Do Investors Save When Market Makers Pay?
Retail Execution Costs Under Payment for Order Flow Models

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November 2021

Abstract

We directly compare retail investor execution costs with exchange execution costs. We find off-exchange retail trades execute at lower effective spreads than comparable exchange trades, primarily due to the uninformed nature of retail trades. These results hold when payment for order flow (PFOF) became the main source of trading revenue for brokers, suggesting that these arrangements do not harm retail investors. Additionally, we find that current standards of retail execution quality overstate economic savings and suggest policy changes to represent these more accurately to investors.

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I. Introduction

In October 2019, five US brokerage firms—Charles Schwab, TD Ameritrade, E*TRADE, Ally Invest, and Fidelity—eliminated trading commissions for their retail clients.[1] And while the brokerage industry touted this move as a victory for retail investors, other interested parties such as advocacy groups, news outlets, and policy makers voiced concern. First, the illusory nature of "free trading" offered in a zero-commission environment obfuscates the execution costs retail traders still bear. Second, removing commissions increased brokers' reliance on payments for order flow (PFOF) as a source of revenue, which in turn may lead to even more expensive executions than before.[2] Therefore, whether retail investors truly benefit from the business model change is an empirical question that requires a close examination of the total trading costs these traders pay.

This question spotlights the controversial payment for order flow model that the retail brokerage industry has embraced for the better part of two decades. Rather than directing marketable retail orders to public stock exchanges, brokers typically route them to wholesalers for off-exchange execution. In return, wholesalers pay brokers a fraction of a penny per share for these orders (i.e., "payment for order flow") and execute the trades typically at a price that is better than the National Best Bid or Offer (i.e., they offer "price improvement"). A key element of controversy is the fact that order flow payments and price improvement are fungible costs to the wholesaler. Thus, on the margin, order flow payments to brokers may offset price improvement for retail investors.

In this paper, we highlight effective spreads as retail traders' primary cost of submitting market orders in the zero-commission environment. Using a proxy for retail trades developed by Boehmer, Jones, Zhang, and Zhang (2021; henceforth BJZZ), we contrast the spreads these traders pay on marketable orders that are routed to off-exchange venues with benchmarks derived from exchange executions. In our initial

[1] TD Ameritrade Press Release on October 1st, 2019; Charles Schwab Press Release on October 1st, 2019, and E*TRADE Press Release on October 2nd, 2019 from businesswire.com; Vanguard Press Release from pressroom.vanguard.com on January 2nd, 2020; Fidelity Press Release from fidelity.com on October 10th, 2019.
[2] See "As behemoth brokerage firms go zero-commission on trades, advisors are concerned" Nov 6th 2019 by Andrew Osterland from CNBC.com and "Commission-free trades: A bad deal for investors" Oct 11th, 2019 by Steven Goldberg from Kiplinger.com.

analysis, we find that for all trade sizes and across market cap subsamples, retail trades have effective spreads that are roughly thirty to fifty percent smaller than those for comparable exchange trades during the two months prior to the zero-commission shift.

Our initial result is interesting in its own right. First, establishing a baseline comparison between retail and exchange orders outside the zero-commission environment offers insight for a payment for order flow debate that has long preceded the recent commission drop. Second, it creates much needed context for the economic magnitude of trading commissions. For example, our estimates indicate a retail trader would incur a half-spread of about $1.76 for a single 250-share trade in a $40 stock. Commissions prior to October 2019 were typically around $5 per trade. Thus, execution costs and commissions both represented non-trivial portions of total trading costs and the elimination of commissions meaningfully reduced trading costs in an all-else-equal framework.

We use a differences-in-differences framework for our main analysis. We compare the baseline retail versus exchange trade differences described above with the same differences for two periods that follow the adoption of zero commissions. The first period, which we label the "Post-Zero Period" is November and December 2019. The difference between retail and exchange spreads narrows slightly for small and medium sized trades and widens slightly for large trades when we compare the Base Period with the Post-Zero Period, but the salient result from this analysis is that retail trades still incur materially smaller spreads than comparable exchange trades in this period. Moreover, the relative increase in spread from the Base Period to the Zero Period is a very small fraction of the commissions that brokers formerly charged.

We also study the "Covid Period" of March and April 2020, in which U.S. cases rose sharply and a host of jurisdictions closed most face-to-face commercial activity. Also during this period, the VIX spiked, major stock indices bottomed out, and retail trading activity rapidly increased. Our results show the magnitudes of retail execution cost savings approximately doubled in the Covid Period compared to the Base Period. While the confluence of events and time lapse preclude us from directly attributing these changes to the commission drop, many believe zero commission trading played some role in the observed increase of retail activity.

Why are retail spreads uniformly lower than spreads for comparable exchange trades? A likely explanation is that (1) retail traders are less informed than other traders, (2) brokers' order routing mechanism correctly segment this uninformed flow, and (3) competition in the market for wholesale executions allows the traders themselves to reap some of the cost savings. We offer two sets of evidence to support this conclusion. First, we decompose effective spread into its price impact and realized spread components. The former measures information asymmetry and is larger for more informed order flow. The latter captures the cost of processing orders as well as rents that accrue to market makers. Our findings clearly show that retail trades have lower price impact than exchange trades and a drop in price impact accounts for the decline in effective spreads during the Covid period. Second, we show that at a daily level, retail order flow is more balanced (i.e., buys offset sells) during the Covid period than in the Base Period. Such balanced order flow is effectively uninformed from the perspective of a market maker desiring net zero positions at the end of the trading day.

While we have thus far emphasized mostly the sign of our results—effective spreads for retail trades are smaller than effective spreads for exchange trades, our reported magnitudes are important as well. We believe that differences in effective spreads between retail trades and appropriately matched benchmarks captures a key element of the cost savings for retail investors. In our final analysis, we therefore assess how the magnitudes of our spread-based estimates line up with price improvement metrics that are reported by market centers and publicized by brokers.

Per Regulation NMS, conventional measures of price improvement compare a trade's execution price to the NBBO that is in force at the time of execution. Price improvement relative to the NBBO may overstate the true economic cost savings of an order for a few reasons. First, the NBBO does not account for either hidden or odd-lot liquidity available on the exchanges within the quote. Of course, such liquidity is available to orders routed to the exchanges and its existence is one contributing factor to the well-known result that effective spreads are generally smaller than quoted spreads. Second, non-retail orders often utilize smart routers that hit the exchanges precisely when quotes are most narrow. Thus, even within granular time intervals, retail trades may execute when quoted spreads are wider, potentially inflating

NBBO-based price improvement. We compute NBBO-based price improvement for each trade and contrast these values with the spread-based results from our main analysis. The result is once again clear. NBBO-based price improvement measures overstate economic savings by a factor of at least three.

Our paper contributes to an ongoing regulatory discourse that has garnered media attention as well as Congressional scrutiny. Since in the absence of trading commissions, spreads represent the main component of retail trading costs, a large-scale study of such costs is warranted. We offer this analysis and provide evidence that market orders submitted by retail traders and routed to wholesalers achieve cheaper executions than exchange trades, both before and after the adoption of zero trading commissions. Moreover, we offer a straight-forward benchmark cost for these retail trades that enables us to estimate the economic magnitude of savings retail traders receive.

In the wake of the Congressional GameStop hearings that criticized PFOF, our results suggest that widespread calls to ban PFOF are premature. If anything, our results indicate that the move to zero commissions, which was facilitated by the PFOF model, proved beneficial to retail investors in terms of overall costs of trading. One caveat to this position is that investors may trade more, and Barber and Odean (2000) find that individual investors who actively trade underperform the market. However, their result emphasizes that underperformance is driven mostly by transactions costs (as opposed to poor security selection). Since we find the removal of commissions likely decreases overall trading costs, the welfare implications for retail traders remains somewhat ambiguous and requires future research.

Finally, our paper draws attention to a possible shortcoming in how execution quality is disclosed. Regulators should take heed to our finding that NBBO-based measures indicate economic benefits are upwards of three times what our benchmark analysis reveals. Disclosure policy could be altered in a number of simple ways to better capture the underlying economics. One improvement would be to require execution-based benchmarks rather than using the NBBO as the basis for computing price improvement. Another improvement could change the NBBO definition to include odd-lot quotes that often lie between the best bid and offer.

II. Institutional Background

II.a. The Zero Commission Cut

On October 1, 2019, Charles Schwab announced that it would cut commissions from $4.95 per trade to zero on all retail trades starting on October 7th. Within hours, TD Ameritrade followed by announcing it would cut commissions to zero from $6.95 beginning on October 3rd. Within the coming days, weeks, and months, other large brokers followed suit. For example, E*TRADE went to zero commissions on October 7th, Ally Invest went to zero commissions on October 9th, Fidelity went to zero commissions on October 10th, and Vanguard went to zero commissions on January 3rd.

Charles Schwab was not the first firm to offer commission-free trading. A week before Charles Schwab announced, Interactive Brokers launched a new service to retail clients called IBKR Lite which allowed investors to trade without paying commissions. Additionally, Robinhood, a popular trading app among young investors, pioneered commission-free investing when it was launched in April of 2013. While Charles Schwab was not the first, they do represent the largest retail broker to adopt such a measure[3] and largely upended one of the most common revenue sources that brokers receive from retail clients.

The announcements by Charles Schwab and other brokers were largely seen as a surprise to the market. After the announcement, Schwab's stock closed down about 10% as they noted that the cut would eliminate about $90 to $100 million in quarterly revenue. Schwab's competitors also took a big hit, with TD Ameritrade shares falling almost 26% and E-TRADE dropping about 16%. Stephen Bigger, director of financial institutions and research at Argus Research wrote that "while the timing and extent of the drop is surprising, we see Schwab's move as accelerating the inevitable."[4] Additional news and industry experts largely recognized this move as unexpected and the next big step in the price war among retail brokerages.

[3] Charles Schwab ranked second in broker-dealers in 2018 for assets under management with $1.85 trillion. Fidelity came in first with $6.85 trillion.

[4] "'Free' Trading has Arrived. Be sure to Read the Fine Print." - Daisy Maxey on October 4, 2019. Barrons.com

II.b. Market Makers, Broker-Dealers, and Payment for Order Flow

When a retail investor submits an order to a broker-dealer such as Charles Schwab or TD Ameritrade, the broker-dealer itself does not execute the order but often sells it to a third-party market maker in return for a rebate.[5] This payment for order flow is often a few cents per 100 shares. For example, E*TRADE received 20 cents per 100 shares for market orders it routed to Citadel Securities in January 2020. Broker-dealers often justify the process as market makers offer marginally better prices than current exchange quotes through a process called price improvement. With the cut in commissions, brokers' reliance on PFOF increased and we would expect brokers to route orders to venues with the highest payment for order flow (Cimon, 2021). Consistent with this prediction, Jain, Mishra, O'Donoghue, and Zhao (2021) find that when commissions were cut to zero, retail brokers routed proportionally more trades to venues that engage in PFOF.

Market makers who pay broker-dealers for their retail order flow take the opposite side of orders aiming to earn the spread. Two necessary conditions give market makers the ability to maintain a stable bid-ask spread, consistently and confidently price improve, and maintain profitability. First, purchased order flow cannot be correlated with common signals. In other words, it should not be informed. Second, shares purchased must roughly balance shares sold over the course of a trading day. If order flow is balanced, a market maker can easily and quickly fill orders while reducing carry risk.

Payment for order flow arrangements have long been a contentious subject. Central to the controversy is that each exchange and market maker sets its own rebates and fees related to execution, and in many cases, the fee structure also varies across brokers. As a result, brokers receive (pay) various rebates (fees) depending on how they route orders. Insofar as market centers also offer varying levels of execution quality whose cost is ultimately borne by traders, an agency problem arises between the retail brokerage firm and the individual investor. Each investor delegates the goal of obtaining best order execution to the

[5] Operating a trade desk is very costly, and because of this most large broker-dealers outsource this operation to third-party market makers. Almost none of the main retail discount brokers execute their own orders.

brokerage firm, which may stand in conflict with the broker's incentive to maximizing profits (Angel, Harris, and Spatt, 2011).

In the wake of the extreme volatility and trading halt experienced with GameStop stock in early 2021, the House Financial Services Committee held hearings and scrutinized the PFOF model. For example, Dennis Kelleher, CEO of Better Markets, emphasized the conflicts of interest between a broker's duty to seek best execution and their duty to maximize profits for shareholders that we discuss above.[6] Many committee members called on regulators to consider banning the practice altogether. More recently, SEC Chairman Gary Gensler has stated publicly that the possibility of banning payment for order flow is "on the table".[7] In response, industry representatives such as Virtu Financial CEO Doug Cifu and Citadel Securities founder Ken Griffin argue that PFOF allows for better execution in the form of price improvement and the introduction of free trading, which encourages investor participation in the market.[8]

Empirically, Battalio, Corwin, and Jennings (2016) find strong evidence brokers respond to variation in fees and rebates as they route orders, and these activities may harm retail investors who submit limit orders.[9] However, Battalio, Jennings, and Selway (2001) suggest that trading costs for brokers who engage in PFOF are not dominated by those of brokers who don't engage in the practice. As noted above, Jain, Mishra, O'Donoghue, and Zhao (2021) find that retail brokers routed more trades to PFOF venues following the shift to zero commissions. And Cimon (2021) offers a theoretical framework for thinking about these various incentives. Our paper adds to this literature by providing a large-scale comparison of execution costs of marketable retail orders with benchmark trades that occur on public stock exchanges.

Research that considers how PFOF arrangements affect overall market quality offers mixed messages as well. Easley, Kiefter, and O'Hara (1996) argue this arrangement may induce cream skimming,

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[6] "Robinhood business model under fire at GameStop hearing in Congress" by Chris Matthews, MarketWatch,March 17, 2021.
[7] "SEC Chairman Says Banning Payment for Order Flow is 'On the Table' by Avi Salzman, Barron's, August 30, 2021.
[8] "Wall Street Pushes Back as SEC Targets Business Practice That Generates Billions" by Paul Kiernan, Wall Street Journal, November 8, 2021.
[9] Battalio, Shkilko, and Van Ness (2016) find that routing US *options* to venues with PFOF is consistent with a broker's fiduciary responsibility to obtain best execution.

which occurs when uninformed orders are selectively executed off-exchange leaving informed orders to execute on exchange and hurt overall market quality because all prices are derived from exchange quotes. Battalio (1997) offers contradictory evidence. He finds that trading costs do not increase with the presence of third-party market makers and argues these venues are effectively cost competitors rather than cream skimmers. Parlour and Rajan (2003) present a theoretical model in which payment for order flow results in higher spreads and serves as a wealth transfer between market orders and limit orders. More recently, Comerton-Forde, Malinova, and Park (2018) study a change in the Canadian markets that effectively eliminated the off-exchange intermediation of retail trading and forced these orders to the exchanges. They find that lit liquidity improved, an outcome they argue to be beneficial to all traders. Finally, Garriot and Walton (2018) study the effects of the NYSE Retail Liquidity Program and find that allowing retail price improvement on the exchange improved effective spreads and reduced price impact.

Our results also contribute to the literature on retail order execution quality and order routing. To our knowledge, we are the first to directly compare retail execution quality against comparable exchange trades within a PFOF structure in a zero-commission environment. This result builds on the literature debating whether payment for order flow is harmful to retail investors. Battalio, Corwin, and Jennings (2016) find that payment for order flow leads to subpar limit order execution quality. Results also depend on the measure of order execution quality, as Battalio, Hatch, and Jennings (2003) show that the NYSE offers better prices, but Trimark, a payment for order flow venue, has better execution prices. Our results show that any negative effects of payment for order flow do not outweigh the benefit from better incentive alignments between brokers and market makers.

III. Data

III.a. Initial Sample and Retail Trading

We study execution costs around several brokers' shifts to zero commission retail trades in October 2019. We establish baseline comparisons in August and September 2019 and henceforth refer to this window as the "Base Period" of our analysis. We then consider two subsequent periods that follow the

zero-commission shift. We define the "Post-Zero Period" to be November and December 2019. Straddling the zero-commission shift with the Base and Post-Zero Periods helps us isolate any effects of the change itself. Ending the Post-Zero Period in December 2019 preserves a symmetric sample around the business model changes and mitigates potential confounding effects related to the Covid-19 pandemic, which hit the US in early 2020. Our second subsequent period is March and April 2020, which we label the "Covid Period". We study this period because the onset of the pandemic in the U.S. was coupled with a rapid rise in retail trading leading some to argue that zero-commission trading contributed in part to this trend.[10]

We identify all U.S. common stocks with market capitalization (*MktCap*) and share price (*Price*) available from CRSP in December 2018. This requirement effectively eliminates from the analysis new listings, whose trading and ownership characteristics may differ from other stocks due to lockup restrictions. We also drop stocks with December 2018 price below $5 or above $1,000. These filters mitigate concerns associated with highly illiquid stocks or stocks for which the minimum tick size of one penny materially alters spread spreads. Finally, we require an average of five retail trades per day according to BJZZ measure (described in detail below) during July 2019. The resulting sample contains 2,420 stocks.

We compute retail trading proxies following BJZZ. Using TAQ data, they label retail trades as executions occurring on Exchange Code "D" and having prices within $0.004 of a whole penny. This measure attempts to capture order flow that market-makers purchase from retail brokers and execute on their own platforms. These wholesale execution venues report the trades to the Trade Report Facilities (Exchange Code = "D") rather than the exchanges, and they offer nominal price improvement relative the closest whole penny, typically in fractions of a penny per share. Since quotes are constrained to whole pennies, these executions occur within the aforementioned price points. Moreover, institutions unlikely receive prices of this nature, but often do trade at quote midpoints. For this reason, BJZZ do not consider transaction prices near $0.005. BJZZ define executions priced between $0.0001 and $0.0039 *below* a whole

[10] "How Robinhood and Covid opened the floodgates for 13 million amateur stock traders" by Sam Rega, CNBC, October 7, 2020.
"Coronavirus turmoil, free trades draw newbies into the stock market" by Alexander Osipovich and Caitlin McCabe,Wall Street Journal,April 29,2020.

penny as retail "buys" and those priced between $0.0001 and $0.0039 *above* a whole penny as retail "sells". We adopt these same conventions.

The retail trading measures are well-suited for our study. BJZZ build their metrics around the payment for order flow model. Trades occurring under such conditions are exactly the set of retail trades we wish to examine. Nevertheless, some caveats remain. First, the metrics only represent market (or marketable) orders. Second, they do not include any retail trades that occur on the exchanges, whether they are directed there by the receiving broker or the retail clients themselves. We suspect those directed orders to be a small minority of all retail trades.[11] Third, they ignore trades that receive price improvement in whole-penny increments as well as those receiving no price improvement at all. Most marketable shares submitted to market makers receive price improvement relative to the NBBO at the time of execution. For example, for market orders submitted to Citadel, G1X, and Virtu Securities from April 2019 to June 2020, the average stock in our sample had 91.9% of their shares price improved per period for trades between 100-499 shares (Appendix II).[12] BJZZ offer a detailed discussion of caveats such as these as well as an empirical analysis that validates their proxies. Henceforth, we often refer to trades captured by the BHZZ measure as "retail trades" for brevity with all caveats in mind.

We present summary statistics from in Table I. Panel A contains results for the filtering variables described above. The interquartile ranges for December 2018 market capitalization and price are $5 Billion to $47 Billion and $14.95 to $56.91, respectively, which indicates the bulk of our sample of 2,420 firms lie within traditional mid-cap and large-cap classifications, and is not dominated by low-priced stocks. Panel B presents summary statistics for trading variables in the Base Period. Importantly, retail investors play a non-trivial role in these firms' trading. For the median stock, retail investors account for 4.9% of share volume and 3.4% of trades. And for some stocks, retail investors are far more influential. The 90th percentile values are 15.6% of share volume and 10.8% of trades.

[11] According to the 606 filing for Q4 2020, Charles Schwab directed less than 1% of all marketable orders to exchanges. Similar statistics are found for other retail brokers and remain relatively constant over time.
[12] We consider the extent to which unobserved trades that execute at the NBBO affect our inferences in Section V below.

III.b. Execution Costs

Our main questions center around retail investors' execution costs in the current environment that is driven by payment for order flow arrangements. We therefore compute for each execution the percentage effective spread:

$$ES\% = \frac{2BuySell(p_t - m_t)}{m_t} \tag{1}$$

which is twice the signed difference between the transaction price (p_t) and the prevailing quote midpoint (m_t) at the time of the trade t, all scaled by the quote midpoint. For retail trades, the *BuySell* indicator variable equals +1 (-1) for buyer-initiated (seller-initiated) trades signed according to the BJZZ procedure. We sign all other trades using the Lee and Ready (1991) algorithm.[13] When aggregating across trades, we always compute share-weighted averages. We present summary statistics for percentage effective spread (*ES*%) and its unscaled counterpart, dollar effective spread (*ES*$), in Table I Panel C. These statistics confirm that our analysis focuses mostly on liquid stocks. The median effective spread is 0.08% of the quote midpoint and more than 95% of all stocks have spreads below one percent. These measures represent all trades, so in that sense, they are stock-level execution cost measures.

We also compute quoted spread at the time of each execution (*QS%*) as the difference between the National Best Offer and National Best Bid scaled by the quote midpoint. When aggregating across executions, we compute quoted spread at the time of each execution and then use shares traded as weights. This procedure differs from the common practice of computing quoted spread over a period of time by weighting intraday observations by time in force. We use shares traded as weights here because our subsequent analysis highlights the concept of "price improvement" which is generally defined as the difference between an execution price and the best quote at the time of execution. Doing so makes the magnitudes of effective spread and quoted spread variables directly comparable to one another.

[13] Following Holden and Jacobsen's (2014) data filters and computational procedures, we require "normal" quote conditions (A, B, H, O, R, W), and we drop quotes that are cancelled or withdrawn, ask and bid =0 or missing, markets are locked or crossed markets, or bid-ask spread >$5. We delete any abnormal trades. If the NBBO has two quotes in same millisecond, we use the one that is last in sequence.

We observe in the summary stats that effective spread is smaller than quoted spread at the mean and at each percentile point. Thus, on average, traders seem to achieve some amount of price improvement according to conventional definitions. For example, the median quoted spread in Panel B is 0.15% while the median *effective* spread is 0.09%, or approximately 40 percent smaller. Since executions on average appear to receive some price improvement relative to prevailing quotes, conventional measures of price improvement that compare trade prices to the NBBO may not appropriately measure any true economic savings that retail investors receive. We visit this point in much detail in Section V below.[14]

As is standard in the literature, we decompose effective spread into the realized spread (*RS*) and price impact (*PI*) components. We calculate percentage realized spread as

$$RS\% = \frac{2BuySell\ (p_t - m_{t+k})}{m_t}, \tag{2}$$

The difference between the transaction and some future quote midpoint m_{t+k} represents the component of the spread that reverses and is a proxy for compensation for market making. We compute price impact as

$$PI\% = \frac{2BuySell\ (m_{t+k} - m_t)}{m_t}, \tag{3}$$

Because it measures the permanent price change associated with a trade, price impact also captures a dimension of liquidity related to compensating market makers for adverse selection. In the analysis presented below we use a *k* of 5 minutes. Like *ES*, we compute dollar measures for realized spread and price impact along with the percentage metrics defined above.

III.c. Developing Appropriate Controls

Any assessment of retail traders' execution costs requires a benchmark for comparison. Ideally, we would compare the execution costs retail traders incur in the current environment with a counterfactual cost they would pay if, say, payment for order flow arrangements did not exist and their trades were

[14] We note that our effective and quoted spreads, as constructed, represent "round-trip" estimates. Conventional price improvement metrics (discussed below) are one-sided. Thus, in subsequent analysis when we reconcile effective and quoted spread estimates with price improvement, we divide our spread measures by two and present results as "half-spreads".

exclusively routed to the public stock exchanges. Of course, such counterfactual is not observable for at least two reasons. First, a hypothetical re-routing of all retail flow might alter the proportion of informed and uninformed traders on exchanges and affect lit market liquidity. Second, non-retail (human or algorithmic) traders could respond to the changing information environment and alter their own order-submission strategies.

With these caveats in mind, we compare retail executions (off exchange) to similar-size executions that occur on exchanges for the same stock at approximately the same time. We include trades from all public stock exchanges in the control sample, and we refer to these trades simply as "exchange trades". We control for trade size by separately analyzing trades in three size ranges based on odd lots (less than 100 shares) and the two smallest breakpoints used in Rule 605 reporting. Thus, we analyze separately (1) "small" trades of 1-99 shares; (2) "medium" trades of 100-499 shares; and (3) "large" trades of 500-1999 shares. While retail trades of 2,000 shares or more may occur, these observations are somewhat rare and likely represent trades of an atypical nature. We highlight within-stock comparisons by including stock fixed effects in all our analyses. Thus, our analysis emphasizes, for example, effective spread differences for retail and exchange executions for "medium" size trades in a given stock.

A crude attempt to control for the time of trade execution is by simply aggregating trades up to the stock-day and including day fixed effects in the models. However, spreads tend to vary *within* the day (e.g., McInish and Wood, 1992), and since retail and non-retail orders may arrive at different intraday rates, an aggregation up to the day level may be too coarse. In Figure 1, we illustrate this intraday variation. We divide the trading day into 15-minute intervals and present the fraction of retail and exchange trades occurring in each interval as vertical bars. The dotted line in the figure represents percentage effective spreads from trades within each interval. Three observations stand out. First, spreads tend to fall throughout the day. Second, the quantities of both retail and exchange trades are elevated near the beginning and ending of trading. Third, the intraday volume patterns are more striking for exchange trades, especially near the close.

To better capture intraday variation in retail and exchange order flow along with spreads, we perform our main analysis using observations aggregated to the 15-minute level, and we include date x intraday interval fixed effects. We also drop the first and last 15-minute interval of each trading day. Trades occurring at these times may be affected by opening and closing procedures. Moreover, dropping these intervals eliminates concerns over the calculation of a prevailing NBBO near the opening bell and a post-trade NBBO (for measures such as realized spread or price impact) near the close of trading.

IV. Retail vs Exchange trades

IV.a. Baseline Comparisons

We commence with a baseline comparison of retail and exchange trades during the two months prior to the zero-commission shift. This initial analysis is interesting in its own right. Long before the adoption of zero commission business models, payment for order flow was commonplace. And as such, practitioners and policy makers have scrutinized the resulting execution costs retail traders pay. Like any other execution venue, wholesalers must issue Rule 605 reports that summarize various cost metrics. Similarly, brokers disclose details about where they route orders and the payments the receive for them on Rule 606 reports. However, to our knowledge, the literature offers no large-scale analysis of the actual execution costs retail traders pay for orders routed to third parties in the U.S. markets and how those costs compare with exchange benchmarks. Our analysis, while it does not consider certain retail orders – most notably limit orders – focuses squarely on the type of retail trades that are of central concern.

We estimate the following fixed effect regression using observations from the Base Period of August-September 2019:

$$Y_{it} = \beta_1 Retail_{it} + \gamma_i + \delta_t + \varepsilon_{it} \tag{4}$$

where the variable Y_{it} is a share-weighted execution cost metric for trades in stock i during intraday period t. We winsorize observations at the 1% and 99% level by day, intraday period, and trade size. We emphasize the t-subscript indexes a date x time interval—for example, the interval from 9:45 AM to 10:00 AM on August 12, 2019. Within each stock-date-time, we include one observation representing retail trading

according to the BJZZ procedure and another representing exchange trading. The indicator variable *Retail* equals unity for retail trading observations. We also include stock fixed effects (γ_i) and day x intraday period fixed effects (δ_t).

How retail execution costs compare to the values we estimate for exchange trades is not a foregone conclusion. On the one hand, payment for order flow proponents have long argued that brokers' selling of order flow to wholesalers results in superior execution costs for their retail clients. Both competition in the market-making sector and the segmentation of less informed order flow may reduce the costs retail traders pay (Battalio, 1997).[15] But on the other hand, diverting these orders from stock exchanges prevents retail traders from accessing any undisplayed liquidity inside the quote that may be resting on many exchanges. Moreover, industry advocates argue that payment for order flow arrangements create a conflict of interest between a broker-dealer's obligation to seek "best execution" and its duty to maximize profits for shareholders. They suggest that a recent SEC enforcement action against Robinhood for preferentially routing orders to the detriment of execution quality as indication that this conflict of interest does harm to investors.[16] And more broadly, retail clients whose orders are typically routed to wholesalers rarely if ever benefit from increasingly popular smart routers that monitor quote changes and execute trades on exchanges when conditions are most favorable.

The coefficient estimates for the *Retail* indicator offer insight. For all order sizes, this coefficient is negative and statistically significant. Thus, for a given stock and controlling for the day and time of execution, we associate off-exchange retail trades with cheaper executions than similar-sized exchange trades. And of particular interest for regulatory discussions, the cost savings is economically meaningful. For small sized orders, the *Retail* coefficient is a statistically significant -0.039, indicating small retail trades receive executions that are about forty percent cheaper than similar exchange trades. For medium sized

[15] Note cost competition and cream skimming may have different effects on overall market quality (Easley et al.,1996; Ballalio, 1997).
[16] Better Markets fact sheet. "Payment for order flow: How Wall Street costs Main Street investors billions of dollars through kickbacks and preferential routing of customer orders.", February 16, 2021.

orders, the cost savings is even larger; the *Retail* coefficient of -0.049 reflects a 52% reduction from the baseline coefficient of 0.095.

We expand the analysis and examine small, medium, and large cap stocks for robustness. We set our breakpoints for small, medium, and large cap stocks at under $2 billion, $2-$10 billion, and above $10 billion, respectively, and we present the results in Table III. Across all subsamples, the *Retail* coefficient for effective spread is negative and statistically significant. Within each order size block, we present results separately for small, medium, and large cap stocks. Unsurprisingly, within each order size block, execution costs are typically larger for smaller stocks. For example, consider odd-lot orders. Average coefficients for exchange trades range from 0.184% for small cap stocks to 0.028% for large cap stocks. Retail trades exhibit similar patterns ranging from 0.120% (0.184% - 0.064%) for small cap stocks to 0.013% for large cap stocks.

The coefficient magnitudes in Table II are also useful because they offer context for the economic importance of brokerage commissions. Estimates for medium-sized orders indicate a retail trader pays a round-trip effective spread of (0.095 – 0.49 =) 0.047%. Therefore, this trader would incur an execution cost of about $1.76 for a single 250-share trade in a $30 stock. Prior to October 2019, brokers typically charged retail trades commissions of less than five dollars per trade, so for this hypothetical order, execution costs would account for more than one-fourth of the trader's total cost. Thus, while eliminating commissions would reduce trading costs, holding all else, zero commissions does not equate to free trading.

So why might retail traders receive the cheaper executions suggested by these results? One popular view is that retail investors tend to be less informed than their institutional counterparts. If the routing mechanisms their brokers use result in successful segmentation of such less-informed order flow, then wholesale market makers who execute trades take on less adverse selection risk than those who trade against exchange flow. We test this conjecture by decomposing the effective spread into its price impact and realized spread components. The price impact component of the spread, that is the "permanent" change in price due to a trade, compensates the market maker for adverse selection risk. All else equal, less informed

order flow should translate to a lower price impact. The realized spread component covers the cost of market making and contains any residual profit to the market.

We estimate Equation (4) using each of the two spread components as the dependent variable. We report results for *PI%* in Table II Panel B and those for *RS%* in Panel C. The *Retail* coefficient corresponding to price impact in Panel B is uniformly negative and statistically significant across order sizes. In all cases, the negative *Retail* coefficient is more than half the magnitude of the intercept, indicating price impact for retail trades, while still positive, is less than half that for exchange trades. This economically and statistically strong result is consistent with these retail trades being less informative than the benchmark exchange trades. Turning to the realized spread results in Panel C, the Retail coefficients are significantly positive and economically meaningful. For small trades, the realized spread is (0.022 + 0.024 =) 0.046%. The realized spread for medium-sized retail trades is (0.000 + 0.021 =) 0.021%. These values account for non-trivial components of the corresponding effective spreads of (0.099 – 0.039 =) 0.060% and (0.095 – 0.049 =) 0.046%. The larger realized spread indicates that market makers enjoy higher trading profits than exchanges despite executing retail trades at a lower effective spread. Coupled with a lower price impact, these results suggest that market maker profits are partly driven by the uninformed nature of retail trades relative to benchmark exchange trades.

Our analysis so far warrants a point of clarification. The results speak to whether retail traders whose orders execute off exchange receive better or worse execution on average than comparable exchange trades. As such, our results inform one specific policy discussion of whether current practices, such as payment for order flow arrangements, disadvantage retail traders. The evidence in Table II suggests they do not. However, our analysis does not speak to the broader question of how routing retail order flow to wholesalers affects market quality overall. For example, the removal of uninformed trades from exchanges could widen spreads quoted there and harm liquidity for all traders. Battalio (1997) finds that this isn't the case as overall bid-ask spreads do not increase when market maker selectively purchase and execute orders. Furthermore, Jain et al. (2021) find that the elimination of commissions improved overall market quality in those stocks preferred by retail investors.

We also show the results of *PI%* and *RS%* for small, medium, and large cap stocks in Table III Panels B and C, respectively. In Panel B, the coefficient for price impact is negative and significant for retail trades across all order sizes and market caps. Unsurprisingly, larger order sizes have a larger price impact. For example, within small cap stocks, exchange price impact ranges from 0.135% for odd-lot trades to 0.225% for large trades. In Panel C, realized spreads are positive and significant for retail trades across all columns. Taken together, the results in Tables II and III presents a consistent message. Retail trades tend to have less price impact than exchange trades and therefore execute at lower costs relative to the exchange, allowing market makers to capture a larger portion of profit in the form of realized spreads.

IV.b. The Zero Commission Shift

The rapid adoption of zero commission trading models spotlight payments for order flow as a primary revenue source for many retail brokerage firms and motivate a reexamination of retail execution costs in the new regime.[17] Since retail brokers have discretion in order routing, they may preferentially direct flow to venues that offer the highest payments so they can partially recoup lost commission revenue. Consistent with this, Jain et al. (2021) finds that retail brokers who eliminated commissions changed their order routing behaviors, shifting trading volume from exchanges to market makers that engage in PFOF. Moreover, they may also attempt to renegotiate existing payment for order flow contracts to receive higher payments per order. In either case, retail traders would ultimately bear the cost of order flow payments to the extent that the wholesale sector treats payments to brokers and execution cost savings for retail traders as fungible expenses. In essence, wholesalers could offset order flow payments by providing worse executions (i.e., capturing greater spreads).

We do not observe order flow payments with the necessary precision or granularity to examine the tradeoff between order flow payments and execution costs in detail. However, we can analyze changes in

[17] Retail brokers receive revenue in a variety of ways. For example, in addition to PFOF, Robinhood receives revenue through interest on uninvested cash, margin lending fees, upgraded service/advisory fees, and hypothecation. *See* https://robinhood.com/us/en/about-us/how-we-make-money/ for more information.

overall retail execution costs around the shift to zero commissions. If order flow payments increase and the increase is passed along to traders via worse executions, then we expect retail traders to pay higher spreads in the new regime. We therefore extend our sample to include two additional two-month time periods that follow the zero-commission shift and analyze execution costs accordingly. The first period is November and December 2019, which we label the "Post-Zero Period". One key advantage of studying this period is that (after leaving out October 2019 as a transition month) all analysis is "close-in" to the structural change. In addition, inferences are unlikely to be contaminated by Covid-19 pandemic which did not hit the U.S. markets until the Spring of 2020. As such, we can associate any changes to execution costs with the shift to zero trading itself. The main disadvantage to this approach is that brokers and wholesale market makers may take a bit of time to fully adjust their contracting and technology to the new regime. Some of this delay may also be attributable to internal assessments of how retail investor behavior changes.

Of course, a confluence of effects during the heart of the Covid pandemic in early 2020 beg for additional study. U.S. cases rose sharply in March and prompted a series of city- and state-level closures. The VIX spiked on March 20, and major stock indices bottomed out around April 1. All the while, retail trading activity rapidly increased. To illustrate, we plot in Figure 2 the daily evolution of mean and median retail share turnover for our sample stocks. While retail trading exhibits some day-to-day volatility in both the Base Period and the Post-Zero Period, its level does not markedly rise until the Covid Period. Many commentators attribute this trend to a confluence of large swaths of the population remaining homebound, the payment of government stimulus checks, and the appearance of free trading in the zero-commission regime.[18] We therefore extend our sample to include a second additional two-month period of March and April 2020, which we label the "Covid Period".

Pooling the three two-month periods as an expanded sample, we estimate the following differences-in-differences regression:

[18] "Pandemic retail trading boom remakes brokerage landscape",S&P Global Market Intelligence, April 14, 2021; "Coronavirus turmoil, free trades draw newbies into stock market",Wall Street Journal, April 29, 2020; "46% of stimulus checks were invested in the stock market?", Forbes, June 27, 2021.

$$Y_{it} = Retail_{it} + Retail_{it}\ x\ Post + Retail_{it}\ x\ Covid + \gamma_i + \delta_t + \varepsilon_{it} \quad (5)$$

The structure is similar to that in the prior section with variables as defined above. The *Post* indicator equals one during the Post-Zero Period and zero otherwise; similarly, the *Covid* indicator equals one during the Covid Period and zero otherwise. in the Pre-Zero Period.[19] Focusing first on the Post-Zero Period for the "close-in" analysis, our primary interest is the coefficient estimate for the *Retail* x *Post* interaction. This coefficient captures how the difference between retail and exchange execution costs change after the cuts to zero commissions.

Table IV contains the results from our estimation of Equation (5). As noted earlier, comparing the Base Period to the Post-Zero Period sample offers cleanest interpretation for the effect of zero commission trading on execution costs due to the periods' close-in proximity to several brokers eliminating commissions. We therefore focus our attention first and foremost on the *Retail* x *Post* interaction coefficient. The *Retail* coefficient represents the difference in effective spreads for retail trades and comparable exchange trades during the Base Period, while the sum of the *Retail* and *Retail* x *Post* coefficients indicate this same difference during the Post-Zero Period. Thus, the interaction coefficient is a difference-in-difference estimator for retail and exchange spreads around the zero-commission event.

In Panel A, for both small and medium order sizes, the *Retail* * *Post* interaction coefficient is positive and statistically significant. Thus, execution costs for retail traders as measured by effective spread increase around the adoption of zero commission trading. The large order results indicate the opposite, as the interaction coefficient in the third column is significantly positive. However, highlighting only the sign and significance of these coefficients undermines the bigger picture our results convey. Particularly, the *Retail* * *Post* interactions are all an order of magnitude smaller than the corresponding *Retail* coefficients. Thus, while differences between retail and exchange spreads change subtly (and even significantly) around the zero-commission event, effective spreads for retail trades are smaller than those for exchange trades in both periods. This salient result holds for small, medium, and large orders alike.

[19] Our incorporation of day-time fixed effects prevents the inclusion of stand-alone *Post* and *Covid* dummy variables.

We next turn to the Covid period. The coefficient for the *Retail * Covid* interaction is the differences-in-differences estimator that reveals how the retail vs. exchange difference in execution costs change from the Base Period to the Covid Period. The interaction coefficient estimates shown in the table are uniformly negative and statistically significantly, which indicates retail executions were relatively less expensive during the rise of Covid in the U.S. More importantly, and unlike the estimates for the *Retail * Post* interaction coefficients discussed above, the magnitudes are economically sizable. The coefficient estimates of -0.047, -0.048, and -0.019 for the small, medium, and large trades, respectively are the same sign and roughly the same magnitude as the *Retail* coefficients. Thus, the cost advantage for retail trades compared to exchange trades approximately doubles when moving from the Base period to the Covid period. For example, the effective spread difference for medium-size trades moves from 0.049% to 0.097%.

At first blush, the large drop in effective spreads is curious. Given the lack of commission revenue and broker's commensurate incentives to reach for order flow payments, one might anticipate the opposite effect. An explanation for the spread decrease could arise from a similar economic story that we argue drives the difference between retail and exchange costs in the Base Period—payment for order flow models segment less informed retail trades, and there is sufficient competition for execution services to pass along some cost savings to the traders themselves.

Of course, whether retail trades in fact became even less informative during the Covid period is an empirical question. On the one hand, the overall environment of this period, which includes more people at home with time to trade stocks and lower explicit trading costs afforded by zero commissions, might have attracted more unsophisticated traders. On the other hand, the commission drop may also have removed barriers to informed (retail) traders desiring to gather and trade on information (e.g. Grossman and Stiglitz, 1980). If the latter dominates the former, the fraction of retail traders who are informed would actually increase.

We offer two tests to shed light on the potential changing nature of retail order flow. The first exploits the same familiar price impact + realized spread decomposition that we use in Table II. We re-estimate Equation (5) using price impact (*PI*%) as the dependent variable and report the results in Table IV

Panel B. Most relevant for the current discussion are the estimates for the *Retail* x *Covid* interaction in Panel B. Each coefficient is significantly negative and slightly larger in magnitude than the corresponding negative coefficient estimate for *Retail*. Thus, price impact falls substantially for retail trades during the Covid Period. The realized spread (*RS*%) results in Panel C paint a similar picture. There, the coefficient estimates for the *Retail* x *Covid* interaction are positive and statistically positive. Each of these main results from Table IV—the reduction in effective spread, the reduction in price impact, and the increase in realized spread—is consistent with retail trades being less informative during the Covid Period than they are during the Base Period.

Our second test is more novel. A market maker who wants to end the day with net zero position in any given stock seeks balanced order flow such that shares bought roughly offset shares sold over the course of the day. Thus, the concept of informed flow refers more to an imbalance in one direction or the other rather than fundamental information per se. With this in mind, we compute for each stock-day the absolute retail order imbalance as

$$Abs\_Imb_{it} = \frac{|retail\ buy\ shares_{it} - retail\ sell\ shares_{it}|}{retail\ buy\ shares_{it} + retail\ sell\ shares_{it}} \tag{6}$$

We use the absolute imbalance because we are more concerned with an imbalance in either direction than its sign.

If retail order flow becomes less informed during the Covid period, we expect absolute imbalance to fall during that time. This is exactly what we find. To illustrate, we first calculate the average *Abs_Imb* across stocks each day in our sample period, and then we smooth the series by taking the 5-day moving average. We plot the resulting time series in Figure 3. The vertical red bars indicate October 2019 (the end of the Base Period) and March 2020 (the beginning of the Covid Period). We observe from the figure that the level of *Abs_Imb* remains roughly constant at just above 0.25 throughout the Base and Zero Periods. However, consistent with the spread results in Table IV, it falls substantially during the Covid period.

V. Understanding Retail Cost Savings

V.a. NBBO-Based Price Improvement vs. Effective (Half) Spread Differentials

Our results thus far suggest retail orders routed to payment for order flow venues receive cheaper executions than exchange-based benchmarks that control for stock, day, time-of-day, and trade size. The coefficient estimates in Table IV indicate retail trades execute at spreads roughly twenty to thirty-five percent lower than comparable exchange trades. These findings qualitatively support claims of both wholesalers and brokers that payment for order flow arrangements benefit retail traders. Such industry statements often rely on price improvement statistics that Regulation NMS requires market centers to disclose on monthly Rule 605 Reports.[20] Broker, in turn, pass along similar information to their clients, often emphasizing the dollar magnitude of such price improvement. For example, as shown in Appendix I, Schwab reported on their website that, for Q3 2021, the average investor saved $5.52 for non-odd lot orders under 500 shares. Similarly, they report the percentage of shares price improved and executing at NBBO or better.[21]

Importantly, the standard reporting per Regulation NMS defines price improvement relative to the NBBO at the time of execution. Thus, the National Best Offer serves as the benchmark price for buy trades, and the National Best Bid serves as the benchmark price for sell trades. However, our results from Table I showing that average effective spreads are lower than quoted spreads (also derived from NBBO) suggest regulatory-based price improvement statistics may overstate any economic savings retail traders receive. And even if the direction of savings conveyed by reported price improvement statistics is correct, their magnitudes remain of high order importance.

[20] On June 9, 2005, the SEC adopted Regulation NMS. Regulation NMS renumbered some prior SEC rules such as the SEC Rule 11Ac11-5 (Dash 5 Report) which was adopted in November 2000. The Dash 5 Report was updated to the Rule 605 report and requires FINRA firms to disclose order execution information in a uniform manner. *See* 17 CFR § 242.605 for more details.

[21] Retail Execution Quality Statistics are reported on Schwab.com and was accessed on November 12, 2021. Their reporting references the Rule 605 Reports for S&P 500 stocks for Q3 2021. Note that Order Size Range of 1-99 is not currently included in the Rule 605 Reports available for public download.

The SEC's recent settlement with Robinhood exemplifies the relevance of exactly how much savings retail traders achieve on their trades. According to the settlement, "at least one principal trading firm communicated to Robinhood that large retail broker-dealers that receive payment for order flow typically receive four times as much price improvement for customers than they do payment for order flow for themselves—an 80/20 split of the value between price improvement and payment for order flow…Robinhood negotiated a payment for order flow rate that was substantially higher than the rate the principal trading firms paid to other retail broker-dealers—which resulted in approximately a 20/80 split of the value between price improvement and payment for order flow. Robinhood explicitly offered to accept less price improvement for its customers than what the principal trading firms were offering, in exchange for receiving a higher rate of payment for order flow for itself."[22]

The extent to which the magnitudes of NBBO-based price improvement for retail trades differ from the effective spread differences we report in Table II is an important empirical question that sheds light on current regulatory disclosure policy's efficacy in communicating economic savings for retail traders. We therefore compute each trade's (regulatory) price improvement as

$$Improve = \begin{cases} Offer - Price, if\ BuySell = 1 \\ Price - Bid, if\ BuySell = -1 \end{cases} \tag{7}$$

$$Improve\ \% = \frac{Improve}{Midpoint} \tag{8}$$

Aggregating trades up to 15-minute bins separately for small, medium, and large trade sizes as before, we estimate Equation (5) using *Improve*% as the dependent variable.

We present the price improvement results in Table VI. We first note the intercepts are positive and statistically significant. Thus, the benchmark exchange trades within each order size receive price improvement relative to the NBBO. For small, medium, and large exchange trades, these price improvements (as percentages of the quote midpoint) are 0.041%, 0.036%, and 0.021%, respectively. For

[22] *See* SEC, In Re Robinhood Financial, LLC, Order Instituting Administrative and Cease and Desist Proceedings (December 17, 2020).

perspective, these magnitudes are roughly one-fifth to one-fourth the size of the effective spreads on exchange trades that we report in Table II above. This finding is also consistent with the generally smaller effective spreads than quoted spreads that we report in Table I.

Turning to retail trades, we see the coefficients for the *Retail* indicator are positive and statistically significant as well. These coefficients are about the same magnitude as the intercepts, indicating that retail trades in the Zero Period receive about twice the price improvement as comparable exchange trades. Summing the intercepts and the *Retail* coefficients, we see that retail price improvement, again relative to the NBBO, for small, medium, and large trades is 0.070%, 0.072%, and .043% of the midpoint, respectively. For a 250-share trade in a $30 stock, the dollar price improvement would be $5.40. This magnitude is very close to Schwab's representative cost savings we highlight in Appendix I.

We next compare the magnitudes of NBBO-based price improvement with our effective spread results. To this end, we insert use various coefficient estimates from Table VI to compute retail price improvement for each order size and in each of the three periods. We display these magnitudes in the black bars in Figure 4. We then perform similar computations using the coefficient estimates from Table IV to represent retail effective spread savings. As argued throughout this paper, we believe these latter estimates better reflect the true economic savings for retail traders because they benchmark retail execution costs with similarly-calculated costs for trades that execute on exchanges. For this figure, we divide the effective spread differentials by two (i.e., express results in terms of "half spreads") so that our numbers are comparable with the price improvement statistics.

We display the effective (half) spread differentials in the dark gray bars and liken the effective spread savings to the price improvement metrics. The message is visually clear. NBBO-based price improvement overstates the cost savings for retail investors by at least a factor of three. For example, keeping with the 250-share trade in a $30 stock referenced above, the effective (half) spread savings for a retail trader is only $1.88 as opposed to the NBBO-based price improvement of $5.40. We also present the price improvement versus effective spread savings differentials for the Zero Commission and Covid Periods. While the magnitudes vary somewhat, the central tenor remains. NBBO-based price improvement

metrics suggest savings for retail traders that are far greater than the values indicated by effective spread comparisons. This is particularly important as most brokers report execution quality in terms of price improvement relative to the NBBO, required by the SEC 605 reports.[23] Figure 4 suggests that this measure of execution quality possibly overstates the level of savings for retail traders, which may lead retail traders to believe they are receiving better execution than they do.

V.b. Quoted Spread Comparisons

If quoted spreads at the time of execution are roughly the same for retail and exchange trades, an alternative way to measure the true economic benefit for retail trades would be through the *Retail* coefficient estimates in Table VI. Doing so would exploit the fact that the *Retail* coefficient in the regression captures the incremental price improvement for retail trades. However, a quick inspection reveals the *Retail* coefficients in Table VI are considerably larger than the effective half spreads conveyed by the dark gray bars in Figure 4 and the coefficient estimates in Table IV. This general pattern seems to suggest that even within the same 15-minute window, retail trades must execute when quoted spreads are higher.

We test this conjecture and repeat our estimation of Equation (5) using percent quoted spread (*QS*%) as the dependent variable. We report the results in Table VII. Indeed, quoted spreads are significantly larger when retail trades execute. We again note that this is true even after controlling for stock, day, time of day, and trade size. Why might this be the case? One reasonable explanation is that non-retail orders often reap the benefits of smart routers that monitor exchange quotes and execute precisely when spreads narrow. Retail traders, on the other hand, do not typically access this routing technology. While we cannot offer direct evidence for why the quoted spreads differ, the finding is certainly interesting in light of ongoing policy debates and should prompt future research.

[23] Virtually all major retail brokerages report execution quality and costs in terms of price improvement savings per order or the percentage of trades that are price improved. In our research, only Vanguard reports execution quality and retail costs in terms of effective spreads.

We now reconcile the various magnitudes in Tables III, VI, and VII by decomposing the difference in retail and exchange effective (half) spreads. We start by noting the that effective half spread (*EHS*) is a function of both quoted half spread (*QHS*) and price improvement (*Improve*) where:

$$EHS = QHS - Improve \tag{9}$$

Consider two trades, one for retail trade *r* and the other for exchange trade *e*. Substituting in and subtracting exchange trades from retail trades yields:

$$EHS_r - EHS_e = (QHS_r - Improve_r) - (QHS_e - Improve_e) \tag{10}$$

$$EHS_r - EHS_e = QHS_r - QHS_e - (Improve_r - Improve_e) \tag{11}$$

where the difference between effective spread of retail trades and exchange trades is equal to the difference between the quoted spreads of retail and exchange trades minus two times the difference in their price improvements. Rearranging equation (11) in terms of price improvement, we see in equation (12) that difference between price improvement of retail and exchange trades is equal to the difference of quoted half spreads minus the difference of effective half spreads.

$$Improve_r - Improve_e = (QHS_r - QHS_e) - (EHS_r - EHS_e) \tag{12}$$

In Figure 4, we illustrate the relative magnitudes coming from differences in effective spreads, price improvement and quoted spreads for each of the three order sizes using magnitudes in Tables III, VI, and VII, respectively. To contrast these findings with regulatory based measures, we also show the magnitude of price improvement relative to the NBBO. Ultimately, Figure 4 shows that the *EHS* difference is decomposed into the price improvement difference minus the *QHS* difference. More importantly, the total NBBO price improvement for retail orders grossly overstates the savings that retail investors actually receive, as reported by the difference in *EHS*.

VII. Possible Misclassification of Retail Trades

As mentioned earlier, the BJZZ retail trading measure only accounts for marketable orders that receive price improvement resulting in sub-penny executions. Thus, any retail orders that executive at the quote are omitted from our analysis. Since those orders, by definition, receive less favorable executions,

their omission biases our estimates of retail execution costs toward zero. Properly accounting for those orders would therefore narrow the gap between retail and exchange effective spreads, and in the extreme, a large mass of retail trades executing at the quote could change the sign of our inferences. Given the ongoing policy implications of our study, we take this concern seriously.

We offer a simple calibration exercise to determine the quantity of omitted trades that would alter our inferences. Assume some fraction p of actual traded retail shares are captured by the BJZZ measure and execute an effective spread of ES_r. Then assume the remaining $(1 - p)$ of actual traded retail shares execute at the NBBO resulting in a spread of QS_r. Thus, in this calibration, the fraction $(1 - p)$ of retail shares is not included in the data. We wish to determine the fraction $(1 - p)$ of trades that, if omitted, would eliminate the observed effective spread differential between retail and exchange trades. Setting the average effective spread for all retail trades equal to the average effective spread for exchange trades, ES_e, we have:

$$ES_r(p) + QS_r(1 - p) = ES_e \tag{13}$$

Solving for p, we have:

$$p = \frac{ES_e - QS_r}{ES_r - QS_r} \tag{14}$$

For each trade size bucket, we then insert the base estimates from Tables III and VII into (14) and report the resulting values of p in Figure 5. We interpret these values as "break-even" fractions of retail trades the Boehmer measure must capture to generate identical effective spreads for retail trades and exchange trades.

Our calibration offers interesting insights. For small, medium, and large trades, the break-even fractions are 0.73, 0.67, and 0.76, respectively. This means that under the simple assumptions above, retail effective spreads would not exceed those for exchange trades if the Boehmer measure captures at least two-thirds to three-fourths. As seen in Appendix I, Schwab reports over 90% of shares price improved for all size categories. Likewise, TD Ameritrade and Fidelity unconditionally reports the fraction of shares price improved at 98% and 85.53%, respectively. To provide further context for these values, we obtain the fraction of shares executed with price improvement by three large retail market makers (Citadel, Virtu Securities, and G1X Susquehanna) according to SEC Rule 605 Reports (Appendix II). We observe these

fractions are well above 80% as well, which suggests our main inferences that compare retail and exchange spreads are unlikely attributable to the omission of orders that execute at the quote.

VIII. Conclusion

Retail brokerage firms often route their clients' marketable orders to wholesale market makers in exchange for rebates of a few cents per hundred shares. These payment for order flow arrangements create a conflict of interest for the brokerage firms who must balance their own profit motives with their "best execution" duties to their clients. The recent shift to zero-commission trading only exacerbated this conflict as the commission drop eliminated a major source of broker revenue. While this conflict is the topic of ongoing public debate, we have surprisingly little empirical knowledge of the execution costs retail traders incur and how these costs compare with a counterfactual in which brokers simply route orders to the exchanges. We attempt to fill this gap with our large-scale analysis of execution costs for marketable retail orders that are routed to wholesalers.

Our key finding that these trades generally receive cheaper executions than comparable exchange trades—both before and after the shift to zero trading commissions—informs the current debate. Our execution cost estimates suggest that, if anything, the shift to zero commissions helped retail investors. At the same time, we show that regulatory-prescribed disclosures that tie price improvement to the NBBO at the time of a trade likely overstate retail traders' economic savings that result from their brokers' order routing arrangements.

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Figure 1 (continued): Intraday Trading

This Figure shows the percent of retail and exchange shares traded during 15-minute trading intervals in August and September 2019 (Base Period). The light (dark) gray bars represent the percent of exchange (retail) shares traded in each 15-minute interval over the period. The percent of shares is represented on the left axis. The solid (dashed) line represents the share weighted effective spread percent for exchange (retail) traded shares. The scale for the execution statistic is represented on the right axis.

Retail and Exchange Trading with Effective Spreads

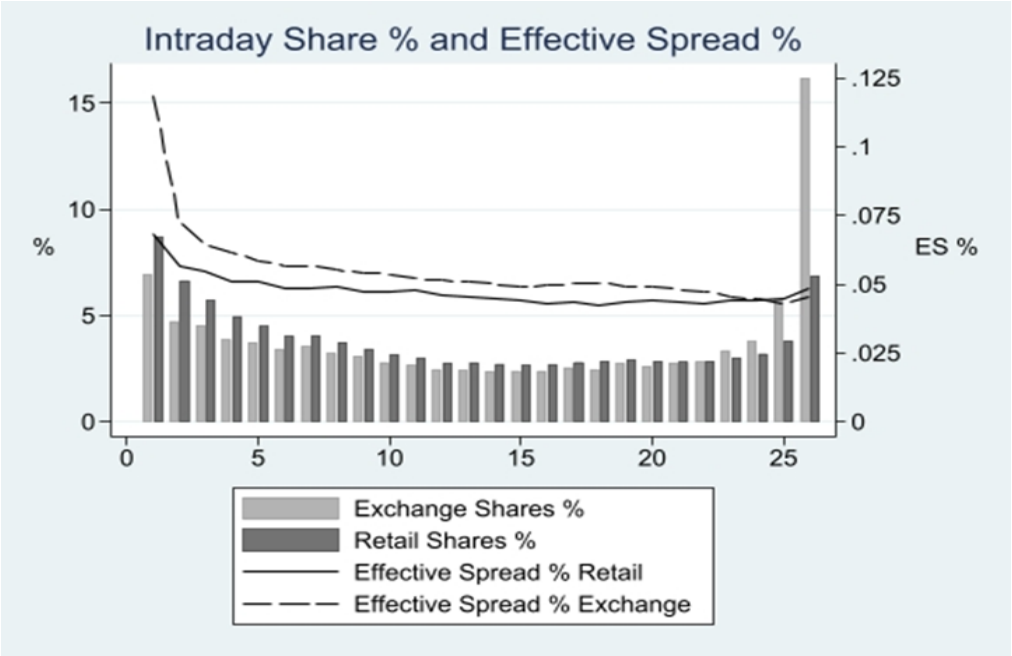


Figure 2: Retail Turnover

This Figure shows distribution statistics for retail turnover from August 2019 to June 2020. Retail turnover is calculated as the ratio of retail shares traded to shares outstanding where retail shares are identified according to the Boehmer et al. 2021 subpenny method and shares outstanding comes from the WRDS Daily CRSP file. Retail turnover is calculated at the stock level daily and with distribution statistics coming from the stock x day turnover. The red line represents the daily 25th percentile. The green line represents the daily 50th percentile (Median). The yellow line represents the daily 75th percentile. The blue line represents the daily average. The first vertical line (from left to right) represents the start of the zero-commission retail trading in October 2019. The second vertical line (from left to right) represent the start of the Covid-19 period in March 2020.

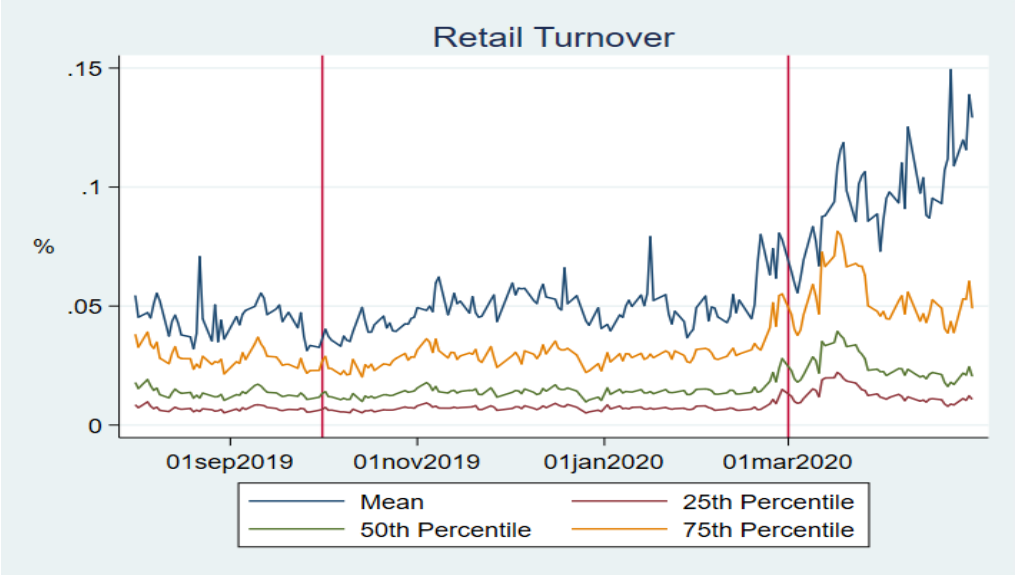


Figure 3: Absolute Imbalance 5-Day Moving Average

This Figure shows the comparison of the 5-day moving average of absolute imbalance for retail trades and the overall market. Retail trades are identified according to the Boehmer et al. 2021 subpenny method. Absolute imbalance is calculated as the absolute value of buys minus sells over buys plus sells. Imbalance is measures in terms of shares. Absolute imbalance is calculated at the stock x day level and then averaged to get a daily measure. The 5-day moving average smoothing of absolute imbalance is calculated as the moving average of the average absolute imbalance over the previous 5 trading days including day t. The red (blue) line represents the retail (market) absolute imbalance respectively. The first vertical line (from left to right) represents the start of the zero-commission retail trading in October 2019. The second vertical line (from left to right) represent the start of the Covid-19 period in March 2020.

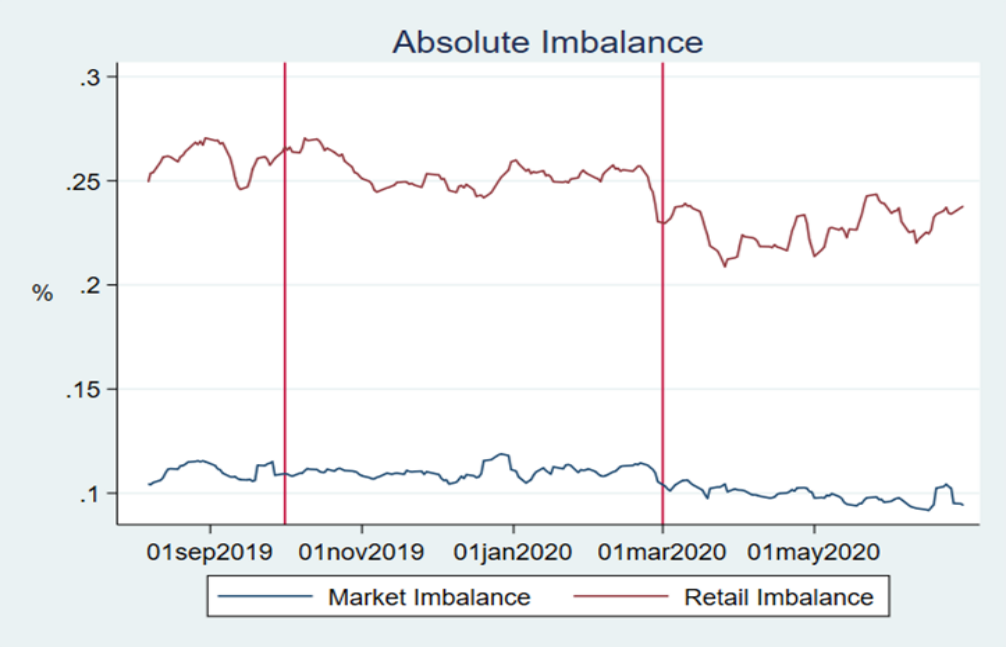


Figure 4: Execution Differences

This Figure shows the difference between retail and exchange execution quality. NBBO Improvement shows the total NBBO price improvement for retail trades. QS Difference shows half the difference in quoted spreads between retail and exchange trades. ES Difference shows half the difference in effective spreads between retail and exchange trades. PI Difference shows difference in NBBO price improvement between retail and exchange trades. All measures are presented as a percent of midpoint. Estimates are pulled from the coefficients in Tables 4, 6, and 7.

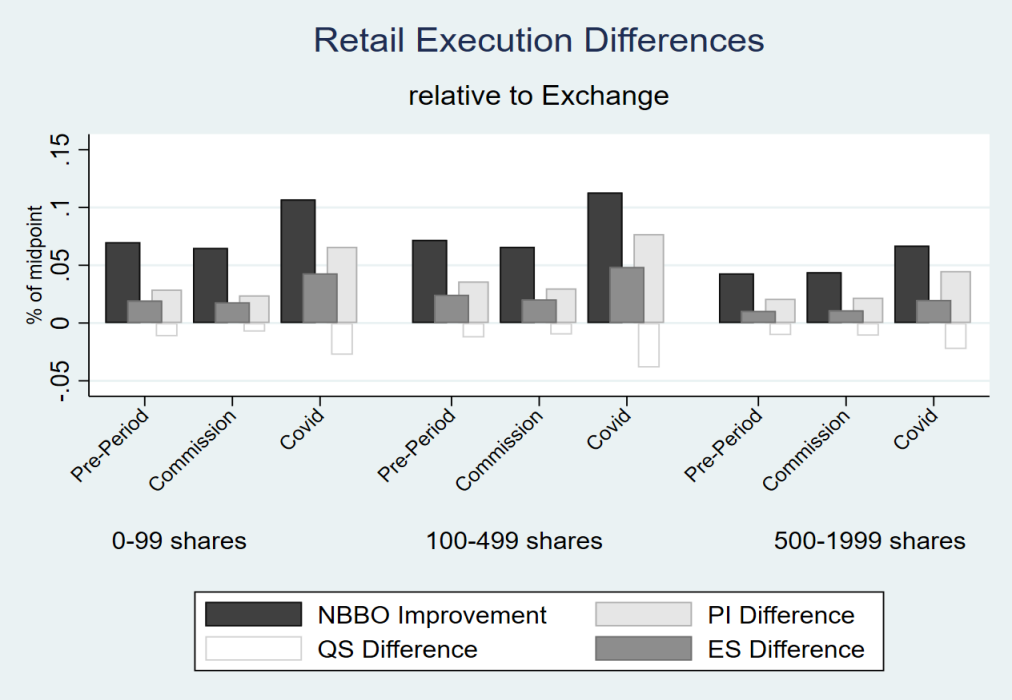


Figure 5: Execution Breakeven

This figure shows the hypothetical breakeven point of execution quality from which retail execution quality will equal exchange execution quality. We use the following equation $ES_r(p)+QS_r(1-p)=ES_e$, where ES_r is the effective spread of retail trades, QS_r is the quoted spread of retail trades, ES_e is the effective spread of exchange trades, p is the percentage of retail shares that execute at the ES_r, and (1-p) is the percentage of retail shares that execute at the NBBO (QS_r). p can be interpreted as the percentage of retail shares that receive price improvement. Estimates are pulled from the coefficients in Tables 4 and 7 for the base period of August and September 2019.

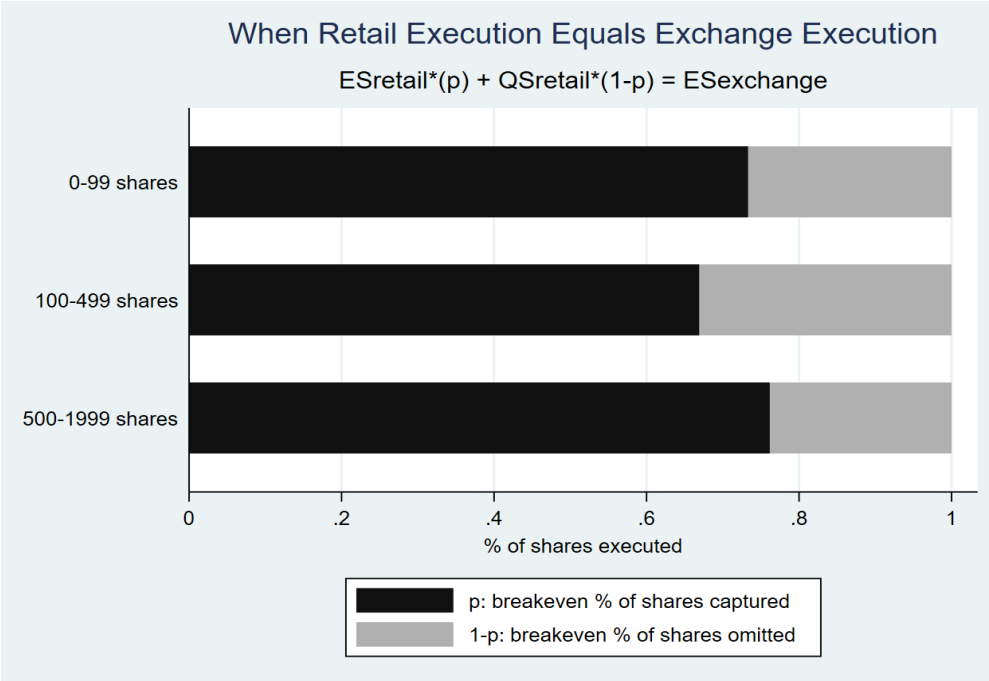


Table I: Descriptive Statistics

This table shows descriptive statistics from August 2019 and September 2019 (Pre-period). Summary statistics are generated by calculating the share weighted item (e.g., effective spread $) for each stock x day then calculating the statistics for each day and averaging across the 42 days in the pre-period (e.g., Max represents the average of 42 daily max values). Panel A shows the stock characteristics of all stocks in the sample. Market Capitalization is reported from December 2018. Panel B and Panel C show the trading statistics for retail trades and exchange trades, respectively. Panel D shows the execution statistics for the market. Panel E shows the execution statistics for those trades that occur on exchange. Panel F shows the execution statistics for retail trades that occur off exchange. Panel G and Panel H show the price improvement statistics for exchange trades and retail trades, respectively. Panel I shows absolute imbalance measures for market trades signed using the Lee and Ready (1991) method, for retail trades signed using the subpenny method by Boehmer et al. (2021), and for retail trades signed using the Lee and Ready (1991) method. Retail trades for all panels are identified according to the subpenny method by Boehmer et al. (2021). In Panel A the market capitalization and price statistics are calculated as of December 31st, 2018. Percent execution statistics (quoted spread, effective spread, realized spread, price improvement and price impact) are calculated as a percent of midpoint unless otherwise specified.

	Mean	Median	Std.	Min	P5	P10	P25	P75	P90	P95	Max
Panel A: Stock Characteristics											
Market Capitalization ($Millions)	9,283	1,499	34,887	15	147	229	514	4,737	16,393	39,301	780,362
Price	47.05	29.54	59.57	5.00	6.75	8.57	14.95	56.91	100.22	142.94	838.34
Turnover %	0.58%	0.32%	1.44%	0.00%	0.02%	0.04%	0.14%	0.61%	1.18%	1.80%	42.37%
Average July 2019 Retail Trades	300	91	872	5	8	14	34	242	616	1,101	15,689
Panel B: Retail Trading Statistics											
Retail Trades	322	87	1,073	1	5	10	30	244	647	1,156	27,869
Retail Turnover	0.04%	0.01%	0.27%	0.00%	0.00%	0.00%	0.01%	0.03%	0.07%	0.13%	9.74%
% Daily Volume in Retail Shares	7.65%	4.89%	8.56%	0.31%	1.94%	2.34%	3.23%	8.31%	15.58%	23.43%	91.19%
% Daily Volume in Retail Trades	5.45%	3.41%	6.44%	0.59%	1.41%	1.68%	2.29%	5.61%	10.80%	17.64%	63.27%
Panel C: Exchange Trading Statistics											
Exchange Turnover	0.41%	0.24%	0.83%	0.00%	0.01%	0.03%	0.10%	0.45%	0.85%	1.27%	21.97%
% Daily Volume in Exchange Shares	73.30%	75.33%	10.80%	4.37%	53.03%	60.62%	69.09%	80.10%	83.84%	86.10%	99.29%
% Daily Volume in Exchange Trades	80.01%	82.03%	8.90%	16.63%	62.91%	70.24%	77.40%	85.40%	87.97%	89.39%	96.46%

Table I (continued): Descriptive Statistics

	Mean	Median	Std. dev.	Min	P5	P10	P25	P75	P90	P95	Max
Panel D: Execution Statistics (Market)											
Quoted Spread %	0.27%	0.14%	0.35%	0.01%	0.03%	0.04%	0.07%	0.29%	0.74%	1.07%	3.51%
Quoted Spread $	0.09	0.04	0.14	0.01	0.01	0.01	0.02	0.11	0.23	0.36	1.41
Effective Spread %	0.15%	0.08%	0.20%	0.01%	0.02%	0.02%	0.04%	0.17%	0.40%	0.59%	2.07%
Effective Spread $	0.05	0.02	0.07	0.00	0.01	0.01	0.01	0.06	0.13	0.20	0.81
Realized Spread %	0.04%	0.01%	0.15%	-1.10%	-0.08%	-0.04%	-0.01%	0.04%	0.15%	0.31%	1.95%
Realized Spread $	0.01	0.00	0.05	-0.36	-0.02	-0.01	0.00	0.02	0.05	0.09	0.64
Price Impact %	0.11%	0.06%	0.16%	-0.61%	0.00%	0.01%	0.03%	0.13%	0.26%	0.41%	1.94%
Price Impact $	0.04	0.02	0.06	-0.16	0.00	0.00	0.01	0.04	0.09	0.14	0.73
Panel E: Execution Statistics (Exchange)											
Quoted Spread %	0.26%	0.13%	0.35%	0.01%	0.03%	0.04%	0.07%	0.27%	0.70%	1.05%	3.72%
Quoted Spread $	0.09	0.04	0.14	0.01	0.01	0.01	0.02	0.10	0.22	0.35	1.49
Effective Spread %	0.16%	0.08%	0.21%	0.00%	0.02%	0.03%	0.04%	0.18%	0.44%	0.64%	2.36%
Effective Spread $	0.06	0.03	0.08	0.00	0.01	0.01	0.01	0.06	0.14	0.21	0.89
Realized Spread %	0.04%	0.00%	0.22%	-1.80%	-0.13%	-0.06%	-0.02%	0.03%	0.15%	0.36%	2.69%
Realized Spread $	0.01	0.00	0.07	-0.60	-0.03	-0.02	-0.01	0.01	0.05	0.10	0.94
Price Impact %	0.13%	0.07%	0.21%	-1.01%	0.00%	0.02%	0.03%	0.15%	0.32%	0.53%	2.54%
Price Impact $	0.05	0.02	0.07	-0.29	0.00	0.01	0.01	0.05	0.11	0.17	0.96
Panel F: Execution Statistics (Retail)											
Quoted Spread %	0.32%	0.18%	0.38%	0.01%	0.03%	0.05%	0.09%	0.37%	0.83%	1.14%	3.83%
Quoted Spread $	0.11	0.06	0.15	0.01	0.01	0.01	0.02	0.14	0.28	0.41	1.63
Effective Spread %	0.09%	0.04%	0.17%	-0.74%	-0.01%	0.00%	0.01%	0.09%	0.23%	0.40%	2.08%
Effective Spread $	0.03	0.01	0.06	-0.26	-0.01	0.00	0.01	0.02	0.06	0.11	0.84
Realized Spread %	0.04%	0.02%	0.26%	-2.15%	-0.28%	-0.15%	-0.04%	0.09%	0.24%	0.41%	2.58%
Realized Spread $	0.01	0.01	0.08	-0.7	-0.09	-0.05	-0.01	0.03	0.07	0.12	0.97
Price Impact %	0.05%	0.01%	0.23%	-1.63%	-0.20%	-0.11%	-0.03%	0.09%	0.24%	0.40%	2.37%
Price Impact $	0.01	0.00	0.07	-0.6	-0.07	-0.04	-0.01	0.03	0.08	0.12	0.85

Table I (continued): Descriptive Statistics

	Mean	Median	Std.	Min	P5	P10	P25	P75	P90	P95	Max
Panel G: Price Improvement Stats (Exchange)											
NBBO PI $	0.0170	0.0069	0.0324	0.0000	0.0008	0.0011	0.0028	0.0173	0.0389	0.0631	0.4364
NBBO PI % Quoted Spread	13.52%	13.74%	4.97%	0.04%	5.60%	7.12%	10.33%	16.54%	18.80%	20.33%	49.21%
NBBO PI % Midpoint	0.045%	0.021%	0.075%	0.000%	0.003%	0.005%	0.010%	0.043%	0.102%	0.180%	1.099%
Panel H: Price Improvement Stats (Retail)											
NBBO PI $	0.0415	0.0209	0.0549	0.0001	0.0019	0.0025	0.0079	0.0526	0.1076	0.1537	0.6162
NBBO PI % Quoted Spread	34.16%	35.36%	11.67%	0.04%	15.62%	18.83%	25.85%	41.88%	47.06%	51.34%	82.70%
NBBO PI % Midpoint	0.103%	0.061%	0.123%	0.000%	0.009%	0.014%	0.028%	0.128%	0.260%	0.355%	1.496%
Subpenny PI $	0.0017	0.0018	0.0004	0.0001	0.0009	0.0012	0.0015	0.0020	0.0022	0.0024	0.0037
Subpenny PI % Quoted Spread	6.62%	4.38%	6.42%	0.03%	0.38%	0.69%	1.68%	10.14%	16.76%	19.12%	74.25%
Subpenny PI % Midpoint	0.009%	0.005%	0.012%	0.000%	0.000%	0.001%	0.002%	0.011%	0.022%	0.032%	0.125%
Panel I: Imbalance											
Market Share Imbalance	0.11	0.08	0.12	0.00	0.01	0.01	0.04	0.14	0.24	0.33	0.95
Market Trade Imbalance	0.09	0.07	0.10	0.00	0.01	0.01	0.03	0.12	0.20	0.28	0.88
Retail Share Imbalance SP	0.26	0.18	0.24	0.00	0.01	0.03	0.08	0.36	0.62	0.80	1.00
Retail Trade Imbalance SP	0.18	0.12	0.19	0.00	0.01	0.02	0.05	0.24	0.42	0.57	1.00
Retail Share Imbalance LR	0.28	0.20	0.25	0.00	0.02	0.03	0.09	0.39	0.65	0.83	1.00
Retail Trade Imbalance LR	0.20	0.14	0.20	0.00	0.01	0.02	0.06	0.26	0.45	0.61	1.00

Table II: Retail Execution

This table presents univariate regressions comparing 15-minute interval intraday execution quality between retail and exchange trades over 15-minute intervals. *Retail* is an indicator variable taking the value of 1 if the trade is a retail trade according to the Boehmer et al. 2021 subpenny method and 0 if the trade is executed on-exchange. Panel A presents the results for the effective spread as a percent of midpoint. Panel B presents the results for the price impact as a percent of midpoint. Panel C presents the results for the 5-minute realized spread as a percent of midpoint. Column 1 shows odd lot trades. Column 2 shows trades under 500 shares but greater than 100. Column 3 shows trades greater than 500 shares but less than 2000 shares. The first and last 15-minute periods of each trading day, 9:30am to 9:45am and 3:45pm to 4:00pm respectively, are excluded. Stock and Date x Time fixed effects are included in all specifications. *T*-statistics in parentheses are calculated from heteroskedasticity-robust standard errors clustered by stock. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A:	Dependent Variable = *Effective Spread as a Percent of Midpoint*		
	0-99 Shares	100-499 Shares	500-1999 Shares
	(1)	(2)	(3)
Retail	-0.039***	-0.049***	-0.021***
	(-46.76)	(-49.94)	(-29.78)
Constant	0.099***	0.095***	0.074***
	(240.23)	(193.01)	(211.77)
Observations	2,926,226	2,772,048	785,024
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.469	0.469	0.567
Panel B:	Dependent Variable = *Price Impact as a Percent of Midpoint*		
	0-99 Shares	100-499 Shares	500-1999 Shares
	(1)	(2)	(3)
Retail	-0.063***	-0.071***	-0.079***
	(-58.91)	(-57.75)	(-25.73)
Constant	0.078***	0.096***	0.105***
	(145.30)	(156.46)	(68.87)
Observations	2,926,226	2,772,048	785,024
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.045	0.071	0.051
Panel C:	Dependent Variable = *Realized Spread as a Percent of Midpoint*		
	0-99 Shares	100-499 Shares	500-1999 Shares
	(1)	(2)	(3)
Retail	0.024***	0.021***	0.057***
	(24.17)	(26.19)	(21.04)
Constant	0.022***	-0.000	-0.030***
	(45.20)	(-0.11)	(-22.52)
Observations	2,926,226	2,772,048	785,024
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.048	0.013	0.011

Table III: Retail Execution and Firm Size

This table presents univariate regressions adjusting for firm size comparing 15-minute interval intraday execution quality between retail and exchange trades over 15-minute intervals. *Retail* is an indicator variable taking the value of 1 if the trade is a retail trade according to the Boehmer et al. 2021 subpenny method and 0 if the trade is executed on-exchange. Panel A presents the results for the effective spread as a percent of midpoint. Panel B presents the results for the price impact as a percent of midpoint. Panel C presents the results for the realized spread as a percent of midpoint. Columns 1 through 3 show odd lot trades. Columns 4 through 6 show trades of 100-499 shares. Column 7 through 9 shows trades of 500-1999 shares. Columns are separated by Small-Cap (<$2B), Mid-Cap ($2B-$10B), and Large-Cap(>$10B) stocks based on market capitalization as of December 31st, 2019. The first and last 15-minute periods of each trading day, 9:30am to 9:45am and 3:45pm to 4:00pm respectively, are excluded. Stock and Date x Time fixed effects are included in all specifications. *T*-statistics in parentheses are calculated from heteroskedasticity-robust standard errors clustered by stock. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A:	Dependent Variable = *Effective Spread as a Percent of Midpoint*								
	0-99 Shares			100-499 Shares			500-1999 Shares		
	Small	Mid	Large	Small	Mid	Large	Small	Mid	Large
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Retail	-0.064***	-0.030***	-0.015***	-0.083***	-0.034***	-0.015***	-0.039***	-0.018***	-0.008***
	(-35.73)	(-33.70)	(-28.71)	(-43.44)	(-36.00)	(-27.65)	(-23.93)	(-18.27)	(-22.26)
Constant	0.184***	0.065***	0.028***	0.168***	0.058***	0.026***	0.156***	0.055***	0.023***
	(205.85)	(143.92)	(109.23)	(176.15)	(124.63)	(94.22)	(189.26)	(112.16)	(126.27)
Observations	1,084,460	1,091,542	750,224	1,126,742	941,270	704,036	244,138	231,342	309,544
Stock FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date x Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.410	0.292	0.503	0.418	0.286	0.416	0.466	0.320	0.443

Panel B:	Dependent Variable = *Price Impact as a Percent of Midpoint*								
	0-99 Shares			100-499 Shares			500-1999 Shares		
	Small	Mid	Large	Small	Mid	Large	Small	Mid	Large
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Retail	-0.104***	-0.050***	-0.023***	-0.116***	-0.051***	-0.024***	-0.159***	-0.067***	-0.024***
	(-55.08)	(-52.58)	(-35.85)	(-56.57)	(-53.36)	(-37.82)	(-24.08)	(-21.72)	(-21.33)
Constant	0.135***	0.057***	0.026***	0.165***	0.063***	0.028***	0.225***	0.081***	0.029***
	(141.99)	(121.47)	(79.79)	(160.93)	(131.87)	(88.57)	(68.20)	(52.10)	(52.23)
Observations	1,084,460	1,091,542	750,224	1,126,742	941,270	704,036	244,138	231,342	309,544
Stock FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date x Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.044	0.025	0.022	0.070	0.027	0.018	0.050	0.016	0.009

Table III (continued): Retail Execution and Firm Size

This table presents univariate regressions adjusting for firm size comparing 15-minute interval intraday execution quality between retail and exchange trades. *Retail* is an indicator variable taking the value of 1 if the trade is a retail trade according to the Boehmer et al. 2021 subpenny method and 0 if the trade is executed on-exchange. Panel A presents the results for the effective spread as a percent of midpoint. Panel B presents the results for the price impact as a percent of midpoint. Panel C presents the results for the realized spread as a percent of midpoint. Columns 1 through 3 show odd lot trades. Columns 4 through 6 show trades of 100-499 shares. Column 7 through 9 shows trades of 500-1999 shares. Columns are separated by Small-Cap (<$2B), Mid-Cap ($2B-$10B), and Large-Cap(>$10B) stocks based on market capitalization as of December 31st, 2019. The first and last 15-minute periods of each trading day, 9:30am to 9:45am and 3:45pm to 4:00pm respectively, are excluded. Stock and Date x Time fixed effects are included in all specifications. *T*-statistics in parentheses are calculated from heteroskedasticity-robust standard errors clustered by stock. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel C:	Dependent Variable = *Realized Spread as a Percent of Midpoint*								
	0-99 Shares			100-499 Shares			500-1999 Shares		
	Small	Mid	Large	Small	Mid	Large	Small	Mid	Large
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Retail	0.038***	0.020***	0.008***	0.030***	0.018***	0.009***	0.116***	0.049***	0.016***
	(16.39)	(24.07)	(18.61)	(18.00)	(24.55)	(22.23)	(18.81)	(16.98)	(14.30)
Constant	0.051***	0.007***	0.002***	0.005***	-0.005***	-0.002***	-0.066***	-0.025***	-0.006***
	(44.22)	(17.08)	(10.18)	(6.20)	(-13.15)	(-10.26)	(-21.42)	(-17.37)	(-11.29)
Observations	1,084,460	1,091,542	750,224	1,126,742	941,270	704,036	244,138	231,342	309,544
Stock FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.050	0.008	0.009	0.015	0.004	0.005	0.015	0.006	0.005

Table IV: Retail Execution during Zero Commissions, and Covid-19

This table presents regressions comparing 15-minute interval intraday effective spreads as percent of midpoint between retail and exchange trades when commissions were cut to zero at the end of 2019 and during the start of the Coronavirus pandemic at the beginning of 2020. *Retail* is an indicator variable taking the value of 1 if the trade is a retail trade according to the Boehmer et al. 2021 subpenny method and 0 if the trade is executed on-exchange. Our base period is August and September 2019. *Post* is an indicator variable denoting trades during November and December 2019, after commissions were cut in October 2019. *Covid* is an indicator variable denoting trades during March and April 2020, the beginning of the pandemic. Panel A presents the results for the effective spread as a percent of midpoint. Panel B presents the results for the price impact as a percent of midpoint. Panel C presents the results for the realized spread as a percent of midpoint. Column 1 shows odd lot trades. Column 2 shows trades under 500 shares but greater than 100. Column 3 shows trades greater than 500 shares but less than 2000 shares. The first and last 15-minute periods of each trading day, 9:30am to 9:45am and 3:45pm to 4:00pm respectively, are excluded. Stock and Date x Time fixed effects are included in all specifications. *T*-statistics in parentheses are calculated from heteroskedasticity-robust standard errors clustered by stock. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A:	Dependent Variable = *Effective Spread as a Percent of Midpoint*		
	0-99 Shares	100-499 Shares	500-1999 Shares
	(1)	(2)	(3)
Retail	-0.039***	-0.049***	-0.021***
	(-46.77)	(-49.94)	(-29.78)
Retail x Post	0.003***	0.008***	-0.001**
	(5.33)	(16.93)	(-2.06)
Retail x Covid	-0.047***	-0.048***	-0.019***
	(-38.46)	(-39.36)	(-16.72)
Constant	0.141***	0.131***	0.109***
	(263.90)	(217.36)	(241.78)
Observations	9,240,036	8,635,684	2,671,856
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.389	0.391	0.447

Panel B:	Dependent Variable = *Price Impact as a Percent of Midpoint*		
	0-99 Shares	100-499 Shares	500-1999 Shares
	(1)	(2)	(3)
Retail	-0.063***	-0.071***	-0.079***
	(-58.91)	(-57.75)	(-25.73)
Retail x Post	0.010***	0.010***	-0.002
	(15.89)	(14.00)	(-0.86)
Retail x Covid	-0.094***	-0.092***	-0.103***
	(-53.69)	(-47.77)	(-22.20)
Constant	0.114***	0.139***	0.166***
	(160.99)	(174.54)	(81.91)
Observations	9,240,036	8,635,684	2,671,856
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.040	0.062	0.041

Table IV (continued): Retail Execution during Zero Commissions, and Covid-19

This table presents regressions comparing 15-minute interval intraday effective spreads as percent of midpoint between retail and exchange trades when commissions were cut to zero at the end of 2019 and during the start of the Coronavirus pandemic at the beginning of 2020. *Retail* is an indicator variable taking the value of 1 if the trade is a retail trade according to the Boehmer et al. 2021 subpenny method and 0 if the trade is executed on-exchange. Our base period is August and September 2019. *Post* is an indicator variable denoting trades during November and December 2019, after commissions were cut in October 2019. *Covid* is an indicator variable denoting trades during March and April 2020, the beginning of the pandemic. Panel A presents the results for the effective spread as a percent of midpoint. Panel B presents the results for the price impact as a percent of midpoint. Panel C presents the results for the realized spread as a percent of midpoint. Column 1 shows odd lot trades. Column 2 shows trades under 500 shares but greater than 100. Column 3 shows trades greater than 500 shares but less than 2000 shares. The first and last 15-minute periods of each trading day, 9:30am to 9:45am and 3:45pm to 4:00pm respectively, are excluded. Stock and Date x Time fixed effects are included in all specifications. *T*-statistics in parentheses are calculated from heteroskedasticity-robust standard errors clustered by stock. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel C:	Dependent Variable = *Realized Spread as a Percent of Midpoint*		
	0-99 Shares	100-499 Shares	500-1999 Shares
	(1)	(2)	(3)
Retail	0.024***	0.021***	0.057***
	(24.17)	(26.19)	(21.04)
Retail x Post	-0.006***	-0.001**	0.000
	(-9.01)	(-2.32)	(0.20)
Retail x Covid	0.046***	0.042***	0.082***
	(26.82)	(26.70)	(19.98)
Constant	0.029***	-0.006***	-0.054***
	(48.82)	(-12.69)	(-32.20)
Observations	9,240,036	8,635,684	2,671,856
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.037	0.011	0.010

Table V: Retail Effective Spread by Firm Size during Zero Commissions and Covid-19

This table presents regressions adjusting for firm size comparing 15-minute interval intraday effective spreads as a percent of midpoint between retail and exchange trades when commissions were cut to zero at the end of 2019 and during the start of the Coronavirus pandemic at the beginning of 2020. *Retail* is an indicator variable taking the value of 1 if the trade is a retail trade according to the Boehmer et al. 2021 subpenny method and 0 if the trade is executed on-exchange. Our base period is August and September 2019. *Post* is an indicator variable denoting trades during November and December 2019, after commissions were cut in October 2019. *Covid* is an indicator variable denoting trades during March and April 2020, the beginning of the pandemic. Panel A presents the results for the effective spread as a percent of midpoint. Column 1 shows odd lot trades. Column 2 shows trades under 500 shares but greater than 100. Column 3 shows trades greater than 500 shares but less than 2000 shares. Columns are separated by Small-Cap (<$2B), Mid-Cap ($2B-$10B), and Large-Cap(>$10B) stocks based on market capitalization as of December 31st, 2019. The first and last 15-minute periods of each trading day, 9:30am to 9:45am and 3:45pm to 4:00pm respectively, are excluded. Stock and Date x Time fixed effects are included in all specifications. *T*-statistics in parentheses are calculated from heteroskedasticity-robust standard errors clustered by stock. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable = *Effective Spread as a Percent of Midpoint*								
	0-99 Shares			100-499 Shares			500-1999 Shares		
	Small	Mid	Large	Small	Mid	Large	Small	Mid	Large
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Retail	-0.064***	-0.030***	-0.015***	-0.083***	-0.034***	-0.015***	-0.039***	-0.018***	-0.008***
	(-35.73)	(-33.70)	(-28.71)	(-43.45)	(-36.00)	(-27.65)	(-23.94)	(-18.27)	(-22.26)
Retail x Post	-0.000	0.006***	0.004***	0.011***	0.008***	0.004***	-0.007***	0.002***	0.002***
	(-0.18)	(18.26)	(20.60)	(10.37)	(23.91)	(24.32)	(-3.99)	(3.90)	(9.89)
Retail x Covid	-0.065***	-0.035***	-0.022***	-0.066***	-0.039***	-0.022***	-0.032***	-0.012***	-0.010***
	(-26.20)	(-25.98)	(-22.24)	(-26.83)	(-27.13)	(-21.42)	(-10.98)	(-8.90)	(-16.89)
Constant	0.250***	0.090***	0.040***	0.226***	0.082***	0.038***	0.225***	0.079***	0.033***
	(232.55)	(154.91)	(107.64)	(196.45)	(132.75)	(93.95)	(226.59)	(152.95)	(143.53)
Observations	3,656,210	3,365,310	2,218,516	3,602,546	2,927,192	2,105,946	873,518	797,066	1,001,272
Stock FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.357	0.301	0.463	0.363	0.277	0.360	0.382	0.275	0.257

Table VI: Retail Price Improvement during Zero Commissions, and Covid-19

This table presents regressions comparing 15-minute interval intraday price improvement relative to the NBBO as a percent of midpoint between retail and exchange trades when commissions were cut to zero at the end of 2019 and during the start of the Coronavirus pandemic at the beginning of 2020. *Retail* is an indicator variable taking the value of 1 if the trade is a retail trade according to the Boehmer et al. 2021 subpenny method and 0 if the trade is executed on-exchange. Our base period is August and September 2019. *Post* is an indicator variable denoting trades during November and December 2019, after commissions were cut in October 2019. *Covid* is an indicator variable denoting trades during March and April 2020, the beginning of the pandemic. Column 1 shows odd lot trades. Column 2 shows trades under 500 shares but greater than 100. Column 3 shows trades greater than 500 shares but less than 2000 shares. The first and last 15-minute periods of each trading day, 9:30am to 9:45am and 3:45pm to 4:00pm respectively, are excluded. Stock and Date x Time fixed effects are included in all specifications. *T*-statistics in parentheses are calculated from heteroskedasticity-robust standard errors clustered by stock. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable = *NBBO Price Improvement as a Percent of Midpoint*		
	0-99 Shares	100-499 Shares	500-1999 Shares
	(1)	(2)	(3)
Retail	0.029***	0.036***	0.021***
	(56.90)	(56.29)	(34.04)
Retail x Post	-0.005***	-0.006***	0.001***
	(-16.19)	(-21.69)	(2.60)
Retail x Covid	0.037***	0.041***	0.024***
	(53.29)	(52.25)	(30.35)
Constant	0.041***	0.036***	0.022***
	(131.66)	(90.99)	(54.11)
Observations	9,240,036	8,635,684	2,671,856
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.384	0.399	0.327

Table VII: Retail Quoted Spreads during Zero Commissions, and Covid-19

This table presents regressions comparing 15-minute interval intraday quoted spreads as a percent of midpoint between retail and exchange trades when commissions were cut to zero at the end of 2019 and during the start of the Coronavirus pandemic at the beginning of 2020. *Retail* is an indicator variable taking the value of 1 if the trade is a retail trade according to the Boehmer et al. 2021 subpenny method and 0 if the trade is executed on-exchange. Our base period is August and September 2019. *Post* is an indicator variable denoting trades during November and December 2019, after commissions were cut in October 2019. *Covid* is an indicator variable denoting trades during March and April 2020, the beginning of the pandemic. Column 1 shows odd lot trades. Column 2 shows trades under 500 shares but greater than 100. Column 3 shows trades greater than 500 shares but less than 2000 shares. The first and last 15-minute periods of each trading day, 9:30am to 9:45am and 3:45pm to 4:00pm respectively, are excluded. Stock and Date x Time fixed effects are included in all specifications. *T*-statistics in parentheses are calculated from heteroskedasticity-robust standard errors clustered by stock. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable = *Quoted Spread as a Percent of Midpoint*		
	0-99 Shares	100-499 Shares	500-1999 Shares
	(1)	(2)	(3)
Retail	0.023***	0.025***	0.021***
	(58.71)	(58.74)	(31.44)
Retail x Post	-0.008***	-0.005***	0.001**
	(-30.08)	(-19.52)	(2.52)
Retail x Covid	0.032***	0.037***	0.030***
	(56.79)	(59.04)	(29.39)
Constant	0.225***	0.205***	0.155***
	(1,021.65)	(756.43)	(357.62)
Observations	9,240,036	8,635,684	2,671,856
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.629	0.643	0.638

Appendix I: Broker Reported Execution Quality

This Figure shows the broker reported execution quality accessed from their various websites in November 2021. Panel A shows the execution quality as reported by Charles Schwab for S&P 500 stocks. Panel B shows the execution quality as reported by Fidelity. Panel C shows the execution quality as reported by TD Ameritrade. Panel D shows the execution quality as reported by Vanguard.

Panel A: Charles Schwab

Price Improvement

93.9%

Orders are often filled at prices better than the National Best Bid and Offer (NBBO).

Average Savings Per Order

$17.19

True cost savings result from better execution prices.

Average Execution Speed[2]

0.04 sec

Marketable orders receive fast execution.

S&P 500 Stocks

Order Size Range (Shares)	Average Order Size (Shares)	Shares Executed at Current Market Quote or Better (%)	Price Improvement (%)	Average Savings Per Order ($)	Average Execution Speed (Seconds)
1 - 99	17	99.3%	97.9%	$0.76	0.04
100 - 499	177	99.1%	97.1%	$5.52	0.04
500 - 1,999	868	98.5%	93.9%	$17.19	0.04
2,000 - 4,999	2,884	97.8%	91.9%	$25.96	0.04

Panel B: Fidelity

The proof is in the numbers

Price improvement
Percentage of shares that are price-improved:

85.53%

How it's measured

Execution price
Percentage of shares that fall within the NBBO:

98.10%

How it's measured

Execution speed
Average execution speed:

0.04 seconds

How it's measured

Effective spread
Average effective spread:

$0.0121

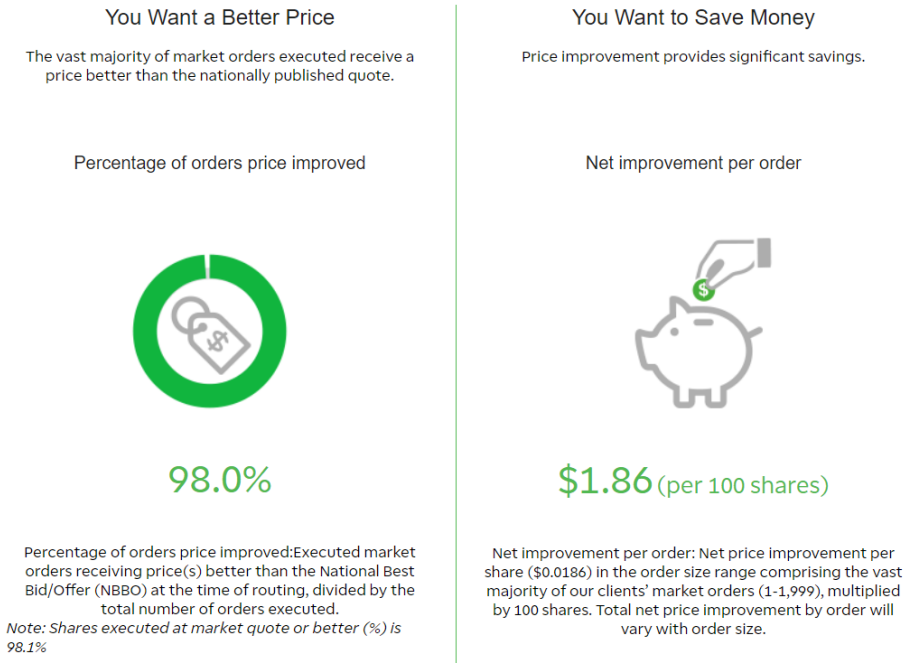
How it's measured

Source: Fidelity Q2 2021 statistics.

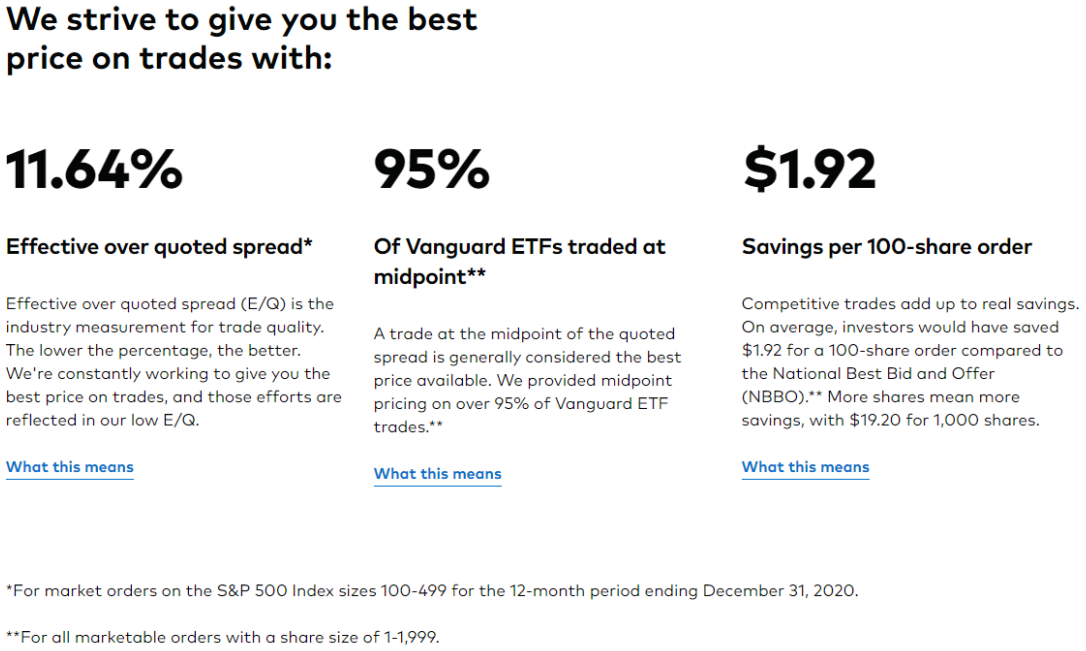
Appendix I (continued): Broker Reported Execution Quality

This Figure shows the broker reported execution quality accessed from their various websites in November 2021. Panel A shows the execution quality as reported by Charles Schwab for S&P 500 stocks. Panel B shows the execution quality as reported by Fidelity. Panel C shows the execution quality as reported by TD Ameritrade. Panel D shows the execution quality as reported by Vanguard.

Panel C: TD Ameritrade



Panel D: Vanguard



Appendix II: 605 Price Improvement for Market Makers

This table shows the percentage of price improved shares to market center executed shares submitted to three large retail market makers (Citadel, Virtu Securities, and G1X Susquehanna) using the SEC form 605 reports. This sample includes 2419 stocks and is separated into three subperiods. Panel A shows August 2019 to September 2019, which covers the period before the commission cut in October 2019. Panel B shows November 2019 to December 2019, which covers the period immediately after the commission cut. Panel C shows March 2020 to April 2020, which covers the Covid-19 shock. The data is separated into market and marketable limit orders, as well as order size. In each period, the total number of executed shares and price improved shares are summed across market makers and within order type and size. The percentage of price improvement is calculated by dividing the total number of price improved shares by the total number of executed shares.

	Market Orders	Marketable Limit Orders
Panel A	August 2019 – September 2019	
100-499 Shares	91.5%	31.6%
500-1999 Shares	83.4%	52.4%
Panel B	November 2019 – December 2019	
100-499 Shares	91.9%	34.3%
500-1999 Shares	82.3%	51.3%
Panel C	March 2020 – April 2020	
100-499 Shares	92.3%	44.5%
500-1999 Shares	84.0%	55.6%
Panel D	All Periods	
100-499 Shares	91.9%	36.7%
500-1999 Shares	83.2%	53.1%