No Such Thing as a Free Trade?
Retail Execution Costs, Zero Commissions, and Payment for Order Flow

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"The ultimate winners in our decision to eliminate commissions were investors."

- Walt Bettinger, President and CEO, Charles Schwab[1]

"Our markets have moved to zero commission, but it doesn't mean it's free. There's still payment underneath these applications. And it doesn't mean it's always best execution."

- Gary Gensler, Chairman, SEC[2]

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.[3] While the brokerage industry touted this move as a victory for retail investors, other interested parties such as the Securities and Exchange Commission, Congressional subcommittees, consumer advocacy groups, and the news media expressed concern. Simply put, the illusory nature of "free trading" offered in a zero-commission environment obfuscates indirect trading costs retail traders still bear. And the conflicts of interest embedded in brokers' order routing processes prompt many to question whether retail clients truly achieve the "best execution" prices our regulatory regime promises.[4]

The current discourse spotlights the controversial payment for order flow model (PFOF) 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

[1] See Charles Schwab Corporation 2019 Annual Report, Letter from the Chief Executive Officer pg. 4.
[2] "SEC chief Gensler says regulator assessing future of payment for order flow." By Thomas Franck, CNBC (October 19, 2021). https://www.cnbc.com/2021/10/19/sec-chief-gensler-says-regulator-assessing-payment-for-order-flow.html.
[3] 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; Fidelity Press Release from fidelity.com on October 10th, 2019.
[4] 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.

for order flow") and execute the trades at a price that is better than the National Best Bid or Offer (i.e., they offer "price improvement"). A key element of strife is that order flow payments and price improvement are fungible costs to the wholesaler. Brokers, who determine where orders are routed, have a profit motive to maximize PFOF. This incentive conflicts with their "best execution" obligation to their clients since order flow payments can offset price improvement and hence, raise execution costs, for retail investors. Ultimately, how the recent shift to zero-commission trading and more broadly, the PFOF framework, have affected retail investors' total trading costs is a contentious empirical question having broad policy implications.

In this paper, we offer new empirical insights by highlighting 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. Our initial analysis reveals 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 "Base Period" comprising the two months prior to the zero-commission shift. This baseline comparison between retail and exchange orders outside the zero-commission environment 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 $1.73 for a single 250-share trade in a $30 stock compared to $3.56 for an exchange trade. As commissions prior to October 2019 were typically around $5 per trade, a back-of-the-envelope calculation indicates for this example order, an increase in total trading costs around the zero-commission shift would require a near tripling of spreads.

We conduct our main analysis within a differences-in-differences framework and compare the baseline retail versus exchange trade differences 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 results from this analysis are twofold: any increase in spread is dwarfed by the drop in commissions; and retail trades still incur materially smaller spreads than comparable exchange trades. For example, relative to a comparable exchange order the half-spread on the hypothetical 250-share trade in a $30 stock referenced earlier would increase by only $0.30, or less than one-tenth of a typical commission. The emerging message is that retail orders subject to PFOF arrangements tend to achieve cheaper executions than comparable exchange orders both before and after the shift to zero commissions.

The second post-zero commission period is 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. This is the same period over which 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. For example, retail trades of 100 to 499 shares had execution costs about five basis points lower than comparable exchange trades in the Base Period. This difference increased to about ten basis points during the Covid 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.

Their uniformly lower effective spreads compared to those of exchange trades suggest retail traders accrue benefits in the PFOF environment. 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 both before the zero-commission shift and in the two post-zero periods. Moreover, the 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 both before and after the shift to zero commissions, our reported magnitudes are important as well. As we argue below, differences in effective spreads between retail trades and appropriately matched benchmarks capture 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 reported by market centers and publicized by brokers that implicitly benchmark execution costs against quoted spreads.

Per Regulation NMS, conventional measures of price improvement compare a trade's execution price to the National Best Bid and Offer (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.

We offer several contributions to the literature. First, our fresh, large-scale analysis of retail transactions costs informs various industry groups and regulators who may disagree about the net effect of zero-commission trading. Our message that overall costs likely decline is particularly interesting in light of Barber and Odean's (2000) classic finding that active retail traders' underperformance is driven mostly by transactions costs. Our results, along with recent papers documenting that retail trading imbalances predict the cross-section of future stock returns (see, e.g., BJZZ; Kaniel, Saar, and Titman (2008); Kelley and Tetlock (2013, 2017)) encourage further analysis of retail trading performance at the portfolio level as data availability allows.

Second, in the wake of the Congressional GameStop hearings that criticized PFOF, our results suggest that widespread calls to ban PFOF are premature. Rather, the economically small increases in execution costs immediately following the zero-commission shift and the subsequent larger reductions in execution costs that we document offer the alternative view that commission cuts serve as a mechanism by which brokers pass order flow payments back to their clients. Nevertheless, we caution the reader and note that we cannot confidently state whether PFOF is beneficial to retail traders or markets overall. Instead, we simply conclude that retail clients achieve cheaper executions than comparable exchange trades and that commission savings in the current environment are not undone by increased execution costs.

Our paper also draws attention to a shortcoming in how market makers and brokers disclose execution quality. 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. The Securities and Exchange Commission's recently adopted Market Data Infrastructure rule makes progress as it contains elements along both dimensions.[5]

Finally, our work contributes to a budding literature that analyzes various aspects of retail trading in a zero-commission environment. The two most closely related papers are Jain, Mishra, O'Donoghue, and Zhao (2021) and Kothari, So, and Johnson (2021). Using data from SEC 605 and 606 reports, Jain et al. (2021) find that brokers offering zero commission trading enjoyed increased market share and tended to route proportionally more orders to wholesalers than exchanges. They also find that retail traders changed

[5] See https://www.sec.gov/news/press-release/2020-311.

their strategies by submitting smaller orders than before. These results suggest the drop in commissions coupled with PFOF influenced the behavior of brokerage firms and their clients. Kothari, So, and Johnson (2021) analyze a proprietary dataset from Robinhood and show the firm's clients received cheaper executions than retail investors trading through other brokers as proxied by odd-lot trades that execute off-exchange. These authors estimate retail investors' aggregate cost savings in the zero-commission environment and conclude that zero-commission trading is indeed economically beneficial to retail traders.

II. Institutional Background

II.a. Retail trading costs and payment for order flow

Retail traders traditionally face two types of transactions costs: commissions and execution costs. A *commission* is usually a fixed out-of-pocket payment the trader remits directly to a broker. Commissions have declined through the years, and leading up to the Fall of 2019, typical charges were on the order of $5.00 per trade.[6] An *execution cost* is the difference between the true value of the shares and the transaction price. Unlike a commission, an execution cost is a variable cost that changes with the number of shares in the transaction, and it could be positive or negative depending on a number of factors such as the trader's desire for immediate execution. Execution costs are far less visible to traders than commissions are since common execution cost estimates require additional real-time information such as bid and ask prices at the time of order submission.

We illustrate the relative magnitudes of commissions and execution costs with a simple example. Suppose a retail trader decides to buy 250 shares of XYZ with immediate execution (i.e., the trader submits a "market order"). The trader's broker charges a fixed commission of $5.00 per trade, and at the time of the order, the bid and ask prices for XYZ are $29.97 and $30.03, respectively. Suppose further the trade fully executes at a price of $30.02 per share. Using the midpoint between the bid and ask price, or $30.00, as an estimate for the stock's true value, the execution cost (also called the effective half-spread) is $0.02 per share. This trader's total transaction cost for the order, therefore, is $5.00 + (250 × $0.02) = $10.00. In this

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[6] For example, Charles Schwab charged its retail clients $4.95; TD Ameritrade charged $6.95 per trade.

simple example, the commission and execution cost each comprise half of the total cost of the trade, but the trader would only immediately observe the commission, which would appear as a debit to her brokerage account. And due to its variable nature, the relative importance of the unobserved execution cost would be greater for larger trades.

The transaction price and, hence, the execution cost of any trade depend critically on how a broker routes client orders. When a retail investor submits an order resembling the one in the example above, her broker likely sells the order to a third-party market maker such as Citadel or Virtu rather than sending it to a stock exchange like the NYSE or Nasdaq.[7] The rebate that the executing market maker pays, also known as payment for order flow, is typically a few cents per 100 shares. For a real-world example of the magnitudes, E*TRADE received 20 cents per 100 shares for market orders it routed to Citadel Securities in January 2020. The brokerage industry justifies the practice of selling client orders because market makers offer marginally better prices than current exchange quotes through a process called price improvement.

Payment for order flow arrangements have long been a contentious subject. Market makers who pay broker-dealers for their retail order flow take the opposite side of orders aiming to earn the spread. The costs these market makers incur include the order flow payments sent to brokers and the price improvement they offer to traders. While these costs may be fungible to a market maker, their relative allocation is not without consequence, giving rise to an agency conflict between brokers and their retail clients. Order flow payments, which are negotiated between brokers and market makers, contribute directly to brokers' profits, while price improvement reduces the execution costs borne by traders. Simply put, brokers' incentive to maximize profit may stand in conflict with their duty to route orders in a manner that ensures best execution for their clients (Angel, Harris, and Spatt, 2011).

While the agency problem affects retail investors directly, payment for order flow arrangements might generate damaging externalities for financial markets more broadly. Market makers are willing to pay broker-dealers for their retail order flow that is roughly balanced between buys and sells or otherwise

[7] 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.

uncorrelated with future price movements so they can easily and quickly fill orders while reducing carry risk. Insofar as routing retail orders to market makers syphons a meaningful mass of uninformed trading interest, the liquidity pools that stock exchanges offer to all market participants may diminish. Moreover, fewer opportunities to interact with uninformed liquidity may discourage the posting of limit orders on exchanges and further hinder price discovery.

Empirical evidence concerning the inherent conflict of interest associated with payment for order flow and commensurate market outcomes informs ongoing discussions between industry participants and policymakers. At this time, there is no strong consensus to date on whether PFOF practices benefit retail investors compared to counterfactual world in which such payments are disallowed. For example, Battalio (1997) studies PFOF arrangements whereby Bernard L. Madoff Investment Securities (Madoff) in 1991 began offering brokers one cent per share for the right to trade against small retail orders. While he shows that Madoff provides significantly costlier executions than comparable trades on the NYSE (his Table III), the differences are somewhat small and retail traders could still benefit from Madoff's presence if brokers pass along a non-trivial share of the order flow payments through some channel such as reduced commissions. Battalio, Jennings, and Selway (2001) study brokers' interactions with another market maker, Knight Securities, in the mid-1990s. They find that trading costs for clients of brokers who engage in PFOF are not dominated by a benchmark broker who does not engage in the practice. More recently, 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.[8]

Research that considers how PFOF arrangements create externalities affecting overall market quality offers mixed messages as well. Easley, Kiefer, and O'Hara (1996) find that purchased order flow that executes on the Cincinnati Stock Exchange contains less information than similar orders that execute on the NYSE. They argue this selective off-exchange execution of uninformed orders, or "cream skimming", leaves informed orders to execute on exchange and hurts overall market quality because all

[8] 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.

prices are derived from exchange quotes. In contrast, Battalio (1997) finds that trading costs did not increase when Madoff began purchasing order flow and argues third-party execution venues function as cost competitors rather than cream skimmers. 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 benefits 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 lowered effective spreads via a reduction in price impact.

II.b. Zero-commission retail trading

In March 2015, upstart retail broker Robinhood foreshadowed an industry transformation by offering commission-free stock trading through its mobile phone app. Subsequently, Robinhood's total number of accounts increased from 300,000 in 2015 to over 5 million in 2019 as the company gained substantial market share.[9] In the Fall of 2019, several industry stalwarts finally followed suit by eliminating commissions for their retail clients as well. On October 1, 2019, Charles Schwab announced it would cut commissions from $4.95 per trade to zero on all retail trades starting on October 7th. Only hours later, TD Ameritrade announcing it too would cut commissions to zero from $6.95 beginning on October 3rd. Within the coming days, weeks, and months, other large brokers acquiesced as well. 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 2nd.

While the brokerage industry touted the change as a victory for retail investors, regulators and various advocacy groups quickly pushed back. For example, SEC Chairman Gary Gensler remarked "Our markets have moved to zero commission, but it doesn't mean it's free. There's still payment underneath

[9] See Robinhood's Form S-1 filed with the Securities and Exchange Commission on July 1, 2021: https://www.sec.gov/Archives/edgar/data/1783879/000162828021013318/robinhoods-1.htm.

these applications. And it doesn't mean it's always best execution."[10] The ensuing discussion then shifted squarely to the societal value of payment for order flow arrangements. We provide more detail below regarding various views in this debate. For now, we note two overarching points. First, moving to zero commission trading does not create a new agency problem. Rather, it simply thrust existing conflicts back into the spotlight. Second, as emphasized by Batallio, Jennings, and Selway (2001), PFOF's net effect on retail clients depends not only on execution costs but on the portion of order flow payments that brokers pass through to them. If one views a commission cut at a mechanism of passing order flow payments along to clients, then these traders could potentially benefit as long as any increase in execution cost is more than offset by commission reductions.

The stock market's reactions to the various commission cuts are informative. After Schwab's announcement, its stock closed down about 10% as the firm noted the cut would eliminate about $90 to $100 million in quarterly revenue. Schwab's competitors also suffered, 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."[11] Additional news and industry experts largely recognized this move as unexpected and the next big step in the price war among retail brokerages. A preliminary interpretation of these stock price changes is that the zero-commission shift represents a reduction in deadweight transactions costs and, hence, signifies a wealth transfer from brokers to retail clients.

II.c. Recent PFOF policy discussions

The U.S. House Financial Services Committee held hearings in the wake of the extreme volatility and trading halt of GameStop from early 2021. Testimony and subsequent commentary scrutinizing the PFOF model succinctly summarize the disparate viewpoints. For example, Dennis Kelleher, CEO of Better

[10] "SEC chief Gensler says regulator assessing future of payment for order flow." By Thomas Franck, CNBC (October 19, 2021). https://www.cnbc.com/2021/10/19/sec-chief-gensler-says-regulator-assessing-payment-for-order-flow.html.

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

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.[12] Other concerns were also raised about PFOF. First, PFOF models entrench dominant HFTs that execute most retail orders, leaving markets vulnerable to disruptions if something were to happen to large market makers such as Virtu Financial or Citadel Securities. Second, exchanges are limited in their capacity to compete for retail order flow due to the private negotiation between wholesalers and brokers and the nuances of sub-penny pricing. Opponents argue that this lack of competition leads to segmentation that disrupts capital formation, price discovery, and useful capital allocation.[13] Finally, price improvement, used as a justification for wholesaler execution, does not accurately reflect cost savings to retail investors. Sal Arnuk of Themis Trading LLC testified that price improvement is flawed because it is based off a slower price feed (the SIP) and it does not take into account odd-lots or hidden liquidity inside the quote. Furthermore, Arnuk argued that the NBBO reference price is set by the same HFT market makers providing price improvement in the off-exchange environment, which suggests possible manipulation.[14]

Others defended the practice and offer a positive perspective of PFOF arrangements. Industry representatives such as Virtu Financial CEO Doug Cifu and Citadel Securities founder Ken Griffin argued 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.[15] In expert testimony, Dr. Vicki Bogan noted that PFOF business models do reduce a significant market friction that historically inhibited access to financial markets for retail investors.[16] From a market quality standpoint, proponents argued that PFOF models lower

[12] "Robinhood business model under fire at GameStop hearing in Congress" by Chris Matthews, MarketWatch,March 17, 2021.

[13] For discussion on these PFOF concerns, see Michael Blaugrund and Dennis Kelleher testimony from "Game Stopped? Who Wins and Loses When Short Sellers, Social Media, and Retail Investors Collide? Part II" on March 17, 2021. www.financialservices.house.gov.

[14] *See* Sal Arnuk testimony from "Game Stopped? Who Wins and Loses When Short Sellers, Social Media, and Retail Investors Collide? Part II" on March 17, 2021. www.financialservices.house.gov.

[15] "Wall Street Pushes Back as SEC Targets Business Practice That Generates Billions" by Paul Kiernan, Wall Street Journal, November 8, 2021.

[16] *See* Vicki Bogan testimony from "Game Stopped? Who Wins and Loses When Short Sellers, Social Media, and Retail Investors Collide? Part II" on March 17, 2021. www.financialservices.house.gov.

costs to retail investors. Not only are upfront commission costs eliminated, but market makers are more confidently able to execute trades without fear of informed orders. Market makers do not want to trade against informed institutional orders. If forced to combine retail and institutional orders, every trader would receive an average price due to adverse selection, much like in Akerlof (1970), and this would result in retail trading costs increasing and institutional trading costs decreasing. Ultimately, market makers are indifferent between PFOF and price improvement, and proponents suggest the elimination of PFOF would result in higher retail execution costs and the elimination of free commissions and fractional trading.[17]

Responses from Congress during the hearings were split along party lines with most Republican committee members calling for less government interference and most Democratic committee members calling on regulators to require greater disclosures or consider banning the practice altogether. During his testimony in May of 2021, SEC Chairman Gary Gensler recognized that PFOF models allow wholesalers to get valuable information from retail order flow that other market participants get with a delay and that other Western countries have banned the practice altogether (Canada and United Kingdom).[18] More recently, SEC Chairman Gary Gensler has stated publicly that the possibility of banning payment for order flow is “on the table”.[19]

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

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[17] *See* Alan Grujic testimony from “Game Stopped? Who Wins and Loses When Short Sellers, Social Media, and Retail Investors Collide? Part II” on March 17, 2021. www.financialservices.house.gov.

[18] *See* Gary Gensler testimony from “Game Stopped? Who Wins and Loses When Short Sellers, Social Media, and Retail Investors Collide? Part III” on May 6, 2021. www.financialservices.house.gov.

[19] “SEC Chairman Says Banning Payment for Order Flow is ‘On the Table’ by Avi Salzman, Barron’s, August 30, 2021.

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.[20]

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 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.

For all stocks meeting the sample criteria, we obtain trade-level observations from TAQ, and we compute retail trading proxies following BJZZ.[21] 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,

[20] "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.

[21] 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.

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 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.[22] 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).[23] 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 BJZZ measure as "retail trades" for brevity with all caveats in mind.

We present summary statistics in Table I. Panel A contains results for the filtering variables described above. The interquartile ranges for December 2018 market capitalization and price are \$519 Million to \$4.7 Billion and \$15.03 to \$57.02, 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

[22] 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.
[23] We consider the extent to which unobserved trades that execute at the NBBO affect our inferences in Section V below.

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.

III.b. Execution costs

We 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.[24] When aggregating across trades, we always compute share-weighted averages. We present market wide, exchange, and retail 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 based on all trades 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%*) according to the NBBO and 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 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.

[24] When buy trades (sell trades) as identified by BJZZ receive sufficient price improvement that they execute below (above) the quote midpoint, the BJZZ and Lee and Ready (1991) algorithms will disagree on the trade sign. For these cases, we rely on BJZZ. These disagreements represent about 24% of trades and 17% of shares traded in our data. Kothari, Johnson, and So (2021) make a similar point and note the Lee and Ready (1991) algorithm misclassifies the trade direction of over 20% of the trades in their proprietary data obtained from Robinhood.

Doing so makes the magnitudes of effective spread and quoted spread variables directly comparable to one another.

We observe in the summary stats that quoted spreads exceed effective spreads 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 C is 0.14% while the median *effective* spread is 0.08%, or approximately 40 percent smaller. This point is particularly relevant to our analysis for two reasons. First, insofar as the NBBO represents a benchmark price for determining best execution at the time of trade, effective spreads for retail investors could increase and still be deemed as acceptable by regulatory standards. Second, since executions on average and irrespective of the trader's identity 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 latter issue in Section V below.[25]

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 15 seconds as recommended in Conrad and Wahal (2020) for this time

[25] 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".

period. We note, however, that our results change very little when we compute realized spread and price impact over longer horizons such 60 seconds and five 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 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 × 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 irrespective of commissions. 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 they 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:

$$Y_{it} = \beta_1 Retail_{it} + \gamma_i + \delta_t + \varepsilon_{it}, \quad (4)$$

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 one for retail trading observations. We also include stock fixed effects (γ_i) and day × 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 order flow to wholesalers generates 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).[26] But on the other hand, diverting these orders to third-party venues prevents retail traders from accessing undisplayed liquidity inside the quote that rests on public stock 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.[27] 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.

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[26] Note cost competition and cream skimming may have different effects on overall market quality (Easley et al.,1996; Battalio, 1997).
[27] 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.

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 40% cheaper than similar exchange trades. For medium sized 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 separately 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 (0.184 – 0.064 =) 0.120% 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 =) 0.046%. Therefore, this trader would incur an execution cost of about \$1.73 for a single 250-share trade in a \$30 stock. Prior to October 2019, brokers typically charged retail trades commissions of less than \$5 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 are 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 (0.018 + 0.033 =) 0.051%. The realized spread for medium-sized retail trades is (0.017 + 0.013 =) 0.03%. 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 is not 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 0.103% for odd-lot trades to 0.205% 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 spotlights order flow payments as a primary revenue source for many retail brokerage firms and motivate a reexamination of retail execution costs in the new regime.[28] 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

[28] 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.

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 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.[29] 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:

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

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.[30] Focusing first on the Post-Zero Period for the "close-in" analysis, our primary interest is the coefficient estimate for the *Retail* × *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* × *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* × *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 negative. However, highlighting only the sign

[29] "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.

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

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.

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* × *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* × *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.[31] 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.[32]

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.

[31] 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.
[32] 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.

And even if the direction of savings conveyed by reported price improvement statistics is correct, their magnitudes remain of high order importance.

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 typically receive four times as much price improvement for customers than they do payment for order flow for themselves—i.e., there exists an "industry standard" 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, resulting in an approximately 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."[33] Thus, the bottom line is that the magnitude of price improvement matters to regulators.

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

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

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

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

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 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.

The estimated intercepts are also interesting in light of the NYSE's Retail Liquidity Program (RLP) in which liquidity providers may quote dark limit orders that are available only to retail traders. One could envision this setting as another potential counter-factual for the retail trades in our sample; rather than routing orders to PFOF venues, brokers could potentially utilize the RLP for their clients. While we do not have detailed data on which trades executed via the RLP, NYSE publications reveal these trades receive an average price improvement of 0.020%.[34] Thus, retail trades executing on exchanges receive no better price improvement than the average exchange trades as indicated by our intercepts in Table VI.

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 0.043% 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

[34] Data as of Q3 2021 according to "The New Your Stock Exchange's Retail Liquidity Program Fact Sheet" available at https://www.nyse.com/publicdocs/nyse/markets/liquidity-programs/RLP_Fact_Sheet.pdf.

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 $1.73 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, and these reports are the primary lens through which retail traders can gauge their execution costs.[35]

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.

[35] 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 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.[36] 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.

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:

$$EHS = QHS - Improve. \tag{8}$$

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

$$EHS_r - EHS_e = QHS_r - QHS_e - (Improve_r - Improve_e), \tag{9}$$

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 (9), we see 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.

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

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,

[36] Interactive Brokers is an interesting anecdote. The firm offers two different plans for retail traders: IBKR LITE charges zero commissions and routes orders to PFOF venues; IBKR PRO charges commissions on a tiered structure and gives traders access to the firm's smart order routing technology.

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 execute 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 the inference that retail traders achieve as good or better executions than comparable exchange trades. 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:

$$ES_r(p) + QS_r(1-p) = ES_e \tag{11}$$

Solving for p, we have:

$$p = \frac{ES_e - QS_r}{ES_r - QS_r} \tag{12}$$

For each trade size bucket, we then insert the base estimates from Tables III and VII into (12) 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: 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 as a percentage of the midpoint for exchange (retail) traded shares. The scale for effective spread appears on the right axis.

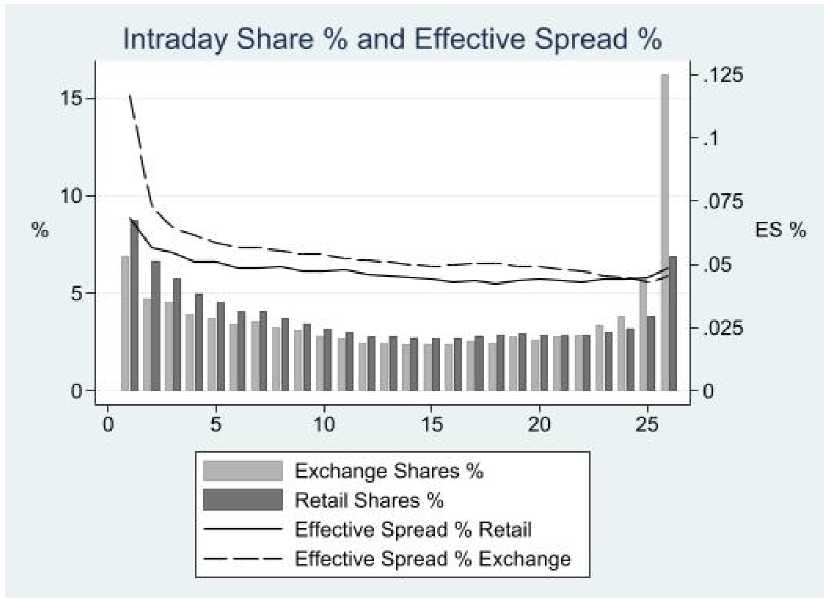

Figure 2: Retail Turnover

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.

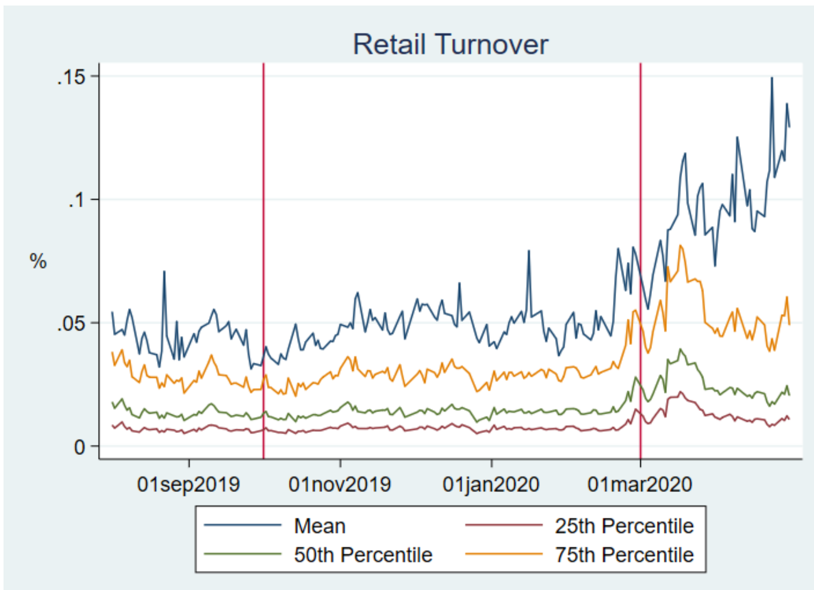

Figure 3: Absolute Imbalance 5-Day Moving Average

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 blue line represents the retail absolute imbalance. 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.

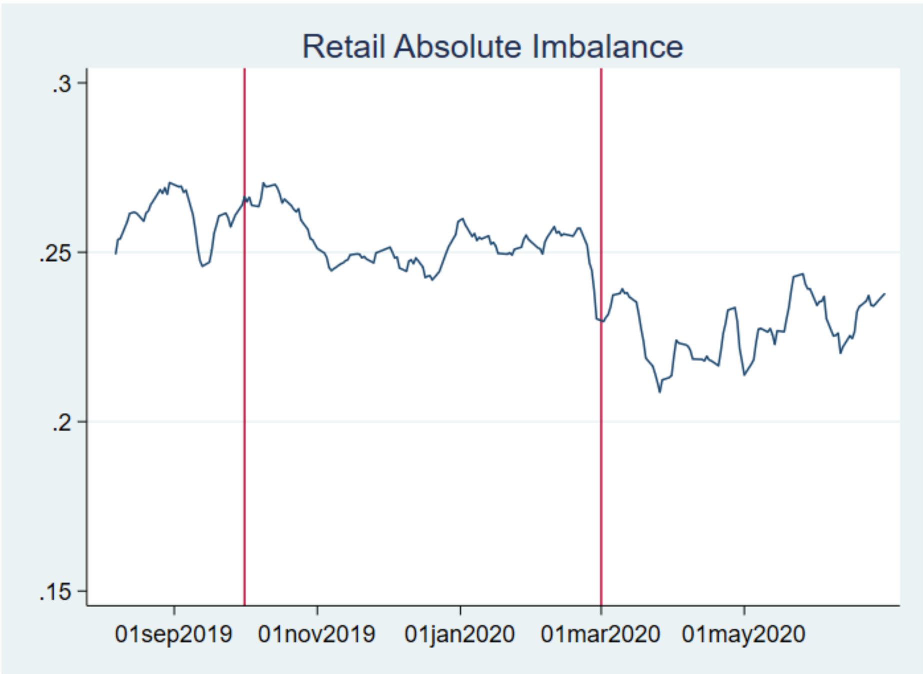

Figure 4: Execution Differences

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.

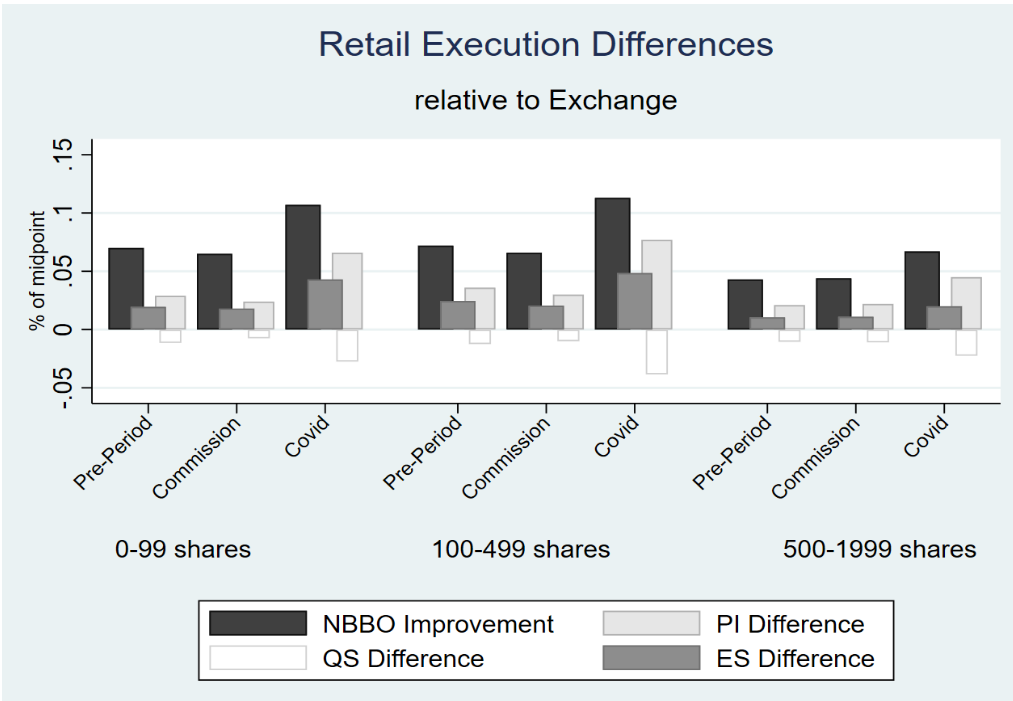

Figure 5: Execution Breakeven

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.

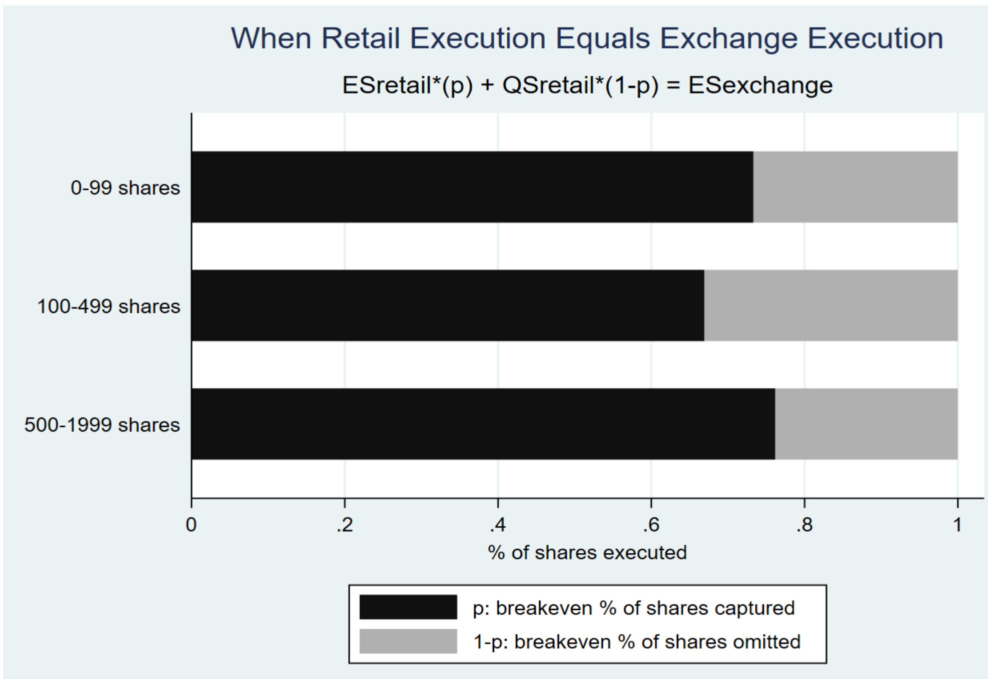

Table I: Descriptive Statistics

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 exchange 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. Realized spread and price impact are calculated using the prevailing NBBO quote 15 seconds after a trade.

	Mean	Median	Std.	Min	P5	P10	P25	P75	P90	P95	Max
Panel A: Stock Characteristics											
Market Capitalization ($Millions)	9,330	1,511	34,999	13	146	231	519	4,749	16,502	39,509	780,362
Price	47.17	29.61	59.70	5.00	6.76	8.61	15.03	57.02	100.85	143.00	838.34
Turnover %	0.58%	0.32%	1.44%	0.00%	0.02%	0.04%	0.14%	0.61%	1.18%	1.80%	42.36%
Average July 2019 Retail Trades	302	92	874	5	9	14	35	243	620	1,105	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%	8.32%	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: 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%	3.49%
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%	2.06%
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 %	0.07%	0.02%	0.15%	-0.42%	-0.01%	0.00%	0.00%	0.06%	0.21%	0.39%	1.85%
Realized Spread $	0.02	0.00	0.05	-0.10	0.00	0.00	0.00	0.02	0.06	0.11	0.63
Price Impact %	0.08%	0.05%	0.09%	-0.16%	0.01%	0.02%	0.03%	0.10%	0.18%	0.25%	1.02%
Price Impact $	0.03	0.02	0.04	-0.04	0.00	0.01	0.01	0.04	0.07	0.10	0.37

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 15-second 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***
	(240.24)	(193.01)	(211.79)
Observations	2,926,224	2,772,038	785,022
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	-0.055***	-0.064***	-0.085***
	(-70.68)	(-64.28)	(-29.25)
Constant	0.064***	0.080***	0.101***
	(164.18)	(161.17)	(69.15)
Observations	2,926,224	2,772,038	785,022
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.210	0.246	0.210
Panel C:	Dependent Variable = *Realized Spread as a Percent of Midpoint*		
	0-99 Shares	100-499 Shares	500-1999 Shares
	(1)	(2)	(3)
Retail	0.018***	0.017***	0.064***
	(21.25)	(21.67)	(24.03)
Constant	0.033***	0.013***	-0.027***
	(76.60)	(33.52)	(-20.55)
Observations	2,926,224	2,772,038	785,022
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.244	0.131	0.073

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 15-second price impact as a percent of midpoint. Panel C presents the results for the 15-second 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, 2018. 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)	(-43.45)	(-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)	(189.28)	(112.16)	(126.27)
Observations	1,084,458	1,091,542	750,224	1,126,732	941,270	704,036	244,136	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	0.410	0.292	0.503	0.418	0.286	0.416	0.466	0.320	0.443

Table III (continued): Retail Execution and Firm Size

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	-0.086***	-0.047***	-0.023***	-0.102***	-0.048***	-0.024***	-0.171***	-0.072***	-0.028***
	(-71.79)	(-65.51)	(-44.39)	(-68.42)	(-63.16)	(-45.71)	(-31.93)	(-25.73)	(-27.79)
Constant	0.103***	0.053***	0.026***	0.131***	0.059***	0.028***	0.205***	0.083***	0.032***
	(171.27)	(147.54)	(97.27)	(175.00)	(153.39)	(108.39)	(76.65)	(59.56)	(63.50)
Observations	1,084,458	1,091,542	750,224	1,126,732	941,270	704,036	244,136	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	0.193	0.204	0.236	0.228	0.205	0.189	0.203	0.145	0.093

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	0.026***	0.017***	0.009***	0.023***	0.015***	0.008***	0.129***	0.053***	0.020***
	(12.45)	(23.35)	(25.93)	(13.36)	(22.54)	(28.99)	(23.80)	(19.70)	(20.87)
Constant	0.076***	0.012***	0.002***	0.033***	-0.000	-0.002***	-0.050***	-0.027***	-0.009***
	(71.85)	(32.42)	(13.01)	(38.94)	(-1.13)	(-13.71)	(-18.46)	(-20.14)	(-19.30)
Observations	1,084,458	1,091,542	750,224	1,126,732	941,270	704,036	244,136	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	0.222	0.077	0.113	0.122	0.033	0.063	0.101	0.064	0.052

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 15-second price impact as a percent of midpoint. Panel C presents the results for the 15-second 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)	(16.96)	(-2.07)
Retail x Covid	-0.047***	-0.048***	-0.019***
	(-38.46)	(-39.36)	(-16.72)
Constant	0.141***	0.131***	0.109***
	(263.91)	(217.36)	(241.78)
Observations	9,239,994	8,635,624	2,671,854
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	-0.055***	-0.064***	-0.085***
	(-70.68)	(-64.29)	(-29.25)
Retail x Post	0.007***	0.008***	-0.001
	(21.10)	(19.29)	(-0.78)
Retail x Covid	-0.067***	-0.066***	-0.077***
	(-72.88)	(-60.89)	(-26.65)
Constant	0.090***	0.110***	0.144***
	(190.36)	(185.70)	(85.24)
Observations	9,239,994	8,635,624	2,671,854
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.198	0.222	0.175

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

Panel C:	Dependent Variable = *Realized Spread as a Percent of Midpoint*		
	0-99 Shares	100-499 Shares	500-1999 Shares
	(1)	(2)	(3)
Retail	0.018***	0.017***	0.064***
	(21.25)	(21.67)	(24.03)
Retail x Post	-0.004***	-0.000	-0.000
	(-7.71)	(-1.05)	(-0.32)
Retail x Covid	0.021***	0.018***	0.056***
	(17.02)	(16.97)	(21.41)
Constant	0.049***	0.019***	-0.035***
	(104.29)	(43.56)	(-25.00)
Observations	9,239,994	8,635,682	2,671,854
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.201	0.099	0.055

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***
	(-0.17)	(18.26)	(20.60)	(10.40)	(23.91)	(24.32)	(-4.00)	(3.89)	(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)	(-26.82)	(-27.13)	(-21.42)	(-10.99)	(-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)	(226.60)	(152.95)	(143.53)
Observations	3,656,168	3,365,310	2,218,516	3,602,486	2,927,192	2,105,946	873,516	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***
	(-16.20)	(-21.72)	(2.60)
Retail x Covid	0.037***	0.041***	0.024***
	(53.29)	(52.25)	(30.35)
Constant	0.041***	0.036***	0.022***
	(131.64)	(90.98)	(54.10)
Observations	9,239,994	8,635,624	2,671,852
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**
	(-30.09)	(-19.53)	(2.52)
Retail x Covid	0.032***	0.037***	0.030***
	(56.80)	(59.03)	(29.39)
Constant	0.225***	0.205***	0.155***
	(1,021.50)	(756.43)	(357.62)
Observations	9,239,994	8,635,624	2,671,854
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.629	0.644	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

You Want a Better Price

The vast majority of market orders executed receive a price better than the nationally published quote.

Percentage of orders price improved

98.0%

Percentage of orders price improved:Executed market orders receiving price(s) better than the National Best Bid/Offer (NBBO) at the time of routing, divided by the total number of orders executed.

Note: Shares executed at market quote or better (%) is 98.1%

You Want to Save Money

Price improvement provides significant savings.

Net improvement per order

$1.86 (per 100 shares)

Net improvement per order: Net price improvement per share ($0.0186) in the order size range comprising the vast majority of our clients' market orders (1-1,999), multiplied by 100 shares. Total net price improvement by order will vary with order size.

Panel D: Vanguard

We strive to give you the best price on trades with:

11.64%

Effective over quoted spread*

Effective over quoted spread (E/Q) is the industry measurement for trade quality. The lower the percentage, the better. We're constantly working to give you the best price on trades, and those efforts are reflected in our low E/Q.

What this means

95%

Of Vanguard ETFs traded at midpoint**

A trade at the midpoint of the quoted spread is generally considered the best price available. We provided midpoint pricing on over 95% of Vanguard ETF trades.**

What this means

$1.92

Savings per 100-share order

Competitive trades add up to real savings. On average, investors would have saved $1.92 for a 100-share order compared to the National Best Bid and Offer (NBBO).** More shares mean more savings, with $19.20 for 1,000 shares.

What this means

*For market orders on the S&P 500 Index sizes 100-499 for the 12-month period ending December 31, 2020.

**For all marketable orders with a share size of 1-1,999.

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%