Who Benefits from Securities Exchange Innovation?*

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Abstract. Securities markets continuously innovate to keep pace with technology. It is often debated if such innovation is beneficial, and which market participants capture the benefits. We contribute to this debate by examining the effects of a wide range of proprietary enhancements to the trading process introduced by the stock exchanges in the United States. Generally, exchange innovation is associated with improvements in liquidity and price efficiency, although the reduction in liquidity costs primarily benefits investors trading in small quantities. Institutional investors experience less favorable outcomes; while their trading costs remain unchanged, their market participation declines.

Key words: liquidity, market quality, equity trading, innovation

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1. Introduction

The role of securities exchanges in the proper functioning of financial markets is difficult to overstate. Exchanges bring together investors, large and small, allowing them to realize gains from trade, mobilize capital, and incorporate relevant information into prices. To keep pace with the unrelenting march of technology and customer expectations, exchanges continuously innovate. Theoretical models predict that such innovation may have both positive and negative effects on market quality. Likewise, empirical studies find that while some exchange initiatives are beneficial for liquidity, others may be disadvantageous.[1] With such varying findings, it is of interest to ask if exchange innovation is beneficial as a whole, and if all market participants capture the benefits.

We address these questions using a multi-year sample of new technological offerings by stock exchanges in the United States. These offerings range from improvements in data dissemination to enhancements in order processing by exchange engines. The results suggest that exchange innovation is generally associated with lower trading costs for investors who trade in small quantities. In the meantime, institutional investors do not appear to derive as much benefit from new exchange technologies; while their trading costs are typically unaffected, their trading volumes decline after new technologies are introduced.

Our sample of technological offerings comes from the public record of patents filed by the exchanges. In the U.S., when a company develops a new technology it files an application for

[1] See the theoretical models by Menkveld and Zoican (2017), Pagnotta and Philippon (2018), and Cespa and Vives (2022) and empirical studies by Hendershott, Jones, and Menkveld (2011), Hendershott and Moulton (2011), Brogaard, Hagströmer, Nordén, and Riordan (2015), Conrad, Wahal, and Xiang (2015), and Foucault, Kozhan, and Tham (2017)). We discuss these studies and other prior research shortly.

a patent with the Patent and Trademark Office describing the invention and claiming an exclusive right to it. We collect a sample of all exchange filings in 2003-2021, for a total of 194 patents. The patents capture a wide range of exchange activities. For example, the NYSE patent US-9450999-B2 (https://bit.ly/4c2nsWy) describes technology for high performance data streaming, while Nasdaq patent US-11671395-B2 (https://bit.ly/3Vvajjd) claims rights to a "message tracking apparatus [that improves] the latency of a message processing system." Like these two examples, the sample patents generally focus on enhancing the speed and efficiency of exchange infrastructure and on improving customer connectivity.

To measure trading costs, we use two data sources, the intraday Trade and Quote (TAQ) database and the Abel Noser institutional trading database. TAQ allows us to compute a set of conventional liquidity metrics such as quoted and effective spreads. The quoted spreads measure displayed liquidity, that is trading costs advertised by liquidity providers. In turn, the effective spreads capture liquidity costs that are actually incurred by market participants. Both liquidity metrics decline following introductions of new exchange technologies.

What may drive liquidity cost reductions associated with innovation? To shed light on this question, we examine two components of effective spreads, the price impact and the realized spread. The former captures adverse selection costs incurred when providing liquidity. The latter reflects liquidity provider inventory costs, fixed costs, and profits. Both components decline post-innovation, consistent with the notion that new exchange technologies help reduce market making costs and may enhance competition for liquidity provision. In addition, the data show that innovation is followed by lower price volatility and greater price efficiency.

Liquidity in the U.S. equity market gradually improves since the early 2000s (Angel, Harris, and Spatt (2015)). It is therefore important to ensure that our tests do not merely pick up this background trend. To do so, we detrend all variables and use only the detrended variables in regression tests. In addition, we verify the results in a difference-in-differences (DID) setting against a sample of Canadian securities, for which long-term liquidity trends are similar to those of U.S. equities. Our findings are robust in the cross-section and replicate for stocks of all sizes.

The TAQ-based liquidity proxies discussed so far apply mainly to market participants, who seek to trade small share quantities represented by the size of the best quotes.[2] Meanwhile, institutional investors often trade large amounts, and for them the TAQ proxies may not be the most suitable (Eaton, Irvine, and Liu (2021)).[3] To shed light on the effects of exchange innovation on institutions, we use Abel Noser data and estimate the execution shortfall, a metric commonly used to measure institutional trading costs (e.g., Conrad, Johnson, and Wahal (2001) and Anand, Puckett, Irvine, and Venkataraman (2013)). The results show that exchange innovation is not associated with changes in execution shortfall. Nevertheless, innovation is followed by lower institutional trading volumes. In summary, while exchange innovation may benefit the seekers of small amounts of liquidity, its effects are more ambiguous for those, who trade large amounts.

What may be behind this dichotomy? We posit that the answer may lie with tech-savvy

[2]Besides traders executing relatively small positions, a group that includes market makers engaging in liquidity demand for inventory management purposes, the TAQ-based proxies also reflect liquidity costs for retail investors. Although retail orders mainly execute off-exchange, their execution prices are benchmarked against exchange quotes. Dyhrberg, Shkilko, and Werner (2024) show that an average retail order is price-improved and executed at 73% of the prevailing exchange spread.

[3]Even though institutional trading algorithms are capable of splitting parent orders into smaller child orders to match the size of the best quote, price pressure originating from a string of child orders and their possible detection by other market participants may result in subsequent child orders trading at worse prices, negatively affecting the overall outcome for the institution.

trading firms that dominate trading in modern markets. These firms are commonly referred to as *high-frequency traders* (HFTs). Fierce competition for speed drives their unrelenting pursuit of innovation. Using a dataset that identifies trading by such firms, we illustrate that they are quick to react to new exchange technologies, with their share of trading volume increasing shortly after exchange patent filings.

Prior research finds that tech-savvy firms play a major role in liquidity provision, yet also points out that they are highly skilled at avoiding adverse selection and maintaining low inventories (Brogaard, Hagströmer, Nordén, and Riordan (2015), Brogaard, Carrion, Moyaert, Riordan, Shkilko, and Sokolov (2018)). Consequently, liquidity provision by such firms may be beneficial for relatively uninformed investors who trade small quantities, but not for institutional liquidity seekers, who are often informed and trade large positions. As new technologies improve the ability of these firms to avoid adverse selection and unwanted inventory accumulation, they may provide less liquidity when institutional investors demand it or provide liquidity at prices greater than what institutions are willing to pay. The probable outcome is a decrease in market participation by institutions.[4]

In addition to liquidity provision, tech-savvy firms regularly engage in liquidity demand. Their inventory management and arbitrage strategies are highly time-sensitive and often require taking liquidity (Brogaard, Hendershott, and Riordan (2014), Chaboud, Chiquoine, Hjalmarsson, and Vega (2014), Boehmer, Li, and Saar (2018), Baron, Brogaard, Hagströmer, and Kirilenko

[4] A plausible alternative expectation is that new technologies should improve the ability of liquidity providers to manage inventories thereby allowing them to better serve institutional customers. Although the data do not allow us to directly examine this alternative, our findings indicating lower institutional trading volumes post-innovation are generally aligned with the notion that market quality for institutional investors does not improve.

(2019)). The ability to harness new technology may give these firms an advantage over institutions in the race to trade against outstanding quotes, potentially resulting in decreased execution probabilities for institutional flow.

Do institutions fail to benefit from exchange innovation because they do not pay attention to new technological developments? To shed light on this question, we examine patent citations with the assumption that market participants affected by a patent will cite it. We find that while most patents are cited primarily by the tech-savvy trading firms and firms that manufacture technologies used for trading, a smaller group of patents is cited mainly by institutional investors. Examining these two patent groups separately, we show that the patents cited by trading and technology firms tend to conform to our main findings. They primarily benefit those who trade small quantities and are associated with lower institutional trading volume. Meanwhile, patents cited by institutions are not linked to any adverse effects for small traders, yet are associated with greater institutional trading volumes. This result suggests that institutions are not entirely disinterested in new technology and are capable of harnessing it. However, the relatively small number of patents cited by institutions indicates that most technologies may be challenging to appropriate for institutional benefit.

Finally, we seek to understand the driving forces behind exchange innovation. Innovation may occur as a routine practice, with exchanges continuously developing new product offerings, or as a strategic response to various challenges such as declining volume or market share and innovation by the rival exchanges. Our results are consistent with both routine practice and strategic response explanations. Nevertheless, the strategic response effect is rather weak. Furthermore, the innovating exchanges do not seem to directly benefit from their new technology; their own trading volumes and market shares do not increase. Overall, the relentless advancement of technology and the need to meet the expectations of tech-savvy customers responsible to liquidity provision seem to be the primary drivers of exchange innovation efforts.

Our findings contribute to an ongoing discussion of modern market structure and the securities exchange industry. Even though exchange innovation by the incumbent exchanges likely does not have nefarious intentions towards any particular group of market participants, it may inadvertently benefit those, who are best equipped for technological change. In this respect, establishing markets that cater to the needs of the institutional community may be socially optimal. This logic echoes Biais, Foucault, and Moinas (2015), who model interactions between investors that operate at different trading speeds and examine the benefits of segmenting markets into those that cater to fast and slow traders.

Related literature. Several theoretical models consider the effects of advancements in exchange technology and predict that such advancements may be both beneficial and detrimental. Menkveld and Zoican (2017) show that increasing exchange engine processing speed may have both positive and negative effects on liquidity. In their model, the direction of the effect may vary over time and in the cross-section and depends on the mix of liquidity traders and news events. Pagnotta and Philippon (2018) show that increasing exchange speed may lead to lower trading fees and greater investor participation. They however caution that purely technological improvements to trading technology may lead to limited welfare gains.

Perhaps the closest to our study, Cespa and Vives (2022) model an environment, in which exchanges supply technology services to various market participants. The fast adopters of new technology use it to improve liquidity, generating a positive welfare effect. In the meantime, traders who are unable to adopt new technology face lower payoffs. Our results echo the implications of this model. While exchange innovation benefits small liquidity consumers by allowing liquidity providers to reduce adverse selection and inventory costs, it also results in lower market participation by institutional investors, who seem to often struggle with adopting new technology.

The empirical literature examines a number of technological enhancements to the exchange trading process. The outcomes of such enhancements vary. Hendershott, Jones, and Menkveld (2011) show that exchange-driven automation of liquidity provision results in liquidity improvements. Conversely, Hendershott and Moulton (2011) and Foucault, Kozhan, and Tham (2017) find that exchange technology enhancements that benefit liquidity-demanding strategies have adverse liquidity effects. Brogaard, Hagströmer, Nordén, and Riordan (2015) and Conrad, Wahal, and Xiang (2015) study co-location, a practice that allows trading firms to place trading servers near exchange matching engines. They report that the practice is mainly used by liquidity suppliers and therefore benefits liquidity. Overall, empirical findings vary across technologies and practices, making it difficult to draw conclusions about the net effects of technological advancement. Our analysis of a multi-year sample of exchange technological offerings therefore brings the literature closer to understanding the general effects of exchange innovation.

Our study is related to the literature that examines liquidity costs and to a sub-set of this literature that focuses on institutional investors. Anand, Puckett, Irvine, and Venkataraman (2013) suggest that despite the overall trend to lower liquidity costs in the 21st century, institutional investor trading costs did not decline. This result echoes the earlier findings by Goldstein and Kavajecz (2000) and Jones and Lipson (2001), who show that regulatory actions that reduce spreads may not have similar liquidity-boosting effects for large traders. Eaton, Irvine, and Liu (2021) generalize this finding to caution that changes in institutional trading costs do not always align with changes in conventional TAQ-based liquidity metrics.

Our arguments are also rooted in the literature on sophisticated trading firms often referred to as HFTs. Such firms fulfill several important functions, among which liquidity provision and arbitrage stand out as dominant (Brogaard, Hendershott, and Riordan (2014), Chaboud, Chiquoine, Hjalmarsson, and Vega (2014), Brogaard, Hagströmer, Nordén, and Riordan (2015), Boehmer, Li, and Saar (2018)). HFTs are highly skilled at avoiding adverse selection and maintaining low inventories (Korajczyk and Murphy (2019), Van Kervel and Menkveld (2019)), and therefore their liquidity supply is likely more beneficial to uninformed investors seeking small amounts of liquidity than to institutional investors.

2. Data and metrics

Our data come from three main sources: (i) a public depository of patents maintained by Google, LLC (`https://patents.google.com`), (ii) the Trade and Quote (TAQ) database maintained by the New York Stock Exchange, and (iii) the Abel Noser (formerly, ANcerno) institutional trading database. For the difference in differences (DID) tests, we also use a fourth data source – Canadian data on liquidity costs and trading volume. These data are from the Canadian Financial Markets Research Centre (CFMRC) (`https://bit.ly/3czbKUV`). Finally, to examine the activity of tech-savvy trading firms around patent filings, we use the Nasdaq HFT dataset available for 2008-2009. This dataset has been previously used by Brogaard, Hendershott, and Riordan (2014), O'Hara, Yao, and Ye (2014), among many others.

2.1 Patent data

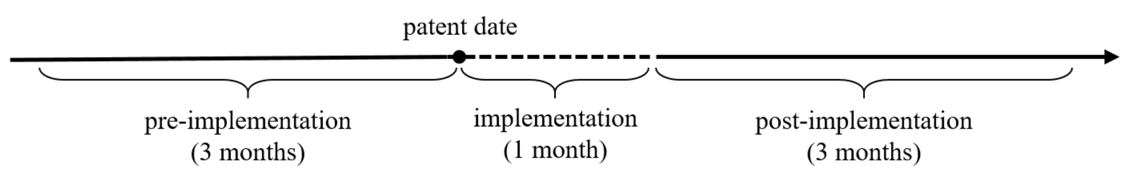
From Google, we obtain 194 patents that are filed by the U.S. exchanges from 2003 through 2021. These patents cover a broad range of exchange activities, with a general focus on optimizing data dissemination and order management. While the patents in our sample capture a wide range of technologies, they do not represent an exhaustive list of new products and services. For instance, during our sample period exchanges start offering co-location – a practice that allows trading firms to place their servers as close as possible to the exchange engine. Co-location is a concept rather than a technology that can be patented, so our sample of events does not include the introduction of co-location. Rather, the sample contains products or services that, among other things, may improve data dissemination and trader connectivity within the co-location facilities. As a result, instead of capturing momentous yet rare market structure changes, the event sample contains numerous incremental adjustments to exchange technology.

In the United States, the prevailing corporate practice is to file a provisional patent application *before* deploying a new product, service, or technology. An alternative of filing after deployment jeopardizes intellectual property rights since inventions are considered unpatentable if they have fallen into the public domain prior to filing.[5] Since product and service implementation involves

[5]See Rule 64 of the Patent Cooperation Treaty, to which the U.S. is a party: https://bit.ly/3eMqRM6.

informing exchange customers and regulators and therefore brings inventions into the public domain, exchanges tend to file provisional applications immediately prior to implementation. We consider the dates of such filings, also known as *patent priority dates*, to be event dates and refer to them as *patent dates*. Supporting this reasoning, Nasdaq data show that HFT market participation changes within just a few days of new filings. We discuss this analysis shortly.

Even though some market participants react to patent filings quickly, others (for instance, institutions) may react to them slowly. Since our main goal is to capture the effects of technology after all market participants have had the opportunity to adjust, we define a one-month window following a patent date as *the implementation period*. As the illustration below shows, in subsequent tests, we compare market quality in the pre-implementation period (the three months preceding the patent date) and the post-implementation period (the three months following the implementation period).



2.2 Liquidity metrics

We obtain liquidity metrics from two sources; the NYSE TAQ database and the Abel Noser database. Using TAQ, we compute quoted and effective spreads as well as the two components of the latter, that is, price impacts and realized spreads. The *quoted spread*, or the National Best Bid and Offer (NBBO), measures displayed liquidity and is computed as the difference between the national best offer (NBO) and the national best bid (NBB). Further, to measure trading costs actually incurred by liquidity demanders, we compute the *effective spread* as twice the signed difference between the traded price and the NBBO midpoint at the time of the trade. The midpoint is the average of the NBO and NBB.

Next, to assess the levels of adverse selection, we compute the *price impact* as twice the signed difference between the NBBO midpoint at the time of the trade and the midpoint five minutes after the trade.[6] Finally, the *realized spread* is computed as the difference between the effective spread and price impact and is often associated with liquidity provider inventory and other costs as well as profits (e.g., Hendershott, Jones, and Menkveld (2011), Brogaard, Hagströmer, Nordén, and Riordan (2015)).

To sign trades, we rely on the Lee and Ready (1991) algorithm. Chakrabarty, Pascual, and Shkilko (2015) show that this algorithm continues to perform well in modern markets. All variables are scaled by the corresponding quote midpoints and winsorized at 1%. When computing daily aggregates, we weight quoted spreads by the time they are outstanding, and we weight all trade-related metrics by the corresponding trading volume.

The above-mentioned TAQ-derived metrics are generally considered representative of small investor trading costs. This is because the NBBO quotes are usually valid for several hundred or several thousand shares, representing quantities sufficient to execute small trades, but not the amounts traded by institutional investors. To estimate liquidity metrics for such larger investors, we use the Abel Noser database. Puckett and Yan (2011) show that even though this database

[6] We use five-minute (300-second) horizons for price impacts because our sample period goes back to 2003 when price adjustments were relatively slow. Studying a 2003 market change, Hendershott, Jones, and Menkveld (2011) also use five-minute horizons. This said, in Section A.1 of the Internet Appendix, we show that our results are unchanged when we use alternative horizons of 15, 30, 60, and 900 seconds.

does not cover trading by all institutions, the activity it captures is representative of that by an average institution.

To proxy for institutional trading costs, we use the *execution shortfall* metric of Conrad, Johnson, and Wahal (2001) and Anand, Puckett, Irvine, and Venkataraman (2013). The metric is computed as the signed difference between the volume-weighted execution price of an institutional order in stock i on day t and the opening price for the stock that day. Abel Noser differentiates between institutional buys and sells, so no additional trade signing is required. As with all trading cost metrics, we volume-weight execution shortfall.[7]

2.3 Price efficiency metrics

In addition to examining the effects of exchange innovation on liquidity costs, we also measure its effects on price efficiency. To do so, we use three standard efficiency metrics: the *variance ratio* of Lo and MacKinlay (1988), *return autocorrelation* as in Hendershott and Jones (2005), and the *price delay* of Hou and Moskowitz (2005).

The first metric, the variance ratio, relies on the notion that if prices follow a random walk, return variance should increase linearly in return horizon. With this in mind, for each stock-day we compute the absolute difference between one and the ratio of the variance of $k \times l$-second

[7] *Execution shortfall* should be distinguished from *implementation shortfall* – the latter being a comprehensive metric that encompasses both execution shortfall and the opportunity costs incurred by not fully executing the initially desired position. Since Abel Noser data lacks information on initially desired positions, studies of institutional trading costs using this database focus on execution shortfall.

midpoint returns to k times the variance of l-second midpoint returns:

$$|1-VR| = \left|1 - \frac{\sigma_{k\times l}^2}{k\times\sigma_l^2}\right|, \tag{1}$$

where $(l,\ k\times l)$ is $(15,60)$ or $(60,300)$. The closer the variance ratio is to zero, the more prices resemble a random walk implying greater efficiency. We choose $k\times l$ of 300 seconds to be consistent with the horizon of price impact estimates. To account for greater speeds that prevail later in the sample period, we follow Comerton-Forde, Grégoire, and Zhong (2019) and also examine $k\times l$ of 60 seconds. In Section A.2 of the Internet Appendix, we report results for additional estimation horizons, i.e., (10, 60) and (30, 60).

The second metric, return autocorrelation, makes use of the notion that in a frictionless market prices should be unpredictable, and therefore midpoint returns should have zero autocorrelation. It is computed for each stock-day as:

$$autocorrelation = corr\left(ret_{l,\ t}, ret_{l,\ t-1}\right), \tag{2}$$

where $ret_{l,\ t}$ is the t^{th} midpoint return at horizon l. For consistency with the metrics discussed earlier, in the main results we focus on return autocorrelations computed at the 15-second and 300-second horizons, with additional horizons of 10, 30, and 60 seconds reported in Section A.2 of the Internet Appendix. Smaller autocorrelation estimates suggest greater efficiency.

The third metric, the price delay, assumes that efficient prices should instantly incorporate public market information. Accordingly, lagged market returns should have no predictive power for individual stocks returns. To compute this metric, we begin by running the following regression for each stock-day:

$$ret_l = \alpha + \beta ret_{m,\,l} + \sum_{\tau=1}^{10} \gamma_\tau ret_{m,\,l-\tau} + \varepsilon_l, \tag{3}$$

where ret_l is the quote midpoint return during time interval l, and $ret_{m,\,l}$ is the return on the S&P 500 index proxied by the SPY ETF. For consistency, we use the same frequencies for l as we did when computing the other price efficiency metrics, with additional horizons of 10, 30, and 60 seconds reported in Section A.2 of the Internet Appendix. We then define the R^2 from regression (3) as unconstrained, R_u^2. Next, we estimate regression (3) without the lagged market returns, effectively constraining γ to zero, and define the corresponding R^2 as constrained, R_c^2. Finally, for each stock-day, we compute:

$$price\ delay = 1 - \frac{R_c^2}{R_u^2}. \tag{4}$$

A smaller delay suggests greater efficiency.

2.4 The sample

To define the sample, we begin with all common equities listed on the NYSE, Nasdaq, or AMEX at the beginning of the sample period, in January 2003. We then drop the stocks that do not survive the entire 2003-2021 period and from the remaining equities select 600 stocks with the greatest market capitalization. Our access to the Abel Noser dataset ends in 2013, so the sample period for institutional analyses is shorter than that for the TAQ analyses. In Section A.3 of the Internet Appendix, we show that the TAQ-based results remain the same if we use the shorter 2003-2013 period that matches the Abel Noser period.

Table 1 reports the summary statistics. The average stock has about $20 billion in market capitalization and trades close to 2.9 million shares a day at a price of $68. From TAQ, its quoted and effective spreads are 10.59 and 9.09 bps, with price impacts of 6.36 bps and realized spreads of 2.81 bps. In turn, daily institutional volume in Abel Noser data is 0.26 million shares per day (about 9% of total volume), and the typical execution shortfall is 13.95 bps. Both these figures are consistent with earlier studies that use Abel Noser data.

[Table 1]

We note the cross-sectional variation in many of the above-mentioned variables that should be expected from a sample of 600 equities. For instance, while the average sample firm has market capitalization of $19.70 billion, the median firm is smaller, with capitalization of $5.98 billion. Trading costs also vary in the cross-section. For example, the effective spreads are 5.17 bps in the 25th percentile of firms and 11.01 bps in the 75th percentile. We observe similar variation for price efficiency metrics, with 15-second return autocorrelations ranging from 0.052 to 0.060. In Section 3.8, we show that the main results are preserved in the cross-section when we separately examine large, medium, and small firms.

2.5 The long-term trend and other possible confounders

The event windows in this study span several months. Consequently, our analyses capture medium-term changes. We note that in the background of these changes are the long-term trends in liquidity and volume. Angel, Harris, and Spatt (2015) report that in the 21st century spreads generally trend downward, while trading volume trends upward. To mitigate these effects, we regress every variable of interest on a time trend as follows:

$$DepVar_{it} = \alpha + \beta Trend_t + \varepsilon_{it}, \tag{5}$$

and use the estimated residuals $\hat{\varepsilon}_{it}$ as dependent variables in all subsequent tests. For illustration, Figure 1 plots four of the resulting detrended series: effective spread, execution shortfall, total trading volume, and institutional volume. The plots confirm that the above-mentioned long-term trends in the variables of interest are removed by the procedure in equation (5). In Section A.4 of the Internet Appendix, we show that the results are robust to alternative methods of trend removal.

[Figure 1]

To further assuage concerns about possible confounding background processes, in Section 3.3 we also introduce a DID analysis that compares the variables of interest from U.S. data to their Canadian equivalents. Our Canadian data are daily and resemble those available through CRSP. Therefore, for the DID analyses we use spread proxies proposed by Corwin and Schultz (2012) and Abdi and Ranaldo (2017).

2.6 Innovation and proprietary traders: an illustration

Throughout the study, we focus on the link between innovation and activities of technologically advanced firms that dominate modern trading. Our premise is that such firms use the latest technology and are therefore likely to be the primary beneficiaries of exchange innovation. Verifying this premise is challenging due to the lack of comprehensive firm-level data for the entire study period. Nonetheless, we have access to a dataset of trades by HFTs on Nasdaq in 2008-2009. This dataset has been widely used by prior research (e.g., Brogaard, Hendershott, and Riordan (2014), O'Hara, Yao, and Ye (2014)) and allows us to divide Nasdaq trades into those with and without HFT involvement.

During the two-year period covered by the dataset, Nasdaq files six patents. Among these, three patents aim to enhance the speed of order processing by the exchange engine, an issue of significant interest for HFT algorithms. The remaining three patents modify the computation of prices during trading halts. These latter three patents also correspond to periods of heightened volatility brought about by the 2008 financial crisis. In contrast, the three speed patents are filed either prior to the onset of the crisis, in very early 2008, or after the crisis-related volatility subsides in late 2009. We therefore focus on these three patents.

If technologically advanced firms quickly adopt the latest technology offered by exchanges, their activity may change soon after the technology is introduced. To gauge if this is the case, we compute the share of volume represented by trades with HFT involvement in the first 15 days after a patent filing. To ease comparison across patents, we standardize the volume share variable. The results in Figure 2 show a discernible uptick in the share of HFT volume within days after a patent filing. In economic terms, this change represents a 1.6% increase per patent. As such, tech-savvy firms respond to exchange innovation and do so quickly.

[Figure 2]

3. Empirical results

3.1 TAQ-based trading costs

We begin the analysis by examining the effects of exchange innovation on liquidity metrics derived from TAQ. Figure 3 provides an illustration of these effects focusing on quoted and effective spreads in a simple event study setting. We report daily averages of the spread metrics in the pre-implementation and the post-implementation periods as well as the average for each period. To better relay the spread magnitudes, in this illustrative exercise we use the figures that are not detrended. The variables are detrended in all subsequent tests. Quoted spreads decline from 10.71 bps to 10.47 bps, a 0.24 bps change. Meanwhile, effective spreads decline by 0.36 bps. We note that the average implementation period contains 4.06 patents, so the above-mentioned reductions are 0.06 and 0.09 bps per patent.

[Figure 3]

Prior studies (e.g., Hendershott, Jones, and Menkveld (2011), O'Hara and Ye (2011), Brogaard, Hagströmer, Nordén, and Riordan (2015)) show that volume and volatility are first-order trading cost determinants that should be controlled for in liquidity cost analyses. Greater volatility is often associated with the arrival of new information and therefore tends to positively affect liquidity costs. Meanwhile, in regressions that control for volatility, greater trading volume is associated with increased participation by the uninformed and therefore tends to be negatively related to liquidity costs. We account for the effects of these variables in the regression framework that follows.

Since our data contain multiple individual patent filings, we rely on a stacked regression approach commonly used in such circumstances (Wooldridge (2010)). Specifically, we arrange the dataset as a panel that stacks, for each patent filing, a set of six-month-long event windows, with one window for each sample stock. The windows contain daily data for the three-month periods preceding and following the implementation period. Using this dataset, we run the following regression:

$$DepVar_{itp} = \alpha + \beta_1 Post_{tp} + \beta_2 Volume_{itp} + \beta_3 Volatility_{itp} + \varepsilon_{itp}, \tag{6}$$

where $DepVar_{itp}$ is the estimated residual from equation (5) representing detrended quoted or effective spreads in stock i on day t for a patent p filing, $Post_{tp}$ is a dummy variable equal to one in the post-implementation period for patent p and zero during the pre-implementation period, $Volume_{itp}$ is the detrended trading volume, and $Volatility_{itp}$ is the detrended difference between the high and low prices on day t scaled by the high price. All non-dummy variables are standardized by stock-year. This means that from each stock-day observation, we subtract the time series mean for the stock-year and then divide this difference by the corresponding standard deviation. With this adjustment, the model incorporates stock and year fixed effects. The standard errors are clustered by stock and day. We do not incorporate day fixed effects as they would be multicollinear with the $Post_{tp}$ variable.

Bessembinder, Hao, and Zheng (2020) show that a shock to market quality on a single exchange transcends through the entire marketplace and affects liquidity on all other markets. In particular, they report that a liquidity improving program administered by the NYSE results in better liquidity across the entire exchange landscape. The authors attribute this finding to strategic complementarity effects, whereby better NYSE liquidity allows market makers on other exchanges to reduce their inventory costs by trading against better NYSE quotations. Based on this finding, the focus of equation (6) is the effects of patent introductions on marketwide liquidity.

Table 2 reports univariate (Panel A) and regression results (Panel B) quantifying innovation-related changes in quoted and effective spreads. Both variables decline post-implementation. In the regression setting without controls (specifications 1 and 3), implementation leads to lower quoted and effective spreads with statistically significant declines of, respectively, 0.049 and 0.052 standard deviations. These figures translate to spread reductions of 0.045 and 0.056 bps per patent.[8]

[Table 2]

Specifications 2 and 4 include volume and volatility controls. The economic significance of the *Post* coefficients declines somewhat, to 0.037 and 0.040 standard deviations, representing per-patent quoted and effective spread reductions of 0.034 and 0.042 bps, respectively. In Section 3.4, we show that volume and volatility also experience changes after the introduction of new

[8]To obtain these figures, we first multiply the estimated coefficients by the time series standard deviations discussed in Section A.5 of the Internet Appendix. Such standard deviations are best suited for assigning economic significance to coefficient estimates from event-study regressions. We then divide the resulting product by the number of patents in a typical post-implementation period.

exchange technologies. As a result, they likely absorb a portion of the economic effect of innovation when used as regressors. We suggest that it may be sensible to view the results with controls as the lower bound of the innovation effects and the results without controls as the upper bound.

At first glance, the economic significance of the estimates mentioned above may appear trivial, but we note that it represents a change per patent. Taken together, the cumulative effect of all patents implemented during the sample period amounts to a 6.60 bps reduction in quoted spreads and a 8.15 bps reduction in effective spreads when we use the estimates from regressions with controls. Given the sample averages of these statistics reported in Table 1, respectively 10.59 and 9.09 bps, the cumulative effects are rather economically significant.

When interpreting the cumulative effects, it is important to remember that exchange innovation does not occur in a vacuum. While innovation may reduce trading costs, other changes to market structure such as the expansion of direct market access (Chakrabarty, Jain, Shkilko, and Sokolov (2021)), the emergence of dark trading (Zhu (2014), Comerton-Forde and Putniņš (2015)), and the proliferation of latency arbitrage (Shkilko and Sokolov (2020), Aquilina, Budish, and O'Neill (2022)), just to name a few, have been shown to increase transaction costs. These effects partly offset those of innovation. Therefore, exchange innovation should be viewed as one of many determinants of liquidity costs in a complex system of interactions.

3.2 Trading cost components

Declines in liquidity costs are often attributed to reductions in the cost of liquidity provision. Researchers typically examine two components of this cost, price impacts and realized spreads.

The former reflects the cost incurred by liquidity providers due to trading against informed order flow, and the latter captures (i) the remaining costs, including those associated with inventory holdings, and (ii) liquidity provider profits.

Table 3 reports that both price impacts and realized spreads decline post-implementation, respectively, by 0.030 and 0.011 standard deviations in regressions without controls and by 0.021 and 0.011 standard deviations in regressions with volatility and volume controls. The former result is consistent with the notion that exchange innovation allows liquidity providers to reduce exposure to adverse selection. The interpretation of the latter result is more nuanced. On the one hand, it may suggest that innovation helps liquidity providers better manage inventory and other costs. On the other hand, it may indicate that a typical round of innovation leads to greater competition among liquidity providers reducing their profits. These effects are not mutually exclusive, and may both be at play.

[Table 3]

3.3 Difference in differences

Events that we study affect all sample stocks at once. It is therefore possible, although unlikely, that another series of events occurring simultaneously with exchange innovation may confound the results. When it comes to this concern, one mitigating factor is that the sample includes multiple patent filings that are spread across multiple years. Aside from the long-term trend that we account for via detrending, it is difficult to think of recurring events that are sufficiently persistent to confound the findings.

This said, to further assuage concerns about confounding effects we turn to a DID setup. To do so, we use a control sample of stocks that trade in Canada matched to the main U.S. sample by market capitalization and trading volume. Firm sizes and trading volumes routinely change during our multi-year sample period. To keep up with these changes, we re-match each sample stock with the most suitable control at the beginning of each calendar year. For instance, in 2012 we match the stocks by their average market capitalization and total trading volume in 2011.

Our Canadian data have daily granularity and resemble CRSP data. To compare the two samples, we rely on three low-frequency spread proxies developed for CRSP-like datasets: (i) the end-of-day quoted spread, EOD, (ii) the Corwin and Schultz (2012) effective spread proxy, *CS*, and (iii) the effective spread proxy of Abdi and Ranaldo (2017), *AR*. Abdi and Ranaldo (2017) show that the EOD quoted spread is the most accurate low-frequency liquidity proxy. Still, since our high-frequency metrics distinguish between quoted and effective spreads, we complement the EOD quoted spreads with the two above-mentioned effective spread proxies.

For the DID analysis, we use the differences denoted by Δ between the sample stocks and their matches in a regression setup similar to that discussed earlier:

$$\Delta DepVar_{itp} = \alpha + \beta_1 Post_{tp} + \beta_2 \Delta Volume_{itp} + \beta_3 \Delta Volatility_{itp} + \varepsilon_{itp}, \tag{7}$$

where $\Delta DepVar_{itp}$ is the difference between the detrended spread proxies (i.e., EOD, *CS*, and *AR*) computed for the event window for patent p on a day t for a pair of stocks i that includes a U.S. sample stock and its matched Canadian counterpart, $Post_{tp}$ is a dummy variable equal to one during the post-implementation period and zero during the pre-implementation period, and $\Delta Volume_{itp}$ and $\Delta Volatility_{itp}$ controls are the differences between the detrended U.S. and Canadian volume and volatility estimates. As previously, this regression standardizes all non-dummy variables by stock-year and uses standard errors clustered by stock and day.

Table 4 confirms the earlier results in that exchange innovation is followed by a decline in quoted and effective spreads. The $Post_{tp}$ coefficient estimates are statistically significant with a negative sign across all specifications. These results allow us to argue with added confidence that liquidity improvements documented thus far are driven by U.S. exchange innovation.

[Table 4]

3.4 Volume and volatility

While volume and volatility serve as controls in the earlier sections, here, we ask if these variables themselves change due to innovation by exchanges. Our expectations are largely informed by the results in Section 3.2 showing that new technologies tend to help market makers reduce adverse selection of their quotes. As market makers become more adept at mitigating the consequences of interactions with informed traders, trading volume generated by the latter is likely to decline, potentially reducing overall volume. Furthermore, given the positive association between informed trading and volatility, volatility is also likely to decline.

Alternatively however, the lower trading costs reported in Section 3.1 may attract additional trading volume, as some market participants may engage in more trading when it is cheaper to do so. This trading is likely to be relatively uninformed, as uninformed traders are more sensitive to the level of liquidity costs (Shkilko and Sokolov (2020)). Consequently, volatility may decline further. To sum things up, while our predictions for volume are somewhat ambiguous, our predictions for volatility are more certain.

Table 5 examines these possibilities using simple differences (Panel A) and the equation (6) framework (Panel B). In volume (volatility) regressions, we naturally omit volume (volatility) as a control. As previously, all non-dummy variables are detrended. Consistent with our expectations, all models point to volatility reductions post-implementation. Meanwhile, the volume results are contingent on the specification; volume decreases in the model without the volatility control and increases in the model with the control. As we mention previously, it is possible that the volatility control proxies for informed trading. With this, it appears that as informed trading volume declines post-implementation, so does total volume, as observed in specification 1. Meanwhile, when the volatility control proxies for informed trading in specification 2, the $Post_{tp}$ coefficient points to a possible increase in uninformed volume.

[Table 5]

3.5 Institutional trading costs and volumes

The reductions in spreads documented in the previous section suggest that innovation by stock exchanges may improve liquidity for small trades. Markets however serve a wide range of participants, and smaller spreads may not necessarily imply lower execution costs for all of them, particularly those who trade large amounts. To examine this issue, we take a closer look at institutional trading volume and execution shortfall.

In Table 6, the results for execution shortfall in the regression model without controls (specification 1) are statistically insignificant, while the results in the full model with controls (specification 2) are significant and positive. Further investigation suggests that the discrepancy is due to the volatility control. It therefore appears that the pure effect of innovation on execution shortfall is to increase it, but this effect is offset by the reduction in volatility. As innovation reduces volatility, and given that volatility is positively related to shortfall, it cancels out the negative direct effect of innovation on shortfall, resulting in a net-zero effect.

Notably, the data also suggest that innovation is accompanied by a decline in institutional trading volume, and this result is independent of specification. Institutional volume declines by at least 0.028 standard deviations, which translates to 0.7% per patent. Considering the earlier finding that execution shortfall remains unchanged, the reduction in volume may be attributed to two possibilities. First, the cost of accessing a marginal unit of liquidity may increase post-implementation. Consequently, institutions may opt to access less liquidity to keep the overall costs unchanged, as measured by execution shortfall. Second, accessing liquidity may become more difficult. As discussed previously, advancements in technology may enable tech-savvy market participants to retract their quotes more efficiently when institutions seek access to these quotes, or to consume others' quotes more effectively when competing with institutions. Setting aside the reasons for the volume decline, when institutions trade less, they potentially obtain diminished gains from trade.

[Table 6]

3.6 Price efficiency

Markets achieve price efficiency in two ways. First, liquidity providers may closely monitor their quotes and adjust them to reflect the latest market conditions. Second, liquidity demanders may trade against the quotes that do not reflect the prevailing conditions, forcing them to adjust and generating adverse selection. Our earlier results show that adverse selection declines with innovation, suggesting that new exchange technology allows market makers to monitor and update their quotes more effectively. On the one hand, improved quote management may simply change the balance of responsibilities for price adjustments between arbitrageurs and market makers with no effect on the overall level of price efficiency. On the other hand, new technology may facilitate the very process of price adjustment and result in improved price efficiency, especially at shorter time intervals where such technology is likely the most beneficial.

We examine these possibilities by estimating equation (6) for the three price efficiency metrics discussed earlier: the variance ratio, return autocorrelation, and price delay. The results in Table 7 suggest that exchange innovation is associated with price efficiency improvements over short horizons, with all three metrics declining post-implementation. At long horizons, the variance ratio and return autocorrelations remain the same, while price delay increases.

When interpreting the price delay result, we suggest that while new technologies may enhance market maker abilities to adjust quotes over short horizons, there may be a slight tedency for market makers to underreact compared to arbitrageurs prolonging the price drift. Setting aside this caveat, the findings are generally aligned with our expectation that new technology primarily enhances price efficiency over shorter horizons. Price efficiency statistics estimated at alternative horizons are reported in Section A.2 of the Internet Appendix and are consistent with this thesis.

[Table 7]

3.7 The cross-section of patents

Our findings so far indicate that a typical technological advance by an exchange benefits small traders while having some negative effects on institutional traders. Do institutions fare worse because they fail to pay attention to new exchange technologies? In this section we address this question by examining the effects across patents. Specifically, we follow the innovation literature (e.g., Akcigit, Baslandze, and Stantcheva (2016), Balsmeier, Fleming, and Manso (2017), Kogan, Papanikolaou, Seru, and Stoffman (2017)) and sort patents according to their citations. Our premise is that market participants affected by a patent are likely to cite it.

Having collected patent citations, we focus on those coming from two distinct groups of firms. The first group includes technologically advanced trading firms (e.g., Virtu Financial) and firms that manufacture technologies used for trading and information transmission (e.g., Intel and Xilinx).[9] In turn, the second group includes institutional investors (e.g., financial services firms such as BGC Group and Northern Trust). We refer to these two groups of firms as *High-tech* and *Institutions*. Patents cited by these two groups represent 92% of all cited patents, with high-tech firms contributing the lion's share of citations. A typical cited patent receives an average (median) of 12.2 (4.0) citations by high-tech firms and 1.9 (0.0) citations by institutions.

[9] Intel and Xilinx are two of the world's leading manufacturers of FPGAs, extensively used in modern trading. A field-programmable gate array (FPGA) is a configurable integrated circuit that can be programmed and reprogrammed after manufacturing. FPGAs are commonly used in applications that require high levels of flexibility, speed, and parallel processing capabilities such as high-speed trading. Xilinx was recently acquired by Advanced Micro Devices (AMD).

We next ask if the patents cited by the high-tech firms differ in their market outcomes from patents cited by institutions. To obtain the answer, we replace the $Post_{tp}$ dummy variable in our base regression model with two new dummies, $High\text{-}tech_{tp}$ and $Institution_{tp}$ that equal to 1 in the post-implementation period for patents that are cited, respectively, by one or more high-tech firms and by one or more institutions:

$$DepVar_{itp} = \alpha + \beta_1 High\text{-}tech_{tp} + \beta_2 Institution_{tp} + \beta_3 Volume_{itp} + \beta_4 Volatility_{itp} + \varepsilon_{itp}. \quad (8)$$

All other variables and the estimation technique are as previously described. The interpretation of the two dummy variables in this model is similar to that of variables in typical regressions. For patents cited solely by the high-tech firms or solely by institutions, the corresponding coefficients capture the effects of the respective patents. For patents cited by both groups, the effects are represented by the sum of the $High\text{-}tech_{tp}$ and $Institution_{tp}$ coefficients. As previously, regressions focusing on the TAQ variables use the 2003-2021 sample period, and Abel Noser regressions use the 2003-2013 period. For the TAQ analyses, the $Institution_{tp}$ dummy is equal to 1 if a patent has an institutional citation at any point between 2003 and 2021.

Table 8 suggests that securities exchanges pursue innovation that is heterogeneous. Our main sample results are most consistent with those for patents cited by the high-tech firms. In models without controls reported in Panel A, such patents are associated with lower trading costs for small investors and lower institutional trading volume. The results remain similar to those reported earlier when we move to models with controls in Panel B, with total volume and execution shortfall coefficients becoming positive and statistically significant as they did in the main analyses.

[Table 8]

In the meantime, patents cited by institutions are associated with no negative changes for institutional investors or small investors. In fact, in models without controls, institutional volume increases in the post-implementation period if a patent is cited only by institutions. This finding suggests that (at least some) institutions do pay attention to and have the ability to use new technologies. However, the relatively limited number of patents with institutional citations is consistent with the possibility that many technologies present challenges in terms of being effectively leveraged for institutional benefit, and therefore institutional interest in these technologies may be relatively low.

3.8 The cross-section of stocks

Prior research suggests that changes in financial markets may have different effects in the cross-section of stocks. For instance, Hendershott, Jones, and Menkveld (2011) show that automation of market making has less of an effect on liquidity in small stocks compared to large stocks. Haslag and Ringgenberg (2023) find that market fragmentation is associated with liquidity improvements in large stocks and worsening of liquidity in small stocks. Given these findings, it is of interest to examine whether the effects of exchange innovation differ in the cross-section.

To do so, we split the sample into terciles by market capitalization, with each tercile containing 200 stocks. We then estimate the following regression model:

$$\begin{aligned} DepVar_{itp} &= \alpha + \beta_1 Post_{tp} + \beta_2 Post \times Medium_{itp} + \beta_3 Post \times Large_{itp} \\ &\quad + \beta_4 Volume_{itp} + \beta_5 Volatility_{itp} + \varepsilon_{itp}, \end{aligned} \tag{9}$$

where $Medium_{itp}$ and $Large_{itp}$ are dummy variables for the medium and large stocks, and all other variables are as previously defined. In this setup, the $Post_{tp}$ dummy absorbs the effect for the small stocks, and the two interaction variables capture the incremental effects for the other two size categories, that is the effects *in addition to* the small stock effect.

Table 9 shows that virtually all findings reported earlier for the full sample are observed in the cross-section, with some results amplified in larger stocks. For instance, specification [1] shows that quoted spreads decline in stocks of all sizes, but the decline is the greatest in large stocks, with the total effect captured by the combination of $Post_{tp}$ and $Post \times Large_{itp}$ coefficients being $-0.049(= -0.025 + (-0.024))$ in the model with controls. In the meantime, trading costs proxied by effective spreads decline similarly across all stocks as indicated by the significant $Post_{tp}$ coefficient and insignificant $Post \times Medium_{itp}$ and $Post \times Large_{itp}$ coefficients (specification [2]). Many other variables of interest exhibit a similar pattern. Hence, it seems that a significant portion of technological innovation by exchanges is linked to changes that are relatively consistent in the cross-section, reflecting the ubiquitous nature of technology and its broad impact.

[Table 9]

The cross-sectional price efficiency results in Table 10 are also consistent with the earlier findings in that exchange innovation generally improves price efficiency, and the improvements tend to concentrate at shorter estimation horizons. The effects at such shorter horizons are occasionally more pronounced for the large stocks. For instance, in the model with controls, while the $Post_{tp}$ coefficient for return autocorrelation computed for the 15-second interval is -0.015 for the small stocks, suggesting a post-innovation improvement, the composite coefficient for the large stocks is $-0.022(=-0.015-0.007)$ pointing to an even greater improvement. The price delay results at long horizons exhibit a pattern similar to that observed in previous results.

[Table 10]

3.9 Why do exchanges innovate?

What drives exchanges to adopt new technologies, especially considering that innovation is costly? On the one hand, innovation may be a routine practice, whereby exchanges continuously engage in the development of new product offerings. On the other hand, innovation may be a response to periodically arising challenges such as reduced market share or advancements made by rivals. If innovation is routine, it is likely that new technologies are introduced without the influence of external triggers, simply when they are ready. Conversely, if innovation is largely driven by external challenges, its introductions should be timed to address these challenges.

We examine these possibilities using the following probit model:

$$Pr(Innovation=1)_{jm} = \alpha + \beta \Delta variable\ of\ interest_{ijm} + \varepsilon_{ijm}, \tag{10}$$

where the dependent variable is a binary indicator equal to one if exchange j innovates in month m and equal to zero otherwise, Δ*variable of interest*$_{ijm}$ is one of the following three variables: a change in exchange j's market share, a change in exchange j's volume, and a change in the number of patents filed by the rival exchanges. All changes are computed over the preceding month. As previously, all variables are detrended, and therefore the changes in variables represent deviations from an expected value of zero. We note that there are two sources of variation in the exchange market share and volume – time series and cross-sectional variation. To account for the latter, for these two variables, we estimate the model using stock-exchange-month panels and use the stock, exchange, and year fixed effects. When estimating the model that uses patents filed by the rivals, we use exchange-month panels and exchange and year fixed effects.

The results in Table 11 are consistent with both the routine and strategic response explanations. Based on the marginal effects reported in square brackets, a one standard deviation decrease in an exchange's market share (specification 1) increases the probability of introducing a new technology by 0.2%. The comparable figures for changes in exchange volume and innovation by the rivals are 0.5% and 0.1%, respectively, indicating that exchanges innovate more after they lose volume and after the competitors innovate. To assess the economic significance, these figures should be compared to the base probability of innovation during the same time period, which is approximately 10.8%. Consequently, even though strategic considerations appear to incentivize exchanges to introduce new technologies, they explain a relatively small share of innovative activities.

[Table 11]

A related question is whether introducing new technologies benefits the innovating exchanges by increasing the volume they attract and their market share. We find no evidence of such benefits. Therefore, it appears that innovation is a routine activity that enables exchanges to stay competitive, rather than one that notably enhances their competitive standing.

4. Conclusion

Securities exchanges continuously innovate, offering new products and services to their customers. Theoretical models predict that such innovation may have both positive and negative effects on market quality. Prior empirical studies have examined individual cases of exchange innovation and found that some new technologies are beneficial for liquidity, whereas others may be disadvantageous. In this study, we ask if exchange innovation is beneficial as a whole, and if all market participants capture the benefits.

To answer this question, we use a multi-year sample of new product and service offerings by the U.S. stock exchanges. These offerings range from technological enhancements in data dissemination to improvements in order processing by exchange engines. The results suggest that exchange innovation is generally associated with lower trading costs for investors who trade in small quantities. In the meantime, institutional investors do not enjoy a similar benefit. Their trading costs do not decrease, and their market participation declines.

We ascribe these results to the unique ecosystem that prevails in most modern securities markets, where tech-savvy trading firms play a dominant role in supplying liquidity and also consume substantial amounts of liquidity in the process of inventory management and arbitrage.

These firms are more technologically nimble than other market participants and therefore are able to capture the benefits of innovation better than others. Since liquidity provided by such firms tends to benefit investors seeking to trade small rather than large quantities, the effects of exchange innovation vary in trader size.

Our results do not imply that exchange innovation is nefarious. Neither do we suggest that exchanges cater to certain groups of market participants over others. Instead, the findings are consistent with the notion that in a highly technologically advanced financial industry, some participants lag behind in their ability to benefit from technological progress. In this light, theoretical models proposing that investors could benefit if trading venues are separated into the fast and the slow may hold merit.

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Table 1: Descriptive Statistics

The table reports sample summary statistics. In Panel A, these include market capitalization, daily trading volume, share price, and price volatility, with the latter computed as the difference between the day's high and low prices scaled by the high price. Panel B reports liquidity metrics computed from TAQ, including quoted, effective, and realized spreads as well as price impacts. Panel C reports Abel Noser institutional daily trading volume and execution shortfall. Panel D reports price efficiency statistics computed from TAQ, including the variance ratio, return autocorrelation, and the price delay. The TAQ data cover the entire sample period of 2003-2021, whereas Abel Noser data end in 2013. Panel D reports price efficiency statistics including the variance ratio, return autocorrelation, and price delay. We first compute the averages for each stock during the sample period and then compute the reported statistics by averaging across stocks.

	Mean	St. dev.	25th	Median	75th
Panel A: Sample characteristics					
Market capitalization, \$B	19.70	44.86	2.51	5.98	16.46
Volume, sh. '000	2,856	6,326	460	1,078	2,684
Price, \$	68.15	124.33	33.30	49.81	73.43
Volatility	0.03	0.01	0.02	0.02	0.03
Panel B: TAQ liquidity metrics, bps.					
Quoted spread	10.59	8.17	4.94	8.40	13.67
Effective spread	9.09	6.43	5.17	7.10	11.01
Price impact	6.36	4.08	3.57	5.30	8.13
Realized spread	2.81	2.66	1.53	1.98	3.06
Panel C: Abel Noser metrics					
Institutional volume, sh. '000	262	451	49	109	281
Execution shortfall, bps	13.95	9.90	7.61	13.03	19.69
Panel D: Price efficiency metrics					
Variance ratio (15s, 60s)	0.123	0.014	0.114	0.120	0.128
Variance ratio (60s, 300s)	0.195	0.012	0.188	0.194	0.200
Return autocorrelation (15s)	0.057	0.008	0.052	0.056	0.060
Return autocorrelation (300s)	0.124	0.006	0.120	0.124	0.128
Price delay (15s)	0.350	0.149	0.228	0.352	0.451
Price delay (300s)	0.468	0.099	0.400	0.467	0.536

Table 2: Liquidity Costs

The table contains univariate results (Panel A) and regression coefficient estimates (Panel B) that measure the effects of exchange innovation on quoted and effective spreads. Panel A compares the metrics in the pre-implementation window to those in the post-implementation window. Panel B reports coefficient estimates from the following model:

$$DepVar_{itp} = \alpha + \beta_1 Post_{tp} + \beta_2 Volume_{itp} + \beta_3 Volatility_{itp} + \varepsilon_{itp},$$

where $DepVar_{itp}$ is the estimated residual from equation (5) representing a detrended quoted or effective spread in stock i on day t for a patent p filing, $Post_{tp}$ is a dummy variable equal to one in the post-implementation period for patent p and zero during the pre-implementation period, $Volume_{itp}$ is the detrended trading volume, and $Volatility_{itp}$ is the detrended difference between the high and low prices on day t scaled by the high price. All non-dummy variables are standardized by stock-year. This means that from each stock-day observation, we subtract the time series mean for the stock-year and then divide this difference by the corresponding standard deviation. With this adjustment, the model incorporates stock and year fixed effects. The standard errors (in parentheses) are clustered by stock and day. We do not incorporate day fixed effects as they would be multicollinear with the $Post_{tp}$ variable. Asterisks *** and * denote the 0.01 and 0.10 levels of statistical significance.

	Quoted Spread				Effective Spread			
	[1]		[2]		[3]		[4]	
Panel A: Univariate results, bps								
Pre	10.71				9.26			
Post	10.47	***			8.90	***		
Panel B: Regression results								
Post	-0.049	***	-0.037	***	-0.052	***	-0.040	***
	(0.01)		(0.01)		(0.01)		(0.01)	
Volume			-0.187	***			-0.002	
			(0.01)				(0.01)	
Volatility			0.302	***			0.246	***
			(0.01)				(0.01)	
Intercept	0.025	***	0.018	*	0.026	***	0.020	***
	(0.01)		(0.01)		(0.01)		(0.01)	

Table 3: Liquidity Cost Components

The table contains univariate results (Panel A) and regression coefficient estimates (Panel B) that measure the effects of exchange innovation on price impacts and realized spreads. Panel A compares the metrics in the pre-implementation window to those in the post-implementation window. Panel B reports coefficient estimates from the following model:

$$DepVar_{itp} = \alpha + \beta_1 Post_{tp} + \beta_2 Volume_{itp} + \beta_3 Volatility_{itp} + \varepsilon_{itp},$$

where $DepVar_{itp}$ is the estimated residual from equation (5) representing one of the detrended spread components (the price impact or realized spread) in stock i on day t for a patent p filing, $Post_{tp}$ is a dummy variable equal to one in the post-implementation period for patent p and zero during the pre-implementation period, $Volume_{itp}$ is the detrended trading volume, and $Volatility_{itp}$ is the detrended difference between the high and low prices on day t scaled by the high price. All non-dummy variables are standardized by stock-year. This means that from each stock-day observation, we subtract the time series mean for the stock-year and then divide this difference by the corresponding standard deviation. With this adjustment, the model incorporates stock and year fixed effects. The standard errors (in parentheses) are clustered by stock and day. We do not incorporate day fixed effects as they would be multicollinear with the $Post_{tp}$ variable. Asterisks *** and * denote the 0.01 and 0.10 levels of statistical significance.

	Price Impact				Realized Spread			
	[1]		[2]		[3]		[4]	
Panel A: Univariate results, bps								
Pre	6.46				2.86			
Post	6.22	***			2.75	***		
Panel B: Regression results								
Post	-0.030	***	-0.021	***	-0.011	***	-0.011	***
	(0.00)		(0.00)		(0.00)		(0.00)	
Volume			-0.037	***			0.057	***
			(0.00)				(0.00)	
Volatility			0.198	***			-0.010	
			(0.00)				(0.00)	
Intercept	0.015	***	0.010		0.005	*	0.005	*
	(0.00)		(0.00)		(0.00)		(0.00)	

Table 4: Difference-in-Differences Analysis

The table contains coefficient estimates from a difference-in-differences regression that compares liquidity metrics for the main sample of U.S. stocks and a sample of matched Canadian stocks. To find suitable matches, we select Canadian stocks that are closest to the sample stocks by market capitalization and trading volume estimated during the previous sample year. Our Canadian data have daily granularity and resemble CRSP data. To compare the two samples, we rely on three low-frequency spread proxies developed for CRSP-like datasets: (i) the end-of-day quoted spread, *EOD*, (ii) the Corwin and Schultz (2012) effective spread proxy, *CS*, and (iii) the effective spread proxy of Abdi and Ranaldo (2017), *AR*. For the DID analysis, we use the differences, denoted by Δ, between the sample stocks and their matches in a regression setup similar to that discussed earlier:

$$\Delta DepVar_{itp} = \alpha + \beta_1 Post_{tp} + \beta_2 \Delta Volume_{itp} + \beta_3 \Delta Volatility_{itp} + \varepsilon_{itp},$$

where $\Delta DepVar_{itp}$ is the difference between the detrended spread proxies (i.e., *EOD*, *CS*, and *AR*) computed for a patent p on a day t that falls into the event window for a pair of stocks i that includes a U.S. sample stock and its matched Canadian counterpart, $Post_{tp}$ is a dummy variable equal to one during the post-implementation period and zero during the pre-implementation period, and $\Delta Volume_{itp}$ and $\Delta Volatility_{itp}$ controls are the differences between the detrended U.S. and Candian volume and volatility estimates. All non-dummy variables are standardized by stock-year. This means that from each stock-day observation, we subtract the time series mean for the stock-year and then divide this difference by the corresponding standard deviation. With this adjustment, the model incorporates stock and year fixed effects. The standard errors (in parentheses) are clustered by stock and day. We do not incorporate day fixed effects as they would be multicollinear with the $Post_{tp}$ variable. Asterisks ***, **, and * denote the 0.01, 0.05, and 0.10 levels of statistical significance.

	Quoted spread		Effective spread			
	Δ EOD		Δ Corwin-Schultz		Δ Abdi-Ranaldo	
	[1]	[2]	[3]	[4]	[5]	[6]
Post	-0.060 ***	-0.051 ***	-0.054 ***	-0.056 ***	-0.046 ***	-0.045 ***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
ΔVolume		-0.242 ***		0.042 ***		-0.054 ***
		(0.01)		(0.01)		(0.01)
ΔVolatility		0.387 ***		0.148 ***		0.161 ***
		(0.01)		(0.01)		(0.01)
Intercept	0.024 **	0.021 *	0.024 ***	0.025 ***	0.020 ***	0.020 ***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)

Table 5: Volume and Volatility

The table contains univariate results (Panel A) and regression coefficient estimates (Panel B) that measure the effects of exchange innovation on trading volume and price volatility. Panel A compares the metrics in the pre-implementation window to those in the post-implementation window. Panel B reports coefficient estimates from the following model:

$$DepVar_{itp} = \alpha + \beta_1 Post_{tp} + \beta_2 Volume_{itp} + \beta_3 Volatility_{itp} + \varepsilon_{itp},$$

where $DepVar_{itp}$ is the estimated residual from equation (5) representing the detrended share volume or volatility in stock i on day t for a patent p filing, $Post_{tp}$ is a dummy variable equal to one in the post-implementation period for patent p and zero during the pre-implementation period, $Volume_{itp}$ is the detrended trading volume, and $Volatility_{itp}$ is the detrended difference between the high and low prices on day t scaled by the high price. All non-dummy variables are standardized by stock-year. This means that from each stock-day observation, we subtract the time series mean for the stock-year and then divide this difference by the corresponding standard deviation. With this adjustment, the model incorporates stock and year fixed effects. The standard errors (in parentheses) are clustered by stock and day. We do not incorporate day fixed effects as they would be multicollinear with the $Post_{tp}$ variable. Asterisks *** and ** denote the 0.01 and 0.05 levels of statistical significance.

	Volume				Volatility			
	[1]		[2]		[3]		[4]	
Panel A: Univariate results								
Pre	2,889				0.025			
Post	2,836	**			0.024	***		
Panel B: Regression results								
Post	-0.014	**	0.011	**	-0.049	***	-0.042	***
	(0.01)		(0.01)		(0.01)		(0.01)	
Volume							0.494	***
							(0.01)	
Volatility			0.494	***				
			(0.01)					
Intercept	0.007		-0.005		0.025	***	0.021	***
	(0.01)		(0.01)		(0.01)		(0.01)	

Table 6: Institutional Trading Costs and Volume

The table contains univariate results (Panel A) and regression coefficient estimates (Panel B) that measure the effects of exchange innovation on execution shortfall and institutional trading volume. Panel A compares the metrics in the pre-implementation window to those in the post-implementation window. Panel B reports coefficient estimates from the following model:

$$DepVar_{itp} = \alpha + \beta_1 Post_{tp} + \beta_2 Volume_{itp} + \beta_3 Volatility_{itp} + \varepsilon_{itp},$$

where $DepVar_{itp}$ is the estimated residual from equation (5) representing the detrended execution shortfall and share volume in stock i on day t for a patent p filing, $Post_{tp}$ is a dummy variable equal to one in the post-implementation period for patent p and zero during the pre-implementation period, $Volume_{itp}$ is the detrended trading volume, and $Volatility_{itp}$ is the detrended difference between the high and low prices on day t scaled by the high price. All non-dummy variables are standardized by stock-year. This means that from each stock-day observation, we subtract the time series mean for the stock-year and then divide this difference by the corresponding standard deviation. With this adjustment, the model incorporates stock and year fixed effects. The standard errors (in parentheses) are clustered by stock and day. We do not incorporate day fixed effects as they would be multicollinear with the $Post_{tp}$ variable. Asterisks *** and ** denote the 0.01 and 0.05 levels of statistical significance.

	Shortfall			Volume			
	[1]	[2]		[3]		[4]	
Panel A: Univariate results							
Pre	11.32			276			
Post	12.37			265	***		
Panel B: Regression results							
Post	-0.004	0.007	**	-0.033	***	-0.028	***
	(0.00)	(0.00)		(0.01)		(0.00)	
Volume		0.007	***			0.512	***
		(0.00)				(0.01)	
Volatility		0.086	***			-0.081	***
		(0.00)				(0.00)	
Intercept	0.002	-0.003		0.016	***	0.009	**
	(0.00)	(0.00)		(0.01)		(0.00)	

Table 7: Price Efficiency

The table contains univariate results (Panel A), regression coefficient estimates from models without control variables (Panel B), and regression coefficient estimates from models with control variables (Panel C) that measure the effects of exchange innovation on price efficiency. We use three efficiency metrics: the variance ratio, return autocorrelation, and price delay. The variance ratio is computed for two intervals: (i) 15 and 60 seconds and (ii) 60 and 300 seconds. The remaining two metrics are computed for 15-second and 300-second intervals. Panel A compares the metrics in the pre-implementation window to those in the post-implementation window. Panel B reports coefficient estimates from the following model:

$$DepVar_{itp} = \alpha + \beta_1 Post_{tp} + \beta_2 Volume_{itp} + \beta_3 Volatility_{itp} + \varepsilon_{itp},$$

where $DepVar_{itp}$ is the estimated residual from equation (5) representing one of the three detrended price efficiency metrics in stock i on day t for a patent p filing, $Post_{tp}$ is a dummy variable equal to one in the post-implementation period for patent p and zero during the pre-implementation period, $Volume_{itp}$ is the detrended trading volume, and $Volatility_{itp}$ is the detrended difference between the high and low prices on day t scaled by the high price. All non-dummy variables are standardized by stock-year. This means that from each stock-day observation, we subtract the time series mean for the stock-year and then divide this difference by the corresponding standard deviation. With this adjustment, the model incorporates stock and year fixed effects. The standard errors (in parentheses) are clustered by stock and day. We do not incorporate day fixed effects as they would be multicollinear with the $Post_{tp}$ variable. Asterisks *** and * denote the 0.01 and 0.10 levels of statistical significance.

	Variance Ratio				Return Autocorrelation				Price Delay			
	(15s, 60s)		(60s, 300s)		15s		300s		15s		300s	
	[1]		[2]		[3]		[4]		[5]		[6]	
Panel A: Univariate results												
Pre	0.124		0.195		0.057		0.124		0.354		0.463	
Post	0.122	***	0.195		0.056	***	0.124		0.345	***	0.469	***
Panel B: Regressions without control variables												
Post	-0.016	***	-0.000		-0.021	***	0.002		-0.052	***	0.022	***
	(0.00)		(0.00)		(0.00)		(0.00)		(0.01)		(0.01)	
Controls	No		No		No		No		No		No	
Panel C: Regressions with control variables												
Post	-0.013	***	-0.000		-0.018	***	0.001		-0.052	***	0.020	***
	(0.00)		(0.00)		(0.00)		(0.00)		(0.01)		(0.01)	
Controls	Yes		Yes		Yes		Yes		Yes		Yes	

Table 8: The Cross-Section of Patents

The table contains regression coefficient estimates from models without control variables (Panel A) and regression coefficient estimates from models with control variables (Panel B):

$$DepVar_{itp} = \alpha + \beta_1 High\text{-}tech_{tp} + \beta_2 Institution_{tp} + \beta_3 Volume_{itp} + \beta_4 Volatility_{itp} + \varepsilon_{itp},$$

where $High\text{-}tech_{tp}$ and $Institution_{tp}$ are dummy variables equal to one during the post-implementation period and zero during the pre-implementation period if a patent is cited by, respectively, at least one high-tech firm and at least one institution, and all other variables are as previously defined. All non-dummy variables are standardized by stock-year. This means that from each stock-day observation, we subtract the time series mean for the stock-year and then divide this difference by the corresponding standard deviation. With this adjustment, the model incorporates stock and year fixed effects. The standard errors (in parentheses) are clustered by stock and day. We do not incorporate day fixed effects as they would be multicollinear with the dummy variables. Asterisks *** and ** denote the 0.01 and 0.05 levels of statistical significance.

	TAQ						Abel Noser	
	QS	ES	RS	PI	Vol	Volat	Shortfall	InstVol
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Panel A: Regressions without control variables								
High-tech	-0.051 ***	-0.052 ***	-0.009 ***	-0.031 ***	-0.011	-0.043 ***	-0.002	-0.038 ***
	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Institution	-0.005	-0.004	-0.006	0.003	0.004	-0.005	0.000	0.019 ***
	(0.02)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Controls	No	No	No	No	No	No	No	No
Panel B: Regressions with control variables								
High-tech	-0.040 ***	-0.041 ***	-0.009 ***	-0.023 ***	0.011 **	-0.038 ***	0.011 ***	-0.025 ***
	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Institution	-0.003	-0.003	-0.006	0.004	0.006	-0.007	-0.005	0.004
	(0.02)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 9: The Cross-Section of Equities: Liquidity, Volume, and Volatility

The table contains regression coefficient estimates from models without control variables (Panel A) and regression coefficient estimates from models with control variables (Panel B):

$$DepVar_{itp} = \alpha + \beta_1 Post_{tp} + \beta_2 Post \times Medium_{itp} + \beta_3 Post \times Large_{itp} + \beta_4 Volume_{itp} + \beta_5 Volatility_{itp} + \varepsilon_{itp},$$

where $Medium_{itp}$ and $Large_{itp}$ are dummy variables for the medium and large stocks, and all other variables are as previously defined. The $Post_{tp}$ dummy absorbs the effect for the small stocks, and the two interaction variables capture the incremental effects for the other two size categories, that is the effects *in addition to* the small stock effect. All non-dummy variables are standardized by stock-year. This means that from each stock-day observation, we subtract the time series mean for the stock-year and then divide this difference by the corresponding standard deviation. With this adjustment, the model incorporates stock and year fixed effects. The standard errors (in parentheses) are clustered by stock and day. We do not incorporate day fixed effects as they would be multicollinear with the $Post_{tp}$ variable. Asterisks ***, **, and * denote the 0.01, 0.05, and 0.10 levels of statistical significance.

	TAQ						Abel Noser	
	QS	ES	RS	PI	Vol	Volat	Shortfall	InstVol
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Panel A: Regressions without control variables								
Post	-0.037 ***	-0.049 ***	-0.011 ***	-0.028 ***	-0.006	-0.044 ***	-0.005	-0.025 ***
	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Post×Medium	-0.014 ***	-0.006*	-0.000	-0.004	-0.004	-0.007 ***	0.002	-0.005
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Post×Large	-0.023 ***	-0.005	-0.000	-0.002	-0.017 ***	-0.008 **	-0.001	-0.017 ***
	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)
Controls	No	No	No	No	No	No	No	No
Panel B: Regressions with control variables								
Post	-0.025 ***	-0.038 ***	-0.011 ***	-0.019 ***	0.015 ***	-0.041 ***	0.005	-0.032 ***
	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Post×Medium	-0.013 ***	-0.004	0.000	-0.003	-0.001	-0.005 **	0.003	0.006 **
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Post×Large	-0.024 ***	-0.002	0.001	-0.001	-0.013 ***	0.000	0.001	0.006
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 10: The Cross-Section of Equities: Price Efficiency

The table contains regression coefficient estimates from models without control variables (Panel A) and regression coefficient estimates from models with control variables (Panel B):

$$DepVar_{itp} = \alpha + \beta_1 Post_{tp} + \beta_2 Post \times Medium_{itp} + \beta_3 Post \times Large_{itp} + \beta_4 Volume_{itp} + \beta_5 Volatility_{itp} + \varepsilon_{itp},$$

where $Medium_{itp}$ and $Large_{itp}$ are dummy variables for the medium and large stocks, and all other variables are as previously defined. The $Post_{tp}$ dummy absorbs the effects for the small stocks, and the two interaction variables capture the incremental effects for the other two size categories, that is the effects *in addition to* the small stock effect. All non-dummy variables are standardized by stock-year. This means that from each stock-day observation, we subtract the time series mean for the stock-year and then divide this difference by the corresponding standard deviation. With this adjustment, the model incorporates stock and year fixed effects. The standard errors (in parentheses) are clustered by stock and day. We do not incorporate day fixed effects as they would be multicollinear with the $Post_{tp}$ variable. Asterisks ***, **, and * denote the 0.01, 0.05, and 0.10 levels of statistical significance.

	Variance Ratio				Return Autocorrelation				Price Delay			
	(15s, 60s)		(60s, 300s)		15s		300s		15s		300s	
	[1]		[2]		[3]		[4]		[5]		[6]	
Panel A: Regressions without control variables												
Post	-0.014	***	0.001		-0.017	***	0.002		-0.048	***	0.015	**
	(0.00)		(0.00)		(0.00)		(0.00)		(0.01)		(0.01)	
Post×Medium	-0.002		-0.000		-0.003		0.000		-0.003		0.008	***
	(0.00)		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)	
Post×Large	-0.005		-0.003		-0.008	**	-0.000		-0.010	*	0.001	***
	(0.00)		(0.00)		(0.00)		(0.00)		(0.01)		(0.01)	
Controls	No		No		No		No		No		No	
Panel B: Regressions with control variables												
Post	-0.011	***	0.001		-0.015	***	0.000		-0.047	***	0.012	
	(0.00)		(0.00)		(0.00)		(0.00)		(0.01)		(0.01)	
Post×Medium	-0.002		-0.000		-0.002		0.001		-0.003		0.008	***
	(0.00)		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)	
Post×Large	-0.004		-0.003		-0.007	**	0.000		-0.010	*	0.002	***
	(0.00)		(0.00)		(0.00)		(0.00)		(0.01)		(0.01)	
Controls	Yes		Yes		Yes		Yes		Yes		Yes	

Table 11: The Drivers of Innovation

The table reports coefficient estimates from the following probit model:

$$Pr(Innovation = 1)_{jm} = \alpha + \beta \Delta \textit{variable of interest}_{ijm} + \varepsilon_{ijm},$$

where the dependent variable is a binary indicator equal to one if exchange *j* innovates in month *m* and equal to zero otherwise, $\Delta \textit{variable of interest}_{ijm}$ is one of the following three variables: the change in exchange *j's* market share, the change in exchange's own volume, and the change in the number of patents filed by rival exchanges. All changes are computed over the preceding month. As previously, all variables are detrended, and therefore the changes in variables represent devaitions from an expected value of zero. We note that the patent data are one-dimensional; when a patent is filed, it affects all stocks. In the meantime, there are two sources of variation in the exchange market share and volume – time series and cross-sectional variation. To account for the latter, for these two variables, we estimate the model using stock-exchange-month panels and use the stock, exchange, and year fixed effects. When estimating the model that uses patents filed by the rivals, we use exchange-month panels and exchange and year fixed effects. The marginal effects are reported in square brackets, and standard errors are in parentheses. Asterisks *** denote the 0.01 level of statistical significance.

	[1]		[2]		[3]	
Δ Exchange's market share	-0.007	***				
	[-0.002]					
	(0.00)					
Δ Exchange's volume			-0.022	***		
			[-0.005]			
			(0.00)			
Δ Patents by rivals					0.006	***
					[0.001]	
					(0.00)	
Intercept	-1.009	***	-1.011	***	-1.210	***
	(0.00)		(0.00)		(0.00)	

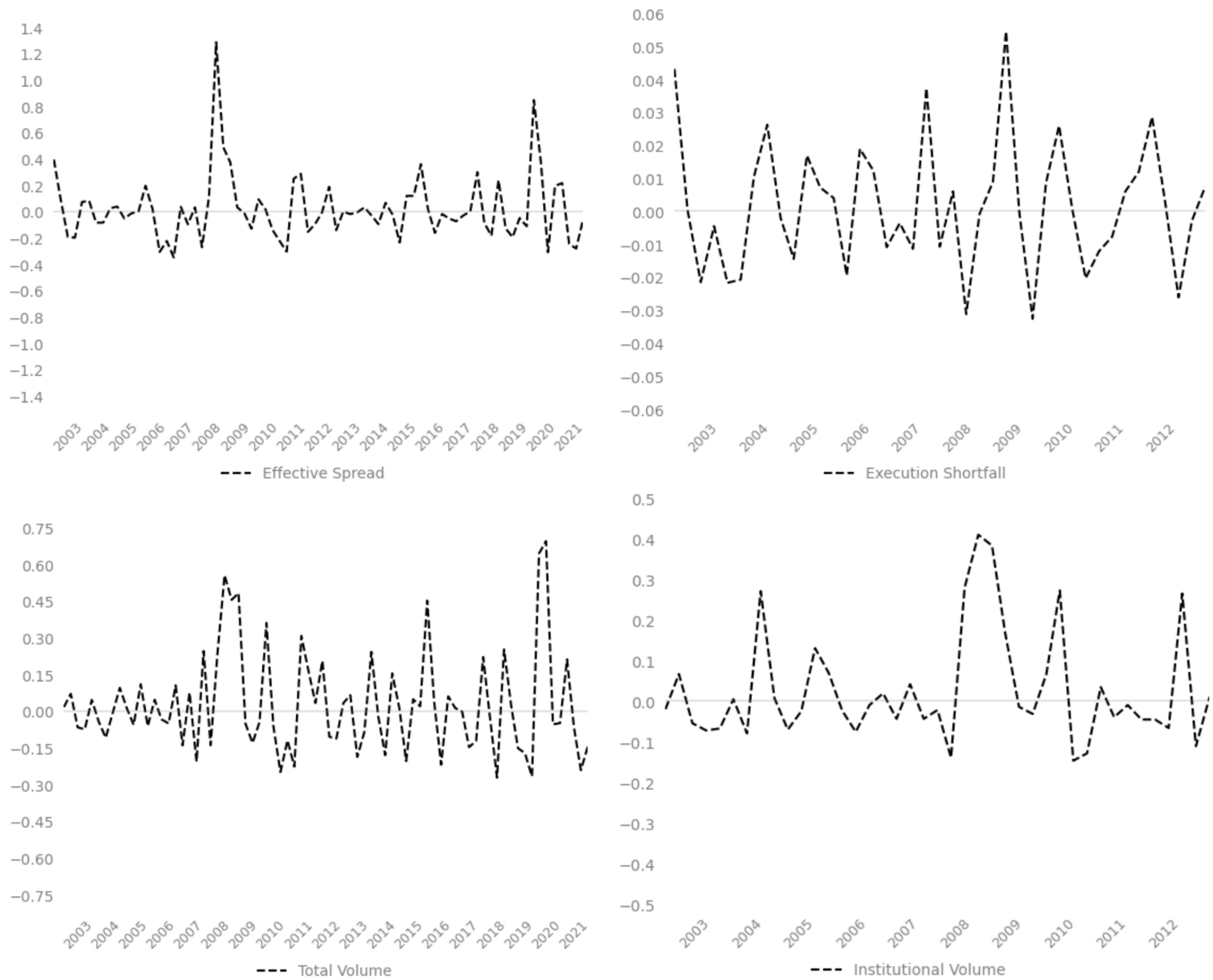


Figure 1. Examples of detrended variables

The figure plots the average residuals from equation (5) regressions of effective spread, execution shortfall, total volume, and institutional volume on a time trend. We perform the same trend adjustments for all variables of interest.

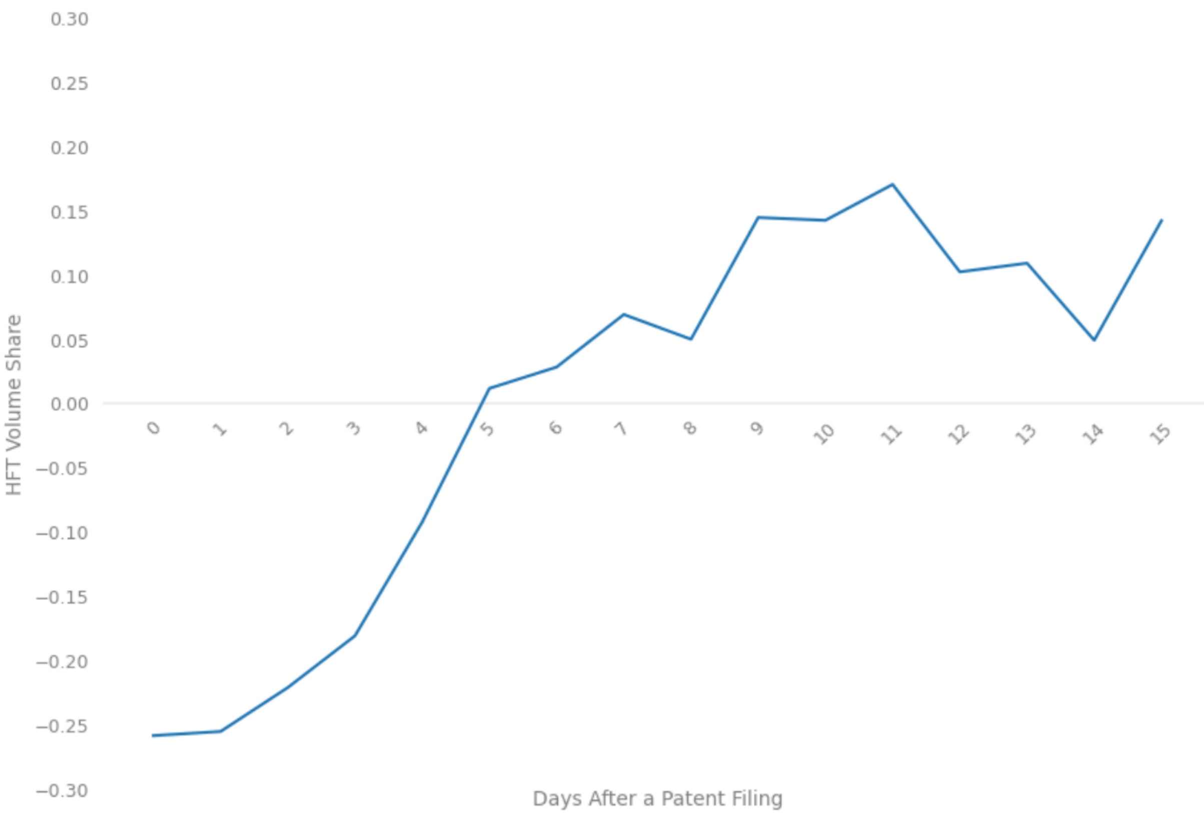
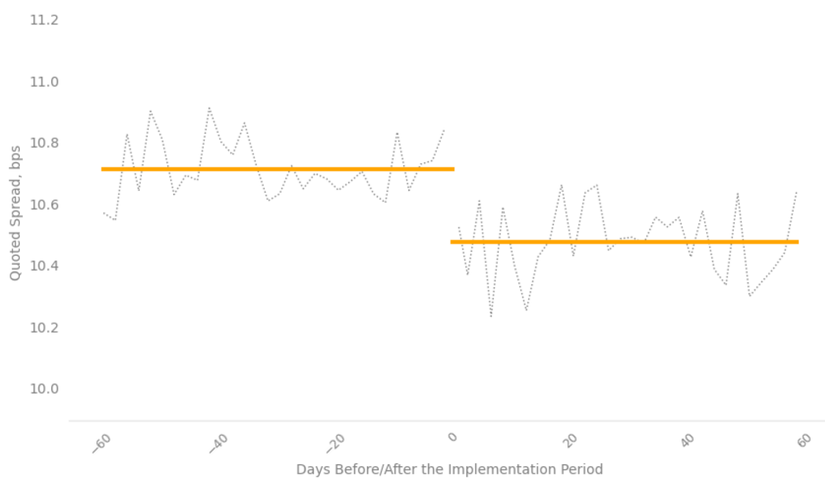


Figure 2. HFT trading after patent filings

The figure plots the share of HFT volume over a period of 15 trading days following the filing of a patent. The variable is standardized for each stock and then averaged across stocks. We focus on three patents related to improving the speed of the Nasdaq engine, filed in early 2008 and late 2009, as HFT data only cover these two years.



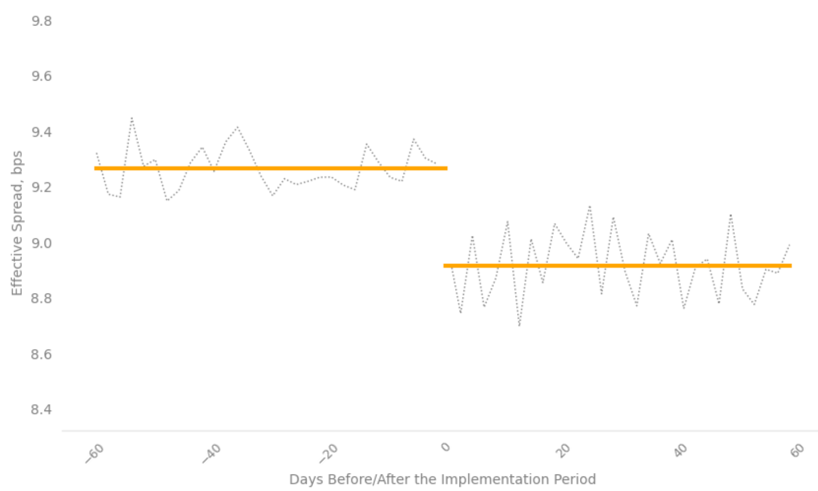


Figure 3. Displayed liquidity and trading costs around patent filings
The figures report daily quoted and effective spread estimates (dotted lines) and their averages (solid orange lines) during the pre- and post-implementation periods.

Internet Appendix

A.1 Additional price impact and realized spread results

In the main text, we report price impacts and realized spreads estimated using a 5-minute (300-second) time horizon. In Table A1, we augment these results with additional horizons using regression models without controls. Specifically, we report price impacts and realized spreads estimated at 15-, 30-, 60-, 300-, and 900-second horizons. The results are consistent with the main text, with both price impacts and realized spreads declining post-implementation.

[Table A1]

A.2 Additional price efficiency results

In the main text, we report price efficiency statistics for two estimation horizons. In Table A2, we augment these results with additional horizons. Specifically, we report the variance ratios estimated at (10s, 60s), (15s, 60s), (30s, 60s), and (60s, 300s) horizons, and we report return autocorrelations and the price delay metrics computed at 10-, 15-, 30-, 60-, and 300-second horizons. As we mention in the main manuscript, price efficiency improves post-innovation, and the improvements are most prominent at shorter estimation horizons.

[Table A2]

A.3 Alternative sample period

TAQ data are available to us between 2003 and 2021, while the Abel Noser data end in 2013. In Table A3, we show that our results are robust to shortening the TAQ sample period to match the Abel Noser sample period. Line 1 references the base case, that is years 2003-2021. Line 2, uses the shorter 2003-2013 sample period for TAQ analyses to match the Abel Noser sample period. The results are consistent with the main text.

[Table A3]

A.4 The polynomial approach to detrending

In the main text, we detrend all variables as in equation (5), which implicitly assumes that the trend affects all stocks similarly. To check if the results are robust to relaxing this assumption, we follow Hausman and Rapson (2018) and detrend the time-series on a symbol-by-symbol basis using polynomial time trend regressions. Besides detrending, we follow our standard data cleaning and adjustment procedures. Line 3 in Table A3 shows that the results obtained using this approach remain similar to those in the main text. Hausman and Rapson (2018) also suggest that it may be useful to account for possible serial dependence in the dependent variable and error terms. Following this suggestion, in line 4, we detrend using the polynomial and an additional AR(1) term. The results remain consistent with the main analysis.

A.5 Standard deviations estimated across time

In Table 1 of the main text, we report summary statistics estimated across stocks. For instance, the standard deviation of 8.17 reported for the quoted spread measures the dispersion of stock-level means. While informative about the sample, this statistic is less useful when interpreting coefficient estimates from fixed effect panel regressions that we use throughout the paper. Therefore, when discussing economic significance we rely on the time-series standard deviations across sample days rather than across sample stocks. For comparison to the above-mentioned figure, the standard deviation for the quoted spread is 3.74 when computed via this method. We report these standard deviations in Table A4.

[Table A4]

Table A1: Additional Price Impact and Realized Spread Results

The table contains regression coefficient estimates that measure the effects of exchange innovation on price impacts and realized spreads. The metrics are computed for five time horizons: (i) 15 seconds, (ii) 30 seconds, (iii) 60 seconds, (iv) 300 seconds, and (v) 900 seconds. We report coefficient estimates from the following model:

$$DepVar_{itp} = \alpha + \beta_1 Post_{tp} + \varepsilon_{itp},$$

where $DepVar_{itp}$ is the estimated residual from equation (5) representing one of the detrended spread components (the price impact or realized spread) in stock i on day t for a patent p filing, $Post_{tp}$ is a dummy variable equal to one in the post-implementation period for patent p and zero during the pre-implementation period. All non-dummy variables are standardized by stock-year. This means that from each stock-day observation, we subtract the time series mean for the stock-year and then divide this difference by the corresponding standard deviation. With this adjustment, the model incorporates stock and year fixed effects. The standard errors (in parentheses) are clustered by stock and day. We do not incorporate day fixed effects as they would be multicollinear with the $Post_{tp}$ variable. Asterisks *** denote the 0.01 level of statistical significance.

Panel A: Price impact										
	15s		30s		60s		300s		900s	
	-0.026	***	-0.046	***	-0.052	***	-0.030	***	-0.023	***
	(0.01)		(0.01)		(0.01)		(0.01)		(0.00)	
Panel B: Realized Spread										
	15s		30s		60s		300s		900s	
	-0.091	***	-0.073	***	-0.053	***	-0.011	***	-0.019	***
	(0.01)		(0.01)		(0.01)		(0.00)		(0.00)	

Table A2: Additional Price Efficiency Results

The table contains regression coefficient estimates that measure the effects of exchange innovation on price efficiency. We use three efficiency metrics: the variance ratio, return autocorrelation, and price delay. The variance ratio is computed for four intervals: (i) 10 and 60 seconds, (ii) 15 and 60 seconds, (iii) 30 and 60 seconds, and (iv) 60 and 300 seconds. The remaining two metrics are computed at 10, 15, 30, 60, and 300-second intervals. We report coefficient estimates from the following model:

$$DepVar_{itp} = \alpha + \beta_1 Post_{tp} + \varepsilon_{itp},$$

where $DepVar_{itp}$ is the estimated residual from equation (5) representing one of the three detrended price efficiency metrics in stock i on day t for a patent p filing, $Post_{tp}$ is a dummy variable equal to one in the post-implementation period for patent p and zero during the pre-implementation period. All non-dummy variables are standardized by stock-year. This means that from each stock-day observation, we subtract the time series mean for the stock-year and then divide this difference by the corresponding standard deviation. With this adjustment, the model incorporates stock and year fixed effects. The standard errors (in parentheses) are clustered by stock and day. We do not incorporate day fixed effects as they would be multicollinear with the $Post_{tp}$ variable. Asterisks *** and * denote the 0.01 and 0.10 levels of statistical significance.

Panel A: Variance ratio										
	(10s, 60s)		(15s, 60s)		(30s, 60s)		(60s, 300s)			
	-0.016	***	-0.016	***	-0.008	***	-0.000			
	(0.00)		(0.00)		(0.00)		(0.00)			
Panel B: Return autocorrelation										
	10s		15s		30s		60s		300s	
	-0.014	***	-0.021	***	-0.013	***	-0.004	*	0.002	
	(0.00)		(0.00)		(0.00)		(0.00)		(0.00)	
Panel C: Price delay										
	10s		15s		30s		60s		300s	
	-0.059	***	-0.052	***	-0.031	***	-0.012		0.022	***
	(0.01)		(0.01)		(0.01)		(0.01)		(0.01)	

Table A3: Alternative Sample Period and Detrending Procedure

The table reports the results of two robustness tests that estimate regression coefficients using (i) an alternative sample period and (ii) an alternative approach to detrending. Line 1 reports the base case, which we use throughout the paper. The base case covers the 2003-2021. In turn, line 2 examines the sample period from 2003 through 2013, for which we have Abel Noser data. In turn, lines 3 and 4 examine specifications that use polynomial detrending and an autoregressive parameter AR(1). The coefficient estimates are from the following model:

$$DepVar_{itp} = \alpha + \beta_1 Post_{tp} + \varepsilon_{itp},$$

where $DepVar_{itp}$ is the detrended quoted, effective, or realized spread, price impact, volume, volatility, execution shortfall, and institutional volume in stock i on day t for a patent p filing, $Post_{tp}$ is a dummy variable equal to one in the post-implementation period for patent p and zero during the pre-implementation period. All non-dummy variables are standardized by stock-year. This means that from each stock-day observation, we subtract the time series mean for the stock-year and then divide this difference by the corresponding standard deviation. With this adjustment, the model incorporates stock and year fixed effects. The standard errors (in parentheses) are clustered by stock and day. We do not incorporate day fixed effects as they would be multicollinear with the $Post_{tp}$ variable. Asterisks *** and ** denote the 0.01 and 0.05 levels of statistical significance.

	TAQ						Abel Noser	
	QS	ES	RS	PI	Vol	Volat	Shortfall	InstVol
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
1. 2003-21	-0.049 ***	-0.052 **	-0.011 **	-0.030 ***	-0.014 **	-0.049 ***		
(base case)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)		
2. 2003-13	-0.116 ***	-0.096 ***	-0.028 ***	-0.046 ***	-0.024 ***	-0.114 ***	-0.004	-0.033 ***
	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
3. 2003-21	-0.040 ***	-0.050 ***	-0.010 ***	-0.029 ***	-0.013 **	-0.047 ***	-0.004	-0.033 ***
(polynomial detrend)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
4. 2003-21	-0.028 ***	-0.042 ***	-0.010 ***	-0.027 ***	-0.007	-0.034 ***	-0.004	-0.022 ***
(polynomial+AR(1))	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)

Table A4: Standard Deviations Estimated Across Time

The table reports sample standard deviations computed across sample days. In comparison, Table 1 in the main text computes standard deviations across sample stocks. We first compute averages for each day during the sample period and then compute the standard deviations across days.

Panel A: Sample characteristics	
Volume, sh. ’000	1,438
Volatility	0.01
Panel B: TAQ liquidity metrics, bps.	
Quoted spread, bps	3.74
Effective spread, bps	4.31
Price impact, bps	5.31
Realized spread, bps	5.41
Panel C: Abel Noser metrics	
Institutional volume, sh. ’000	274
Execution shortfall, bps	138.20
Panel D: Price efficiency metrics	
Variance ratio (15s, 60s)	0.092
Variance ratio (60s, 300s)	0.142
Return autocorrelation (15s)	0.042
Return autocorrelation (300s)	0.092
Price delay (15s)	0.146
Price delay (300s)	0.243