Subsidizing Liquidity with Wider Ticks:
Evidence from the Tick Size Pilot Study

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**Abstract:**

Using data from the 2016-2018 tick size pilot study, we examine the efficacy of using wider tick sizes to subsidize market-making in small capitalization stocks. We demonstrate that realized spreads decay quickly within the initial microseconds of a trade. The effect reduces the subsidy offered by wider tick sizes, particularly for non-HFT market makers. The profit subsidy from wider tick sizes is also compromised by a significant shift in trading to "taker/maker" exchanges and to midpoint trading in non-exchange venues. The pilot's exception for midpoint trades also accounts for the fact that nearly a third of trading remains in non-exchange venues despite the inclusion of a trade-at rule. Overall, these findings point to considerable inefficiencies in the pilot study's goal of using wider tick sizes to subsidize liquidity provision in small capitalization stocks.

Draft Date: December 16, 2019
JEL codes: G10, G15, G18, G23, G28, K22
Keywords: tick size pilot; high-frequency trading; market making; market structure

***Statement of Financial Disclosure and Conflict of Interest*: This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.**

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## I. Introduction

In October 2016, the Securities and Exchange Commission (SEC) commenced a two-year pilot study that widens the minimum quoting increment—or "tick size"— from $0.01 to $0.05 for nearly 1,200 small capitalization stocks. Crafted in the wake of a steady decline in the number of listed companies on U.S. exchanges, the pilot study aimed to examine whether widening the tick size can enhance the liquidity and trading of small capitalization stocks.[1]

A central premise behind the tick size pilot was that "a widened tick increment could increase market maker profits and that the increased profits could foster a more robust secondary market for small capitalization stocks (and ultimately a more robust primary market) by, for example, increasing liquidity, enhancing the attractiveness of acting as a market maker, and possibly increasing the provision of sell-side research" (SEC 2015). In this regard, the pilot study was consistent with the theoretical literature on tick sizes that evaluates the regulatory choice of tick size as involving a tradeoff between minimizing transaction costs for investors and subsidizing liquidity providers to make a market in a security (Angel 1997). According to this theory, a wider tick size (such as existed prior to decimalization in 2001) should encourage dealers to make a market in a security because the market maker "pockets the spread" in a stationary market: the tick represents the minimum round-trip profit to a dealer who can buy at a lower bid price and sell at a higher offer price. Yet by the same reasoning, larger tick sizes increase transaction costs for liquidity takers who buy at the offer and sell at the bid. The tick size can thus be thought of as taxing liquidity takers in order to subsidize liquidity provision.

[1] The pilot study itself was a product of a mandate contained in the 2012 Jumpstart Our Businesses and Startups Act (JOBS Act) for the SEC to study the effects of the decimalization of stock prices in 2001 on IPOs and small and middle capitalization companies (Nallengara and Ramsay, 2013).

The notion that the market maker pockets the spread is a theoretical proposition, as it posits a stationary market. Empirically, evidence from the decimalization of stock prices in 2001 highlights how institutional features of the trading market can interfere with this straightforward relationship between tick sizes and market maker profits. For instance, while average quoted and effective spreads declined following decimalization (see, e.g., Chakravarty, Harris, and Wood (2001); Bacidore, Battalio, and Jennings (2003)), the effect was minimal for small capitalization stocks, which had large spreads prior to decimalization (Bessembinder, 2003). As another example, the smaller spreads of decimalization permitted dealers to offset any drop in profits through participating in more trades (Ronen and Weaver, 2001; Coughenour and Harris, 2004). These second order effects complicate understanding how tick sizes relate to liquidity providers' incentives.

In this paper, we use new data from the tick size pilot to examine the central premise in the theoretical literature on tick sizes and market making—namely, to what extent does widening spreads increase market maker profitability? We focus on this first order effect of wider tick sizes for the simple reason that market maker profitability is the primary channel through which wider ticks are believed to improve the liquidity of small capitalization stocks. Evidence that wider ticks result in greater market maker profitability is accordingly a necessary condition for wider ticks to achieve their stated policy objective. Conversely, evidence that market makers capture only a fraction of the enhanced transaction costs created by wider ticks would call into question the efficiency of using wider ticks to subsidize liquidity in small capitalization stocks.

We begin with an empirical finding. As noted by Rindi and Werner (2019), daily market maker profits reported to the SEC by FINRA increased by approximately 40% in transactions involving securities that were treated with nickel tick sizes during the pilot study.[2] This is a notable increase in profitability, but given that the tick size for these securities was quintupled, the increase in market maker profits is in many ways more modest than what proponents of the tick size pilot might have envisioned.

We posit that there are at least three reasons to question the efficacy of wider ticks in enhancing market maker profitability in contemporary equity markets. The first concerns the risk of informed trading in a context where liquidity provision is increasingly performed by high-frequency trading (HFT) firms. Consider, for instance, the framework of Aït-Sahalia and Saglam (2017) in which a strategic high frequency trader receives a signal about future order flows and exploits its speed advantage to optimize its quoting policy. For instance, if the bid-ask in a penny quoting environment is $10.02 x $10.08, a market maker receiving an incoming buy-order might fill the order at $10.08 by selling short with an expectation of covering at the bid of $10.02, earning 6 cents per share. In an Aït-Sahalia and Saglam (2017) framework, however, HFT liquidity providers observing the transaction may update their own buy orders rapidly, moving the best bid above $10.02 before the market maker can cover. Such a result will force the initial market maker to quote at or better than this new price, reducing its expected profits.

The ability of HFT liquidity providers to adjust quotes in this fashion can jeopardize the market maker subsidy ostensibly created by wider spreads. For instance, if the original bid-ask spread were $10.00 x $10.10 with nickel tick sizes, our market maker's ability to capture the 10 cent spread will be compromised by the incentives of HFT liquidity providers to update bids to $10.05 upon observing buying interest. Price-time priority rules observed by exchanges further complicate our market maker's ability to profit from the wider spreads: As HFT liquidity

[2] The pilot study required broker-dealers to report daily market-maker profitability to FINRA. The data, aggregated across stocks by day, is available at http://tsp.finra.org/finra_org/ticksizepilot/tsp_index.html.

providers react to the buying interest, the wider ticks should create longer queue lines at the best bid of $10.05, enhancing the challenge of covering the short position (Yao and Ye, 2014).

These long queue lines point to a second reason to question the efficacy of wider ticks in enhancing market maker profitability. Using a regression discontinuity design, Bartlett & McCrary (2012) and Kwon et al. (2015) find that, relative to the $0.0001 minimum price variation (MPV) for stocks that trade below $1.00 per share, the penny MPV for orders priced above $1.00 per share has the result of creating long queue lines at the national best bid or offer (NBBO). Because the MPV regulates quotes but not trades, however, traders seeking to avoid these long queue lines can trade in non-exchange venues at sub-penny prices, generally by means of pegged mid-point orders (Bartlett & McCrary, 2019a). In the prior example, for instance, our hypothetical liquidity provider—having gone short at $10.10—may seek to cover its position by submitting a midpoint buy order to a dark pool rather than wait in line on a displayed venue at the best bid of $10.05.

To the extent wider tick sizes result in longer queue lines, the migration of order flow away from exchanges towards midpoint orders in dark venues should accordingly place a further limitation on the effectiveness of wider spreads to subsidize liquidity providers, particularly those who display orders on exchanges. Indeed, concerns that wider tick sizes might drive trading to non-exchange venues induced the SEC to divide the pilot into three separate treatment groups. In group 1 ("TG1"), quotes were required to be priced in nickels, but trade prices were left unconstrained (similar to the current penny MPV rule.) In group 2 ("TG2"), quotes and trades were required to be priced in nickels. Finally, in group 3 ("TG3"), quotes and trades were required to be priced in nickels, and trading venues were also subject to a "trade-at" rule. The trade-at rule generally prohibited price matching by trading centers that were not already displaying a quotation at that price. As such, the trade-at rule was intended to keep more trades on exchanges. However, as we document below, trades in both TG2 and TG3 were subject to various exceptions including one for midpoint orders, which risked undermining the effectiveness of the trade-at rule given the common use of pegged midpoint orders in non-exchange venues (Bartlett & McCrary, 2019a).

The final reason to question the efficacy of wider ticks in enhancing market maker profitability relates to exchange pricing models. Most exchanges compete for orders by using either a maker/taker or taker/maker price schedule. Under a maker/taker schedule, an exchange charges liquidity takers a per-share fee for removing liquidity from the exchange, a portion of which is used to pay a rebate to the trader providing liquidity. In contrast, a smaller group of exchanges use an inverse taker/maker schedule that pays the rebate to liquidity takers, while charging a per-share fee to the liquidity provider. As Angel, Harris, and Spatt (2015) document, inverted pricing schedules have the effect of creating a finer pricing grid than the minimum tick size. In effect, the liquidity provider is paying a liquidity taker to trade on the venue, reducing the net cost to the liquidity taker and increasing the probability a liquidity taker looks first to an inverted exchange. Thus, among exchanges, inverted exchanges should stand to gain market share in the pilot study for the same reasons that non-exchanges should gain market share. The fact that liquidity providers are assessed a fee for posting liquidity on inverted exchanges, however, also reduces the nominal subsidy offered by wider tick sizes.

We find that all three of these institutional features are empirically relevant in contemporary markets, which greatly compromised the pilot study's goal of subsidizing liquidity providers by using wider tick sizes. With respect to the role of informed trading, we document that in today's trading market, price impact begins to reduce realized spreads in the initial microseconds following a trade and is larger for securities treated with nickel pricing.[3] These results are consistent with wider ticks causing greater price impact due to HFT liquidity providers optimizing quotes in response to trades. Specifically, for securities assigned to the control group in the pilot (i.e., penny priced quotes), our estimate of the price impact from a trade—measured by the percentage change in the NBBO midpoint following an observed trade—grows from approximately 2 basis points in the first microsecond after a trade to 10.7 basis points by the time one second elapses, a 4.6-fold increase.[4] However, we also show that for securities that were randomly assigned to the nickel treatment, price impact was greater and increased at a faster rate in the microseconds after a trade. For instance, for securities assigned to TG1, our estimate of price impact grows from approximately 2.8 basis points in the first microsecond after a trade to 20.4 basis points by the time one second elapses, an increase of more than 700%. Moreover, approximately 60% of this change occurs within the first millisecond of a trade. In contrast, approximately 52% of the one-second price impact change occurs in the first millisecond for the control group. Overall, these findings highlight how the HFT quoting environment can undermine efforts to subsidize liquidity providers through using wider tick sizes. They also underscore the fact that to capture the wider spread, liquidity providers need to be faster than human market makers are capable of being and indeed probably even collocated, highlighting that any movement to a nickel quoting environment is likely to favor HFT market making.

We additionally document the extent to which the longer queue lines associated with a nickel tick size can interact with the competition for order flow among exchange and non-exchange trading venues. Among exchanges, we find that inverted exchanges experienced a significant

[3] As described below, we use realized spreads as our primary proxy for liquidity provider profits.

[4] When a trade fills an existing quote on an exchange, the exchange produces a trade report and a quote report stamped to the same microsecond, reflecting the trade and the change in the exchange's best bid or offer, respectively. As such, we attribute the price impact at one microsecond to trades absorbing the displayed depth at the NBBO, forcing a mechanical change to the NBBO. We discuss this phenomenon in more detail in Section VI.

increase in quoting activity and trading volume relative to maker/taker exchanges. Likewise, consistent with our predictions, we document notable changes in both the frequency and the form of non-exchange trading. Within TG1 and TG2, for instance, the size-weighted fraction of trades occurring in non-exchange venues increased from approximately 42% to approximately 46% and 44%, respectively. The result was the opposite in TG3 where non-exchange trading dropped due to the trade-at rule, yet it still accounted for 32% of all trades.

Finally, we find that the nickel tick size resulted in a significant increase in the incidence of midpoint trades in both exchange and non-exchange venues. For instance, among non-exchange venues, the incidence among midpoint trades increased by 14% and 22.5% for securities in TG1 and TG2, respectively. Because of the trade-at rule, the effect was even more pronounced for securities within TG3, where the incidence of midpoint trades grew to over 65% of all non-exchange trades—an increase of nearly 200%. As such, the existence of the midpoint exception provides an important explanation for why nearly one-third of trades in TG3 remained in non-exchange venues despite the trade-at rule. For the same reason, it greatly limited the effectiveness of the nickel tick size to deliver a subsidy to providers of displayed liquidity.

Our findings have several implications for researchers and policy-makers. For researchers, our results provide compelling evidence that the institutional features of trading markets can interact with the tick size in ways that substantially reduce the ability of wider ticks to enhance liquidity provider profits. Moreover, by examining realized spreads beginning with the first microsecond after a trade, our study complements recent work by Conrad and Wahal (2019) who examine the term structure of realized spreads from 100 milliseconds to 600 seconds after a trade. They similarly emphasize that in electronic markets, realized spreads should be measured with much shorter time frames than has been the conventional practice. Using recent changes to the timestamps of the publicly available TAQ data, we show both the feasibility and the empirical relevance of measuring realized spreads from the first microsecond following a reported trade. This empirical strategy is also consistent with the theoretical work of Aït-Sahalia and Saglam (2017) who demonstrate that market makers are subject to adverse selection risk arising from both information asymmetries and speed asymmetries.

Finally, for policy-makers, our findings point to an inherent inefficiency in trying to use the tick size to subsidize liquidity provision for small company securities. Whatever merit this approach may have had in the past, the speed with which contemporary prices react to trades makes it extraordinarily difficult for all but the fastest traders to capture the subsidy created by the nickel tick size. To the extent the tick size pilot was motivated by a desire to reverse the effects of the decimalization of stock prices in 2001 (see, e.g., Weild, Kim and Newport, 2012), the policy accordingly suffers from a fundamental anachronism. Moreover, while liquidity takers have reduced some of the costs of wider ticks through exploiting the midpoint exception, they nevertheless pay effective spreads that are on average more than twice what they were in the penny environment. Thus, while market maker profits may have increased, our findings make it questionable whether the pilot study provides sufficient evidence that widening tick sizes would pass the type of cost-benefit analysis increasingly expected of financial reforms.

This paper proceeds as follows. Section II situates our study in light of other papers examining the tick size pilot. Section III provides an overview of our research design, describing both the tick size pilot and our use of the new microsecond timestamps. Section IV discusses our sample of tick size securities and provides, to our knowledge, the first formal test of whether the tick size pilot study produced a balanced set of control and treatment securities. Section V discusses our estimation strategy, and Section VI presents our results. Section VII concludes.

## II. Prior Studies of the Tick Size Pilot

Several papers have examined the results of the tick size pilot. Penalva and Tapia (2017) find an increase in quoted and effective spreads, as well as an increase in quoted depth up to the prices that are 100 bps (one percentage point) away from the NBBO. These finding suggest that the wider tick sizes increased trading costs for small trades but might have produced more mixed effects for large trades given the greater depth around the NBBO. In contrast, Griffith and Roseman (2019) find that among test stocks where the nickel tick size was binding, nickel pricing lead to wider quotes and a deterioration in limit order depth beyond the NBBO, suggesting nickel pricing forced large trades to walk through multiple levels of the order book indicating higher trading costs for both small and large orders.

With regard to market maker profitability, Rindi and Werner (2019) examine how the pilot affected liquidity provider profits by examining realized spreads in pilot securities. They find that spreads (quoted and effective) widened after the tick size increase, as did realized spreads for securities assigned to TG1 and TG2, while in a puzzling finding realized spreads for TG3 declined. Similar to our approach, they also examine the term structure of realized spreads and estimate realized spreads at 30, 60, 120, 180, 240, and 300 seconds after a trade. However, by beginning at 30 seconds after a trade, they do not observe how price impact affected trading across treatment groups within the initial microseconds of a trade, and thus, cannot observe how the tick size pilot may have altered the adverse selection risk posed by high speed market makers. Rindi and Werner (2019) also rely on data from Thomson Reuters Tick History. In contrast, we utilize the TAQ data with the new microsecond timestamps, allowing us to observe all trades and quote updates at the microsecond a transaction occurred at a trading venue. Using these data, we find that realized spreads increased for all three treatment groups in the initial microseconds after a trade, including for trades that occur in TG3.

Three other papers expressly examine how the pilot affected the incidence of market activity across competing venues. Lin et al. (2017) find that the first two treatment arms increased trading on non-exchange venues, while TG3 drove trading to exchanges. Focusing on the effect of the trade-at rule, Farley et al. (2018) similarly find that the market share of trading on dark venues fell from 35% to 23% for securities assigned to TG3. Comerton-Forde et al. (2019) show that TG3 resulted in a decline in the market share of trading on dark venues but an increase in the market share of trading on inverted exchanges, while TG1 increased the market share for both dark venues and inverted exchanges. Cox et al. (2017) focus exclusively on trading volume across exchange trades and similarly find that the pilot resulted in a significant increase in trading volume at inverted exchanges.

None of these papers examine the relationship of these observed effects on the incidence of market activity within the context of the pilot's goal of increasing market maker profitability. Moreover, none examine the prediction made by Bartlett & McCrary (2019a) that the pilot study would increase the incentive to trade at the midpoint of the NBBO and its implications for market marker profitability. By expressly grappling with the midpoint exception, we help explain why, despite a quintupling of the tick size, average daily market maker profits increased by just 40%. Moreover, Comerton et al. (2019), Lin et al. (2017), and Cox et al. (2017) each examine trading only with respect to the final months of 2016. These sample selection choices may explain why Comerton et al. (2019) find no change in the market share of trading for TG2 securities and Cox et al. (2017) find no statistically significant decrease in overall maker/taker trading volume. Our longer study periods and methodological approach reveal across all three treatment groups an increase in trading volume among inverted exchanges and a decline among maker/taker exchanges.

## III. Overview of Research Design

To assess empirically the extent to which a nickel environment subsidizes market markers in small capitalization firms, we exploit two recent policy interventions. The first policy intervention is the tick size pilot itself, which utilized a randomized experimental design to explore the effects of imposing a nickel tick size on small capitalization stocks. The second is the introduction of new microsecond timestamps in August 2015 by the two Securities Information Processors (SIPs), which permit us to estimate market makers' profits in the microseconds following an observed trade.[5] We provide institutional details regarding both interventions in this section.

### A. Tick Size Pilot

In all of our analyses, we rely on the fact that in implementing the two-year tick size pilot study, the SEC utilized a randomized controlled trial to study the effects of widening the tick size from a penny to a nickel. Under the terms of the pilot study, the SEC used a measurement period (the "Measurement Period") commencing three months prior to September 2, 2016 to determine which exchange-listed securities were eligible for inclusion either as a control security or a security subject to a nickel tick (collectively, the "Pilot Securities"). Specifically, a security was eligible for inclusion as a Pilot Security if (a) on the last day of the Measurement Period, the security had an aggregate market capitalization of $3 billion or less and a closing price of at least

[5] All exchanges and brokers are required to submit quote updates and trade reports occurring on a venue to one of two SIPs. Quotes and trades in NYSE-listed securities ("Tape A" securities) and securities listed on regional exchanges and their successors ("Tape B" securities) must be submitted to the Securities Industry Automation Corporation ("SIAC"), a subsidiary of the NYSE which acts as the central SIP for any transaction in Tape A and Tape B securities. Quote updates and trade reports in Nasdaq securities must be sent to the SIP managed by Nasdaq. The quote updates and trade reports sent to these two SIPs constitute the publicly available TAQ data.

$2.00 per share, and (b) during the Measurement Period, the security had a closing price on every trading day of at least $1.50 per share, a consolidated daily average volume of 1 million shares or less, and a volume-weighted average price of at least $2.00 per share. In total, 2,399 securities were identified as Pilot Securities.[6]

Following the identification of Pilot Securities, an Operating Committee consisting of representatives from all exchanges and FINRA assigned each Pilot Security to either a control group or one of three treatment groups. As noted previously, the SEC utilized three different treatment groups in light of the concerns that widening the tick size might induce traders to trade at subpenny prices in non-exchange venues given that the tick size rule conventionally applies only to quotations and not to trades. As such, Pilot Securities were assigned to one of the following four groups:

- **Control Group**: Securities in this group were quoted at their current tick size increment of a penny and remained subject to the existing rule that trades could be made in penny and subpenny prices.
- **Treatment Group 1 (TG1)**: Securities in this group were quoted in $0.05 increments (subject to limited exceptions), but otherwise could be traded in pennies or sub-pennies.
- **Treatment Group 2 (TG2)**: Securities in this group were quoted in $0.05 increments (subject to limited exceptions) and were also required to trade in $0.05 increments (subject to exceptions, including executions at the midpoint of the NBBO, certain retail investor executions and negotiated trades.)
- **Treatment Group 3 (TG3)**: Securities in this group were subject to the quoting and trading requirements of TG2 and were additionally subject to a "trade-at" requirement, which generally prevented price matching by trading centers that were not already displaying a quotation at that price (subject to certain exceptions, including those for TG2.)

[6] The original list of Pilot Securities can be found at: http://www.finra.org/sites/default/files/Tick_Size_Pilot_Selection_Process.pdf. All but 28 of the 2,399 Pilot Securities were matchable to TAQ data as of October 24, 2016, the first date of treatment assignment. The lack of a match for these securities was primarily due primarily to acquisitions and delistings. See Appendix Table 1 for an overview of the 28 unmatched stocks and why they were not matched to TAQ data. The Data Appendix provides additional discussion of how Pilot Securities were merged to the TAQ data.

Speaking loosely, TG1 can be thought of as "quote in nickels," TG2 can be thought of as "quote and trade in nickels," and TG3 can be thought of as "quote and trade in nickels, and the trade-at rule."

Operationally, in assigning securities to one of these groups, the Operating Committee first assigned all Pilot Securities to a stratum, after which securities were selected at random within stratum for inclusion in one of the three treatment groups. The strata were defined based on a security having: (1) a low, medium, or high share price based on the value-weighted average price during the Measurement Period, (2) a low, medium, or high market capitalization based on the last day of the Measurement Period, and (3) a low, medium, or high trading volume based on the average daily trading volume during the Measurement Period. In each of these three classifications, Pilot Securities were classified based on the tercile (i.e., low, medium, or high) in which it fell.

In all, this classification scheme resulted in Pilot Securities being assigned to a total of 27 strata of Pilot Securities (i.e., H-H-H, H-H-M, and so on). Because some of these strata had a small number of securities, however, the Operating Committee elected prior to randomization to combine some of the smaller strata, resulting in 21 "revised strata." Randomization yielded approximately 400 securities in each of TG1, TG2, and TG3. Those Pilot Securities not placed into the three treatment groups constituted the control group.

Announcement of the Pilot Securities and their assignments was publicly made on September 6, 2016. At the time of the initial assignment, 397 (16.66%) securities were assigned to TG1, 395 (16.58%) securities were assigned to each of TG2 and TG3, and 1,196 (50.19%) securities were assigned to the control group.[7] The tick size pilot commenced on October 3, 2016 as

[7] See http://www.finra.org/industry/test-group-assignments. While we have been unable to confirm the probability of selection and in particular whether it was constant across strata—the key condition which would obligate any analysis to condition

securities were phased into their assignment treatment over the ensuing weeks with full implementation occurring on October 31, 2016.[8]

B. Microsecond Timestamps

Because we seek to examine the quoting environment at the time of a trade as well as in the microseconds following it, our design also exploits the fact that since August 2015, the TAQ data records in microseconds the precise time at which a quote update or trade report occurred on a trading venue (see Bartlett & McCrary, 2019b). These new data permit a novel look at how adverse selection costs affect liquidity providers in today's trading environment.

In the classical setting, market makers face the possibility that a liquidity taker is better informed about the fundamental value of the security, generating adverse selection risk for the market maker (see Glosten and Milgrom, 1985). For instance, a market maker might sell to a buyer who knows that posted asks undervalue the security, resulting in an overall increase in the NBBO following the trade as the informed buyer picks off underpriced orders. Such a market maker will then have to cover the short position by buying at the higher national best bid, potentially causing a loss for the market maker. Given this adverse selection risk, conventional approaches to examining the effect of adverse selection costs on liquidity provider profits have focused on decomposing effective spreads into a *realized spread* at time horizon *t* that is captured by a liquidity provider and a *price impact* at time horizon *t* that reflects the post-trade movement in the quote midpoint that undermines a liquidity provider's ability to capture the full spread.

appropriately on strata—there is little evidence of meaningful departures from a constant probability of selection. However, conditioning on strata here guards against bias and substantially improves the precision of estimated treatment effects. Consequently, as discussed in more detail below, we fully condition on strata in all of our analyses.

[8] See http://www.finra.org/industry/tick-size-pilot-program-implementation-plan for a description of the plan rollout during October 2016.

Our framework differs from this classical setting insofar that we follow Aït-Sahalia and Saglam (2017) in assuming that price impact can also arise from HFT liquidity providers who rapidly update quotes in response to trades. To illustrate, consider again the example provided in the Introduction in which a stock was quoted at \$10.02 x \$10.08, during which a market maker filled an incoming buy-order by selling short a share at \$10.08. Assuming a post-trade movement of the NBBO to \$10.04 x \$10.08, the market maker that covered its short position at the new midpoint of \$10.06 would earn 2 cents (its realized spread) rather than 3 cents (the realized spread had she covered at the prior midpoint of \$10.05). The 1 cent increase in the quote midpoint reflects the price impact associated with this hypothetical trade.

Estimating price impact from this form of adverse selection naturally raises an empirical challenge given the need to examine price impact in the microseconds following a trade. Prior to the introduction of the SIPs' new microsecond timestamps, researchers using TAQ data to estimate price impact and realized spreads faced the practical challenge that the timestamp assigned to a quote or trade report was made in milliseconds and reflected the time that one of the two SIPs finished processing the transaction report received from the reporting venue. Given reporting latencies in the quote and trade reports sent to the SIPs, researchers were accordingly required to make assumptions about the quoting environment at the time of a trade to estimate effective spreads and price impact (see Bessembinder 2003 for a discussion). These timestamp issues also posed challenges for understanding the quoting environment in the moments after a trade. However, conventional approaches to measuring realized spreads (including the formal definition of realized spreads in SEC Rule 605) generally averted these challenges by using a 5-minute lag (or longer) following a trade for measuring realized spreads and price impact (Bessembinder 2003; Goyenko et al. 2009).

In contrast, by using the new microsecond timestamps, we can empirically estimate the price impact and realized spreads following a trade from the initial microsecond after it occurs through any point in time. As such, our approach permits not only an evaluation of whether a wider tick size produces enhanced adverse selection due to HFT optimizing, but also whether realized spreads today are more appropriately measured in the microseconds and seconds following a trade, rather than in the minutes following it.

## IV. Data

### A. Sample Construction

We obtain our sample data from the trade and quote reports from the NYSE TAQ data for Pilot Securities from March 7, 2016 through September 28, 2018, the last date of the tick size pilot.[9] Due to the implementation of the pilot study, we collect data across the following five time periods:

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| March 7, 2016 – September 2, 2016: | Pre-Pilot Period |
| September 6, 2016 – September 30, 2016: | Pilot Securities Announcement Period |
| October 3, 2016- October 31, 2016: | Pilot Phase-In Period |
| November 7, 2016-June 30, 2017: | Full Implementation Period |
| November 7, 2016-September 28, 2018: | Extended Evaluation Period |

*Pre-Pilot Period.* We commence with March 7, 2016 as this is the first date on which FINRA published a list of pre-pilot securities that were to be used to evaluate the pre-pilot trading environment. Under the Tick Size Pilot Program Implementation Plan (the "Plan"), trading centers and market makers were required to collect trading data for these pre-pilot securities until September 2, 2016, after which the data collection requirements would apply only

[9] While the tick size pilot was originally scheduled to run through October 3, 2018, the exchanges and FINRA requested and received in early September 2018 an exemption from the SEC, allowing cessation of the tick size pilot on September 28, 2018. See https://www.sec.gov/divisions/marketreg/mr-noaction/2018/tick-size-pilot-exemption-091018-608e.pdf.

to the Pilot Securities.[10] As discussed below, and to improve statistical efficiency, we use measures collected during the Pre-Pilot Period as control variables in our analyses as appropriate.

*Pilot Securities Announcement Period.* On September 2, 2016, the identity of Pilot Securities was announced by Nasdaq and the NYSE following the three-month measurement period used to identify exchange-listed securities that were eligible to be Pilot Securities. Assignment of Pilot Securities to control and treatment groups was published by Nasdaq and NYSE on September 6, 2016.

*Pilot Phase-In Period*. Implementation of the pilot occurred on five different Mondays during October 2016. Securities from TG1 and TG2 were phased in on October 3, October 10, and October 17. Securities from TG3 were phased in on October 17, October 24, and October 31.

*Full Implementation Period.* Under the Plan, all securities assigned to a treatment group were subject to the experimental treatment as of November 1, 2016. Given the technical challenges of implementing the pilot study, however, we begin our sample period on November 7, 2016, giving market participants a one-week transition period from the date of full implementation (see Figure 1, below, for evidence of phased implementation). Our primary analyses pertain to a large set of stocks that traded persistently from November 7, 2016 to June 30, 2017. Due to acquisitions, delistings, and other corporate actions following the commencement of the pilot study, the total number of Pilot Securities that satisfies this criterion is 2,026 securities.[11]

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[10] For a discussion of the Plan, see http://www.finra.org/industry/tick-size-pilot-program-implementation-plan.
[11] Details on the construction of the sample and the harmonization of information on the Plan and TAQ data are provided in the Data Appendix.

*Extended Evaluation Period.* As a robustness check and to investigate longer-term patterns, we also examine the 1,735 Pilot Securities that traded persistently from November 7, 2016 to September 28, 2018.

As in Rindi and Werner (2019), we also focus our analysis to account for the fact that the treatment effect—and therefore, the possibility for a market-maker subsidy—will generally be concentrated in those securities where quoted spreads tend to be narrower than a nickel. Yet the selection criteria for Pilot Securities implicitly focus on less liquid stocks, suggesting that many of these securities should be expected to trade at spreads in excess of 5 cents. In Table 1, we examine average quoted spreads for Pilot Securities across control and treatment groups during the five time periods noted previously, classifying securities into whether they had an average pre-pilot quoted spread of less than 5 cents (panel A) as opposed to more than 5 cents (panel B). In our analyses below, we limit our sample to those Pilot Securities having average pre-pilot quoted spreads of less than 5 cents in light of the fact that any rents provided to market makers by virtue of widening the tick size from a penny to a nickel will be largely concentrated in these securities.

[Insert Table 1]

In total, these restrictions result in a core sample of 1,087 securities that traded during the Full Implementation Period, consisting of 547 securities assigned the control group (50%), 178 securities assigned to TG1 (16%), 173 securities assigned to TG2 (16%), and 189 securities assigned to TG3 (17%). We focus below on this sample of securities, which we refer to as the "full binding sample." As noted above, we also examine trading in the 1,735 Pilot Securities that traded persistently during the Extended Evaluation Period. Among this group of securities, there were 930 with pre-period spreads below a nickel, which we refer to as the "long binding sample." Of these securities, 471 were assigned to the control group (51%), 157 were assigned to TG1 (17%), 147 were assigned to TG2 (16%), and 155 were assigned to TG3 (17%). For both the full binding sample and the long binding sample, the proportion of securities assigned to treatment and control conditions are generally in line with the original assignment of Pilot Securities discussed in Section III(A). In the interest of space, and since results for the full binding and long binding samples are highly similar, we focus in the main text on results for the full binding sample and reserve for footnotes and the Appendix analogous examinations of the long binding sample.

Additionally, to ensure that all quotes and trades occur during the trading day after the opening cross and before the closing auction, we subset the data to exclude quotes and trades occurring before 9:45:00 and after 15:34:59.999999. As noted in Holden and Jacobsen (2014), the NBBO file of the Daily TAQ file is incomplete; therefore, we manually calculate the NBBO for each security for each microsecond during our sample period using quote updates from the daily TAQ data and the standard Hasbrouck algorithm. In so doing, we restrict our analysis to those quotations that are eligible to establish an exchanges' best offer or best bid (i.e., quotation updates having a condition of A, B, H, O, R, W, or Y).

B. Balance Tests and Summary Statistics

In Table 2, we present balancing tests for the 1,087 Pilot Securities in the full binding sample. In the table, we estimate differences between TG1, TG2, TG3, and control in a manner consistent with how we approach estimating treatment effects, below. We defer until later a detailed discussion of our methodology, but at a high level, the method we use is the statistically efficient method of estimating treatment effects semiparametrically, and this implies that the balancing tests in Table 2 are the most powerful tests possible in the absence of invoking potentially erroneous parametric assumptions.

[Insert Table 2]

Turning to the results, Table 2 examines 15 different market measures. Estimates for each measure are based on observed trades from the Pre-Pilot Period, where there should be no systematic difference between control and treatment groups. The control group mean is a regression-adjusted size-weighted average for securities assigned to the control group, while estimates for each treatment group reflect treatment effects. As Table 2 shows, the difference between the treatment and control assignment is statistically insignificant at conventional levels for each of the 15 measures examined, with only the fraction of trades marked as part of an Intermarket Sweep Order having a p-value below 0.10. We conclude from the evidence in Table 2 that our sample selection criteria do little to dislodge the similarity between control and treatment groups generated by conditional randomization. Balancing tests for the long binding sample are presented in Appendix Table 2, and likewise establish similarity of pre-period market measures between control and treatment groups, with no p-value being below 0.10.

C. Compliance with Treatment Assignment

Our final preliminary test examines whether market participants complied with the treatment assignments of the Pilot Study. Figure 1 plots the fraction of quote updates priced in nickels over time by treatment assignment.[12] The figure reveals a stark increase in nickel-priced updates as Pilot Securities were phased into treatment groups. Vertical lines in the figure indicate important dates: March 7, 2016, when the tick size pilot began; September 6, 2016, when treatment assignments were announced; October 3, 2016 when treatment assignments began to be phased in; November 7, 2016, when the pilot was fully implemented; and June 30, 2017, when our core sample period closes.[13] Overall, the evidence is consistent with participants complying with the nickel-quote rule applicable to securities assigned to TG1, TG2, and TG3. For the control group, the baseline prevalence of nickel quotes is approximately one-fifth throughout the Full Implementation Period.[14] For the three treatment groups, the prevalence of nickel quotes is also around one-fifth prior to implementation, but as of early November is approximately one for all three treatment arms. As the length of the window widens, the extent of compliance declines ever so slightly as a small number of firms exit treatment status and revert to control. Overall, we view compliance with the prescriptions of the tick size pilot as nearly perfect and thus throughout this paper we take conditional differences between control and treatment group, using

[12] By "nickels," we refer to quotes with a second decimal of either 5 or 0, which is sometimes referred to in the literature as "nickels or dimes" as opposed to just "nickels." Strictly speaking, we generate the estimates in Figure 1 by applying the same methodology as in Table 2, but on a week-by-week basis. The control group curve in Figure 1 is thus analogous to the control group mean in the first column of Table 2. The curves for TG1, TG2, and TG3 represent the sum of the control group mean and the estimated treatment effect.

[13] Appendix Figure 1 is analogous to Figure 1, but pertains to the long binding sample. That figure likewise shows a high degree of compliance. Relative to Figure 1, Appendix Figure 1 contains an additional vertical line at September 28, 2018, when the pilot ended.

[14] The discerning eye will note that the rate of nickel quotes is in fact slightly above one-fifth. This is actually the expected pattern given quote clustering at nickel and dime increments. See, for example, Chung, Van Ness, and Van Ness (2004) and Blau, Van Ness, and Van Nesss (2012).

treatment assignments as of October 24, 2016, as identifying the average treatment effect of the treatment arms for both the full binding sample and the long binding sample.

[Insert Figure 1]

## V. Estimation Strategy

Given its experimental design, the pilot study permits a unique opportunity to identify the effects of the treatment assignments on trading activity. However, the structure of the pilot also raises a number of methodological questions regarding conditional randomization—a point that has yet to be examined in prior work. While the Plan repeatedly states that Pilot securities are to be assigned to control and treatment arms by randomization, a careful examination of the record reveals that randomization was conditional on strata, as opposed to unconditional. Moreover, there is no clarification in the Plan about whether the probability of assignment to TG1, TG2, and TG3 was the same across strata. This means that a simple comparison of average outcomes between the treatment and control groups does not necessarily identify the average treatment effect (ATE) typically delivered by an unconditionally randomized evaluation.[15] Instead, attention must be paid to the conditioning set upon which the Operating Committee relied before randomly selecting stock from within strata.

Conditional rather than unconditional randomization raises questions not only of possible bias alluded to above, but also questions of statistical efficiency. Traditional econometric approaches to conditional randomization would assume constant coefficients and would approach the problem using regression with indicators for treatment arms as well as strata indicators. However, the literature on treatment effects has established that greater efficiency can be obtained from somewhat different estimators (Hahn 1998, Imbens 2004). In the context

[15] Perhaps for this reason, some analyses of the pilot utilize a difference-in-differences methodology (see, e.g., Rindi and Werner, 2019), as opposed to taking advantage of the knowledge that randomization was conditional.

of randomization conditional on strata, the easiest way to describe the semiparametric efficient estimator is as a matching estimator: within each stratum, one computes differences in mean outcomes between treatment arms and control, and those stratum-specific differences are averaged using the number of stocks in each stratum as a weight. The resulting estimator—the weighted average of stratum-specific differences—is both asymptotically unbiased and semiparametrically efficient.[16]

Obtaining valid standard errors for matching estimators can be challenging computationally, however. Following Wooldridge (2010, Chapter 21), we obtain standard errors using a computational trick that allows for recovery of the efficient semiparametric estimator from a particular saturated regression model. The computational trick relies on a numerical equivalence result.[17] To obtain the regression version of the semiparametric efficient estimator, one first de-means the set of (all but one) strata indicators prior to forming interactions between the three treatment arm indicators with those strata indicators. A regression of the outcome on treatment arm indicators, strata indicators, and the interaction terms then yields coefficients on the treatment arm indicators that are numerically equivalent to the matching estimator described above. Because this particular regression estimator is numerically equivalent to the matching estimator described above, it too must be semiparametrically efficient. Moreover, as Wooldridge (2010) notes, heteroscedasticity- or cluster-robust standard errors from the regression are, up to a minor adjustment, equivalent to their corresponding matching estimator standard errors. In

[16] In light of the evidence shown in Figure 1, we ignore compliance problems.

[17] In the special case where the conditioning set for the semiparametric estimator is a set of mutually exclusive and exhaustive indicators, there are at least three numerically equivalent approaches to recovering the semiparametrically efficient estimator: reweighting (using a logit, although not a probit, model for the propensity score), matching, and regression with the described saturated model. When the conditioning set is not a set of such indicators, these three approaches can and typically do differ.

addition, the constant term from that regression recovers the efficient estimator of the outcome under the control regime.[18]

Finally, we note that while our regressions are based on large scale data sets, such as quotes or trades, the regressions involve a covariate grouping structure: the covariates only take on a fixed number of values, since neither stratum nor treatment assignment varies except cross-sectionally across stocks. This fact implies that one can reduce the data through averaging prior to running the regression with no loss of information.[19] Given the dramatic computational savings associated with running the regression at the cross-sectional level (a thousand observations instead of several million or even billion, cf., Data Appendix), we focus on cross-sectional regressions where the outcome variable is an average of the underlying data, weighted as appropriate (e.g., by size or value).

## VI. Results

In this section, we analyze the effectiveness of using the tick size pilot to enhance liquidity provider profits. As noted previously, a central goal of the pilot was to increase liquidity provider profits through increasing the quoting increment from a penny to a nickel, which was anticipated to lead to wider quoted and effective spreads. The wider tick size was also anticipated to increase quoted depth at the NBBO given that traders would have fewer price points at which to

[18] Below, we consider not just treatment effects in the sense of differences in average outcomes between control and treatment groups, but also ratios, where it is useful to have an efficient estimator of the control group mean.

[19] Formally, suppose the outcome is $Y_{ij}$ and the covariates are $X_j$, with each of the $n_j$ observations within group $j$ having the same value of the covariate. Then a direct approach computes the microdata regression coefficients $\hat{\beta}$ by computing sums over the microdata, i.e., $\hat{\beta} = \left\{\sum_j \sum_i X_j X_j'\right\}^{-1} \sum_j \sum_i X_j Y_{ij}$, where the outer sum is over the groups and the inner sums are over the observations within the group. However, we could instead recognize that a numerically equivalent computation is available from the grouped data regression $\hat{\beta} = \left\{\sum_j n_j X_j X_j'\right\}^{-1} \sum_j n_j X_j \overline{Y}_j$, where $\overline{Y}_j = n_j^{-1} \sum_i Y_{ij}$ is the sample mean of the outcome for group $j$. Moreover, up to a degrees of freedom adjustment, heteroscedasticity-robust standard errors from the grouped data regression are numerically equal to cluster-robust standard errors from the microdata regression, clustering on group. The results of Abadie, Athey, Imbens, and Wooldridge (2017) imply that unless one is willing to assert homogeneity of treatment effects, as would be implausible here, the clustered standard error approach is the correct one.

express trading interest. Quoted depth was also anticipated to increase for TG3 on account of the trade-at rule. Accordingly, we begin with an analysis of the extent to which the pilot produced these anticipated effects.

A. Quoted Spreads, Effective Spreads, and Quoted Depth

In Table 3, we present estimated effects of the pilot on spread measures and inside depth. The estimates in the table are based on the regression framework discussed in Section V and are based on the full binding sample. (In Appendix Table 3, we present analogous estimates using the long binding sample.) For each treatment arm $t$, the estimate labeled $\overline{Y_t} - \overline{Y_0}$ provides the gross change in the outcome over the control group mean, or the regression-adjusted difference in means. The estimates labeled $(\overline{Y_t} - \overline{Y_0})/\overline{Y_0}$ provide the percentage increase in the outcome relative to the control group mean, $\overline{Y_0}$.[20] In even-numbered columns, we examine the role of the pre-period outcome as a control variable for additional precision.[21]

The first two columns provide results for quoted spreads.[22] As expected, quoted spreads increased considerably across all three treatment groups. Turning to column 1, compared to the control group mean of $0.03 per share, quoted spreads for TG1, TG2, and TG3 increased by $0.029, $0.029, and $0.027 per share, respectively. For securities in our sample, the pilot thus had the effect of almost doubling the average quoted spread. The percentage differences reported in the table quantify these effects at 96%, 96%, and 89% across TG1, TG2, and TG3, respectively.

[20] Estimated percentage increases are based on the ratio of the treatment effect for arm $t$ to the control group mean, and standard errors are based on the delta method.
[21] To accommodate the pre-period quoted spreads, we simply computed the average quoted spread for each security for the Pre-Pilot Period.
[22] To prepare the data to run the regression corresponding to these columns, we identified the spread at the microsecond of each trade and computed the size-weighted mean of quoted spread across trades and days for each security, separately for the five time periods outlined in Table 1. The outcome variable for the analysis in the first two columns is the size-weighted mean over the Full Implementation Period and the sample is the full binding sample. Note that our analysis here is taking advantage of the microsecond timestamps and is based on the interleaved file described in the Data Appendix.

[Insert Table 3]

As noted, we also estimate quoted spreads including as an additional control a security's average quoted spread during the Pre-Pilot Period. These results, presented in column 2, are qualitatively the same but with standard errors that are about 10 percent smaller than those in the first column. The enhanced precision of the estimates in column 2 reflects the strong improvement in prediction afforded by including the pre-period outcome as a control. The coefficient on the average size-weighted quoted spread from the Pre-Pilot Period of 0.68 has an associated t-ratio of almost 17. Correspondingly, the inclusion of this one variable increases the R-square of the regression by about 7 percentage points, from 0.67 to 0.75. Throughout the remainder of this paper, in light of the additional precision obtained by using these pre-period controls, we focus our discussion of results on models that utilize pre-period controls for the outcome variable, but report models with and without those controls where space permits.

One concern with relying on quoted spreads, however, is that the measure is sometimes affected by extreme outliers.[23] We therefore consider two alternative spread measures; both measures indicate that the pilot increased spreads in the full binding sample by more than implied by the first two columns of the table. In the third and fourth columns, we present results for relative quoted spreads, defined as the difference between the natural log of the NBO and the natural log of the NBB as of an observed trade.[24] The relative quoted spread can be thought of as

[23] While outliers do not substantially affect our analysis during the Full Implementation Period, they do affect our analysis during the longer Extended Evaluation Period. As noted above, in Appendix Table 3, we extend our results for the full binding sample to those for the long binding sample where the influence of a remarkable outlier is apparent. In particular, for a single stock in TG2 on a single day at a single moment—SKYW on November 24, 2017 around 13:55:18—liquidity on the sell-side vanished, pushing the ask from $50, where it had fluctuated most of the day, to a stub quote of $99,999. Remarkably, this single market moment (a) increases the estimated treatment effect for TG2 by a factor of 2.5 and its estimated standard error by a factor of 30 and (b) decreases the effect of the pre-period quoted spread by a factor of 15 and increases its standard error by a factor of 15, and (c) reduces the R-square from 0.70 to 0.05. Appendix Table 3 provides detail on these points (cf., columns 1a through 2b). Generally speaking, once SKYW on November 24, 2017, is excluded from the results for quoted spread, the estimates in Appendix Table 3 are qualitatively similar to those in Table 3, but with somewhat smaller magnitudes.

[24] As with the first two columns, we first prepared the data by identifying the spread at the microsecond of each trade and then computing the size-weighted relative quoted spread across trades, separately for each security for the six time periods outlined in Table 1. The outcome is the average for a given security in the full binding sample over the Full Implementation Period.

a first order approximation of calculating the quoted spread relative to the NBBO midpoint. For readability of coefficients, we multiply the outcome by 100 (relative quoted spread is generally smaller than the quoted spread because the quoted spread (e.g., $0.01) is small relative to the midpoint (e.g., $10)). As compared to the results using the quoted spread, and as noted, using this measure of spread increases the estimated impact of the pilot on spreads. The estimated impact of the pilot was 39, 37, and 41 basis points for TG1, TG2, and TG3, respectively. Relative to the control group mean of 30 basis points, these treatment effects indicate that the relative quoted spread more than doubled. Percentage differences quantify this conclusion and are estimated to be 129%, 121%, and 134% for TG1, TG2, and TG3, respectively. These effects are estimated with some precision: across treatment arms, the smallest t-test of the null hypothesis of no effect is 10. Indeed, we can even rule out at the 95 percent confidence level the null hypothesis that the treatment effect was less than double that of the control mean.[25]

The fifth and sixth columns of Table 3 present results for relative effective spread, which is a spread measure based on the price and direction for observed trades, relative to the midpoint of the NBBO at the time of the trade.[26] For buy orders, we multiply by two the difference between the log of an observed trade price and the log of the NBBO midpoint prevailing at the moment of the trade; for sell orders, we multiple by two the difference between the log of the NBBO midpoint and the log of the trade price. As with relative quoted spread, we multiply the outcome by 100 for readability of coefficients.

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[25] The smallest t-test was 1.77 for TG2 in the fourth column, and the one-sided critical value of the t-test at the 5% level is 1.65. The other five estimates have t-tests of the null that $(\overline{Y_t} - \overline{Y_0})/\overline{Y_0} < 1$ ranging from 2.16 (TG2, third column) to 3.31 (TG3, third column).

[26] We classify trades as having been buy- or sell-side initiated using the Lee and Ready (1991) algorithm with no lag (see Bessembinder and Venkataraman, 2010).

Turning to the results for relative effective spreads, we see again that all three treatment arms witnessed a large increase in spreads. In particular, TG1 had a mean relative effective spread that was over 32 basis points higher than the control group mean of 23 basis points, while TG2 and TG3 had mean relative effective spreads that were higher by 29 and 28 basis points, respectively. In terms of percentage differences, the increase in relative effective spreads is 139%, 128%, and 121% for TG1, TG2, and TG3 respectively. As with our prior two spread measures, the estimated treatment effects are decisively different from zero in statistical terms. As well, the conclusion that relative effective spreads increased by more than a factor of two relative to control securities continues to be statistically significant at conventional levels for all three treatment arms.[27]

The final two columns of Table3 present estimated treatment effects for inside depth. We define inside depth as the average of the depth at the NBB and NBO, or one-half the sum of quoted inside depth at the bid and quoted inside depth at the ask, and we estimate treatment effects using a log dependent variable to handle inside depth's long right-hand tail.[28]

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[27] A notable difference between relative quoted spreads and relative effective spreads concerns the rank ordering of the estimated treatment effects across the three treatment arms. For instance, while TG3 had the highest relative quoted spread, it had the lowest relative effective spread. In unreported tests, we formally test the hypothesis of equal treatment effect across all three treatment arms, as well as equal treatment between any two treatment groups. In none of these tests can we reject the hypothesis of equal treatment. Nonetheless, we note here the differential rank ordering, as it highlights the importance of evaluating the tick size pilot in light of how traders respond to wider ticks by searching for price improvement in non-exchange venues. (Because relative effective spreads are based on a trade's observed price, relative effective spreads—but not relative quoted spreads—will be reduced when a trade receives price improvement, for instance when a trade executes at the midpoint of the NBBO in a dark pool.) Somewhat surprisingly, Table 3 suggests this phenomenon is most dramatic for TG3, notwithstanding the fact that the trade-at rule should result in the greatest share of trades occurring on exchanges at nickel prices. We explore this issue in more detail in Section VI(C).

[28] Without taking logs, statistical analysis of inside depth could easily be misleading. For example, in the full binding sample, the 90$^{th}$, 95$^{th}$, and 99$^{th}$ percentiles of inside depth are 6,100, 11,700, and 51,900, respectively, but the maximum (3,675,900) is over 70 times higher than the 99$^{th}$ percentile. A closer examination of the distribution reveals that the middle portion of the distribution is well-behaved in terms of transactions or shares (Appendix Figure 2A), but that above 5,000 or so, inside depth can be well approximated as a Pareto distribution with parameters $x_m = 5{,}195$ and $\alpha = 2.15$ (Appendix Figure 2B). This latter result implies that the first and second moments of inside depth likely exist, but none higher than that, potentially problematizing regression. For regression, moments above two are minimally sufficient for the application of central limit theorems and the second and first moments are minimally sufficient for the application of the weak law of large numbers. However, since the log of the Pareto is exponential, log inside depth has moments that exist to all orders. Analyzing the data in terms of quantiles (e.g., median regression) is an alternative approach, but is less practical given the scale of the data. Even analyzing the quantiles reported in this footnote required collecting a dataset of all tied and unique values of inside depth for all stocks and all days, producing a dataset of 74 million observations.

The results show that all three treatment arms experienced a significant increase in quoted inside depth. This is the expected result since under nickel quoting there are fewer available price points. Focusing on column 8, the estimated differences in logs, labeled as $\hat{\theta}_t = \overline{\ln Y_t} - \overline{\ln Y_0}$, are 1.38, 1.34, and 1.63 for TG1, TG2, and TG3, respectively. If these differences were small, they would admit an interpretation as the percentage increase over control.[29] However, since they are large, it is known that these results understate the percentage increase. Estimating the percentage increase over control requires a nonlinear transformation; in large samples, the correct transformation is $\exp(\hat{\theta}_t) - 1$, and this is how we label the percentage change estimates in the table.[30] The percentage increases in inside depth under treatment versus control are in fact 298%, 282%, and 407% for TG1, TG2, and TG3, respectively. The pronounced increase in inside depth for TG3 is consistent with the expected effects of the trade-at rule, since that rule generally prevents price matching by trading centers that are not already displaying a quotation at that price. Traders seeking to trade in a TG3 security therefore have incentives to display quotes at the NBBO, increasing quoted depth. As we discuss below, however, the increase in quoted depth can create strong incentives to utilize the midpoint trade exception to the trade-at rule, potentially explaining why relative effective spreads for TG3 are no higher than those of TG1 and TG2.

[29] That is, $\ln Y_t - \ln Y_0 = \ln Y_t / Y_0 = \ln\left(1 + (Y_t - Y_0)/Y_0\right) \approx (Y_t - Y_0)/Y_0$, where the approximation emerges from the Mercator approximation $\ln(1+x) \approx x$, valid for small $x$.

[30] Somewhat technically, we additionally employ a finite-sample correction. Let $\hat{V}_t$ be the estimated standard error for $\hat{\theta}_t$. The nonlinear transformation we use for estimating the percentage change between treatment arm *t* and control can then be written $\exp(\hat{\theta}_t)\exp(-\hat{V}_t/2) - 1$. This can be thought of as a finite-sample improvement upon the transformation $\exp(\hat{\theta}_t) - 1$, which is another way to see that small estimated treatment effects admit an interpretation as percentage change: $\exp(x) - 1 \approx x$ for small $x$. See Jan van Garderen and Shah (2002) Equation (1.4) for the formula for the bias-adjusted point estimate and their Equation (2.4) for the formula for the standard error for the same. We estimate the control group mean analogously, including the bias-adjustment term. That being said, bias-adjustment in this context is minor due to the large sample size and concomitant small standard errors.

B. Realized Spreads

While the prior section reveals that relative quoted spreads and relative effective spreads more than doubled for all three treatment groups, it would be a mistake to conclude that these are necessarily the spreads liquidity providers can expect to earn by virtue of a nickel quoting rule: a security's price is likely to move against a liquidity provider as she seeks to capture the spread. The extent of this post-trade movement in the price of a security—or what is known as a trade's price impact—reduces the portion of the spread that can be captured by a liquidity provider. In keeping with the literature, we therefore estimate profits to liquidity takers from the tick size pilot by decomposing realized effective spreads into a price impact component and a realized spread component. Formally, we define the $i$th trade's price impact at time horizon $t$ as

$$Price\ Impact_i(t) = 2D_i\ (\ln N_i(t) - \ln N_i(0))$$

where $N_i(0)$ represents the midpoint of the NBBO for the security at the precise moment of the $i$th trade, $N_i(t)$ represents the NBBO midpoint $t$ seconds later, and $D_i$ represents a trade's direction (where $D_i = 1$ corresponds to a buy and $D_i = -1$ corresponds to a sell). Conversely, we define the $i$th trade's realized spread at time horizon $t$ as

$$Realized\ Spread_i(t) = 2D_i\ (\ln P_i - \ln\ N_i(t))$$

where $P_i$ represents the $i$th trade's observed execution price. Our use of log-differences can be thought of as measuring realized spreads and price impact on price-adjusted basis.[31] As noted in Section III(B), these measures take advantage of the new microsecond timestamps in TAQ, which allow for precise temporal alignment of trade reports with the NBBO that prevailed at the

[31] Specifically, for realized spreads, the measure produces a first order approximation of the realized spread for a trade as a percentage of the observed price of the trade; for price impact, the measure produces a first order approximation of the price impact for a trade as a percentage of the quote midpoint in effect at the time of an observed trade.

time of a trade. We estimate these measures for time horizons $t$ commencing with the first microsecond ($t = 1\mu s$) following a trade and for an approximately log-equispaced grid of microseconds thereafter, covering a millisecond ($t = 1ms$), a second ($t = 1$), 5 minutes ($t = 300$), and up through 15 minutes ($t = 900$).[32]

Figures 2A and 2B present the time path of average price impact and average realized spread, respectively, for the control group and the three treatment arms. Because they decompose realized effective spread, which does not vary with time horizon, the two figures exhibit inverted patterns.[33] In the first microsecond following a trade, average price impact is near zero for control and all three treatment arms, while average realized spread is 20 basis points for control and ranges from 45 to 50 basis points depending on the treatment arm. These results reflect the fact that in the first microsecond after a typical trade, market participants are unlikely to have had sufficient time to respond to the trade to move prices, diminishing the likelihood of any price impact. Moreover, because quoted prices are unlikely to have changed, a trader who provided liquidity for the trade retains the opportunity of capturing nearly all of the effective spread by completing an off-setting passive trade at the NBBO.

At the same time, while price impact is small in magnitude in the first microsecond following a trade, it is worth emphasizing that it is nevertheless positive and that despite the small scale of price impact at such a high frequency, differences between control and treatment arms are statistically meaningful.[34] While it is possible that these results reflects the presence of informed

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[32] We use points $j = 1,2, \ldots, J$ defined by $t_j = \mu\left[(900/\mu)^{j/65}\right]$, where $\mu = 1e - 6$ is a microsecond (where, following software, units are in seconds), and where $[x]$ is notation for $x$ rounded to the nearest integer. For $J = 65$, this yields time horizons $1\mu, 2\mu, 3u, 4\mu, 5\mu, 6\mu, 7\mu, 9\mu, 13\mu, 17\mu, \ldots, 900$.

[33] Technically speaking, the patterns in Figure 2A and 2B are not exactly inverted except at the shorter time horizons. This follows because the composition of trades changes slightly as the time horizon lengthens: for trades occurring within $t$ seconds of the end of the trading day, there is no corresponding observed $N_i(t)$. In unreported results, the sum of price impact and realized spread equals a constant value until around 15 seconds, at which point differences in the sixth decimal place emerge.

[34] We return to this issue in Table 4, below, but already at the first microsecond for all three treatment arms we reject at the 95th level the null hypothesis of zero, both for treatment arms individually as well as jointly across the three treatment arms.

traders, we suspect the primary explanation stems from trade and quote reporting conventions among exchanges. In particular, when a trade fills an existing quote on an exchange, the exchange matching engine produces separate trade reports and quote updates that are each stamped to the same microsecond to reflect the trade and the change in the exchange's best bid or offer, respectively. If an exchange happens to hold the remaining depth at the NBBO at the time of a trade, the transaction will accordingly produce a "mechanical" change in the NBBO by the first microsecond of the trade.

Notably, the existence of this mechanical effect is itself informative of how the wider tick size is affecting the liquidity of pilot securities and reflects two competing effects. The first effect relates to the greater inside depth enjoyed by treated securities, which implies that for a fixed trade size, a trade in a treated security is less likely to "dislodge" the prevailing NBBO in this mechanical sense. The second effect relates to the magnitude of any change in the NBBO: under any of the three treatment arms, any dislodging of the prevailing NBBO moves the bid or ask by at least a nickel. In unreported results, we decompose price impact at the first microsecond into the product of the rate of dislodging the NBBO and the magnitude of the shift in price impact. Those results show that the former effect is quantitatively more important.[35]

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[35] That is, define price impact at the first microsecond as $\pi_i = 2D_i(\ln N_i(1\mu s) - \ln N_i(0))$ and define the value of the associated trade as $V_i$. Then since $\sum_i V_i\pi_i = \sum_i V_i\pi_i 1(\pi_i \neq 0)$, the value-weighted average of price impact can be re-written as $\sum_i V_i\pi_i / \sum_i V_i = \{\sum_i V_i 1(\pi_i \neq 0)/\sum_i V_i\}\{\sum_i V_i 1(\pi_i \neq 0)\pi_i / \sum_i V_i 1(\pi_i \neq 0)\} \equiv \{A\}\{B\}$, where $A$ is the value-weighted rate of the NBBO being dislodged (extensive margin) and $B$ is the value-weighted average price impact conditional on the NBBO being dislodged (intensive margin). To understand the influence of these two factors separately, we estimate treatment effects for $A$ and $B$ using a seemingly unrelated regressions framework controlling for the Pre-Pilot value-weighted rate of dislodge and the Pre-Pilot value-weighted average price impact conditional on dislodge. In seeking to understand the roles of $A$ and $B$ in assessing the extent to which $A_tB_t > A_0B_0$, it is most helpful to estimate treatment effects as ratios rather than differences, because the inequality is equivalent to $(A_t/A_0)(B_t/B_0) > 1$. For the full binding sample, and pooling effects across TG1, TG2, and TG3, ratio treatment effects are $0.47 \pm 0.02$ for the extensive margin, and $2.95 \pm 0.33$ for the intensive margin, explaining why treatment effects for price impact at the first microsecond are positive overall: under treatment, the NBBO changes half as often as it does under control, but when it does, it increases by nearly a factor of 3, for a net increase of $1.39 \pm 0.16$, or unambiguously in excess of 1.

Perhaps the most significant aspect of the results displayed in Figures 2A and 2B pertains to the high frequency decline of realized spreads or, equivalently, the high frequency increase in price impact. These results show that the likelihood a liquidity provider can capture the wider spreads caused by the tick size pilot dissipates rapidly within the first millisecond following a trade, suggesting the value of colocation for market makers in the modern market. Across treatment arms, the high frequency change in realized spreads and price impact is especially strong with respect to trades subject to the trade-at rule in TG3. By the first second following a trade, Figure 2B indicates that average realized spreads are just over one-half of their initial size for this group, while realized spreads for TG1 and TG2 are roughly two-thirds of their initial size. Thereafter, our estimate of average realized spreads across all three groups continues to decline, though at a more modest rate through the fifth minute following a trade.[36]

In Table 4, we further explore the decay rate of realized spreads by examining quantitative estimates of realized spreads at time horizons of the first microsecond, the first second, and the fifth minute. In columns 1 and 2, we first present the value-weighted average relative effective spread for the control group and estimated treatment effects for each treatment arm, both without and with a control variable of the pre-pilot realized effective spread.[37] Including relative effective spread in the table allows for a straightforward assessment of the magnitude of realized spread. Realized spread at time horizon $t$ is the profit available to a liquidity provider able to complete the other side of a transaction within $t$ seconds of a trade, and should be gauged against the relative effective spread, which is what a liquidity provider would be able to capture if she were able to transact instantaneously at $t=0$.

[36] For the long binding sample, the shape of these results as a function of the time horizon are highly similar to those for the full binding sample, but scaled down by about four-fifths. See Appendix Figures 3A and 3B.

[37] Recall that in Table 3 we define relative effective spread as involving a leading factor of 2, leading to comparability of magnitudes across the columns of Table 4. To further facilitate comparisons, the estimates of relative effective spread in Table 4 are value-weighted; those of relative effective spread in Table 3 were size-weighted.

[Insert Table 4]

In columns 3 and 4, we present the average price impact for an observed trade after the first microsecond for the control group as well as treatment effects for each of the three treatment arms. As was largely discernable visually in Figure 2A, the control group mean and the estimated treatment effects are all non-zero, consistent with the mechanical effect of trades dislodging the NBBO. Even if this effect is mechanical, it is worth noting that any such price impact remains a real barrier to a liquidity provider seeking to capture the wider spreads afforded by the tick size pilot. Reflecting this fact, average realized spreads in columns 5 and 6 are uniformly lower than average relative effective spreads for control and for each treatment arm.

A surprising aspect of Table 4 is that the point estimates for price impact at one microsecond are higher for TG3 than for TG1 and TG2. After all, Table 3 shows that there is substantially greater inside depth for TG3 than for either TG1 or TG2, and thus for a fixed trade size it is less likely, rather than more likely, that there would be relatively more price impact at one microsecond for trades in TG3. This pattern reflects more than just chance variation: in unreported results, we test and reject the hypothesis of equal treatment effects.[38] In further unreported results, and as discussed in note 35, we decompose price impact at the first microsecond into the rate at which the NBBO is dislodged (e.g., because a trade exhausts remaining displayed inside depth), on the one hand, and the magnitude of the shift in the NBBO, on the other. Comparing TG3 to the average of TG1 and TG2, we see for TG3 both a higher rate at which trades dislodge the prevailing NBBO, on the one hand, as well as a higher magnitude of the shift in the NBBO, on the other.[39]

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[38] For example, in column 4, the F-test for the joint restriction of equal treatment effects across the three arms is 9.96 (2 numerator dof, $p<0.01$). Results are similar for column 3.

[39] For dislodge, TG3 has a statistically higher rate than either TG1 or TG2 ($\chi^2$ equal to 14.75 and 5.12, respectively, $p<0.01$), whereas for the magnitude of the shift, TG3 is statistically higher than TG2 ($\chi^2$=4.49, $p<0.01$) but not than TG1 ($\chi^2$=0.23).

Columns 7 through 10 present average price impact and realized spreads as of the first second following an observed trade. At that time horizon, average realized spreads remain significantly higher than those for the control group across all three treatment arms, consistent with the pilot's goal of enhancing liquidity provider profits through the wide tick size. For example, treatment effects in column 10 are 170%, 150%, and 110% of the control group mean for TG1, TG2, and TG3, respectively. However, realized spreads are also much lower than they were immediately following the trade. At one microsecond, TG1, TG2, and TG3 have realized spreads of 0.51, 0.48, and 0.46, respectively, but by one second, those fall to 0.33, 0.30, and 0.25.[40] The decline is notably faster for TG3: the percentage decline in average realized spreads between one microsecond and one second are 34.6% and 36.4% for TG1 and TG2, respectively, but 44.4% for TG3.[41] As with the higher "mechanical" effect on the NBBO for TG3, the greater decay rate for TG3 may be symptomatic of the trade-at rule: to the extent the rule forces more trading to occur on lit, public venues, a greater number of trades should result in greater price impact arising from both the mechanical effect noted previously as well as more trades being observed by fast market makers looking to adjust their quotes.

Columns 11 through 14 present results for average price impact and realized spreads for the fifth minute following an observed trade. At this time horizon, price impact remains higher for treated securities, though all three treatment arms now show similar levels of price impact; all three price impact levels are just below double that of control. For realized spread, we see that despite the fact that the time since an observed trade is 300 times longer in columns 13 and 14 than in columns 9 and 10, realized spreads have decayed by about the same percentage as they

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These results are analogous to those of the seemingly unrelated regressions framework discussed in footnote 35, but allow for separate treatment effects for the three treatment arms.

[40] Levels cited are computed as the sum of the treatment effect for the given treatment arm and the control group mean.

[41] Margins of error on the percentage decline are quite narrow and rule out chance variation. The difference in the percentage decline cited between TG3 and the average of TG1 and TG2 is 8.6 percentage points, and the t-ratio of no difference is -6.

do within the very first second after a trade: at 5 minutes, realized spreads are 0.20, 0.19, and 0.15 for TG1, TG2, and TG3, respectively, which reflects a percentage decline in realized spreads from 1 second to 5 minutes of 39.6%, 39.2%, and 39.6%.[42] That the decay in realized spread from 1 second to 5 minutes is as large as that from the instant of a trade to 1 second highlights the dramatic return to speed in the modern marketplace for U.S. equities.[43]

## C. Trading Venues and Midpoint Trading

As noted in the Introduction, one of the more notable institutional features that can affect the relationship between wider ticks and liquidity provider profits is the fact that trading occurs across competing trading venues that vie for liquidity, trades, and market share. Here, we evaluate how this competition among venues can interact with the pilot study's policy goal of subsidizing liquidity takers with wider tick sizes.

### *1. Liquidity Provider Profits and Exchange Pricing*

We first evaluate the extent to which wider tick sizes shifted exchange trading from exchanges using maker/taker price schedules to those using taker/maker schedules. During our sample period, the majority of exchanges adhered to a maker/taker schedule, with a smaller group of exchanges adhering to an inverse taker/maker schedule that pays the rebate to liquidity takers, while charging a per-share fee to the liquidity provider. Two exchanges during our sample period (the Investors Exchange and the National Stock Exchange) either charged no fees at all or simply charged a small fee on all liquidity taking orders without paying a rebate.[44]

These different fee structures should be expected to interact with the tick size pilot for a number of reasons. First, because exchanges observe price-time priority, the increase in inside

[42] The data fail to reject the null hypothesis of equal percentage declines across treatment arms ($\chi^2$ of 0.04, 2 dof).
[43] In the interest of space, we do not discuss in the main text results for the long sample. We merely note that those results, presented in Appendix Table 4, are qualitatively similar to those reported in Table 4.
[44] The National Stock Exchange ceased trading on February 1, 2017 when it was acquired by the NYSE. Our trading data for this venue therefor runs from March 7, 2016 through January 31, 2017.

depth across treatment arms shown in Table 3 would have lowered the likelihood that a limit order is filled when priced at the NBBO, particularly when the spread between the NBB and NBO equals the tick size.[45] Relative to maker/taker venues, however, taker/maker venues should have fewer liquidity providers posting at the NBBO in light of the fee a liquidity provider would pay for an order filled at the NBBO. Yet, for the same reason, liquidity-taking traders should be expected to look first for liquidity at the NBBO at taker/maker venues given the rebate they receive from them. In short, the tick size pilot should induce aggressive liquidity providers to turn to inverted exchanges as a means to bypass the longer queue lines on maker/taker exchanges.

We present our results for exchange market share in Table 5. In Panel A, we first assess the extent to which the pilot study induced liquidity providers to transition away from maker/taker exchanges to taker/maker exchanges by looking at the market share of trading across venues. For purposes of this analysis, we assess market share in terms of exchange fraction of the NBO.[46] That is, we calculate the fraction of the NBO for a security that was accounted for by the Best Offer (BO) posted on a particular exchange during the sample period. In column 1, we present the control group mean across all exchanges. The rows of the table correspond to exchanges, organized in descending order by the type of exchange (maker/taker, taker/maker, and zero-rebate).[47] Columns 2 through 4 present means for each treatment group. Overall, the first four columns highlight that the effect of the pilot for maker/taker exchanges is a decrease in market

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[45] When the spread between the NBB and NBO equals the tick size, it is not possible to post a limit order that improves the NBBO because doing so would cause the NBBO to be locked or crossed in violation of Rule 610 of Regulation National Market System. Under Rule 610, all exchanges must establish and enforce rules that prohibit their members from displaying orders that lock or cross the NBBO.

[46] Results using the NBB are highly similar and omitted.

[47] As with prior analyses, the control group mean and treatment group means are obtained from the efficient semiparametric estimator using the pre-period outcome as a control. Treatment effects, discussed next, are obtained from the same model.

share, whereas the effect of the pilot for taker/maker and zero-rebate exchanges in an increase in market share.

[Insert Table 5]

We formally examine the significance of this treatment effect in columns 5 through 7. As shown in these columns, maker/taker venues showed a statistically significant drop in the fraction of the NBO for a security that was accounted for by the venue's BO across all three treatment groups. The only exception was with respect to NYSE MKT and the Chicago Stock Exchange, where their BOs made only a de minimis contribution to the NBO even in the case of control securities. Conversely, all three taker/maker venues experienced a significance increase in the fraction of the NBO that was represented by the BO on these exchanges. Indeed, across all three treatment arms, Nasdaq OMX saw its contribution to the NBO more than double from approximately 5% to 11-13%, depending on treatment arm, and BATS Y similarly rose from 5% to 13%. The two no-rebate exchanges likewise witnessed a general increase in the extent to which their BO contributed to the NBO across all three treatment arms. These findings are consistent with the tick size pilot inducing liquidity providers to compete on price by turning to taker/maker exchanges.

In Panel B, we present the same analysis but using a different measure of market share, namely the exchange's size-weighted fraction of all exchange trades. (We examine below the market share of exchange versus non-exchange venues.) Across all three treatment arms, the increase (decrease) in the fraction of the NBO that was represented by an exchange's BO during the pilot period was also reflected in a similar increase (decrease) in the fraction of exchange trades completed on that venue.[48]

[48] Results for the long binding sample, presented in Appendix Table 5, are highly similar and point to the same qualitative conclusions.

Finally, in column 8 we examine whether these effects differ within the three treatment arms. As shown in Table 3, a primary consequence of the trade-at rule was a dramatic increase in inside depth for securities assigned to TG3. The incentive to post orders on inverted exchanges should therefore be especially pronounced within this treatment arm. This is what we find: treatment effects differ between TG3 and the other two treatment arms. Focusing on the results in Panel B, where the effects of the trade-at rule on the distribution of trades are relatively more pronounced, we see that for the top five maker/taker venues, TG3 implies a decline in the size-weighted fraction of trades of between a half a percentage point to two percentage points, relative to the average of TG1 and TG2.[49] Effects are of the opposite sign for the top two taker/maker venues.[50] Effects for Panel A are somewhat smaller in magnitude but follow the same qualitative pattern.

Overall, these results are consistent with the tick size pilot increasing queue lines at the NBBO and the incentive these queue lines create for liquidity providers to turn to inverted exchanges, especially when the spread between the NBB and NBO is constrained by the tick size. They also underscore how the total profitability of a trade for a liquidity provider depends on the joint effects of the tick size and pricing schedule used among competing exchanges. For instance, ignoring other transaction fees, a trader entitled to "top tier" pricing on BATS X who bought and sold passively at the NBBO in a control security with a penny quoted spread would have earned \$0.0164 per share (\$0.01 of spread, plus rebates of \$0.0032 x 2) by posting liquidity on BATS X given its fee schedule in effect in April 2017. Conversely, the same market maker

[49]The size-weighted fraction of trades for these five exchanges ranges from 33% at the high end (NASDAQ) to 9% at the low end (BATS). Consequently, these differences between TG3, on the one hand, and TG1 and TG2, on the other, might be said to be small economically. On the other hand, they are generally persistent across exchanges of the same type and statistically significant at conventional levels. For example, t-tests for no effect for NASDAQ, ARCA, NYSE, Direct Edge X, and BATS are -4.8, -5.7, -1.5, -5.6, and -3.6, respectively.

[50]Together, these seven exchanges account for 90% of the shares traded for stocks in the full binding sample.

who made such a pair of trades at the NBBO in a Pilot Security with a nickel quoted spread would have earned $0.0476 ($0.05, less fees of $0.0012 x 2) by posting liquidity on BATS Y given its fee schedule.[51] It is unclear whether, in a broader utilization of nickel pricing, exchanges would find it in their interest to leave fee schedules unchanged or whether there would be an endogenous response and a new equilibrium. However, this simple example establishes that at least in partial equilibrium, the interaction of the tick size pilot with exchanges' pricing schedules can reduce the effect of the subsidy on liquidity providers.

*2. Liquidity Provider Profits and Non-Displayed Order Types*

In Tables 6 and 7, we turn to an examination of the effect of the pilot on the use of non-displayed liquidity—both with respect to non-displayed orders on exchanges and to non-exchange venues. As noted in the Introduction, prior research (Bartlett & McCrary, 2012; Kwan et al., 2014) suggests that the greater depth associated with the tick size pilot should also induce liquidity providers to "queue jump" exchanges by posting non-displayed orders in non-exchange venues, particularly by means of using orders that are "pegged" to the midpoint of the NBBO. Similar to the rebate offered to liquidity providers by inverted exchanges, the possibility of receiving price improvement by hitting an undisplayed midpoint order should induce liquidity takers to look to these venues prior to routing orders to orders displayed at the NBBO. Moreover, many exchanges also permit traders to submit non-displayed orders that are pegged to the NBBO midpoint which, because exchanges observe price-time priority, will be filled on the exchange before orders that are posted at the exchange's BBO.[52] As a result, pegged midpoint

[51] Fee and rebate data are from Wah et al. (2017). Exchanges often provide favorable pricing based on a market participant's volume of trading on a given venue; the figures provided by Wah et al. (2017) reflect the most favorable, top-tier fees and rebates available to a market participant at BATS X and BATS Y based on fee schedules accessed on April 3, 2017.
[52] See, e.g., Nasdaq Global Trading and Market Services, *Midpoint Liquidity*, available at https://www.nasdaqtrader.com/content/productsservices/trading/midpointpeg.pdf.

orders can permit liquidity providers to queue-jump the long queue lines induced by the tick size pilot whether submitted to an exchange or non-exchange venue.

In short, the greater depth associated with the pilot study should encourage liquidity providers to queue-jump by means of trading in non-exchange venues, as well as by using non-displayed midpoint orders on both exchange and non-exchange venues.  Moreover, while the trade-at rule will limit trading in non-exchange venues, recall that the rule exempts trades completed at the midpoint of the NBBO, making this form of queue-jumping viable even within TG3. At the same time, a liquidity provider who trades at the midpoint will by definition receive less than the quoted spread, underscoring how the exception for midpoint trades within the pilot study can work to reduce the subsidy to liquidity providers of nickel tick sizes.

In Table 6, we first evaluate how the tick size pilot affected the competition for order flow between exchange and non-exchange venues.  In keeping with the format of Table 5, the first four columns present for the control group and each treatment group the mean size-weighted fraction of trades that occur on all exchanges and on all non-exchange venues.  As shown in column 1, approximately 42% of all trades within the control group were made in non-exchange venues.  This figure increases to approximately 46% and 45% respectively, for TG1 and TG2, respectively.  In contrast, the figure declines to 32% for TG3, consistent with the trade-at rule reducing the number of trades that occur in non-exchange venues. We formally examine the significance of the treatment effect in columns 5 through 7.  As shown in these columns, treatment effects on the fraction of trades occurring in non-exchange venues are statistically significant for all three treatment arms.  In column 8, we additionally test whether the treatment effect for TG3 was significantly different from the average of the treatment effects for TG1 and TG2—that is, the effect of the trade-at rule itself, holding fixed nickel pricing.  As shown in the table, the trade-at rule associated with TG3 reduced the incidence of off-exchange trading by some 13.5 percentage points.[53]

[Insert Table 6]

Finally, Table 7 examines the incidence of midpoint trades. Columns 1 and 2 pertain to estimated treatment effects where the dependent variable is the rate of midpoint trades regardless of venue. Under control, 12% of trades are midpoint trades. Focusing on the more precise results in column 2, under TG1 and TG2 the fraction of trades that are midpoints rises by approximately 5.5% and 6.0%, respectively. However, as predicted, the average treatment effect is higher for TG3. The null hypothesis that the treatment effect is the same across treatment arms is easily rejected (F-stat=662, 2 numerator dof, $p<0.01$), and the rejection is due overwhelmingly to the different results for TG3. From the rows corresponding to percentage changes, we see that TG3 nearly doubles the incidence of midpoint trades relative to the control group.

[Insert Table 7]

In columns 3 and 4 we conduct the same analysis for all exchange trades, where midpoints may reflect non-displayed midpoint orders, and in columns 5 and 6 we do so for all non-exchange trades, where midpoints are a common form of price improvement, as noted. As shown in columns 3 and 4, the incidence of midpoint trades on exchanges increased across all three treatment arms; however, the magnitude of the effect was smallest for TG3. A likely explanation is that liquidity taking traders in TG3 securities were looking first to non-exchange venues for price improvement over the NBBO, reducing the probability that an in-bound marketable order would be placed on an exchange with a resting non-displayed midpoint order.

[53] Results for the long binding sample, presented in Appendix Table 6, are qualitatively similar to those for the full binding sample, differing typically by less than one percentage point.

Consistent with this interpretation, columns 5 and 6 highlight that on non-exchange venues, TG3 causes a massive increase relative to control in the frequency of midpoint trades.

More generally, for each treatment arm, the sizes of the estimated treatment effects are consistent with each treatment group providing an incrementally greater incentive to post a pegged midpoint order in a non-exchange venue. The effect is especially notable for TG3 where the frequency of midpoint trades increases by nearly 192%, with an estimated two-thirds of all non-exchange trades under TG3 taking the form of a midpoint execution. This finding helps explain why nearly one-third of trades in TG3 securities were filled in non-exchange venues despite the trade-at rule. The sizeable fraction of midpoint trades within TG3 also helps explain why, despite the trade-at rule, relative effective spreads were statistically indistinguishable across the three treatment groups.

## VII. Conclusion

The tick size pilot represents a unique opportunity to study the efficiency with which a wider tick size creates a subsidy for liquidity provision in contemporary equity markets. While policy discussions over the past decade have commonly asserted that decimalization of stock prices in 2001 has harmed the incentive to provide liquidity in small capitalization equities, regulatory and technological changes since that time make it an open question whether reversing decimalization can produce liquidity benefits for smaller companies that outweigh the immediate increase in trading costs associated with wider ticks. As we show, the emergence of HFT market-making and the growing role of non-exchange trading can both interfere with the extent to which a wider tick size provides a subsidy for liquidity providers.

As noted in the Introduction, regulatory filings made by brokers in connection with the pilot study indicate that market maker profits increased by approximately 40% in transactions involving securities that have been treated with nickel tick sizes. While this represents a notable increase in market maker profitability, our results underscore important institutional challenges facing the central policy goal of using wider tick sizes to subsidize liquidity providers. First, the exception for midpoint trades substantially reduces the aggregate subsidy available from wider tick sizes. Equally important, while liquidity taking trades in our sample nevertheless pay effective spreads that are on average more than twice what they were with a penny MPV, the sub-second decay rate of realized spreads makes the ability of liquidity providers to capture these spreads dependent on their capacity for trading at sub-second frequencies. Moreover, this challenge is made all the more acute to the extent trades are encouraged to occur on exchanges, making the relationship between the trade-at rule and the profitability of exchange market makers considerably more complicated than proponents of the trade-at rule have acknowledged. For instance, based on the estimates from Table 4, a liquidity provider who takes just one second to off-set a transaction will capture two-thirds of the wider spreads for securities in TG1 or TG2. For trades in TG3, such a liquidity provider will capture just over one-half of the wider trading costs.

Overall, these findings help explain why the profitability of making a market in treated securities is not higher than it is despite a quintupling of the tick size. For the same reason, they also point to the tick size as being a highly inefficient means to subsidize liquidity providers in contemporary equity markets.

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## Data Appendix

*A. Strata/TAQ Crosswalk*

To construct our analysis sample, we began with two lists: (1) a list of 2,399 ticker symbols with information on the strata used in the conditional randomization and (2) an exhaustive list of all (sym_root,sym_suffix) pairs that ever appeared in any of the trade, quote, or master file of the microsecond TAQ data at any time during March 7, 2016 to September 28, 2018, inclusive.

The list of 2,399 ticker symbols came from a spreadsheet provided to us by FINRA ("TickSizePilotSelectionProcess.xlsx"). That spreadsheet details which stocks were assigned to which of the 27 initial strata and the 21 revised strata.[54] The list of TAQ (sym_root,sym_suffix) pairs was obtained using a day-by-day DOW-loop over the trade and quote files, respectively, followed by merging the resulting pair with the master file for that day.[55] Those were then stacked up over days to obtain a list of all possible (sym_root,sym_suffix) pairs and the dates over which they were quoted, traded, or listed in the master file.

Because the list of 2,399 tickers were reported as a combined field, we next needed to develop a rule for mapping those tickers to the TAQ (sym_root,sym_suffix) pairs. Inspection of the list of 2,399 tickers suggested strongly that a different convention was applied for NASDAQ vs NYSE/Arca listed stocks. For NASDAQ stocks, the ticker represents the concatenation of (sym_root,sym_suffix), but for NYSE/Arca listed stocks, the ticker represents the concatenation of (sym_root,sym_suffix) with a space inserted. We thought of this particular mapping from ticker to (sym_root,sym_suffix) pair as a tentative rule and then sought to falsify it.

To do so, we first took the listing exchange specific rule described above and applied it to October 24, 2016, the first date as of which information on treatment assignment status is available in the TAQ master file. This led to an exact match on listing exchange and (sym_root,sym_suffix) for 2,371 out of 2,399 of the records in the FINRA list. For the remaining 28 records, we then engaged in a manual search for those records in the exhaustive TAQ list and a review of public domain documents, such as Edgar filings. The results of this inquiry are presented in Appendix Table 1.[56] That inquiry provides no evidence against our rule: in each of the 28 instances, non-matches are due to fundamental changes to the underlying economic entity prior to October 26, 2016, rather than to a bad match between ticker and (sym_root,sym_suffix) pair.

The rule outlined above allows for matching 2,371 (98.8%) of the 2,399 stocks identified by FINRA as pilot eligible. Conceptually, only those 2,371 stocks belong in study population.

*B. Selection of Stocks for Study*

While 2,371 stocks were matchable to TAQ, not all of those stocks were used for this study. As discussed in the draft, we study both a full sample (that traded with the same CUSIP over the period November 7, 2016 to June 30, 2017) and a long sample (that traded with the

[54] We stripped hidden carriage returns from the FINRA list using the Excel formula =SUBSTITUTE(B2,CHAR(13),CHAR(32)).

[55] Strictly speaking, the TAQ master file, unlike the trade and quote files, does not contain (sym_root,sym_suffix) pairs, but rather a single field symbol_15 which is space delimited. We define the first and second words from symbol_15 as sym_root and sym_suffix, respectively.

[56] Summarizing the results of that table, prior to October 26, 2016, the 28 unmatched tickers experienced an array of fundamental changes to the underlying economic entity, such as a corporate combination. For example, the first unmatched ticker, ASEI, corresponding to American Science and Engineering, Inc., was acquired by OSI Systems in mid-2016 and ceased trading on September 9, 2016. In such a case, the appropriate course of action is to drop the ticker from the analysis. In only one case, FXCM, could there be a case for retaining the entity.

same CUSIP over the period November 7, 2016 to September 28, 2018).[57]

Of the 2,371 matchable stocks, 2,026 (85.4%) traded and had the same CUSIP over the entire period November 7, 2016 to June 30, 2017. We refer to these as the "full sample stocks." Of the 2,026 that traded over that time period, 1,735 (85.6%) traded and had the same CUSIP over the entire period November 7, 2016 to September 28, 2018. We refer to these as the "long sample stocks."

Starting with the full sample stocks, we used the Pre-Pilot observations to classify stocks into those for which the pilot would likely be binding and those for which it would likely not be binding, as discussed in the draft. Our proxy for whether the pilot would be binding is whether the stock had a Pre-Pilot size-weighted average spread below a nickel: stocks with spreads above a nickel are those for which trading or quoting in nickels is much less likely to have an effect. Conversely, those with spreads below a nickel are likely to have the quoting and trading environment affected meaningfully. Of the 2,026 full sample stocks, just over half (1,087) were binding in the sense of having a Pre-Pilot spread of below $0.05. These 1,087 stocks are our primary analysis sample. Of the 1,735 long sample stocks, again just over half (930) were binding.

*C. Computation*

While our models are in each instance simple linear models, there are nonetheless challenges associated with computation due to the scale of the data. From March 7, 2016 to September 28, 2018, there are roughly 1.76 billion (1,758,702,992) trades corresponding to the 2,026 stocks in the full sample, or about 8.3% of all trade reports.[58] Over the same time period, there were 30.6 billion (30,596,863,843) quote updates corresponding to the 2,026 stocks in the full sample, or about 7% of all quote updates.[59]

We performed all of our computations in SAS on the platform run by Wharton Research Data Services, where the data are stored on a day-by-day basis, with a fast index corresponding to (sym_root,sym_suffix). The index facilitates the creation of study extracts, and we created daily SAS binary extracts corresponding to the 2,026 stocks in the full sample for trade reports and quote updates. We then processed quote updates to obtain NBBO updates and finally interleaved NBBO updates with trade reports. Thus, for each day we obtain four SAS binaries on the 2,026 stocks. Each extract was compressed using pixz, a parallel compression program. Compressed, the total scale of our extracts is just over 1 Terabyte (1,016 Gigabytes).[60]

All of our analyses leverage the fact that, for linear models, it is straightforward to calculate daily sufficient statistics for an overall analysis and aggregate (e.g., the weighted average of daily averages replicates the ensemble average). In addition, we make extensive use of named pipes, as this allows pixz, which is very fast, to funnel decompressed data on the fly to SAS. This has the obvious advantage of avoiding decompressing the underlying extracts. A less obvious advantage is that, relative to the use of decompressed data, using named pipes and parallel compression *speeds* computation. This occurs because modern processors are very fast relative to disk, and reading from compressed data means reading *less* data.

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[57] While it is also true that we require information prior to the pilot, this constraint is not binding. For all 2,371 stocks, information is available from the pre-period for 59 or more trading days, with fully 2,157 stocks having 161 trading days (the most possible) available from the pre-period.

[58] Over the same time period, there were just over 21 billion (21,094,405,389) trade reports for the overall market.

[59] Over the same time period, there were just over 436 billion (436,141,064,223) quote updates for the overall market.

[60] Compression ratios range from approximately 6 to 35, with the general tendency being that trade, quote, and interleaved extracts have compression ratios closer to 6 and NBBO extracts have compression ratios of 20 or more.

## Table 1. Treatment Effects on Spreads: Choosing the Binding and Excluded Samples

Table examines the effect of the pilot on percent quoted spreads, measured at the time of an observed trade, for Pilot Securities with pre-pilot size-weighted quoted spreads of less than $0.05 (Panel A) compared to those with spreads of $0.05 or higher (Panel B). Each row represents a cross-sectional regression where the outcome is 100 times a stock-specific size-weighted percent quoted spread. Analysis is presented separately for five different time periods for a given stock. Heteroskedasticity-consistent standard errors presented in parentheses below point estimates. See text for details.

| | Security | Number of | Control | Treatment Effect for Group: | | |
|---|---|---|---|---|---|---|
| | Group | Securities | Group Mean | TG1 | TG2 | TG3 |
| *A. BINDING SAMPLE: Stocks with Pre-Pilot Size-Weighted Quoted Spreads BELOW $0.05* | | | | | | |
| Pre-Pilot Period | Full Binding Sample | 1087 | 0.3013 | 0.0059 | 0.0144 | 0.0200 |
| 3/7/2016-9/2/2016 | | | (0.0069) | (0.0119) | (0.0140) | (0.0144) |
| Announcement Period | Full Binding Sample | 1087 | 0.2867 | 0.0014 | 0.0108 | 0.0043 |
| 9/6/2016-9/30/2016 | | | (0.0079) | (0.0126) | (0.0148) | (0.0147) |
| Phase-In Period | Full Binding Sample | 1087 | 0.3214 | 0.2362 | 0.2582 | 0.1187 |
| 10/3/2016-11/4/2016 | | | (0.0102) | (0.0205) | (0.0251) | (0.0243) |
| Full Implementation Period | Full Binding Sample | 1087 | 0.3000 | 0.4001 | 0.3840 | 0.4285 |
| 11/7/2016-6/30/2017 | | | (0.0082) | (0.0298) | (0.0347) | (0.0341) |
| Full Implementation Period | Long Binding Sample | 930 | 0.2812 | 0.3606 | 0.3360 | 0.4078 |
| 11/7/2016-6/30/2017 | | | (0.0080) | (0.0313) | (0.0336) | (0.0343) |
| Extended Evaluation Period | Long Binding Sample | 930 | 0.2843 | 0.3324 | 0.3133 | 0.3459 |
| 11/7/2016-9/28/2018 | | | (0.0093) | (0.0283) | (0.0365) | (0.0347) |
| *B. EXCLUDED SAMPLE: Stocks with Pre-Pilot Size-Weighted Quoted Spreads ABOVE $0.05* | | | | | | |
| Pre-Pilot Period | Full Excluded Sample | 939 | 0.8779 | -0.0183 | 0.0040 | 0.0206 |
| 3/7/2016-9/2/2016 | | | (0.0307) | (0.0640) | (0.0503) | (0.0620) |
| Announcement Period | Full Excluded Sample | 939 | 0.7704 | -0.0124 | 0.0744 | -0.0530 |
| 9/6/2016-9/30/2016 | | | (0.0296) | (0.0529) | (0.0549) | (0.0494) |
| Phase-In Period | Full Excluded Sample | 939 | 0.8558 | -0.0132 | 0.0460 | -0.0692 |
| 10/3/2016-11/4/2016 | | | (0.0354) | (0.0583) | (0.0565) | (0.0496) |
| Full Implementation Period | Full Excluded Sample | 939 | 0.7310 | 0.0573 | 0.0653 | 0.0726 |
| 11/7/2016-6/30/2017 | | | (0.0249) | (0.0514) | (0.0474) | (0.0489) |
| Full Implementation Period | Long Excluded Sample | 805 | 0.7100 | 0.0585 | 0.1001 | 0.0276 |
| 11/7/2016-6/30/2017 | | | (0.0256) | (0.0531) | (0.0522) | (0.0462) |
| Extended Evaluation Period | Long Excluded Sample | 805 | 0.6495 | 0.0590 | 0.0891 | 0.0535 |
| 11/7/2016-9/28/2018 | | | (0.0227) | (0.0488) | (0.0445) | (0.0452) |

## Table 2. Balancing Tests

This table reports balance tests for control and treatment securities. Estimates of the control group mean reflect regression-adjusted means after controlling for strata. Treatment effects estimates are relative to control and condition on strata. Heteroskedasticity-robust standard errors in parentheses. Final column presents the F-statistic for the null hypothesis of zero treatment effects in the pre-treatment period and the associated p-value. For all rows, n=1,087.

| Outcome | Control Group Mean | Treatment Effect for Group: TG1 | TG2 | TG3 | Joint Test of Randomization |
|---|---|---|---|---|---|
| Log Trades | 6.8862 | -0.0150 | 0.0062 | -0.0453 | 0.4246 |
| | (0.0213) | (0.0469) | (0.0440) | (0.0434) | p=0.7354 |
| Log On-Exchange Trades | 6.5857 | -0.0213 | 0.0036 | -0.0584 | 0.5806 |
| | (0.0233) | (0.0500) | (0.0467) | (0.0471) | p=0.6278 |
| Log Off-Exchange Trades | 5.4372 | -0.0048 | 0.0106 | -0.0111 | 0.0682 |
| | (0.0186) | (0.0429) | (0.0407) | (0.0385) | p=0.9768 |
| Log Volume | 11.7661 | -0.0183 | 0.0176 | 0.0012 | 0.1322 |
| | (0.0199) | (0.0474) | (0.0428) | (0.0409) | p=0.9409 |
| Log On-Exchange Volume | 11.2315 | -0.0235 | 0.0063 | -0.0247 | 0.1886 |
| | (0.0216) | (0.0499) | (0.0457) | (0.0435) | p=0.9042 |
| Log Off-Exchange Volume | 10.7531 | -0.0199 | 0.0250 | 0.0405 | 0.4911 |
| | (0.0209) | (0.0495) | (0.0443) | (0.0431) | p=0.6885 |
| Size-weighted ISO fraction | 0.3196 | -0.0045 | -0.0068 | -0.0081 | 2.2351 |
| | (0.0020) | (0.0039) | (0.0036) | (0.0037) | p=0.0826 |
| Quoted Spread | 0.0275 | 0.0004 | -0.0002 | -0.0010 | 0.6608 |
| | (0.0004) | (0.0009) | (0.0009) | (0.0008) | p=0.5762 |
| Percent Quoted Spread | 0.3122 | 0.0134 | 0.0081 | 0.0085 | 0.1849 |
| x100 | (0.0097) | (0.0220) | (0.0190) | (0.0162) | p=0.9067 |
| Locked or Crossed | 0.0129 | 0.0002 | 0.0012 | 0.0003 | 0.7631 |
| | (0.0003) | (0.0008) | (0.0008) | (0.0007) | p=0.5149 |
| Effective Spread | 0.0217 | 0.0002 | -0.0002 | -0.0007 | 0.5842 |
| | (0.0003) | (0.0007) | (0.0007) | (0.0006) | p=0.6254 |
| Relative Effective Spread | 0.0025 | 0.0001 | 0.0001 | 0.0001 | 0.1906 |
| | (0.0001) | (0.0002) | (0.0002) | (0.0001) | p=0.9028 |
| EQ Ratio | 0.8440 | -0.0040 | 0.0011 | 0.0034 | 1.6199 |
| | (0.0014) | (0.0028) | (0.0027) | (0.0027) | p=0.1831 |
| Price Improvement | 0.0032 | 0.0001 | 0.0000 | -0.0001 | 0.7803 |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) | p=0.5050 |
| Log Inside Depth | 1.5427 | 0.0323 | 0.0523 | 0.0603 | 1.1880 |
| | (0.0187) | (0.0395) | (0.0389) | (0.0384) | p=0.3131 |

## Table 3. Spreads and Depths

Table presents estimated treatment effects for quoted spreads, relative quoted spreads, relative effective spreads, and log inside depth, both in terms of differences between control and treatment groups as well as percent changes. Standard errors in parentheses. See text for details.

| Estimate | Quoted Spread | | Relative Quoted Spread | | Relative Effective Spread | | Estimate | Log Inside Depth | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | | (7) | (8) |
| TG1 | | | | | | | | | |
| $\overline{Y_1} - \overline{Y_0}$ | 0.029 | 0.029 | 0.40 | 0.39 | 0.32 | 0.32 | $\hat{\theta}_1 = \overline{\ln Y_1} - \overline{\ln Y_0}$ | 1.40 | 1.38 |
| | (0.001) | (0.001) | (0.03) | (0.03) | (0.03) | (0.03) | | (0.06) | (0.05) |
| $(\overline{Y_1} - \overline{Y_0})/\overline{Y_0}$ | 0.958 | 0.951 | 1.33 | 1.29 | 1.39 | 1.39 | $\exp(\hat{\theta}_1)$-1 | 3.05 | 2.98 |
| | (0.047) | (0.040) | (0.11) | (0.10) | (0.13) | (0.13) | | (0.26) | (0.22) |
| TG2 | | | | | | | | | |
| $\overline{Y_2} - \overline{Y_0}$ | 0.029 | 0.029 | 0.38 | 0.37 | 0.29 | 0.29 | $\hat{\theta}_2 = \overline{\ln Y_2} - \overline{\ln Y_0}$ | 1.38 | 1.34 |
| | (0.001) | (0.001) | (0.03) | (0.03) | (0.03) | (0.03) | | (0.06) | (0.06) |
| $(\overline{Y_2} - \overline{Y_0})/\overline{Y_0}$ | 0.960 | 0.956 | 1.28 | 1.21 | 1.27 | 1.28 | $\exp(\hat{\theta}_2)$-1 | 2.95 | 2.82 |
| | (0.050) | (0.043) | (0.13) | (0.12) | (0.14) | (0.13) | | (0.25) | (0.21) |
| TG3 | | | | | | | | | |
| $\overline{Y_3} - \overline{Y_0}$ | 0.027 | 0.027 | 0.43 | 0.41 | 0.28 | 0.28 | $\hat{\theta}_3 = \overline{\ln Y_3} - \overline{\ln Y_0}$ | 1.69 | 1.63 |
| | (0.001) | (0.001) | (0.03) | (0.03) | (0.03) | (0.02) | | (0.06) | (0.05) |
| $(\overline{Y_3} - \overline{Y_0})/\overline{Y_0}$ | 0.891 | 0.903 | 1.43 | 1.34 | 1.23 | 1.21 | $\exp(\hat{\theta}_3)$-1 | 4.39 | 4.07 |
| | (0.045) | (0.040) | (0.13) | (0.11) | (0.13) | (0.12) | | (0.35) | (0.27) |
| Pre-Period Outcome | | 0.685 | | 0.90 | | 0.43 | | | 0.97 |
| | | (0.041) | | (0.10) | | (0.14) | | | (0.05) |
| Control Group Mean | | | | | | | | | |
| $\overline{Y_0}$ | 0.030 | 0.030 | 0.30 | 0.31 | 0.23 | 0.23 | $\exp(\overline{Y_0})$ | 602.0 | 613.8 |
| | (0.001) | (0.001) | (0.01) | (0.01) | (0.01) | (0.01) | | (16.3) | (12.3) |
| Observations | 1087 | 1087 | 1087 | 1087 | 1087 | 1087 | | 1087 | 1087 |
| R-square | 0.67 | 0.75 | 0.69 | 0.75 | 0.66 | 0.69 | | 0.67 | 0.79 |

## Table 4. Price Impact and Realized Spreads

Table presents estimated treatment effects (differences and percentage changes) on price impact and realized spreads at 1 microsecond, 1 second, and 5 minutes after a trade. Relative effective spread is value weighted. Standard errors in parentheses. See text for details.

| Estimate | Relative Effective Spread | | $1\mu s$ | | | | 1 second | | | | 5 minute | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Price Impact | | Realized Spread | | Price Impact | | Realized Spread | | Price Impact | | Realized Spread | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
| TG1 | | | | | | | | | | | | | | |
| $\bar{Y}_1 - \bar{Y}_0$ | 0.308 | 0.307 | 0.0039 | 0.0047 | 0.304 | 0.303 | 0.0991 | 0.1019 | 0.209 | 0.208 | 0.1588 | 0.1620 | 0.149 | 0.149 |
| | (0.026) | (0.025) | (0.0010) | (0.0009) | (0.025) | (0.025) | (0.0055) | (0.0049) | (0.023) | (0.022) | (0.0135) | (0.0122) | (0.016) | (0.016) |
| $(\bar{Y}_1 - \bar{Y}_0)/\bar{Y}_0$ | 1.381 | 1.373 | 0.1790 | 0.2193 | 1.509 | 1.497 | 0.9755 | 1.0009 | 1.720 | 1.703 | 0.9249 | 0.9336 | 2.915 | 2.907 |
| | (0.133) | (0.128) | (0.0510) | (0.0432) | (0.146) | (0.141) | (0.0639) | (0.0560) | (0.217) | (0.213) | (0.0912) | (0.0806) | (0.431) | (0.430) |
| TG2 | | | | | | | | | | | | | | |
| $\bar{Y}_2 - \bar{Y}_0$ | 0.278 | 0.280 | 0.0019 | 0.0027 | 0.276 | 0.277 | 0.0963 | 0.0969 | 0.182 | 0.183 | 0.1471 | 0.1397 | 0.131 | 0.133 |
| | (0.027) | (0.025) | (0.0009) | (0.0009) | (0.026) | (0.025) | (0.0060) | (0.0051) | (0.023) | (0.022) | (0.0148) | (0.0125) | (0.017) | (0.017) |
| $(\bar{Y}_2 - \bar{Y}_0)/\bar{Y}_0$ | 1.245 | 1.250 | 0.0865 | 0.1266 | 1.369 | 1.371 | 0.9483 | 0.9520 | 1.494 | 1.498 | 0.8570 | 0.8050 | 2.553 | 2.590 |
| | (0.135) | (0.128) | (0.0431) | (0.0427) | (0.148) | (0.142) | (0.0683) | (0.0571) | (0.211) | (0.207) | (0.0967) | (0.0804) | (0.432) | (0.434) |
| TG3 | | | | | | | | | | | | | | |
| $\bar{Y}_3 - \bar{Y}_0$ | 0.268 | 0.264 | 0.0106 | 0.0099 | 0.257 | 0.254 | 0.1339 | 0.1299 | 0.134 | 0.133 | 0.1655 | 0.1591 | 0.103 | 0.103 |
| | (0.024) | (0.023) | (0.0018) | (0.0015) | (0.023) | (0.022) | (0.0087) | (0.0072) | (0.018) | (0.017) | (0.0149) | (0.0131) | (0.013) | (0.013) |
| $(\bar{Y}_3 - \bar{Y}_0)/\bar{Y}_0$ | 1.201 | 1.177 | 0.4906 | 0.4620 | 1.277 | 1.254 | 1.3183 | 1.2761 | 1.103 | 1.088 | 0.9640 | 0.9172 | 2.002 | 2.005 |
| | (0.124) | (0.117) | (0.0905) | (0.0763) | (0.131) | (0.125) | (0.0954) | (0.0785) | (0.166) | (0.162) | (0.0991) | (0.0848) | (0.343) | (0.341) |
| Pre-Period Outcome | | 0.451 | | 0.6787 | | 0.421 | | 0.8865 | | 0.274 | | 0.6933 | | 0.108 |
| | | (0.141) | | (0.1019) | | (0.138) | | (0.1038) | | (0.108) | | (0.1026) | | (0.063) |
| Control Group Mean | | | | | | | | | | | | | | |
| $\bar{Y}_0$ | 0.223 | 0.224 | 0.0216 | 0.0215 | 0.202 | 0.202 | 0.1015 | 0.1018 | 0.122 | 0.122 | 0.1717 | 0.1735 | 0.051 | 0.051 |
| | (0.007) | (0.007) | (0.0006) | (0.0005) | (0.006) | (0.006) | (0.0020) | (0.0017) | (0.005) | (0.005) | (0.0049) | (0.0041) | (0.004) | (0.004) |
| Observations | 1087 | 1087 | 1087 | 1087 | 1087 | 1087 | 1087 | 1087 | 1087 | 1087 | 1087 | 1087 | 1087 | 1087 |
| R-square | 0.66 | 0.70 | 0.68 | 0.78 | 0.65 | 0.69 | 0.69 | 0.78 | 0.61 | 0.63 | 0.65 | 0.74 | 0.53 | 0.54 |

## Table 5. Exchange Trading

Panel A shows the fraction of the National Best Offer accounted for by the Best Offer posted on each exchange, as well as estimated treatment effects. Panel B is analogous but shows the size-weighted fraction of exchange trades for each exchange. Treatment effects estimates are the efficient semiparametric estimator and control for the pre-period outcome (except for the Investors Exchange, where no pre-period exists). Standard errors in parentheses. Some columns may not add to one due to rounding. Analysis based on long binding sample. See text for details.

| | Fraction of the NBO on Given Exchange | | | | Estimated Treatment Effects | | | |
|---|---|---|---|---|---|---|---|---|
| | Control | Treatment Mean | | | TG1 vs. | TG2 vs. | TG3 vs. | TG3 vs. |
| Venue | Mean | TG1 | TG2 | TG3 | Control | Control | Control | TG1 & TG2 |
| *A. Distribution of NBO Among Exchanges* | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *- Maker-Taker Venues:* | | | | | | | | |
| NASDAQ Stock Exchange | 0.3602 | 0.2706 | 0.2644 | 0.2588 | -0.0896 | -0.0958 | -0.1014 | -0.0087 |
| | (0.0022) | (0.0032) | (0.0044) | (0.0033) | (0.0039) | (0.0049) | (0.0040) | (0.0043) |
| NYSE Arca | 0.1350 | 0.1243 | 0.1262 | 0.1203 | -0.0106 | -0.0087 | -0.0146 | -0.0050 |
| | (0.0011) | (0.0015) | (0.0018) | (0.0018) | (0.0019) | (0.0021) | (0.0021) | (0.0021) |
| New York Stock Exchange | 0.1288 | 0.0942 | 0.0981 | 0.0902 | -0.0346 | -0.0306 | -0.0386 | -0.0060 |
| | (0.0017) | (0.0027) | (0.0031) | (0.0030) | (0.0032) | (0.0036) | (0.0035) | (0.0036) |
| Direct Edge X Stock Exchange | 0.1153 | 0.1024 | 0.0994 | 0.0992 | -0.0129 | -0.0159 | -0.0162 | -0.0018 |
| | (0.0011) | (0.0016) | (0.0021) | (0.0017) | (0.0020) | (0.0024) | (0.0020) | (0.0022) |
| BATS Exchange | 0.1029 | 0.0897 | 0.0880 | 0.0851 | -0.0132 | -0.0149 | -0.0178 | -0.0037 |
| | (0.0010) | (0.0011) | (0.0015) | (0.0015) | (0.0015) | (0.0018) | (0.0018) | (0.0017) |
| NASDAQ OMX PSX Stock Exchange | 0.0199 | 0.0150 | 0.0145 | 0.0172 | -0.0049 | -0.0054 | -0.0028 | 0.0024 |
| | (0.0003) | (0.0004) | (0.0003) | (0.0005) | (0.0005) | (0.0004) | (0.0005) | (0.0005) |
| NYSE MKT (American) Stock Exchange | 0.0082 | 0.0081 | 0.0078 | 0.0058 | -0.0001 | -0.0004 | -0.0025 | -0.0022 |
| | (0.0005) | (0.0004) | (0.0006) | (0.0011) | (0.0006) | (0.0007) | (0.0012) | (0.0011) |
| Chicago Stock Exchange | 0.0008 | 0.0007 | 0.0008 | 0.0008 | 0.0000 | 0.0000 | 0.0000 | 0.0001 |
| | (0.0000) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) |
| *- Taker-Maker Venues:* | | | | | | | | |
| NASDAQ OMX BX Stock Exchange | 0.0505 | 0.1091 | 0.1098 | 0.1289 | 0.0586 | 0.0592 | 0.0784 | 0.0195 |
| | (0.0006) | (0.0014) | (0.0016) | (0.0020) | (0.0015) | (0.0017) | (0.0021) | (0.0023) |
| BATS Y-Exchange | 0.0493 | 0.1285 | 0.1290 | 0.1308 | 0.0792 | 0.0797 | 0.0816 | 0.0021 |
| | (0.0007) | (0.0014) | (0.0016) | (0.0014) | (0.0015) | (0.0017) | (0.0016) | (0.0018) |
| Direct Edge A Stock Exchange | 0.0199 | 0.0350 | 0.0351 | 0.0352 | 0.0151 | 0.0153 | 0.0153 | 0.0001 |
| | (0.0003) | (0.0005) | (0.0005) | (0.0005) | (0.0005) | (0.0005) | (0.0005) | (0.0006) |
| *- Zero Rebate Venues:* | | | | | | | | |
| Investors Exchange | 0.0085 | 0.0271 | 0.0266 | 0.0256 | 0.0186 | 0.0181 | 0.0171 | -0.0013 |
| | (0.0002) | (0.0004) | (0.0004) | (0.0004) | (0.0005) | (0.0004) | (0.0005) | (0.0005) |
| National Stock Exchange | 0.0001 | 0.0002 | 0.0002 | 0.0002 | 0.0001 | 0.0001 | 0.0001 | 0.0000 |
| | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) |

| Venue | Size-weighted Fraction of Trades | | | | Estimated Treatment Effects | | | |
|---|---|---|---|---|---|---|---|---|
| | Control Mean | Treatment Mean | | | TG1 vs. Control | TG2 vs. Control | TG3 vs. Control | TG3 vs. TG1 & TG2 |
| | | TG1 | TG2 | TG3 | | | | |
| *B. Distribution of Trading Among Exchanges* | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *- Maker-Taker Venues:* | | | | | | | | |
| NASDAQ Stock Exchange | 0.3302 | 0.2564 | 0.2596 | 0.2386 | -0.0738 | -0.0706 | -0.0916 | -0.0194 |
| | (0.0021) | (0.0032) | (0.0040) | (0.0032) | (0.0038) | (0.0045) | (0.0038) | (0.0041) |
| NYSE Arca SM | 0.1193 | 0.0992 | 0.1003 | 0.0893 | -0.0200 | -0.0189 | -0.0299 | -0.0105 |
| | (0.0011) | (0.0018) | (0.0016) | (0.0014) | (0.0021) | (0.0020) | (0.0018) | (0.0018) |
| New York Stock Exchange | 0.1185 | 0.0848 | 0.0875 | 0.0805 | -0.0337 | -0.0310 | -0.0379 | -0.0056 |
| | (0.0016) | (0.0026) | (0.0033) | (0.0030) | (0.0030) | (0.0037) | (0.0035) | (0.0037) |
| Direct Edge X Stock Exchange | 0.1369 | 0.1055 | 0.1042 | 0.0925 | -0.0315 | -0.0327 | -0.0444 | -0.0123 |
| | (0.0013) | (0.0019) | (0.0020) | (0.0017) | (0.0022) | (0.0023) | (0.0022) | (0.0022) |
| BATS Exchange | 0.0923 | 0.0847 | 0.0827 | 0.0777 | -0.0076 | -0.0096 | -0.0146 | -0.0060 |
| | (0.0008) | (0.0011) | (0.0013) | (0.0014) | (0.0014) | (0.0016) | (0.0017) | (0.0017) |
| NASDAQ OMX PSX Stock Exchange | 0.0106 | 0.0090 | 0.0084 | 0.0093 | -0.0016 | -0.0022 | -0.0013 | 0.0006 |
| | (0.0001) | (0.0002) | (0.0002) | (0.0002) | (0.0002) | (0.0003) | (0.0003) | (0.0003) |
| NYSE MKT (American) Stock Exchange | 0.0065 | 0.0064 | 0.0061 | 0.0049 | -0.0001 | -0.0003 | -0.0016 | -0.0014 |
| | (0.0004) | (0.0003) | (0.0004) | (0.0011) | (0.0005) | (0.0005) | (0.0012) | (0.0011) |
| Chicago Stock Exchange | 0.0018 | 0.0021 | 0.0009 | 0.0013 | 0.0003 | -0.0008 | -0.0005 | -0.0002 |
| | (0.0004) | (0.0007) | (0.0002) | (0.0003) | (0.0008) | (0.0005) | (0.0004) | (0.0005) |
| *- Taker-Maker Venues:* | | | | | | | | |
| NASDAQ OMX BX Stock Exchange | 0.0545 | 0.1242 | 0.1245 | 0.1665 | 0.0697 | 0.0699 | 0.1120 | 0.0422 |
| | (0.0005) | (0.0017) | (0.0018) | (0.0025) | (0.0018) | (0.0019) | (0.0026) | (0.0028) |
| BATS Y-Exchange | 0.0606 | 0.1444 | 0.1424 | 0.1596 | 0.0838 | 0.0819 | 0.0990 | 0.0162 |
| | (0.0006) | (0.0017) | (0.0018) | (0.0017) | (0.0018) | (0.0019) | (0.0018) | (0.0021) |
| Direct Edge A Stock Exchange | 0.0269 | 0.0288 | 0.0287 | 0.0271 | 0.0018 | 0.0018 | 0.0002 | -0.0016 |
| | (0.0003) | (0.0004) | (0.0004) | (0.0004) | (0.0006) | (0.0006) | (0.0005) | (0.0005) |
| *- Zero Rebate Venues:* | | | | | | | | |
| Investors Exchange | 0.0418 | 0.0573 | 0.0558 | 0.0500 | 0.0155 | 0.0140 | 0.0082 | -0.0065 |
| | (0.0007) | (0.0015) | (0.0011) | (0.0011) | (0.0017) | (0.0013) | (0.0012) | (0.0014) |
| National Stock Exchange | 0.0001 | 0.0001 | 0.0001 | 0.0001 | 0.0000 | 0.0000 | 0.0000 | 0.0000 |
| | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) |

## Table 6. Exchange vs. Non-Exchange Trading

Table shows size-weighted fraction of shares that were traded on all exchanges relative to all non-exchange trading venues. Treatment effects estimates are the efficient semiparametric estimator and control for the pre-period outcome. Some figures may not add to one due to rounding. Standard errors in parentheses. See text for details.

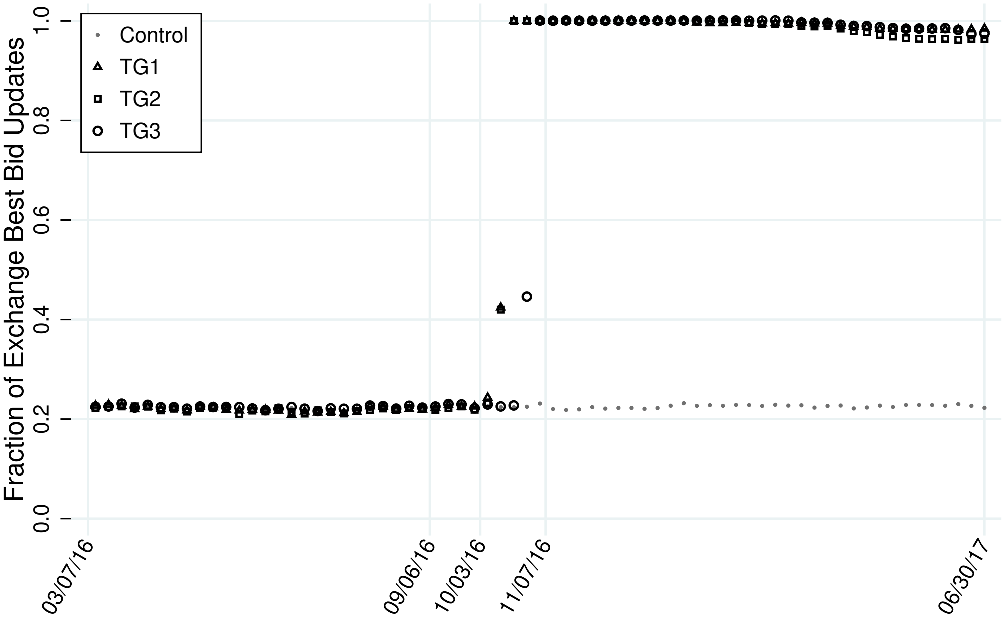
| | Size-weighted Fraction of Trades | | | | Estimated Treatment Effects | | | |
|---|---|---|---|---|---|---|---|---|
| | Control | Treatment Mean | | | TG1 vs. | TG2 vs. | TG3 vs. | TG3 vs. |
| Venue | Mean | TG1 | TG2 | TG3 | Control | Control | Control | TG1 & TG2 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| On Exchange | 0.584 | 0.540 | 0.551 | 0.680 | -0.044 | -0.032 | 0.097 | 0.135 |
| | (0.002) | (0.004) | (0.004) | (0.004) | (0.004) | (0.005) | (0.004) | (0.005) |
| Off Exchange | 0.416 | 0.460 | 0.449 | 0.320 | 0.044 | 0.032 | -0.097 | -0.135 |
| | (0.002) | (0.004) | (0.004) | (0.004) | (0.004) | (0.005) | (0.004) | (0.005) |

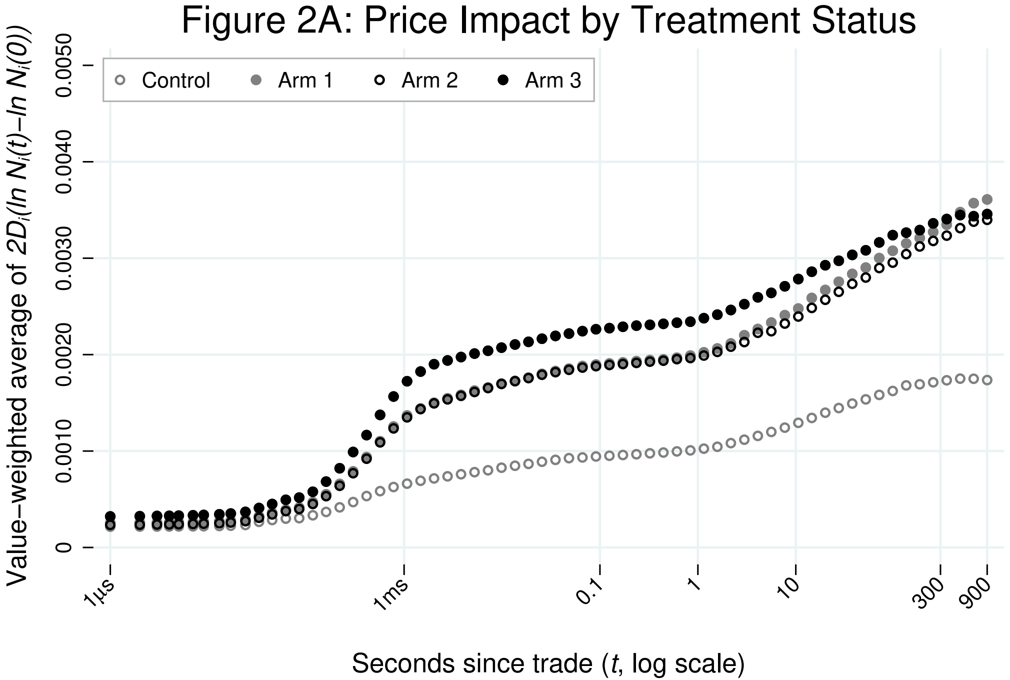
## Table 7. Midpoint Trading

Table presents estimated treatment effects for the incidence of trades priced at the midpoint of the NBBO, both in terms of differences between control and treatment groups as well as percent changes. Standard errors in parentheses. See text for details.

| Estimate | Overall | | On Exchanges | | Off Exchange Venues | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| TG1 | | | | | | |
| $\bar{Y}_1 - \bar{Y}_0$ | 0.056 | 0.055 | 0.052 | 0.051 | 0.040 | 0.038 |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.004) | (0.003) |
| $(\bar{Y}_1 - \bar{Y}_0)/\bar{Y}_0$ | 0.471 | 0.460 | 0.596 | 0.592 | 0.170 | 0.162 |
| | (0.025) | (0.022) | (0.035) | (0.034) | (0.018) | (0.014) |
| TG2 | | | | | | |
| $\bar{Y}_2 - \bar{Y}_0$ | 0.060 | 0.060 | 0.049 | 0.050 | 0.060 | 0.058 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.004) | (0.004) |
| $(\bar{Y}_2 - \bar{Y}_0)/\bar{Y}_0$ | 0.501 | 0.501 | 0.562 | 0.571 | 0.258 | 0.248 |
| | (0.023) | (0.021) | (0.028) | (0.028) | (0.020) | (0.016) |
| TG3 | | | | | | |
| $\bar{Y}_3 - \bar{Y}_0$ | 0.099 | 0.099 | 0.039 | 0.039 | 0.445 | 0.448 |
| | (0.003) | (0.003) | (0.002) | (0.002) | (0.008) | (0.006) |
| $(\bar{Y}_3 - \bar{Y}_0)/\bar{Y}_0$ | 0.827 | 0.824 | 0.450 | 0.450 | 1.905 | 1.915 |
| | (0.028) | (0.026) | (0.027) | (0.027) | (0.042) | (0.032) |
| Pre-Period Outcome | | 0.567 | | 0.414 | | 0.652 |
| | | (0.047) | | (0.081) | | (0.033) |
| Control Group Mean | | | | | | |
| $\bar{Y}_0$ | 0.120 | 0.120 | 0.087 | 0.087 | 0.234 | 0.234 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) | (0.002) |
| Observations | 1087 | 1087 | 1087 | 1087 | 1087 | 1087 |
| R-square | 0.689 | 0.742 | 0.574 | 0.594 | 0.895 | 0.935 |

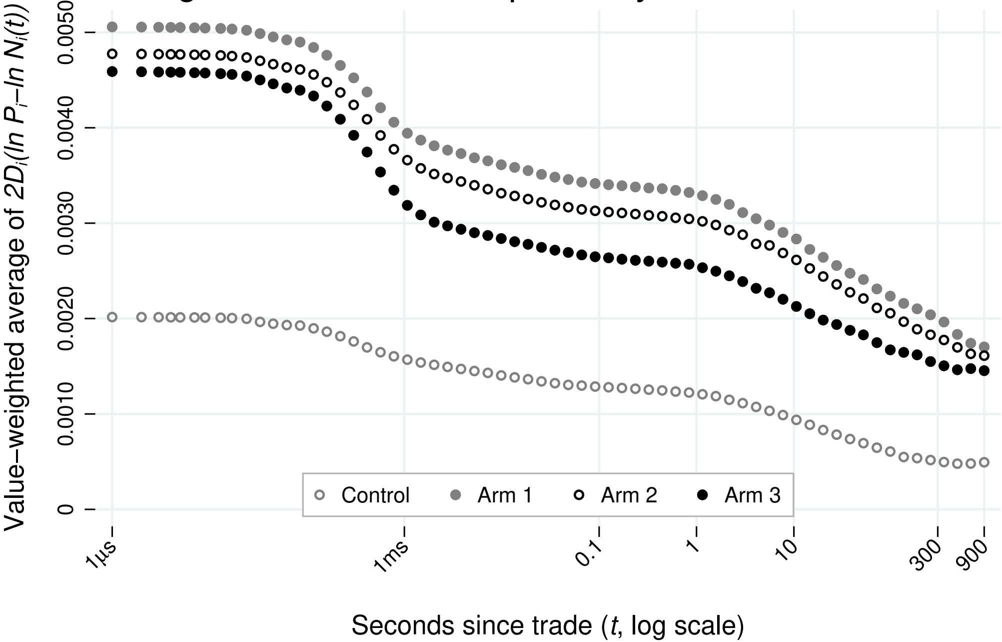
Figure 1. Incidence of Nickel–Priced Quotes
by Treatment Assignment Over Time






Note: $N_i(t)$ is the NBBO midpoint $t$ seconds after the $i$th trade. See text for details.

# Figure 2B: Realized Spread by Treatment Status




Note: $P_i$ is the price of the $i$th trade. See text for details.

## Appendix Table 1. Reasons for Non-Matches for 28 Unmatched Stocks

| Row | Ticker | FINRA name | Stratum [1] | Notes | Last Traded Date | Selected Public Domain Documents |
|---|---|---|---|---|---|---|
| 1 | ASEI | AMERICAN SCIENCE AND ENGINEERING INC [2] | M-H-M | In 2016, American Science and Engineering was acquired by OSI Systems. | 9/9/16 | en.wikipedia.org/wiki/American_Science_and_Engineering |
| 2 | AUMA | AR CAPITAL ACQUISITION CORP [2] | M-L-L | In 2016, AR Capital Acquisition Corp combined with Axar Acquisition Corp. | 10/7/16 | www.streetinsider.com/SEC+Filings/Form+8-K+Axar+Acquisition+Corp.+For%3A+Oct+06/12131686.html |
| 3 | CBNK | CHICOPEE BANCORP INC [2] | L-M-L | In 2016, Chicopee Bancorp combined with Westfield Financial. | 10/7/16 | www.sec.gov/Archives/edgar/data/1157647/000138713116004894/ex2-1.htm |
| 4 | CSH | CASH AMERICA INTERNATIONAL | H-H-M | In 2016, Cash America combined with First Cash Financial Services. | 9/1/16 | www.streetinsider.com/SEC+Filings/Form+S-8+POS+CASH+AMERICA+INTERNATION/11996980.html |
| 5 | CYNA | CYNAPSUS THERAPEUTICS INC [3] | M-M-M | In 2016, Sunovion Pharmaceuticals acquired Cynapsus Therapeutics. | 10/21/16 | news.sunovion.com/press-release/sunovion-pharmaceuticals-completes-acquisition-cynapsus-therapeutics |
| 6 | EPIQ | EPIQ SYSTEMS INC [2] | M-M-H | In 2016, Epiq Systems was acquired by OMERS Private Equity and Harvest Partners, LP, and combined with DTI. | 9/30/16 | www.omersprivateequity.com/News/Press-Release/Epiq-Systems-Reaches-Agreement-to-be-Acquired-for |
| 7 | FCFS | FIRST CASH FINANCIAL SERVICES INC [4] | H-H-M | cf., above entry regarding CSH. Ticker remained FCFS but CUSIP changed 8/31/2016 to 9/1/2016. | 8/31/16 | www.streetinsider.com/SEC+Filings/Form+S-8+POS+CASH+AMERICA+INTERNATION/11996980.html |
| 8 | FCLF | FIRST CLOVER LEAF FINANCIAL CORP [2] | L-M-L | In 2016, First Clover Leaf was acquired by First Mid-Illinois Bancshares | 9/7/16 | www.bizjournals.com/stlouis/morning_call/2016/04/first-clover-leaf-bank-parent-to-be-sold-in-90.html |
| 9 | FXCM | FXCM INC [5] | L-L-M | FXCM transitioned from a NYSE to a NASDAQ listing between 9/23/2016 and 9/26/2016 | NA | phx.corporate-ir.net/phoenix.zhtml?c=238885&p=irol-newsArticle&ID=2201403 |
| 10 | GLDC | GOLDEN ENTERPRISES INC [2] | L-M-L | In 2016, Utz Quality Food acquired Golden Enterprises | 9/30/16 | www.foodbusinessnews.net/articles/6962-utz-finalizes-acquisition-of-golden-enterprises |
| 11 | IDI | IDI INC | M-L-M | On 9/23/2016, IDI transitioned from trading on NYSE under IDI to on NASDAQ under COGT (new CUSIP) | 9/22/16 | www.businesswire.com/news/home/20160914005290/en/IDI-Announces-Corporate-Change-Cogint-Move-NASDAQ |
| 12 | IMPR | IMPRIVATA INC | M-M-H | In 2016, Imprivata was acquired by Thoma Bravo. | 9/15/16 | www.imprivata.com/company/press/imprivata-agrees-be-acquired-thoma-bravo |
| 13 | MDGN | MEDGENICS INC | M-L-M | On 12/15/2016, Medgenics changed its name to Aevi Genomic Medicine and changed its ticker from MDGN to GNMX. In March 2017, a related entity trades under the same ticker. | 12/15/16 | seekingalpha.com/news/3230811-medgenics-become-aevi-genomic-medicine-tomorrow |
| 14 | MFRM | MATTRESS FIRM HOLDING CORP [2] | H-H-H | In 2016, Mattress Firm was acquired by Steinhoff International Holdings. | 9/16/16 | newsroom.mattressfirm.com/press/mattress-firm-acquired-steinhoff-international-64-00-per-share-cash/ |
| 15 | OUTR | OUTERWALL INC [2] | H-H-H | In 2016, Outerwall was acquired by Apollo Global Management. | 9/27/16 | en.wikipedia.org/wiki/Outerwall |
| 16 | PCCC | PC CONNECTION INC [2] | M-H-L | On 9/7/2016, PC Connection reorganized several subsidiaries to trade under a new common ticker, CNXN. | 9/7/16 | en.wikipedia.org/wiki/PC_Connection |
| 17 | PGND | PRESS GANEY HOLDINGS INC | H-H-H | In 2016, Press Ganey was acquired by EQT Equity Fund. | 10/21/16 | www.bloomberg.com/news/articles/2016-08-09/private-equity-firm-eqt-to-buy-press-ganey-in-2-35-billion-deal |
| 18 | RDEN | ELIZABETH ARDEN INC [2] | M-M-H | In 2016, Elizabeth Arden was acquired by Revlon. | 9/6/16 | en.wikipedia.org/wiki/Elizabeth_Arden,_Inc. |
| 19 | REXI | RESOURCE AMERICA INC CLASS A [2] | M-L-M | In 2016, Resource America was acquired by C-III Partners. | 9/7/16 | bluevaultpartners.com/news/c-iii-capital-partners-acquires-resource-america/ |
| 20 | ROVI | ROVI CORPORATION [2] | H-M-H | In 2016, Rovi acquired TiVo. | 9/6/16 | www.sec.gov/Archives/edgar/data/1675820/000167582017000006/tivocorp12311610-k.htm |
| 21 | RRMS | ROSE ROCK MIDSTREAM LP | H-H-M | In 2016, Rose Rock was acquired by SemGroup. | 9/29/16 | en.wikipedia.org/wiki/SemGroup |
| 22 | SKUL | SKULLCANDY INC [2] | L-L-H | In 2016, Skullcandy was acquired by Mill Road Capital. | 9/30/16 | en.wikipedia.org/wiki/Skullcandy |
| 23 | SZMK | SIZMEK INC [2] | L-L-H | In 2016, Sizmek was acquired by affiliates of Vector Capital. | 9/26/16 | www.globenewswire.com/news-release/2016/08/03/861208/0/en/Sizmek-to-be-Acquired-by-Vector-Capital.html |
| 24 | TOF | TOFUTTI BRANDS INC | L-L-L | Tofuti Brands voluntarily delisted on or before 10/24/2016. | 10/20/16 | www.prnewswire.com/news-releases/tofutti-announces-transfer-to-otcqb-market-300349473.html |
| 25 | USBI | UNITED SECURITY BANCSHARES INC [2] | L-L-L | On 10/10/2016, United Security Bancshares changed names to First US Bancshares and tickers to FUSB with no change to outstanding shares. | 10/7/16 | www.reuters.com/article/idUSFWN1CG0G4 |
| 26 | USMD | USMD HOLDINGS INC [2] | M-M-L | In 2016, WellMed acquired USMD Holdings. | 9/29/16 | www.expressnews.com/business/health-care/article/WellMed-acquiring-Irving-firm-for-almost-255M-9195094.php |
| 27 | WFD | WESTFIELD FINANCIAL INC [2] | L-L-L | cf., above entry regarding CBNK. | 10/20/16 | www.sec.gov/Archives/edgar/data/1157647/000138713116004894/ex2-1.htm |
| 28 | YCB | YOUR COMMUNITY BANKSHARES INC [2] | M-H-L | In 2016, Your Community Bankshares was acquired by WesBanco. | 9/8/16 | www.bloomberg.com/research/stocks/private/snapshot.asp?privcapId=341203 |

Notes: 1 For each of the 28 tickers, the initial and revised stratum are equal.
2 FINRA name appended "COMMON STOCK" to name displayed in table
3 FINRA name appended "COMMON SHARES" to name displayed in table
4 Full FINRA name was "FIRST CASH FINANCIAL SERVICES INC COMMON STOCK - MERGED WITH CSH BEFORE INITIAL LIST PUBLISHED"
5 Traded under the same ticker throughout, but changed listing exchange. See notes field for detail.

## Appendix Table 2. Balancing Tests: Long Sample

This table reports balance tests for control and treatment securities in the long binding sample. Estimates of the control group mean reflect regression-adjusted means after controlling for strata. Treatment effects estimates are relative to control and condition on strata. Heteroskedasticity-robust standard errors in parentheses. Final column presents the F-statistic for the null hypothesis of zero treatment effects in the pre-treatment period and the associated p-value. For all rows, n=930.

| Outcome | Control Group Mean | Treatment Effect for Group: TG1 | TG2 | TG3 | Joint Test of Randomization |
|---|---|---|---|---|---|
| Log Trades | 6.9297 | -0.0434 | -0.0243 | -0.0597 | 0.6527 |
| | (0.0220) | (0.0505) | (0.0462) | (0.0480) | p=0.5814 |
| Log On-Exchange Trades | 6.6332 | -0.0494 | -0.0235 | -0.0741 | 0.8131 |
| | (0.0241) | (0.0537) | (0.0493) | (0.0519) | p=0.4867 |
| Log Off-Exchange Trades | 5.4692 | -0.0305 | -0.0217 | -0.0208 | 0.2298 |
| | (0.0193) | (0.0458) | (0.0419) | (0.0429) | p=0.8757 |
| Log Volume | 11.7905 | -0.0389 | -0.0080 | -0.0015 | 0.1974 |
| | (0.0209) | (0.0512) | (0.0449) | (0.0456) | p=0.8982 |
| Log On-Exchange Volume | 11.2642 | -0.0441 | -0.0200 | -0.0336 | 0.3275 |
| | (0.0226) | (0.0543) | (0.0480) | (0.0485) | p=0.8055 |
| Log Off-Exchange Volume | 10.7667 | -0.0398 | 0.0024 | 0.0447 | 0.5963 |
| | (0.0221) | (0.0523) | (0.0465) | (0.0482) | p=0.6175 |
| Size-weighted ISO fraction | 0.3201 | -0.0030 | -0.0059 | -0.0079 | 1.6177 |
| | (0.0021) | (0.0040) | (0.0039) | (0.0040) | p=0.1837 |
| Quoted Spread | 0.0275 | 0.0008 | 0.0001 | -0.0010 | 0.8642 |
| | (0.0004) | (0.0010) | (0.0009) | (0.0008) | p=0.4592 |
| Percent Quoted Spread | 0.2906 | 0.0274 | 0.0139 | 0.0146 | 0.6606 |
| x100 | (0.0088) | (0.0241) | (0.0195) | (0.0151) | p=0.5764 |
| Locked or Crossed | 0.0129 | 0.0002 | 0.0008 | 0.0006 | 0.4132 |
| | (0.0004) | (0.0008) | (0.0009) | (0.0008) | p=0.7436 |
| Effective Spread | 0.0216 | 0.0006 | 0.0001 | -0.0006 | 0.7561 |
| | (0.0003) | (0.0007) | (0.0007) | (0.0006) | p=0.5190 |
| Relative Effective Spread | 0.0023 | 0.0002 | 0.0001 | 0.0001 | 0.7270 |
| | (0.0001) | (0.0002) | (0.0002) | (0.0001) | p=0.5360 |
| EQ Ratio | 0.8434 | -0.0029 | 0.0011 | 0.0042 | 1.2687 |
| | (0.0015) | (0.0030) | (0.0030) | (0.0030) | p=0.2839 |
| Price Improvement | 0.0032 | 0.0001 | 0.0000 | -0.0002 | 0.9936 |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) | p=0.3951 |
| Log Inside Depth | 1.5220 | 0.0447 | 0.0571 | 0.0860 | 1.6878 |
| | (0.0199) | (0.0425) | (0.0422) | (0.0433) | p=0.1681 |

## Appendix Table 3. Spreads and Depths: Long Sample

Table presents estimated treatment effects for quoted spreads, relative quoted spreads, relative effective spreads, and log inside depth, both in terms of differences between control and treatment groups as well as percent changes. Standard errors in parentheses. See text for details.

| Estimate | Quoted Spread | | | | Relative Quoted Spread | | Relative Effective Spread | | Estimate | Log Inside Depth | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1a) | (2a) | (1b) | (2b) | (3) | (4) | (5) | (6) | | (7) | (8) |
| TG1 | | | | | | | | | | | |
| $\overline{Y}_1 - \overline{Y}_0$ | 0.029 | 0.029 | 0.029 | 0.028 | 0.33 | 0.32 | 0.25 | 0.25 | $\hat{\theta}_1 = \overline{\ln Y_1} - \overline{\ln Y_0}$ | 1.33 | 1.30 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.03) | (0.03) | (0.02) | (0.02) | | (0.06) | (0.05) |
| $(\overline{Y}_1 - \overline{Y}_0)/\overline{Y}_0$ | 0.860 | 0.859 | 0.860 | 0.846 | 1.17 | 1.12 | 1.16 | 1.14 | $\exp(\hat{\theta}_1)$-1 | 2.76 | 2.66 |
| | (0.053) | (0.054) | (0.053) | (0.045) | (0.12) | (0.11) | (0.14) | (0.14) | | (0.24) | (0.18) |
| TG2 | | | | | | | | | | | |
| $\overline{Y}_2 - \overline{Y}_0$ | 0.069 | 0.069 | 0.028 | 0.027 | 0.31 | 0.30 | 0.22 | 0.23 | $\hat{\theta}_2 = \overline{\ln Y_2} - \overline{\ln Y_0}$ | 1.33 | 1.30 |
| | (0.042) | (0.043) | (0.001) | (0.001) | (0.04) | (0.03) | (0.03) | (0.03) | | (0.07) | (0.06) |
| $(\overline{Y}_2 - \overline{Y}_0)/\overline{Y}_0$ | 2.080 | 2.079 | 0.833 | 0.821 | 1.11 | 1.04 | 1.04 | 1.04 | $\exp(\hat{\theta}_2)$-1 | 2.78 | 2.65 |
| | (1.273) | (1.287) | (0.056) | (0.050) | (0.14) | (0.13) | (0.16) | (0.15) | | (0.28) | (0.23) |
| TG3 | | | | | | | | | | | |
| $\overline{Y}_3 - \overline{Y}_0$ | 0.024 | 0.024 | 0.024 | 0.025 | 0.35 | 0.33 | 0.22 | 0.21 | $\hat{\theta}_3 = \overline{\ln Y_3} - \overline{\ln Y_0}$ | 1.56 | 1.49 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.03) | (0.03) | (0.03) | (0.02) | | (0.06) | (0.05) |
| $(\overline{Y}_3 - \overline{Y}_0)/\overline{Y}_0$ | 0.733 | 0.734 | 0.733 | 0.747 | 1.22 | 1.14 | 1.01 | 0.98 | $\exp(\hat{\theta}_3)$-1 | 3.77 | 3.42 |
| | (0.048) | (0.049) | (0.048) | (0.043) | (0.14) | (0.13) | (0.15) | (0.14) | | (0.31) | (0.21) |
| Pre-Period Outcome | | 0.053 | | 0.759 | | 0.82 | | 0.47 | | | 0.92 |
| | | (0.739) | | (0.051) | | (0.14) | | (0.14) | | | (0.05) |
| Exclude SKYW on 11/24/2017? | | | | | | | | | | | |
| | No | No | Yes | Yes | No | No | No | No | | No | No |
| Control Group Mean | | | | | | | | | | | |
| $\overline{Y}_0$ | 0.033 | 0.033 | 0.033 | 0.033 | 0.28 | 0.29 | 0.22 | 0.22 | $\exp(\overline{Y}_0)$ | 586.1 | 599.3 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.01) | (0.01) | (0.01) | (0.01) | | (16.0) | (12.3) |
| Observations | 930 | 930 | 930 | 930 | 930 | 930 | 930 | 930 | | 930 | 930 |
| R-square | 0.05 | 0.05 | 0.61 | 0.70 | 0.65 | 0.70 | 0.59 | 0.63 | | 0.67 | 0.79 |

## Appendix Table 4. Price Impact and Realized Spreads: Long Sample

Table presents estimated treatment effects (differences and percentage changes) on price impact and realized spreads at 1 microsecond, 1 second, and 5 minutes after a trade. Relative effective spread is value weighted. Standard errors in parentheses. Analysis pertains to long binding sample. See text for details

| | Relative Effective | | $1\mu s$ | | | | 1 second | | | | 5 minute | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Estimate | Spread | | Price Impact | | Realized Spread | | Price Impact | | Realized Spread | | Price Impact | | Realized Spread | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
| TG1 | | | | | | | | | | | | | | |
| $\bar{Y}_1 - \bar{Y}_0$ | 0.229 | 0.225 | 0.0031 | 0.0033 | 0.226 | 0.222 | 0.0851 | 0.0837 | 0.144 | 0.142 | 0.1198 | 0.1164 | 0.109 | 0.109 |
| | (0.023) | (0.021) | (0.0013) | (0.0012) | (0.022) | (0.021) | (0.0059) | (0.0053) | (0.019) | (0.018) | (0.0111) | (0.0104) | (0.015) | (0.014) |
| $(\bar{Y}_1 - \bar{Y}_0)/\bar{Y}_0$ | 1.115 | 1.090 | 0.1591 | 0.1677 | 1.217 | 1.189 | 0.8619 | 0.8384 | 1.350 | 1.326 | 0.7341 | 0.7028 | 2.601 | 2.607 |
| | (0.141) | (0.134) | (0.0704) | (0.0631) | (0.154) | (0.148) | (0.0683) | (0.0608) | (0.245) | (0.238) | (0.0800) | (0.0732) | (0.695) | (0.694) |
| TG2 | | | | | | | | | | | | | | |
| $\bar{Y}_2 - \bar{Y}_0$ | 0.204 | 0.206 | 0.0017 | 0.0021 | 0.202 | 0.204 | 0.0799 | 0.0799 | 0.124 | 0.125 | 0.1130 | 0.1090 | 0.091 | 0.093 |
| | (0.028) | (0.026) | (0.0010) | (0.0009) | (0.028) | (0.026) | (0.0070) | (0.0055) | (0.023) | (0.023) | (0.0153) | (0.0126) | (0.017) | (0.016) |
| $(\bar{Y}_2 - \bar{Y}_0)/\bar{Y}_0$ | 0.994 | 0.995 | 0.0838 | 0.1061 | 1.091 | 1.089 | 0.8088 | 0.8007 | 1.166 | 1.170 | 0.6925 | 0.6582 | 2.171 | 2.230 |
| | (0.160) | (0.151) | (0.0532) | (0.0461) | (0.176) | (0.167) | (0.0775) | (0.0620) | (0.268) | (0.261) | (0.1021) | (0.0840) | (0.654) | (0.660) |
| TG3 | | | | | | | | | | | | | | |
| $\bar{Y}_3 - \bar{Y}_0$ | 0.211 | 0.204 | 0.0082 | 0.0074 | 0.203 | 0.197 | 0.1086 | 0.1042 | 0.103 | 0.100 | 0.1234 | 0.1158 | 0.088 | 0.088 |
| | (0.026) | (0.025) | (0.0016) | (0.0015) | (0.024) | (0.024) | (0.0071) | (0.0064) | (0.020) | (0.020) | (0.0133) | (0.0126) | (0.015) | (0.015) |
| $(\bar{Y}_3 - \bar{Y}_0)/\bar{Y}_0$ | 1.030 | 0.987 | 0.4161 | 0.3730 | 1.096 | 1.053 | 1.0998 | 1.0438 | 0.966 | 0.932 | 0.7562 | 0.6992 | 2.099 | 2.099 |
| | (0.150) | (0.144) | (0.0885) | (0.0795) | (0.161) | (0.156) | (0.0822) | (0.0726) | (0.233) | (0.227) | (0.0919) | (0.0845) | (0.621) | (0.621) |
| Pre-Period Outcome | | 0.466 | | 0.6311 | | 0.449 | | 0.7349 | | 0.320 | | 0.5733 | | 0.191 |
| | | (0.151) | | (0.1252) | | (0.151) | | (0.0878) | | (0.128) | | (0.1026) | | (0.091) |
| Control Group Mean | | | | | | | | | | | | | | |
| $\bar{Y}_0$ | 0.205 | 0.207 | 0.0198 | 0.0198 | 0.186 | 0.187 | 0.0987 | 0.0998 | 0.107 | 0.107 | 0.1632 | 0.1656 | 0.042 | 0.042 |
| | (0.010) | (0.010) | (0.0006) | (0.0005) | (0.009) | (0.009) | (0.0021) | (0.0019) | (0.009) | (0.009) | (0.0048) | (0.0045) | (0.007) | (0.007) |
| Observations | 930 | 930 | 930 | 930 | 930 | 930 | 930 | 930 | 930 | 930 | 930 | 930 | 930 | 930 |
| R-square | 0.59 | 0.62 | 0.61 | 0.70 | 0.58 | 0.61 | 0.68 | 0.75 | 0.51 | 0.53 | 0.65 | 0.71 | 0.40 | 0.41 |

## Appendix Table 5. Exchange Trading: Long Sample

Panel A shows the fraction of the National Best Offer accounted for by the Best Offer posted on each exchange, as well as estimated treatment effects. Panel B is analogous but shows the size-weighted fraction of exchange trades for each exchange. Treatment effects estimates are the efficient semiparametric estimator and control for the pre-period outcome (except for the Investors Exchange, where no pre-period exists). Standard errors in parentheses. Some columns may not add to one due to rounding. Analysis based on long binding sample. See text for details.

| | Fraction of the NBO on Given Exchange | | | | Estimated Treatment Effects | | | |
|---|---|---|---|---|---|---|---|---|
| | Control | Treatment Mean | | | TG1 vs. | TG2 vs. | TG3 vs. | TG3 vs. |
| Venue | Mean | TG1 | TG2 | TG3 | Control | Control | Control | TG1 & TG2 |
| *A. Distribution of NBO Among Exchanges* | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *- Maker-Taker Venues:* | | | | | | | | |
| NASDAQ Stock Exchange | 0.3466 | 0.2529 | 0.2434 | 0.2415 | -0.0937 | -0.1032 | -0.1051 | -0.0066 |
| | (0.0024) | (0.0038) | (0.0038) | (0.0037) | (0.0044) | (0.0045) | (0.0044) | (0.0045) |
| NYSE Arca | 0.1320 | 0.1256 | 0.1285 | 0.1117 | -0.0064 | -0.0035 | -0.0203 | -0.0154 |
| | (0.0011) | (0.0014) | (0.0019) | (0.0016) | (0.0018) | (0.0022) | (0.0019) | (0.0020) |
| New York Stock Exchange | 0.1307 | 0.0907 | 0.0969 | 0.0830 | -0.0400 | -0.0338 | -0.0477 | -0.0108 |
| | (0.0021) | (0.0032) | (0.0034) | (0.0036) | (0.0039) | (0.0040) | (0.0042) | (0.0043) |
| Direct Edge X Stock Exchange | 0.1181 | 0.1076 | 0.1057 | 0.1007 | -0.0105 | -0.0124 | -0.0173 | -0.0059 |
| | (0.0011) | (0.0019) | (0.0021) | (0.0018) | (0.0022) | (0.0023) | (0.0021) | (0.0023) |
| BATS Exchange | 0.1109 | 0.0886 | 0.0894 | 0.0798 | -0.0223 | -0.0215 | -0.0310 | -0.0092 |
| | (0.0010) | (0.0010) | (0.0011) | (0.0012) | (0.0014) | (0.0015) | (0.0016) | (0.0014) |
| NASDAQ OMX PSX Stock Exchange | 0.0187 | 0.0129 | 0.0127 | 0.0138 | -0.0058 | -0.0059 | -0.0049 | 0.0009 |
| | (0.0003) | (0.0003) | (0.0003) | (0.0004) | (0.0005) | (0.0004) | (0.0005) | (0.0004) |
| NYSE MKT (American) Stock Exchange | 0.0086 | 0.0102 | 0.0093 | 0.0081 | 0.0016 | 0.0007 | -0.0005 | -0.0016 |
| | (0.0006) | (0.0007) | (0.0009) | (0.0009) | (0.0009) | (0.0011) | (0.0010) | (0.0010) |
| Chicago Stock Exchange | 0.0015 | 0.0008 | 0.0008 | 0.0007 | -0.0007 | -0.0007 | -0.0009 | -0.0001 |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) | (0.0001) |
| *- Taker-Maker Venues:* | | | | | | | | |
| NASDAQ OMX BX Stock Exchange | 0.0554 | 0.1168 | 0.1184 | 0.1506 | 0.0614 | 0.0630 | 0.0952 | 0.0330 |
| | (0.0007) | (0.0020) | (0.0020) | (0.0025) | (0.0021) | (0.0021) | (0.0026) | (0.0029) |
| BATS Y-Exchange | 0.0472 | 0.1346 | 0.1339 | 0.1477 | 0.0874 | 0.0867 | 0.1005 | 0.0135 |
| | (0.0006) | (0.0018) | (0.0020) | (0.0019) | (0.0019) | (0.0021) | (0.0020) | (0.0023) |
| Direct Edge A Stock Exchange | 0.0147 | 0.0260 | 0.0265 | 0.0268 | 0.0113 | 0.0119 | 0.0122 | 0.0006 |
| | (0.0002) | (0.0004) | (0.0003) | (0.0003) | (0.0004) | (0.0004) | (0.0004) | (0.0004) |
| *- Zero Rebate Venues:* | | | | | | | | |
| Investors Exchange | 0.0141 | 0.0303 | 0.0303 | 0.0255 | 0.0162 | 0.0161 | 0.0114 | -0.0048 |
| | (0.0003) | (0.0005) | (0.0006) | (0.0005) | (0.0006) | (0.0006) | (0.0006) | (0.0006) |
| National Stock Exchange | 0.0011 | 0.0067 | 0.0063 | 0.0082 | 0.0056 | 0.0053 | 0.0071 | 0.0017 |
| | (0.0001) | (0.0003) | (0.0003) | (0.0004) | (0.0004) | (0.0004) | (0.0004) | (0.0005) |

| Venue | Size-weighted Fraction of Trades: Control Mean | Treatment Mean: TG1 | Treatment Mean: TG2 | Treatment Mean: TG3 | Estimated Treatment Effects: TG1 vs. Control | TG2 vs. Control | TG3 vs. Control | TG3 vs. TG1 & TG2 |
|---|---|---|---|---|---|---|---|---|
| *B. Distribution of Trading Among Exchanges* | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *- Maker-Taker Venues:* | | | | | | | | |
| NASDAQ Stock Exchange | 0.3213 | 0.2354 | 0.2343 | 0.2208 | -0.0859 | -0.0870 | -0.1005 | -0.0140 |
| | (0.0022) | (0.0037) | (0.0039) | (0.0035) | (0.0043) | (0.0045) | (0.0041) | (0.0044) |
| NYSE Arca SM | 0.1199 | 0.1070 | 0.1061 | 0.0894 | -0.0128 | -0.0138 | -0.0305 | -0.0172 |
| | (0.0011) | (0.0016) | (0.0015) | (0.0015) | (0.0020) | (0.0018) | (0.0018) | (0.0018) |
| New York Stock Exchange | 0.1207 | 0.0806 | 0.0860 | 0.0719 | -0.0401 | -0.0347 | -0.0488 | -0.0114 |
| | (0.0020) | (0.0029) | (0.0035) | (0.0037) | (0.0036) | (0.0041) | (0.0042) | (0.0044) |
| Direct Edge X Stock Exchange | 0.1403 | 0.1106 | 0.1092 | 0.0945 | -0.0297 | -0.0311 | -0.0458 | -0.0153 |
| | (0.0014) | (0.0022) | (0.0026) | (0.0021) | (0.0026) | (0.0030) | (0.0026) | (0.0027) |
| BATS Exchange | 0.0904 | 0.0806 | 0.0796 | 0.0683 | -0.0098 | -0.0108 | -0.0221 | -0.0118 |
| | (0.0008) | (0.0011) | (0.0010) | (0.0011) | (0.0013) | (0.0013) | (0.0013) | (0.0013) |
| NASDAQ OMX PSX Stock Exchange | 0.0095 | 0.0077 | 0.0075 | 0.0073 | -0.0017 | -0.0020 | -0.0022 | -0.0003 |
| | (0.0001) | (0.0002) | (0.0002) | (0.0002) | (0.0002) | (0.0002) | (0.0002) | (0.0002) |
| NYSE MKT (American) Stock Exchange | 0.0071 | 0.0088 | 0.0078 | 0.0070 | 0.0017 | 0.0007 | 0.0000 | -0.0012 |
| | (0.0006) | (0.0005) | (0.0008) | (0.0008) | (0.0007) | (0.0009) | (0.0009) | (0.0008) |
| Chicago Stock Exchange | 0.0018 | 0.0021 | 0.0016 | 0.0011 | 0.0004 | -0.0001 | -0.0007 | -0.0008 |
| | (0.0002) | (0.0005) | (0.0005) | (0.0001) | (0.0006) | (0.0005) | (0.0002) | (0.0004) |
| *- Taker-Maker Venues:* | | | | | | | | |
| NASDAQ OMX BX Stock Exchange | 0.0615 | 0.1274 | 0.1299 | 0.1800 | 0.0659 | 0.0684 | 0.1185 | 0.0514 |
| | (0.0006) | (0.0022) | (0.0023) | (0.0029) | (0.0023) | (0.0024) | (0.0029) | (0.0033) |
| BATS Y-Exchange | 0.0588 | 0.1525 | 0.1521 | 0.1741 | 0.0937 | 0.0932 | 0.1152 | 0.0218 |
| | (0.0006) | (0.0022) | (0.0023) | (0.0022) | (0.0023) | (0.0024) | (0.0022) | (0.0027) |
| Direct Edge A Stock Exchange | 0.0198 | 0.0209 | 0.0210 | 0.0195 | 0.0011 | 0.0012 | -0.0004 | -0.0015 |
| | (0.0002) | (0.0003) | (0.0003) | (0.0003) | (0.0004) | (0.0004) | (0.0004) | (0.0004) |
| *- Zero Rebate Venues:* | | | | | | | | |
| Investors Exchange | 0.0480 | 0.0625 | 0.0630 | 0.0565 | 0.0145 | 0.0151 | 0.0086 | -0.0062 |
| | (0.0007) | (0.0014) | (0.0015) | (0.0011) | (0.0015) | (0.0016) | (0.0013) | (0.0015) |
| National Stock Exchange | 0.0009 | 0.0051 | 0.0048 | 0.0069 | 0.0042 | 0.0039 | 0.0060 | 0.0020 |
| | (0.0000) | (0.0003) | (0.0003) | (0.0004) | (0.0003) | (0.0003) | (0.0004) | (0.0004) |

## Appendix Table 6. Exchange vs. Non-Exchange Trading: Long Sample

Table shows size-weighted fraction of shares that were traded on all exchanges relative to all non-exchange trading venues. Treatment effects estimates are the efficient semiparametric estimator and control for the pre-period outcome. Some figures may not add to one due to rounding. Standard errors in parentheses. Analysis corresponds to long binding sample over the extended evaluation period. See text for details.

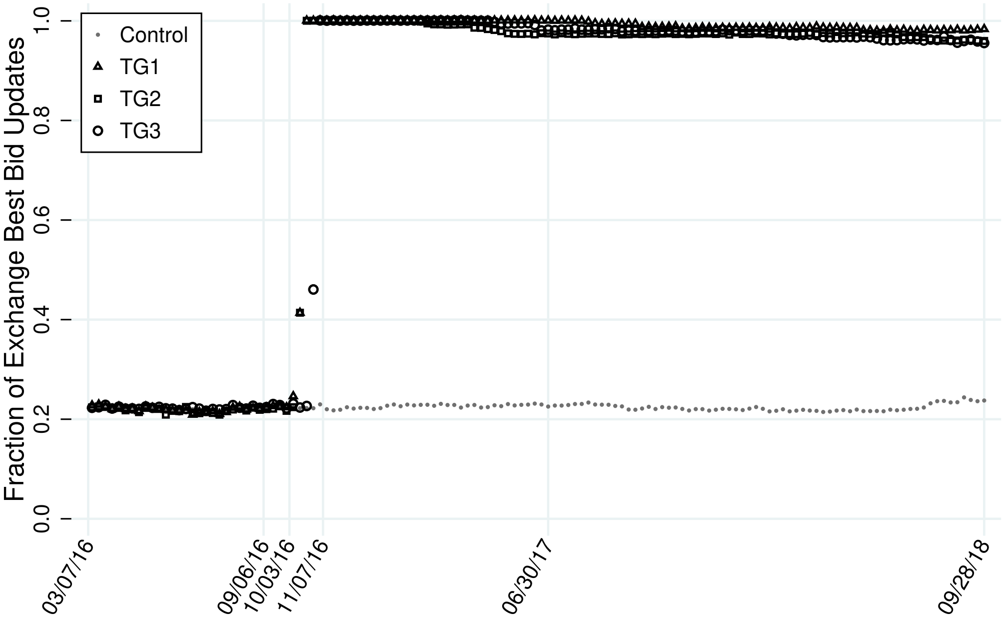
| | Size-weighted Fraction of Trades | | | | Estimated Treatment Effects | | | |
|---|---|---|---|---|---|---|---|---|
| | Control | Treatment Mean | | | TG1 vs. | TG2 vs. | TG3 vs. | TG3 vs. |
| Venue | Mean | TG1 | TG2 | TG3 | Control | Control | Control | TG1 & TG2 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| On Exchange | 0.590 | 0.551 | 0.569 | 0.683 | -0.039 | -0.021 | 0.092 | 0.122 |
| | (0.002) | (0.004) | (0.005) | (0.004) | (0.004) | (0.006) | (0.005) | (0.005) |
| Off Exchange | 0.410 | 0.449 | 0.431 | 0.317 | 0.039 | 0.021 | -0.092 | -0.122 |
| | (0.002) | (0.004) | (0.005) | (0.004) | (0.004) | (0.006) | (0.005) | (0.005) |

## Appendix Table 7. Midpoint Trading: Long Sample

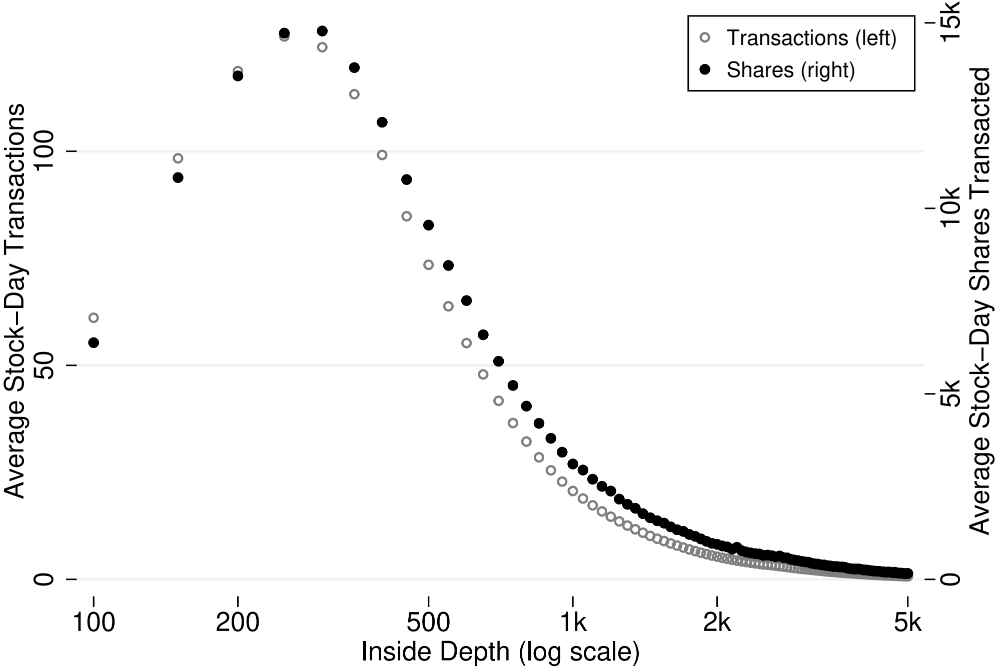
Table presents estimated treatment effects for the incidence of trades priced at the midpoint of the NBBO, both in terms of differences between control and treatment groups as well as percent changes. Standard errors in parentheses. Analysis corresponds to long binding sample over the extended evaluation period. See text for details.

| Estimate | Overall | | On Exchanges | | Off Exchange Venues | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| TG1 | | | | | | |
| $\overline{Y_1} - \overline{Y_0}$ | 0.066 | 0.066 | 0.058 | 0.058 | 0.060 | 0.060 |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.005) | (0.004) |
| $(\overline{Y_1} - \overline{Y_0})/\overline{Y_0}$ | 0.551 | 0.551 | 0.667 | 0.668 | 0.252 | 0.255 |
| | (0.026) | (0.024) | (0.033) | (0.032) | (0.021) | (0.017) |
| TG2 | | | | | | |
| $\overline{Y_2} - \overline{Y_0}$ | 0.072 | 0.072 | 0.059 | 0.059 | 0.082 | 0.083 |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.006) | (0.005) |
| $(\overline{Y_2} - \overline{Y_0})/\overline{Y_0}$ | 0.596 | 0.602 | 0.672 | 0.678 | 0.347 | 0.351 |
| | (0.029) | (0.029) | (0.036) | (0.036) | (0.025) | (0.022) |
| TG3 | | | | | | |
| $\overline{Y_3} - \overline{Y_0}$ | 0.100 | 0.100 | 0.043 | 0.044 | 0.435 | 0.439 |
| | (0.003) | (0.003) | (0.002) | (0.002) | (0.009) | (0.008) |
| $(\overline{Y_3} - \overline{Y_0})/\overline{Y_0}$ | 0.830 | 0.834 | 0.496 | 0.499 | 1.836 | 1.858 |
| | (0.030) | (0.027) | (0.029) | (0.028) | (0.047) | (0.039) |
| Pre-Period Outcome | | 0.449 | | 0.319 | | 0.640 |
| | | (0.057) | | (0.089) | | (0.043) |
| Control Group Mean | | | | | | |
| $\overline{Y_0}$ | 0.120 | 0.120 | 0.087 | 0.087 | 0.237 | 0.236 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) | (0.002) |
| Observations | 930 | 930 | 930 | 930 | 930 | 930 |
| R-square | 0.718 | 0.746 | 0.622 | 0.633 | 0.877 | 0.935 |

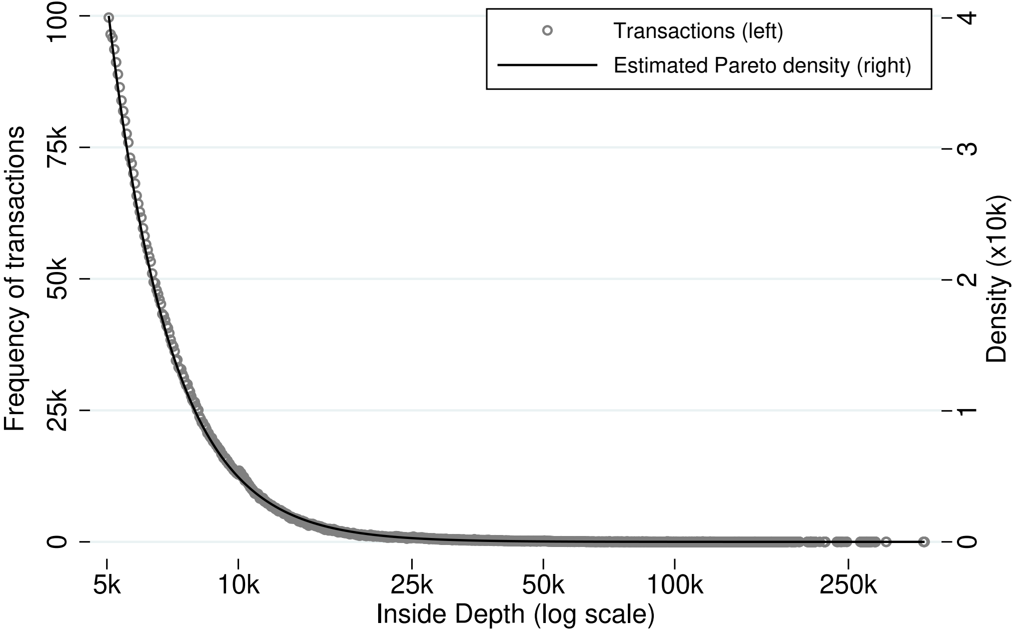
Appendix Figure 1. Incidence of Nickel–Priced Quotes by Treatment Assignment Over Time: Long Sample



Appendix Figure 2A. Distribution of Inside Depth

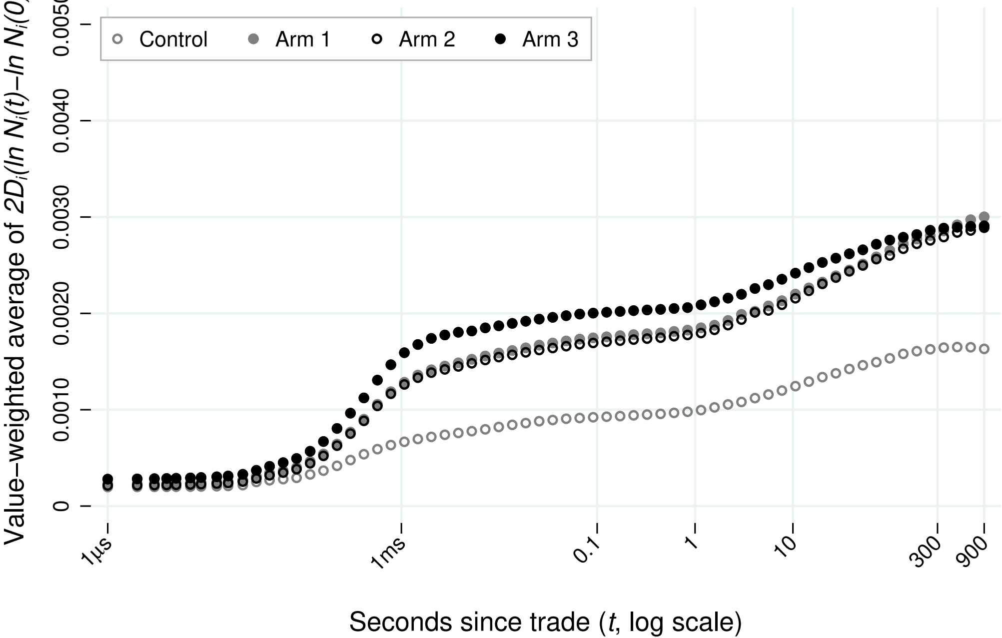


Appendix Figure 2B. Tail Behavor of Inside Depth



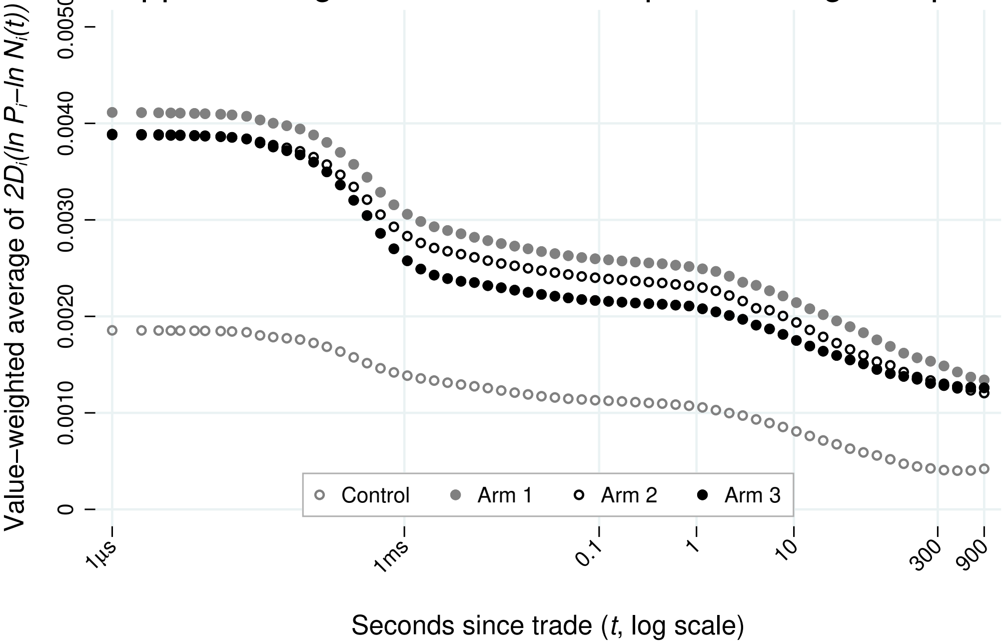

Note: Pareto parameter estimates (standard errors) are $x_m$=5,195.3 (9.9) and $\alpha$=2.150 (0.003).

# Appendix Figure 3A. Price Impact: Long Sample




Note: $N_i(t)$ is the NBBO midpoint $t$ seconds after the $i$th trade. See text for details.

# Appendix Figure 3B. Realized Spread: Long Sample




Note: $P_i$ is the price of the $i$th trade. See text for details.