0.094***
11.9	0.050	0.111***	0.102***	0.063	0.103***	0.094***
16.67	-0.060	0.007	0.096***	-0.059	-0.013	0.077***
25	-0.118**	-0.194***	-0.098***	-0.113**	-0.129***	0.079***
50	-0.042	0.037*	0.031	-0.043	-0.009	-0.016
60	-0.218***	-0.258***	-0.196***	-0.214***	-0.249***	-0.185***
70	-0.434***	-0.344***	-0.286***	-0.429***	-0.359***	-0.301***
80	-0.360***	-0.420***	-0.382***	-0.362***	-0.437***	-0.403***
90	-0.290***	-0.356***	-0.324***	-0.286***	-0.344***	-0.320***
100	0.615**	-0.193***	-0.129***	0.140**	-0.222***	-0.159***

Table 9: ITCH reaction times

We report the distance between a trade execution and the two types of events: (i) the most recent order placed on the side of the book that provides liquidity for the execution and (ii) the most recent book event (e.g., order submission, cancellation, modification, etc.) on any side of the book. Columns 1 and 2 contain pre- and post-ban univariate means. Column 3 contains the percent difference between the post and pre statistics. Columns 4 and 5 report regression coefficients on the BAN dummy from models in eqs. 10 and 11. Asterisks *** and ** indicate statistical significance at, respectively, 1% and 5% levels.

	pre	post	diff		β_1		β_2	
	[1]	[2]	[3]		[4]		[5]	
All LP order	0.520	0.842	0.619	***	0.370	***	0.276	***
All LOB event	0.221	0.344	0.557	***	0.366	***	0.270	***
Large LP order	0.246	0.341	0.095	***	0.287	***	0.218	***
Large LOB event	0.106	0.138	0.032	***	0.341	***	0.193	***
Medium LP order	0.375	0.580	0.205	***	0.386	***	0.296	***
Medium LOB event	0.142	0.215	0.073	***	0.433	***	0.357	***
Small LP order	0.939	1.607	0.668	***	0.381	***	0.290	***
Small LOB event	0.414	0.680	0.266	***	0.375	***	0.276	***

Table 10. Difference-in-differences analysis

In this table, we report the results from the difference-in-differences regressions that compare trading activity and liquidity in the main sample of U.S. stocks and a matched sample of non-interlisted Canadian stocks. We obtain the list of interlisted (cross-listed) stocks from the September 2011-January 2012 eReviews published by the Toronto Stock Exchange. We match the stocks by market capitalization, price (adjusted for the exchange rate), and the percent quoted spread in September 2011. The following daily metrics for the sample U.S. stocks are derived from CRSP: the number of shares traded ($VOLUME$); the end-of-day quoted spread, QSP, computed as the difference between the closing ask and bid prices divided by the midpoint of these prices; the Amihud's (2002) illiquidity measure, $ILLIQ$; and the spread estimator of Corwin and Schultz (2012), $CSSP$. To estimate Canadian equivalents of the abovementioned metrics, we use the Canadian Financial Markets Research Centre (CFMRC) database. The table contains coefficient estimates on the BAN dummy from the fixed effects regressions of the differences in the abovementioned metrics between the main sample stocks (the treatment sample) and their Canadian matches (the control sample). Among dependent variables are differences in the previously defined regressors: $IPRICE$, $MCAP$, $VOLAT$, and $TURN$. We exclude the $TURN$ variable from the $VOLUME$ regression. Asterisks ***, **, and * indicate statistical significance at, respectively, 1%, 5%, and 10% levels.

	all stocks		*large*		*medium*		*small*	
	β_1	β_2	β_1	β_2	β_1	β_2	β_1	β_2
$VOLUME$	-0.195***	-0.140***	-0.160***	-0.138***	-0.259***	-0.175***	-0.164***	-0.085***
QSP	-0.129***	-0.107***	-0.101***	-0.091**	-0.161***	-0.154**	-0.103***	-0.051**
$ILLIQ$	-0.113***	-0.146***	-0.067**	-0.120*	-0.105***	-0.102	-0.144***	-0.139
$CSSP$	-0.182***	-0.181***	-0.186***	-0.100**	-0.192***	-0.170***	-0.147***	-0.078**
$VOLAT$ & $TURN$	No	Yes	No	Yes	No	Yes	No	Yes

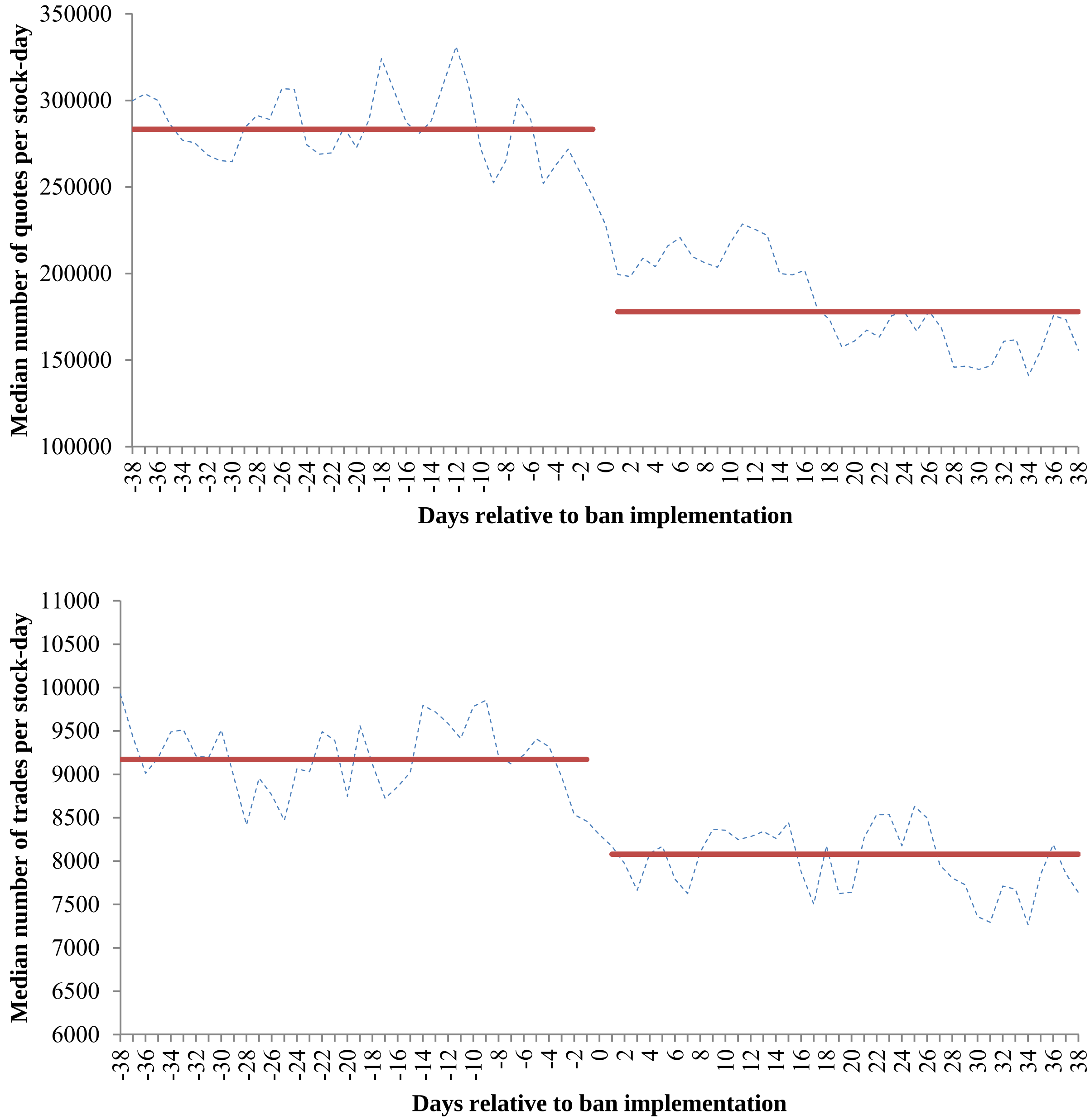

Figure 1: Daily quotes and trades around the ban implementation
The upper figure reports the median number of quote submissions around the date of ban implementation, and the lower figure reports the median number of trade submissions. The horizontal axes reflect a timeline around the implementation of the ban, with day 0 representing the first day of the ban. The vertical axes reflect quote and trade volumes. The red lines are pre- and post-ban medians of daily quote and trade volumes. The dashed lines are the two-day moving averages of daily median quote and trade volumes.

Table A1

The table reports changes in incremental average depth of ITCH limit order book around the ban. This table replicates Table 7 of the main text, yet splits sample stocks into groups by firm size. Incremental average depth is additional depth available by moving from one book layer to another averaged between the two sides of the book. To build the book, we record orders according to price-time priority. We then allow executions (full or partial) and cancellations (full or partial) to remove shares from the book. We build order books for each stock-day for all days in our sample period, and record the incremental share depth at the following distances away from INET's best bid and ask: 0.119%, 0.166%, 0.6%, 0.833%, 2.38%, 3.333%, 11.9%, 16.67%, 25%, 50%, 60%, 70%, 80%, 90%, and 100%. A 0.119% cutoff for a $42 stock (about the average price of our sample stocks) captures depth up to $0.05 away from the best quote. We then take snapshots of the state of the order book at every half hour interval beginning at 9:30 a.m. and ending at 4:00 p.m. Given that the first and the last half hours may be affected by the opening and closing procedures, we report the results twice: for the 10:00 a.m. – 3:30 p.m. interval (columns 1-3) and for the full day (columns 4-6). Columns 1 and 4 report percentage differences between the post and pre periods. Columns 2 and 3 as well as 5 and 6 report regression coefficients on the BAN dummy from models in eqs. 10 and 11. Asterisks ***, **, and * indicate statistical significance at, respectively, 1%, 5%, and 10% levels.

% from the inside quote	10:00 a.m. – 3:30 p.m.						9:30 a.m. – 4:00 p.m.					
	post-pre		β_1		β_2		post-pre		β_1		β_2	
Panel A: large stocks												
0.119	0.399	**	0.297	***	0.254	***	0.306	***	0.277	***	0.230	***
0.166	0.163	**	0.096	**	0.096	**	0.145	***	0.062		0.063	
0.6	0.221	**	0.130	***	0.115	***	0.212	***	0.127	***	0.111	***
0.833	0.468	**	0.168	***	0.186	***	0.393	***	0.167	***	0.181	***
2.38	0.425	**	0.199	***	0.188	***	0.398	***	0.186	***	0.174	***
3.333	0.158	*	0.046	*	0.077	***	0.162	*	0.058	**	0.083	***
11.9	0.042		0.084	***	0.076	***	0.056		0.091	***	0.081	***
16.67	-0.063		0.068	***	0.064	***	-		0.067	***	0.060	***
25	-0.120	**	-		0.013		-	**	-		0.010	
50	-0.069		-	***	-	***	-		-	***	-	***
60	-0.228	**	-0.24	***	-	***	-	***	-	***	-	***
70	-0.442	**	-	***	-	**	-	***	-	***	-	**
80	-0.363	**	-	***	-	***	-	***	-	***	-	***
90	-0.301	**	-	***	-	***	-	***	-	***	-	***
100	0.622	**	0.020		0.067		0.133		-	***	-	***

Panel B: medium stocks												
0.119	0.278	***	0.271	***	0.243	***	0.170	***	0.214	***	0.176	***
0.166	0.048	*	0.188	***	0.160		0.033		0.182	***	0.139	***
0.6	-0.136	***	-0.020		-0.024	*	-0.092	***	0.000		-0.006	
0.833	-0.122	***	-0.190	***	-0.187	***	-0.105	***	-0.150	***	-0.139	***
2.38	-0.026		-0.021		-0.004		-0.068	**	-0.060	***	-0.038	***
3.333	0.171	***	-0.033		0.011		0.119	**	-0.036		0.015	
11.9	0.085	***	-0.097		0.093	***	0.084	***	0.096	***	0.089	***
16.67	0.006		-0.076		0.013		-0.005		-0.093	*	-0.008	
25	-0.073		-0.321	***	-0.226	***	-0.079	*	-0.164	***	-0.131	***
50	0.191	***	0.100	**	0.093	*	0.146	***	0.029		0.019	
60	-0.308	***	-0.385	***	-0.308	***	-0.291	***	-0.358	***	-0.272	***
70	-0.286	***	-0.381	***	-0.328	***	-0.311	***	-0.414	***	-0.364	***
80	-0.273	***	-0.428	***	-0.352	***	-0.268	***	-0.484	***	-0.418	***
90	-0.119		-0.270	***	-0.224	***	-0.163	*	-0.297	***	-0.252	***
100	1.757	***	-0.092		-0.027		1.666	***	-0.032		0.031	
Panel C: small stocks												
0.119	0.537	***	0.417	***	0.368	***	0.209	***	0.305	***	0.234	***
0.166	0.137	***	0.22	***	0.196	***	0.083		0.191	***	0.155	***
0.6	0.096	***	0.113	***	0.102	***	0.077	***	0.082	***	0.065	***
0.833	-0.015		0.079	***	0.092	***	0.031		0.117	***	0.13	***
2.38	-0.025		-0.122	***	-0.123	***	-0.055		-0.147	***	-0.139	***
3.333	0.506	***	0.121	***	0.157	***	0.439	***	0.107	***	0.156	***
11.9	0.234	***	0.163	***	0.143	***	0.229	***	0.136	***	0.117	***
16.67	-0.052		0.078		0.246	***	-0.042		0.024		0.202	***
25	-0.125	***	-0.291	***	-0.112	*	-0.129	***	-0.227	***	-0.12	***
50	0.086	***	0.073	*	0.047		0.061	**	0.013		-0.012	
60	0.178		-0.122	*	-0.038		0.159		-0.121	*	-0.038	
70	-0.300	***	-0.402	***	-0.315	***	-0.284	***	-0.413	***	-0.318	***
80	-0.348	***	-0.446	***	-0.434	***	-0.341	***	-0.46	***	-0.45	***
90	-0.158		-0.192	***	-0.117	**	-0.166		-0.166	***	-0.107	**
100	-0.324	***	-0.504	***	-0.441	***	-0.301	***	-0.413	***	-0.357	***