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How free is free? Retail trading costs with zero commissions

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ABSTRACT

We examine the economics that underlie retail trading costs around discount brokers' widespread adoption of zero commission trading in October 2019. Our analysis of participating brokers' Rule 606 filings and financial statements reveals little change in payment for order flow, which suggests brokers absorbed the cost of eliminating commissions in a competitive environment. We then perform a difference-in-differences analysis of effective spreads and report economically trivial changes in retail execution costs around the commission change. Finally, we assess the total trading costs of an aggregate retail portfolio compared to a host of counterfactuals. We find that following the zero-commission change, total retail transaction costs dropped substantially even under the extreme counterfactual that these traders pay exchange quoted spreads and receive zero price improvement. Our findings support the brokerage industry's claim that dropping commissions helped retail investors and should ease regulators' concerns to the contrary.

"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]

1. Introduction

Brokerage commissions have long been the most salient and easily observable transaction cost retail investors incur. As such, five US brokerage firms—Charles Schwab, TD Ameritrade, E*Trade, Ally Invest, and Fidelity—captured widespread attention when they introduced commission-free trading for their retail clients.[3] While the brokerage industry touted this shift as a triumph for retail investors, highlighting the reduced cost and increased accessibility to trading, regulators and various advocacy groups quickly raised concerns. As exemplified by SEC Chairman Gary Gensler's quote at the beginning of this paper, the illusory nature of "free trading" offered in a zero-commission environment obfuscates indirect trading costs (i.e., execution costs), and the underlying mechanisms that have allowed for the elimination of commissions do not guarantee the "best execution" prices promised by our regulatory regime.

In this paper, we study the economics surrounding transactions costs before and after the shift to zero-commission trading to provide a more complete picture of how the change affected retail investors. This analysis is important for two reasons. First, brokers' reliance on order flow payments as primary source of revenue in stock trading places pressure on the execution quality their clients receive. Rather than

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[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-future-of-payment-for-order-flow.html.

[3] TD Ameritrade Press Release and Charles Schwab Press Release on October 1st, 2019; E*Trade Press Release on October 2nd, 2019; Ally Press Release on October 4, 2019; Fidelity Press Release on October 10th, 2019.

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routing marketable retail orders to public stock exchanges, brokers typically direct them to wholesalers for off-exchange execution. In return, these wholesalers pay brokers a fraction of a penny per share for these orders (i.e., "payment for order flow" or "PFOF") and execute the trades at a price that is better than the National Best Bid or Offer (i.e., they offer "price improvement"). Regulators routinely express concerns that such arrangements harm traders as brokers, who determine where orders are routed, have a profit motive to maximize their PFOF. This incentive may conflict with the "best execution" obligation to their clients since order flow payments can offset price improvement, and because execution costs are less salient and often difficult to determine, the average investor may be left vulnerable to paying higher execution costs.[4]

Second, unlike a commission, an execution cost is not an "out-of-pocket" cost displayed on a trade confirmation or account statement. An investor who naively associates zero commissions with free trading may change their behavior and trade more (see., e.g., Constantinides, 1986)). Such an outcome is particularly alarming since Barber and Odean (2000) show that retail investors who trade the most frequently exhibit the worst investment performance, largely due to the transactions costs they pay. Whether zero commission trading ultimately benefits investors is ultimately an empirical question. On the one hand, eliminating commissions might decrease retail trading costs if the foregone commissions outweigh any increase in execution costs. On the other hand, brokers and wholesalers may manipulate order flow or renegotiate order flow payments to maximize broker revenue at the expense of price improvement and increase execution costs in a nontrivial manner.

We first analyze payments that wholesale execution venues sent to retail brokerage firms as disclosed in broker-level Rule 606 reports. For the zero-commission brokers with publicly available reports, we find that total order flow payments remained roughly flat during the final two quarters of 2019 that surrounded zero-commission event. These patterns continued throughout the year 2020, suggesting no short-term or long-term increase in payment for order flow to compensate for lost commission revenue. We find complimentary evidence in brokers' 10Q and 10 K income statement filings of these brokers. Surrounding the elimination of commissions in the Fall of 2019, every broker we analyzed reported a stark drop in revenue per trade, which includes both commissions and payment for order flow. Moreover, we obtain commissions and payment for order flow separately for three different brokers and report that in all cases, foregone commissions clearly drove the drop in revenue per trade while payment for order flow remained flat. This evidence, combined with large negative stock price reactions that brokerage firms experienced in the Fall of 2019, is consistent with brokers absorbing losses in a zero-commission regime.

We next turn to a formal statistical analysis of execution costs at the trade level using the retail trading proxy developed by Boehmer et al. (2021; henceforth BJZZ). Our estimates in the Base Period, which is the two months before brokers eliminated commissions, indicate a retail trader would incur a half-spread of about $2.37 for a single 200-share trade in a $30 stock. As commissions prior to October 2019 were typically around $5 per trade, a back-of-the-envelope calculation for this example $6000 order reveals round-trip transaction costs of ((2 × 2.37)/6000 =) 0.08 % in spreads and ((2 × 5.00)/6000 =) 0.17 % in commissions. These values are strikingly smaller than the estimates of 1 % in spreads and 3 % in commissions that Baber and Odean (2000) report from their sample in the 1990s, though the relative importance of spreads and commissions is similar. To add contemporaneous context, we benchmark our spread estimates with those from similar-sized trades that occur on exchanges in the same stock, day, and intraday trading interval. Given brokers' standard practice of routing marketable retail orders to wholesalers, we refer to these benchmark exchange trades as "institutional." We report that for all trade sizes and across market cap subsamples, retail trades have effective spreads that are between two and twenty percent smaller than those for comparable institutional trades.

Using a difference-in-differences framework, we compare the baseline retail versus institutional trade differences discussed above with the same differences for the period following the adoption of zero commissions. During the "Zero Period" consisting of November and December 2019, the retail spreads increase slightly for medium-sized trades and decrease small and large trades compared to their respective institutional benchmarks. For perspective on the magnitude of the increase for medium-sized trades, the half-spread on the hypothetical 200-share trade in a $30 stock referenced earlier would increase by only $0.06, or almost two orders of magnitude smaller than a typical commission. The salient results from this analysis are twofold: the drop in commissions dwarfs even the largest detectable spread increase; and retail trades still incur materially smaller spreads than comparable institutional trades.

The effective spread analysis suggests the elimination of brokerage commissions reduced retail trading costs in the average stock. But since investors who perceive a drop in transactions could end up trading more (see., e.g., Constantinides, 1986)), small increases in spreads could combine with large increases in trading activity to drive aggregate transactions costs even higher. We therefore aggregate retail transactions across stocks each day into a single hypothetical portfolio so that we can estimate the total amount retail traders spent, in dollars, both before and after the zero-commission change. We find that aggregate retail dollar volume and the total number of trades both increased under zero-commission trading. Perhaps not surprisingly since eliminating commissions provide greater cost savings for smaller trades, the average trade size fell and the fraction of retail trades representing odd lots increased. More importantly, we argue the magnitude of foregone commissions far surpassed the costs associated with the spreads paid for additional trading, and consequently, aggregate retail trading costs declined. Using spreads computed from the data, an assumed $5 per trade commission prior to October 2019, and zero commissions thereafter, we estimate a drop in aggregate trading costs from about $5.1 Million per day in the Base Period to less than $1.3 Million per day in the Zero Period. Furthermore, even considering the worst-case counterfactual in which all retail orders are sent to exchanges and trade at the quoted spread (i.e., no price improvement), aggregate trading costs visually fell under zero-commission trading.

For our final analysis, we turn to the accuracy of the cost savings measures reported by market centers and publicized by brokers that implicitly benchmark execution costs against quoted spreads. 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. 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 order receipt. Price improvement relative to the NBBO may overstate the true economic cost savings of an order for multiple reasons. First, the NBBO does not account for either hidden or odd-lot liquidity available on the exchanges within the quote. Second, effective spreads for trades on the exchanges are also generally smaller than quoted spreads so the NBBO is likely the wrong counter-factual for cost of trading. 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 clear. NBBO-based price improvement measures consistently overstate economic savings, in some subsamples by a factor of four or more.

We offer a host of contributions to the literature. First, our fresh, large-scale analysis of retail transaction costs informs industry groups and regulators who disagree about the net effect of zero-commission trading as our paper suggests significant cost savings for retail

[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.

investors. More broadly, the finding that retail trading costs in the current environment are but a fraction of those in the Barber and Odean (2000) era, along with recent papers documenting that retail trading imbalances predict the cross-section of future stock returns (see, e.g., BJZZ; Kaniel et al. (2008); Kelley and Tetlock (2013, 2017)), encourage further analysis of retail trading performance at the portfolio level as data availability allows. Even-Tov et al. (2022) make progress with their analysis of data from the international brokerage firm eToro. They find that around staggered commission drops, clients traded around 30 percent more without experiencing a decline in net performance.

Second, our analysis informs current regulatory policy discussions. We show that retail traders attain lower execution costs off-exchange than benchmarked institutional orders. In a contemporaneous working paper, Dyhrberg et al. (2022) draw similar conclusions to ours using data from SEC Rule 605 reports covering more than 70 execution venues. We view these finding as complimentary as the authors do not directly study the impact of zero commission trading and due to the nature of Rule 605 data, they do not include odd lot trades in their analysis. Together, these finding challenge the notion imbedded in the SEC's December 2022 proposal that forcing brokers to route retail orders to auctions run by the exchanges would benefit retail traders. Likewise, 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 changes in execution costs immediately following the zero-commission shift and over the succeeding year offer the alternative view that commission cuts serve as a mechanism by which brokers pass order flow payments back to their clients. This alternative view is also consistent with Schwarz et al. (2023), who find no link between execution cost variation and order flow payments in their experimental dataset of self-placed trades at several retail brokerages in the post-zero commission environment and Battalio and Jennings (2022), who show in proprietary data that even brokers who do not charge payment for order flow route marketable orders to wholesalers.

Third, our paper exposes shortcomings in how market makers and brokers disclose execution quality. Regulators should take heed to our finding that NBBO-based measures indicate economic benefits are several times what our benchmark analysis reveals. Policy makers could easily alter disclosure 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. Echoing our findings on NBBO-based measures of price improvement, Schwarz et al. (2023) find that price improvement overstates cost savings in their experimental setting. The Securities and Exchange Commission's adoption of the Market Data Infrastructure Rule in 2020 makes progress as it contains elements along both dimensions.[5] More recently, the Commission amended Rule 605 in 2024 to modernize the included metrics and require that large broker-dealers report execution quality statistics just as market centers currently do.[6]

Finally, our work contributes to the literature that analyzes retail trading in a zero-commission environment. The two most closely related papers are Jain et al. (2021) and Kothari et al. (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 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 et al. (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.

2. An initial analysis of payment for order flow

An agency problem between retail investors and their brokers drives concerns that these investors may achieve inferior execution quality on their trades (see, e.g., Angel et al., 2011). Specifically, brokers benefit from routing client orders to wholesalers that offer the highest payment for order flow, while their clients benefit from routing to venues that offer the most price improvement on executions. And since order flow payments and price improvement are fungible expenses for wholesale market makers, such payments can be "passed through" to retail investors in the form of higher execution costs. The elimination of retail trading commissions, which prior to 2019 represented a large swath of brokerage revenue, spotlights this agency problem and prompts fresh empirical questions about the costs retail traders ultimately bear. More formally, the agency hypothesis predicts an increase in order flow payments coupled with a rise in execution costs following the zero commission announcements.

We utilize data from brokers' Rule 606 disclosures to conduct an initial analysis of payment for order flow around the commission changes in 2019. While Regulation NMS stipulates that brokers use these disclosures to report payments they receive for order flow, two issues complicate a rigorous analysis. First, per regulation, brokers only maintain historical Rule 606 reports for a rolling three-year window, which limits data availability in the months surrounding the zero-commission event. Second, the lack of granularity in Rule 606 reports prior to 2020 only allows a highly aggregated before vs. after comparison around the adoption of zero commissions for the data that are available.

Six brokers eliminated commissions for retail stock trading in 2019: Ally Invest, Charles Schwab, E*Trade, Fidelity, TD Ameritrade, and Vanguard. We obtained historical Rule 606 data for Ally Invest, E*Trade, Fidelity, and Vanguard. The other two brokers, Charles Schwab or TD Ameritrade, no longer publicly display their Rule 606 data from 2019, and our attempts to obtain the data by contacting the companies directly, searching current online sources, and using the Wayback Machine were unsuccessful. We observe from the filings that neither Fidelity nor Vanguard accepted order flow payments before or after zero commission trading, so we limit the Rule 606 analysis to Ally Invest and E*Trade.

In Fig. 1, we display quarterly average order flow payments (in cents per 100 shares) for Ally Invest and E*Trade. As revealed in the Rule 606 filings, Ally Invest primarily routed orders to Citadel and Virtue, while E*Trade routed to Citadel, Virtue, and Two Sigma.[7] We report payments separately in the figure for each of these wholesalers. The 2019 data are from quarterly Rule 606 reports during that year, and we extend the figure through 2020 by aggregating the more granular monthly data available starting in January 2020.[8] The most salient feature of the figure is the lack of any discernible trend in the data. While we do note a

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

[6] See https://www.sec.gov/files/rules/final/2024/34-99679.pdf.

[7] For each broker, we only analyzed the wholesalers who were directed order flow throughout the entirety of our sample timeframe.

[8] For consistency with the 2019 reports, we aggregate the 2020 data by first estimating the number of shares executed at the venue/month/trade type/class level by using the total revenue and cents per 100 shares. Next, we calculate a share weighted average PFOF at each venue in each month. Finally, we compute the average PFOF for each venue in each quarter as a simple three-month average. The PFOF estimate includes market orders, marketable limit orders, limit orders, and other orders for both S&P 500 and non-S&P 500 stocks.

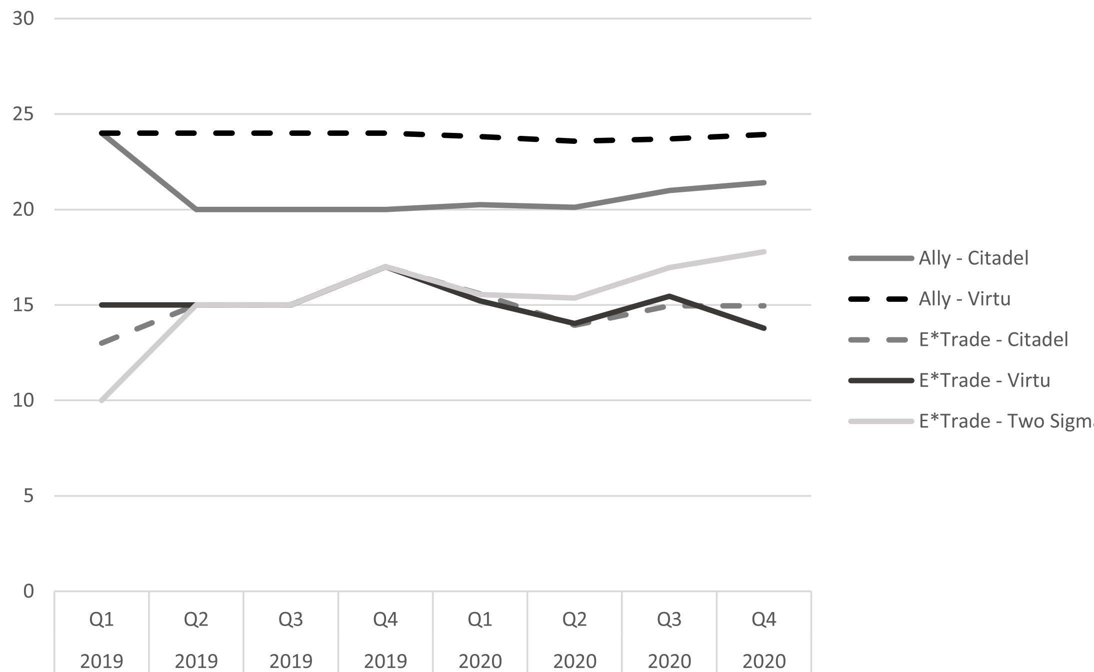

Fig. 1. Quarterly PFOF for Ally and E*Trade.
This Figure shows the average quarterly payment for order flow (PFOF) in cents per 100 shares. The data is taken from 606 reports filed by Ally Financial and E*Trade over 2019 and 2020. The dark grey solid line (black dashed line) shows the PFOF paid to Ally Financial from orders routed to Citadel (Virtu). The dark grey dashed line (solid black line) shows the PFOF paid to E*Trade from orders routed to Citadel (Virtu). The light grey solid line shows the PFOF paid to E*Trade from orders routed to Two Sigma. The 2019 data represents quarterly filings, while the 2020 data represents monthly filings.

slight and transient uptick for E*Trade in the second half of 2019, over the full two years, the lines are on average flat across the two brokers and three wholesalers.[9]

The magnitudes of order flow payments displayed in Fig. 1 are also noteworthy. These payments range between 10 and 24 cents per 100 shares. To put these numbers in perspective, payments for a 100-share order are between one-fiftieth and one-twentieth that of a typical commission prior to the event—for example, the commission would be on the order of five dollars per 100 shares. Thus, any increase in order flow payments sufficient to recover a meaningful fraction of commission revenue would generate an egregiously large increase in execution costs borne by traders. From the pictures displayed here, such an increase in order flow payments did not occur. We explore how execution costs changed in the next section.

Broker-level financials offer a complementary reporting of order flow payments. We therefore collect trading revenue data from the 10-Q filings all four brokers who accepted payment for order flow: Ally Invest, Charles Schwab, E*Trade, and TD Ameritrade. In Fig. 2 Panel A, we display these metrics from the first quarter of 2019 through the second quarter of 2020.[10] Like the Rule 606 data above, these data lack granularity. In addition to being broker-level aggregates, the data includes revenue related to other securities such as ETFs and options, and we therefore again interpret this evidence with caution. That said, we observe a clear shift in the second half of 2019. For each of the four brokers represented, revenue per trade drops sharply from Q3 2019 to Q4 2019. Comparing the fourth quarter of 2019 to the second quarter of the same year, the drop in revenue per trade ranges from $2.12 (Charles Schwab) to $6.51 (Ally Invest). Of the four brokers represented in Panel A, TD Ameritrade, Charles Schwab, and E*Trade report order routing revenue per trade separately from commissions. We display the quarterly values for those brokers Fig. 2 Panel B-D. We observe that for each one of these brokers, lost commissions drive the drop in revenue per trade, while order flow revenue per trade does not meaningfully change.

In sum, our broker-level analysis offers no evidence that order flow revenue somehow “offset” brokers’ substantial loss in commissions in the Fall of 2019. In contrast, brokers themselves appear to have internalized losses due to the increasingly competitive landscape for retail brokerage services. Broker-level stock price reactions around their announcements of zero-commission trading, consolidation amongst brokerage firms, and industry commentary bolster this interpretation. In the next section, we turn to a formal statistical analysis of execution costs.

3. Execution costs and zero-commission trading

In the previous section, we found no broker-level evidence in support of the agency hypothesis. We now consider the trader-level implication of the agency hypothesis that execution costs will rise in the zero-commission environment. This analysis addresses concerns expressed by regulators and industry advocates that retail traders do not receive best execution in the current environment. Moreover, it speaks more broadly to whether zero-commission trading provides a benefit to retail investors.

3.1. Data

Since current data sources do not explicitly identify retail traders, we

[9] In unreported analysis, we obtain 2020 monthly Rule 606 data for Charles Schwab and TD Ameritrade. Order flow payments for those brokers are also flat through 2020.

[10] No data are available for E*Trade or TD Ameritrade after the second quarter of 2020 due to the firms’ respective mergers with Morgan Stanley and Charles Schwab.

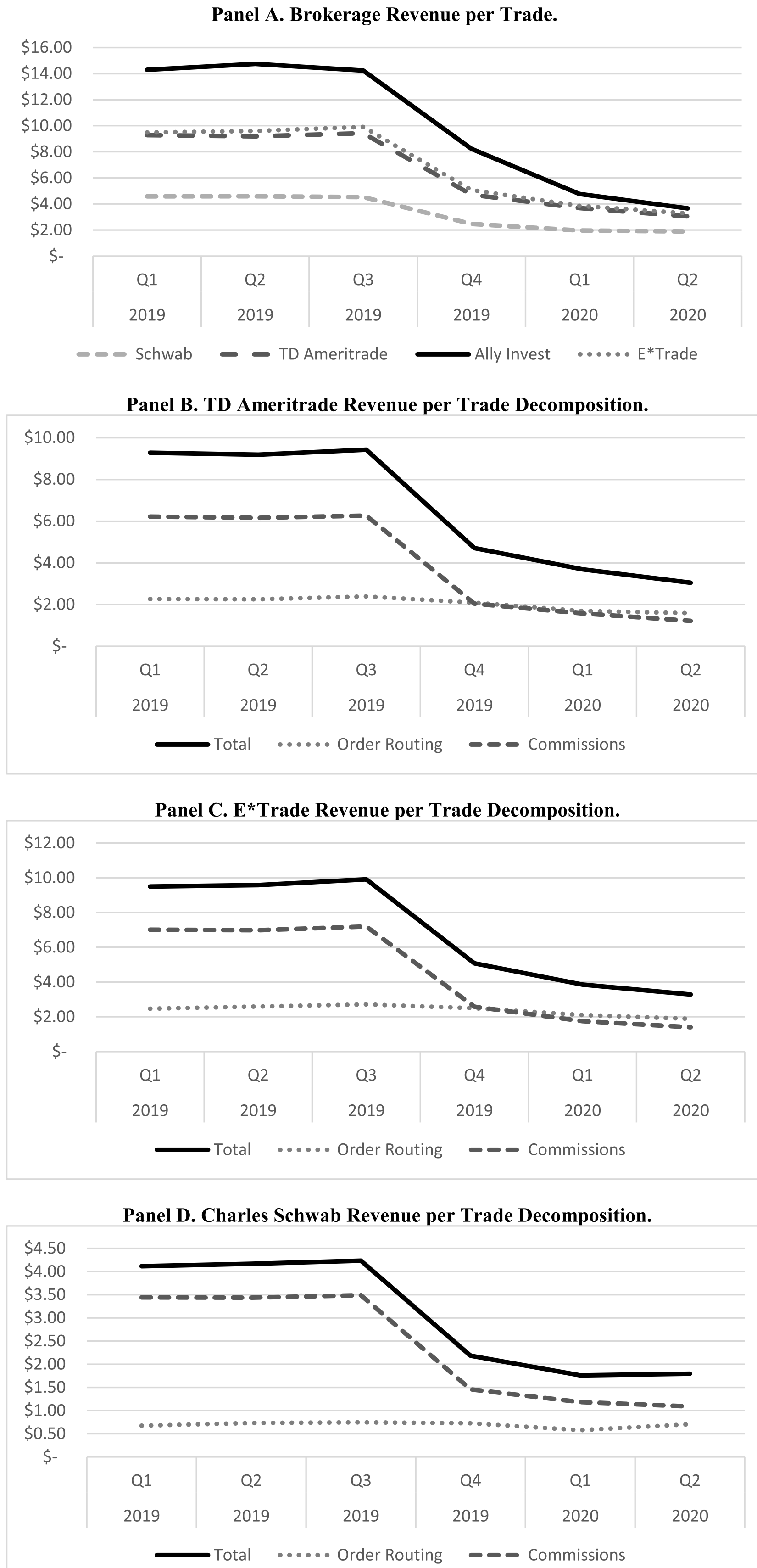

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Fig. 2. Brokerage trade revenue.
This Figure shows the revenue per trade for discount brokers that adopted zero commission trading. Revenue per trade statistics and decomposition are collected from brokerage annual and quarterly financial reports in 2019 and 2020. Panel A shows the revenue per trade reported by Charles Schwab (dashed grey line), TD Ameritrade (dashed black line), Ally Invest (solid black line) and E*Trade (dotted grey line). Panels B, C, and D show the decomposition of revenue per trade for TD Ameritrade, E*Trade, and Charles Schwab, respectively. The solid line represents revenue per trade, the dotted line represents the revenue from order routing per trade, and the dashed line represents the revenue from commissions per trade. Reported statistics can include stock, ETF, options, and other financial product revenue.

use BJZZ's proxy, which we compute using TAQ data.[11] Those authors label retail trades as executions occurring on Exchange Code "D" and having prices that depart from a whole penny by at least $0.0001 but no more than $0.0040.[12] 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 to 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. We sign "buys" and "sells" following the Barber et al. (2022) correction that uses the Lee and Ready (1991) algorithm.[13]

Several caveats accompany our use of the BJZZ method. First, the metrics only represent market (or marketable) orders. Second, they do not include any retail trades that occur on the exchanges, whether 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.[14] Third, they ignore trades that receive price improvement in whole-penny increments or execute at the quote midpoint as well as those receiving no price improvement at all.[15] 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 and 499 shares (Appendix III).[16] Finally, using proprietary wholesaler data, Battalio et al. (2022) find that some institutional trades print on non-mid-point sub-penny prices in violation of the BJZZ assumption that institutional orders trade only on penny or half-penny increments.[17] Henceforth, we refer to trades captured by the BJZZ measure as "retail trades" for brevity with all caveats in mind.

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 $1000. 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 during July 2019. The resulting sample contains 2384 stocks.

We present summary statistics in Table 1. Panel A contains results for the filtering variables described above. The interquartile ranges for December 2018 market capitalization and price are $512 Million to $4.7 Billion and $14.84 to $56.90, respectively, which indicates the bulk of our sample of 2384 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 August and September 2019, the two months prior to the zero-commission announcements. Importantly, retail investors play a non-trivial role in these firms' trading. For the median stock, the trades BJZZ label as retail account for 3.94 % of share volume and 2.58 % of trades. And for some stocks, retail investors are far more influential. The 90th percentile values are 11.18 % of share volume and 7.05 % of trades.

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. We sign all trades using the Lee and Ready (1991) algorithm. When aggregating across trades, we always compute share-weighted averages. We present market-wide summary statistics for percentage effective spread (*ES*%) and its unscaled counterpart, dollar effective spread (*ES*$), in Table 1 Panel C. These statistics confirm that our analysis focuses mostly on liquid stocks. The median effective spread based on all trades is 0.09 % of the quote midpoint and more than 95 % of all stocks have a spread 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. Doing so makes the magnitudes of effective spread and quoted spread

[11] 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.

[12] The BJZZ measure only considers marketable orders. While we recognize the omission of limit orders, retail investors tend to demand liquidity. For example, according to Charles Schwab's 2020 Q1 606 report, approximately 32% of all non-directed orders were non-marketable limit orders in January 2020.

[13] 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". Barber, Huang, Jorion, Odeon, and Schwarz (2022) show that procedure misidentifies the trade direction of about 30% of their retail trades. Our conclusions regarding changes around the zero-commission event are not sensitive to whether we sign trades according to the original BJZZ method or the Barber et al correction.

[14] 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.

[15] Barber et al. (2022) find that such misidentification of retail trades is most common among stocks with spreads above one penny due to retail trades in these stocks executing at subpenny midpoints and whole pennies even with price improvement. Our results are conservative as trades at brokers associated with greater price improvement are more likely to price at the subpenny midpoint.

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

[17] We note that data from Rule 605 disclosures suffer from similar concerns as those observations are reported at the execution venue level without separation of retail and institutional orders that execute on the same venue.

Table 1
Descriptive statistics.

	Mean	Median	Std.	Min	P5	P10	P25	P75	P90	P95	Max
Panel A: Stock Characteristics											
Market Capitalization ($Millions)	9275	1497	34,874	10	138	220	512	4732	16,393	39,301	780,362
Price	46.96	29.27	59.57	5.00	6.75	8.53	14.84	56.90	100.22	142.94	838.34
Turnover%	0.72 %	0.45 %	1.79 %	0.00 %	0.10 %	0.16 %	0.27 %	0.75 %	1.33 %	1.96 %	57.83 %
Average July 2019 Retail Trades	292	90	838	5	8	13	34	237	599	1076	15,381
Panel B: Retail Trading Statistics											
Retail Trades	578	111	3612	1	7	12	37	323	909	1816	114,549
Retail Turnover	0.06 %	0.02 %	0.35 %	0.00 %	0.00 %	0.00 %	0.01 %	0.04 %	0.09 %	0.16 %	13.17 %
% Daily Volume in Retail Shares	5.62 %	3.94 %	5.67 %	0.01 %	1.19 %	1.62 %	2.50 %	6.58 %	11.18 %	15.77 %	66.20 %
% Daily Volume in Retail Trades	3.63 %	2.58 %	3.67 %	0.14 %	0.86 %	1.12 %	1.67 %	4.16 %	7.05 %	9.90 %	45.64 %
Panel C: Execution Statistics (Market)											
Quoted Spread%	0.35 %	0.15 %	0.53 %	0.02 %	0.03 %	0.04 %	0.08 %	0.35 %	0.85 %	1.41 %	3.24 %
Quoted Spread $	0.11	0.05	0.17	0.01	0.01	0.01	0.02	0.12	0.26	0.41	1.17
Effective Spread%	0.21 %	0.09 %	0.34 %	0.01 %	0.02 %	0.03 %	0.05 %	0.21 %	0.52 %	0.87 %	2.07 %
Effective Spread $	0.06	0.03	0.10	0.01	0.01	0.01	0.01	0.07	0.15	0.24	0.68
Realized Spread%	0.10 %	0.02 %	0.26 %	−0.04 %	−0.01 %	0.00 %	0.00 %	0.07 %	0.28 %	0.57 %	1.65 %
Realized Spread $	0.03	0.01	0.07	−0.01	0.00	0.00	0.00	0.02	0.07	0.13	0.45
Price Impact%	0.10 %	0.06 %	0.12 %	0.00 %	0.02 %	0.02 %	0.04 %	0.12 %	0.22 %	0.33 %	0.75 %
Price Impact $	0.03	0.02	0.04	0.00	0.01	0.01	0.01	0.04	0.08	0.12	0.28

This table shows descriptive statistics from August and September 2019 (Base 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 and filtering variables of the 2384 stocks in the sample. Market Capitalization and price are calculated as of December 31st 2018. Panel B shows the trading statistics for retail trades. Panel C shows the execution statistics for the market. Trades are signed using the Lee and Ready (1991) method. 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 s after a trade.

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 0.15 % while the median *effective* spread is 0.09 %, or approximately 40 % 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 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.[18]

3.2. Developing appropriate controls

Any assessment of retail traders' execution costs requires a benchmark. Ideally, we would compare the execution costs retail traders incur in the current environment with a counterfactual cost they would pay if 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. Instead, 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. We refer to these benchmark trades as "exchange trades" and "institutional trades" interchangeably. 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.[19] 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 2000 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.

Since trading activity and spreads vary within the trading day (e.g., McInish and Wood, 1992), we aggregate observations to the 15-minute level, and we include date × intraday interval fixed effects. Fig. 3 illustrates this important intraday variation. The solid and dotted line in the figure represents percentage effective spreads from retail and exchange trades within each interval, respectively. 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. We believe this level of analysis gives our study advantages over those primarily relying on 605 data, which only presents monthly averages of execution statistics. 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.

3.3. Regression analysis

We now analyze execution costs around the zero-commission shift.

[18] 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".

[19] The 605 reports currently do not report odd lot trades, which underscores one main advantage of the BJZZ measure using TAQ. Odd lots represent a non-trivial amount of trading that 605 reports do not capture. For example, a 2021 CBOE study found that 54.8% of trades were odd lot. (https://www.cboe.com/insights/posts/an-in-depth-view-into-odd-lots/).

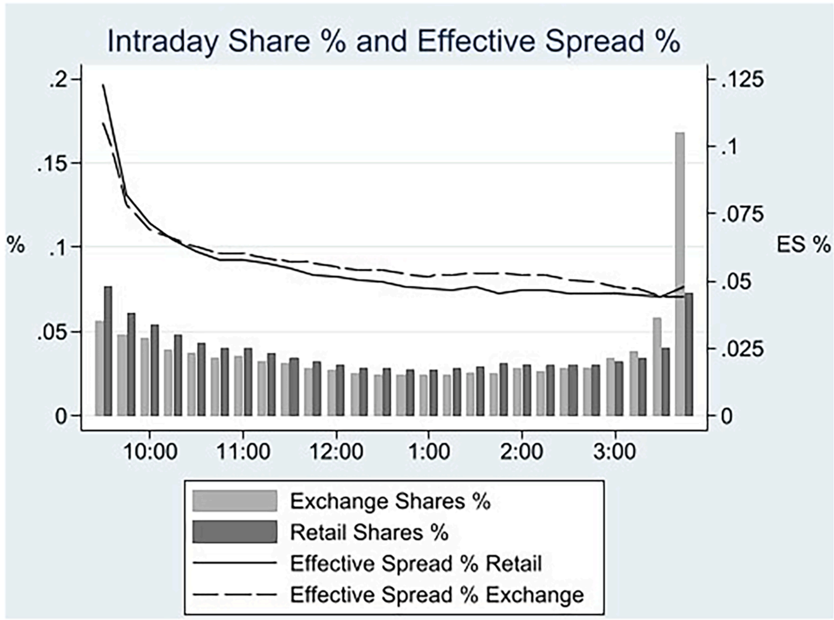


Fig. 3. Intraday trading.
This Figure shows the percent of retail and exchange shares traded during 15-minute trading intervals in August and September 2019 (Base Period). The light (dark) gray bars represent the percent of exchange (retail) shares traded in each 15-minute interval over the period. The 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.

We refer to the two months prior to the event, August and September 2019, as the "Base Period" and the two months following the event, November and December 2019, as the "Zero Period." We drop from the sample all observations from October 2019 as the relevant brokers eliminated commissions at various points within that month. We estimate the following fixed effect regression:

$$Y_{it} = \alpha_0 + \alpha_1 Retail_{it} + \alpha_2 Retail_{it} \times Zero + \gamma_i + \delta_t + \varepsilon_{it}, \quad (2)$$

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 × time interval—for example, the interval from 9:45AM to 10:00AM on August 12, 2019. Within each stock-date-time, we include one observation representing retail trading 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).

We report the results for an all-stock sample in Table 2, and those for small, medium, and large stocks separately in Table 3. The coefficient estimates for the *Retail* indicator (α_1), which reflect the difference between retail and exchange execution costs during the Base Period, offer initial 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. The economic magnitude of the costs savings is modest. For small-sized orders, the *Retail* coefficient reported in Table 2 is a statistically significant −0.4 basis points, indicating small retail trades receive executions that are about 4 % cheaper than similar exchange trades, which are represented by the intercept value of 9.5 basis points. For medium-sized orders, the cost savings is larger; the *Retail* coefficient of −1.4 basis points reflects a 15 % reduction from the intercept of 9.3 basis points.[20] Across the stock size subsamples analyzed in Table 3, the *Retail* coefficient for effective spread remains negative and statistically significant, and unsurprisingly, within each order size block, execution costs are typically larger for smaller stocks.

The coefficient magnitudes in Table 2 offer useful context for the economic importance of brokerage commissions. Estimates for medium-sized orders indicate a retail trader pays a round-trip effective spread of (0.093 – 0.014 =) 0.079 %. Therefore, this trader would incur an execution cost of about $2.37 for a single (one-way) 200-share trade in a $30 stock. Prior to October 2019, brokers typically charged retail trades commissions of about $5 per trade, so for this hypothetical order, execution costs would account for about one-third 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. At the same time, any potential increase in execution cost that would fully offset the trader's commission savings would likely be egregious by any standard.

The main coefficient of interest is α_2, which is akin to a difference-in-differences coefficient for retail trades in the Zero Period. Thus, a positive value for α_2 reflects an increase in retail execution costs relative to exchange execution costs in the Zero Period. The results are somewhat mixed. In Table 2, for both small and large order sizes, the *Retail* × *Zero* interaction coefficient is negative and statistically significant, while the same coefficient for medium-size orders is significantly positive. More importantly, highlighting only the sign and significance of these coefficients undermines the bigger picture our results convey. A quick comparison of the coefficient estimates with the magnitudes of foregone commissions illuminates the key message. Consider again the

[20] We also estimate the model separately using each of the effective spreads two components—price impact and realized spread—as the dependent variable. Consistent with retail order flow being less informed than institutional order flow, the *Retail* coefficient estimates (not reported) indicate smaller price impact and larger realized spreads for retail trades.

Table 2
Retail execution costs during zero commissions.

Panel A:	Dependent Variable = *Effective Spread as a Percent of Midpoint*		
	0–99 Shares (1)	100–499 Shares (2)	500–1999 Shares (3)
Retail	−0.004***	−0.014***	−0.009***
	(−11.58)	(−42.11)	(−12.04)
Retail x Post	−0.003***	0.002***	−0.004***
	(−9.47)	(7.63)	(−7.02)
Constant	0.095***	0.093***	0.083***
	(687.37)	(581.78)	(224.86)
Observations	5672,286	5382,386	1495,938
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.624	0.643	0.676
Panel B:	Dependent Variable = *Price Impact as a Percent of Midpoint*		
	0–99 Shares (1)	100–499 Shares (2)	500–1999 Shares (3)
Retail	−0.054***	−0.059***	−0.083***
	(−69.28)	(−61.62)	(−27.68)
Retail x Post	0.007***	0.008***	−0.002
	(20.95)	(18.03)	(−1.20)
Constant	0.061***	0.077***	0.106***
	(163.12)	(166.19)	(70.61)
Observations	5672,286	5382,386	1495,938
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.206	0.246	0.223
Panel C:	Dependent Variable = *Realized Spread as a Percent of Midpoint*		
	0–99 Shares (1)	100–499 Shares (2)	500–1999 Shares (3)
Retail	0.053***	0.048***	0.076***
	(57.85)	(57.74)	(28.39)
Retail x Post	−0.010***	−0.006***	−0.002
	(−22.24)	(−13.92)	(−1.23)
Constant	0.034***	0.015***	−0.023***
	(83.26)	(39.26)	(−17.48)
Observations	5672,286	5382,386	1495,938
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.400	0.279	0.115

This table presents regressions comparing 15-minute interval intraday execution quality between retail and exchange trades when commissions were cut to zero at the end of 2019. 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. 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 between 100 and 499 shares. Column 3 shows trades between 500 and 1999 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.

hypothetical single 200-share trade of a $30 stock, and note the sum of the alpha coefficients ($\alpha_0 + \alpha_1 + \alpha_2$) represents the round-trip execution cost for a retail trade. In the Zero Period, the cost of this trade would be ((0.093 – 0.014 + 0.002)/2 =) 0.020 %, or $2.43, as the commission is eliminated. While this trade's execution cost is a trivial six-cent increase from a comparable trade in the Base Period, the trader would no longer pay a commission, representing a cost savings on the order of five dollars.

In sum, the reduction in commissions dwarfs any change in execution costs reflected in the coefficient estimates reported in Table 2. This finding is inconsistent with the agency hypothesis articulated in Section II above, and it should temper concerns that the elimination of brokerage commissions harmed retail investors. Similar exercises of our estimates across all size buckets would reveal that retail investor trading costs almost universally decreased. We repeat the effective spread tests for small, medium, and large stocks in Table 3. Once again, we find that changes in effective spreads, while sometimes statistically significant, are again uniformly tiny in economic magnitude within subsamples based on firm size.

4. Aggregate retail trading

Our analysis up to this point suggests the widespread adoption of zero commission benefited retail investors. We find no substantive evidence supporting the agency hypothesis. Instead, the evidence is more consistent with competitive forces in the brokerage industry driving down trading costs in a manner that helped retail investors. From broker-level regulatory reporting and financials, order flow payments did not increase. In TAQ data, subject to the various caveats associated with the BJZZ proxy for retail trading, execution costs did not change materially. Nevertheless, we caution the reader against over-interpreting our results as evidence retail traders ultimately spent less on trading since investors who perceive a drop in transactions costs will likely trade more (see., e.g., Constantinides, 1986)). As such, small increases in spreads combined with heavy trading could drive aggregate transactions costs even higher. Second, retail trading is not uniformly distributed across stocks. For example, Barber et al. (2023) ???show that retail purchases concentrate in attention-grabbing stocks and fail to recover their cost of trading within the day of trade.

In this section, we aggregate retail transactions across stocks each day into a single hypothetical portfolio so that we can examine overall trader behavior and the resulting costs in the zero-commission regime. Using this aggregate portfolio, we track share and dollar volume, the number of transactions, and average trade size. More importantly, we estimate the total amount retail traders spent, in dollars, both before and after the zero-commission change compared to various counterfactuals. For context, we extend the analysis to two full years (2019 and 2020) to observe any changes during the rise of the Covid pandemic as well.

4.1. Aggregate trading activity

In Fig. 4, we display the time series of aggregate retail trading activity, both in dollars and number of trades. The figure indicates retail investors did, in fact, trade more after brokers eliminated commissions. Daily retail dollar volume, depicted by the dashed line, remains stable in 2019 until the commission event, and then it gradually rises in the final quarter of 2019 through the onset of the Covid pandemic in the first half of 2020. Highlighting our main test periods, average daily retail volume increased from $7.84 Billion in the Base Period to $8.94 Billion in the Zero Period. The subsequent increase during Covid is striking as total trading rose to over $16 Billion per day between March and May of 2020.

The number of retail trades, depicted by the solid line, exhibits a similar pattern. We observe a gradual increase under zero commissions from 752 thousand per day in the Base Period to more than two million per day during the Covid Pandemic.[21] These increases do not simply reflect a trend in overall trading volume; retail dollar volume as a percentage of total dollar volume (not reported) increases over these

[21] Due to the BJZZ measure's inability to identify retail trades that occur at or near the midpoint or trade at whole penny values, the trading volume and number of trade statistics almost certainly serve as lower bounds for the total amount of retail trading activity. Indeed, Barber et al (2022) report the sub-penny identification procedure labels only about 35%. Thus, one could arrive at a more reasonable approximation of total retail trading activity by multiplying the numbers in Fig. 4 by a factor of two to two and one-half.

Table 3
Retail execution costs by firm size during zero commissions.

	Dependent Variable = *Effective Spread as a Percent of Midpoint*								
	0–99 Shares			100–499 Shares			500–1999 Shares		
	Small (1)	Mid (2)	Large (3)	Small (4)	Mid (5)	Large (6)	Small (7)	Mid (8)	Large (9)
Retail	−0.004***	−0.004***	−0.004***	−0.025***	−0.008***	−0.005***	−0.015***	−0.008***	−0.004***
	(−4.36)	(−10.13)	(−18.74)	(−36.53)	(−26.37)	(−26.35)	(−8.74)	(−6.99)	(−7.85)
Retail x Post	−0.009***	−0.000	0.001***	0.002***	0.002***	0.002***	−0.008***	−0.003***	−0.000**
	(−10.99)	(−0.61)	(10.68)	(3.90)	(10.09)	(17.15)	(−5.25)	(−5.32)	(−2.24)
Constant	0.176***	0.061***	0.027***	0.165***	0.056***	0.025***	0.177***	0.060***	0.024***
	(563.60)	(334.41)	(241.23)	(529.90)	(392.15)	(290.16)	(203.77)	(99.43)	(107.98)
Observations	2124,534	2107,728	1440,024	2201,156	1829,254	1351,976	472,986	438,354	584,598
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	0.564	0.487	0.652	0.576	0.482	0.605	0.577	0.497	0.590

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. *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. Columns 1 through 3 show odd lot trades. Columns 4 through 6 show trades between 100 and 499 shares. Columns 7 through 9 show trades between 500 and 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.

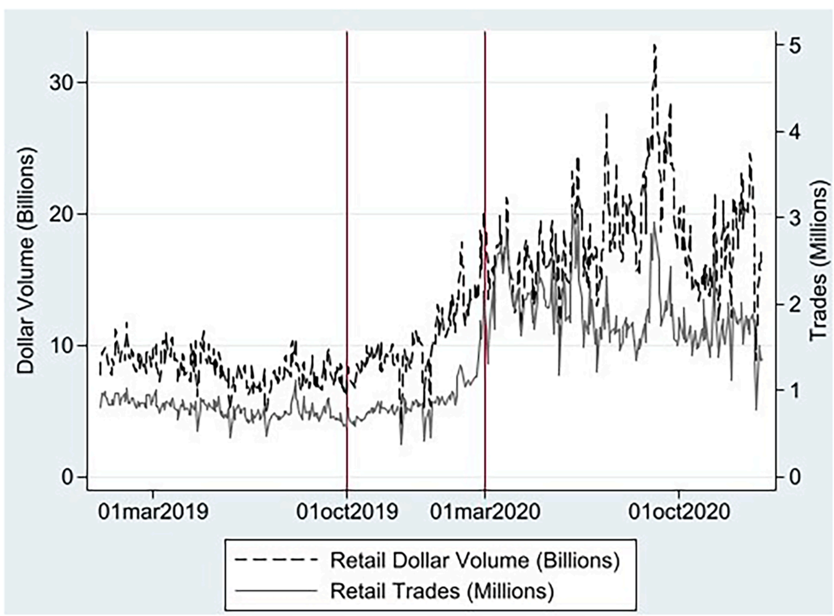


Fig. 4. Retail dollar volume and trades.
This figure shows the aggregate retail dollar volume from January 2019 to December 2020. The solid line represents the total retail trades in millions presented on the right axis. The dashed line represents the retail dollar volume in billions presented on the left axis. Dollar volume and trades statistics are calculated at the daily level for those stocks in our underlying sample and for trades between 1 and 1999 shares. 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.

periods as well. Others report similar findings using different datasets. Even-Tov et al. (2022) report an increase in retail trading around staggard commission cuts in international data; Ozik et al. (2022) infer a sharp rise in Robinhood trading during the Covid pandemic.

Since commissions account for a greater proportion of transaction costs for smaller trades, we expect the increase in trading to be particularly acute for small trades. The evidence indicates this is the case. In Fig. 5, we show that average retail trade size, depicted by the solid line, drops steadily throughout the Zero Period shifting from 148 shares during the Base Period to 122 shares during the onset of Covid. In that same figure, we report odd lot trades as a percentage of total retail trades. The retail odd lot percentage series, depicted in the dashed line, roughly mirrors that for average trade size—as average trade size falls, the relative frequency of odd lot trades rises.[22]

4.2. Aggregate trading costs

So how much did retail traders pay in transactions cost before and

[22] To the extent reported single-share trades in TAQ represent fractional share trades, the average trade sizes reported in Fig. 5 are upward biased. However, this bias should not affect the fraction of trades that are odd lot trades since both a single-share trade and a fractional share trade would represent an odd lot.

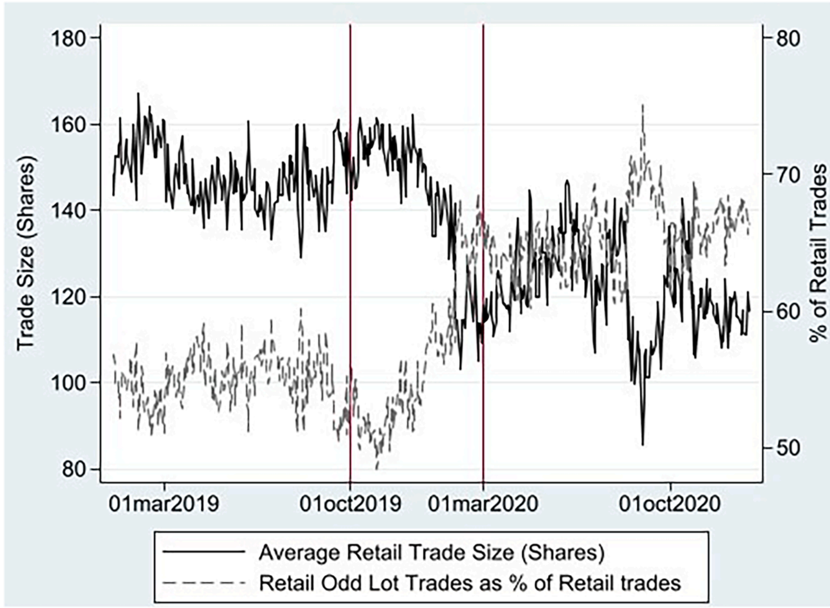


Fig. 5. Average retail trade size.
This figure shows the average trade size in shares and the percent of retail trades made up by odd lot trades. The solid line represents the daily average trade size in shares. Average trade size in shares is calculated as the daily aggregate retail shares divided by the daily aggregate number of retail trades. The dashed line represents the percent of daily retail trades made up of odd lot retail trades. Retail odd lot trades as a percent of retail trades is calculated as the daily aggregate number of retail odd lot trades divided by the daily aggregate number of retail trades. Retail trades are identified according to the Boehmer et al. (2021) subpenny method. Odd lot trades are trades between 1 and 99 shares. 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.

after zero commissions? To answer this question, we estimate daily dollar trading costs for the hypothetical aggregate portfolio described above. For each individual transaction, we compute trading cost as the effective half-spread (in dollars) observed in the data plus an assumed commission. We use $5 as the per-trade commission on days prior to October 2019 and zero thereafter. The solid line in Fig. 6 displays the

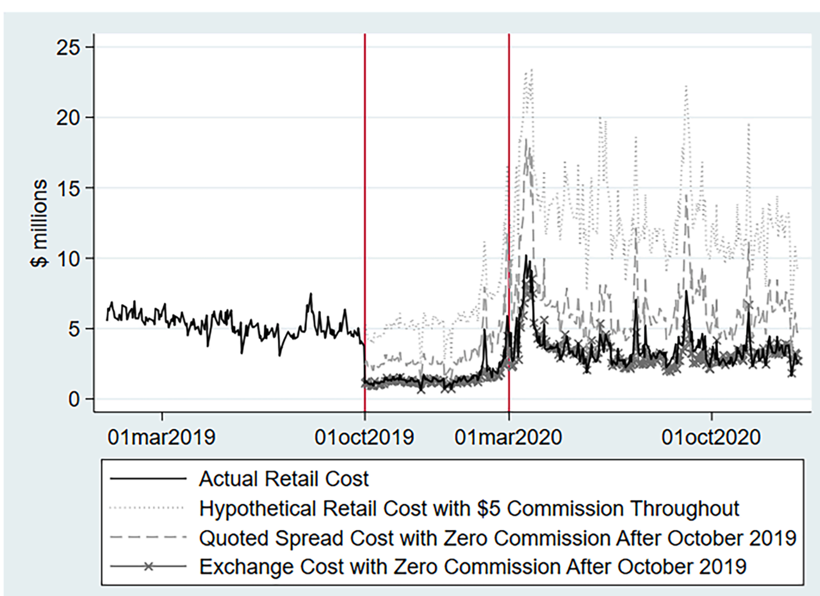


Fig. 6. Aggregate trading costs.
This figure shows aggregate retail trading costs under different commission regimes. The solid line represents the actual one-way trading costs in millions for all the retail trades in the stocks in our sample and for trades between 1 and 1999 shares. The actual trading costs assume a $5 commission pre-October 2019 and a $0 commission thereafter. The dotted line represents the one-way trading costs in millions for retail trades assuming the $5 commission existed throughout the entire sample. The dashed line represents the one-way trading costs in millions for retail trades assuming the zero-commission cut and retail traders received half quoted spreads. The solid line with (x) markers represents the one-way trading costs in millions for retail trades assuming the zero-commission cut and retail traders received similar half effective spreads to exchange trades. 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. Retail trades are identified according to the Boehmer et al. (2021) subpenny method.

daily time series of aggregate retail trading costs. The sharp drop in October 2019 suggests the reduction in commissions dwarfed the increased trading costs associated with more trading. Highlighting various time periods, average daily trading costs in the Base Period was $5.12 Million, which fell to $1.29 Million in the Zero Period. And even with the sharp spike in trading, the average daily trading cost of $4.93 Million during Covid was still below the Base Period level. Importantly, since we study a portfolio aggregated across trades and stocks, the visually striking decline in costs around the zero commission events reflects increases in retail trading and the unequal distribution of trades across stocks.

We next compare the trading costs of the aggregate retail portfolio in the zero-commission regime to those of three counterfactual aggregate portfolios for perspective. First, the dashed line represents the trading costs using the actual volume of trade and a $5 commission throughout the entire time series. Note the solid and dashed lines would be identical leading up to October 2019 as both would incorporate a per-trade commission of $5. Second, the line denoted by "x" marks, represents the aggregate trading costs using the effective spread estimates from institutional trades and $5 (zero) commissions prior to (after) October 2019. Third, the dotted line represents the aggregate trading costs using quoted spread estimates and zero commissions after October 2019. We interpret this third counterfactual as a worst-case scenario in which retail trades are routed to exchanges and receive no price improvement. The salient finding in this figure is that even under the extreme worst-case counterfactual, retail traders spent less to trade after the shift to zero commissions. Furthermore, because the aggregate trading costs likely capture the well-documented increase in the number of retail investors during the pandemic, the trading cost for the subset of investors that existed before the commissions were cut are almost certainly less than our reported aggregate costs.

5. Understanding retail cost savings

The retail brokerage industry touts that arrangements to route orders to wholesale execution venues achieve superior executions for their clients than those offered on the organized exchanges. These arguments rely on price improvement statistics that Regulation NMS requires market centers to disclose on monthly Rule 605 Reports.[23] Brokers pass similar information to their clients, often emphasizing the dollar magnitude of such price improvement. For example, as shown in Appendix II, 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.[24] Per Regulation NMS, this "price improvement" represents savings relative to the NBBO at the time of order receipt. 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.

Our results highlight the more subtle empirical fact that trades routed to the exchanges, i.e., our benchmark trades, receive price improvement relative to the NBBO as well. Indeed, our summary statistics in Table 1 illuminate the well-known result that across all trades, average effective spreads are lower than quoted spreads (also derived from NBBO). Thus, even if the direction of savings conveyed by reported price improvement statistics is correct, using regulatory-based price improvement statistics to quantify benefits that accrue to investors may overstate the economic savings retail traders receive. This point is timely given regulators' current emphasis on how accurately brokers convey the quantitative aspects of cost savings to their clients.

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 PFOF for themselves—i.e., there exists an "industry standard" 80/20 split of the value between price improvement and PFOF. 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 PFOF. 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 PFOF for itself."[25]

The extent to which the magnitudes of NBBO-based price improvement for retail trades differ from the effective spread differences we report in Table 2 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

$$Improve\% = \begin{cases} \dfrac{Offer - Price}{Midpoint}, & if\ BuySell = 1 \\ \dfrac{Price - Bid}{Midpoint}, & if\ BuySell = -1 \end{cases} \tag{3}$$

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 4. We first note the intercepts are positive and statistically significant. Thus, the benchmark institutional trades within each order size receive price improvement relative to the NBBO. For small, medium, and large exchange trades, these price improvements (as percentages of the quote midpoint) are 0.026 %, 0.024 %, and 0.014 %, respectively. For perspective, these magnitudes are roughly fifteen to twenty-five percent of the effective spreads on institutional trades that we report in Table 2 above. This finding is also consistent with the generally smaller effective spreads than quoted spreads that we report in Table 1.

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 %.[26] Thus, retail trades executing on exchanges receive similar price improvement to the average exchange trades as indicated by our intercepts in Table 4.

Turning to retail trades, we see the coefficients for the *Retail* indicator are positive and statistically significant as well. These coefficients are between 40 % and 80 % of the magnitude as the intercepts, indicating that retail trades in the Zero Period receive about 50 % higher

[23] 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.

[24] 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.

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

[26] 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.

Table 4
Retail price improvement during zero commissions.

	Dependent Variable = NBBO Price Improvement as a Percent of Midpoint		
	0–99 Shares (1)	100–499 Shares (2)	500–1999 Shares (3)
Retail	0.011***	0.017***	0.011***
	(54.20)	(58.58)	(30.43)
Retail x Post	−0.002***	−0.004***	0.002***
	(−11.79)	(−23.97)	(6.34)
Constant	0.024***	0.016***	0.007***
	(72.04)	(36.81)	(13.89)
Observations	5672,286	5382,386	1495,938
Stock FEs	Yes	Yes	Yes
Date x Time FEs	Yes	Yes	Yes
Adj R^2	0.486	0.505	0.383

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. 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. Column 1 shows odd lot trades. Column 2 shows trades between 100 and 499 shares. Column 3 shows trades between 500 and 1999 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.

price improvement as comparable institutional 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 0.037 %, 0.041 %, and 0.025 % of the midpoint, respectively. For a 200-share trade in a $30 stock, the dollar price improvement would be $2.46. This magnitude is similar to Schwab's representative cost savings we highlight in Appendix II.

We next compare the magnitudes of NBBO-based price improvement with our effective spread results. To this end, we use various coefficient estimates from Table 4 to compute retail price improvement for each order size and in each period. We display these magnitudes in the black bars in Fig. 7. We then perform similar computations using the coefficient estimates from Table 2 to represent retail effective spread savings. As argued throughout this paper, we believe these latter estimates better reflect the true economic savings for retail traders because they benchmark retail execution costs with similarly-calculated costs for trades that execute on exchanges. For this figure, we divide the effective spread differentials by two (i.e., express results in terms of "half spreads") so that our numbers are comparable with the price improvement statistics.

We display the effective (half) spread differentials in the dark gray bars and liken the effective spread savings to the price improvement metrics. The message is visually clear. NBBO-based price improvement substantially overstates the cost savings for retail investors. For example, keeping with the 200-share trade in a $30 stock referenced above, the effective (half) spread savings for a retail trader is only $0.42 as opposed to the NBBO-based price improvement of $2.46. 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 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.[27]

6. Conclusion

How does the reduction in commissions affect retail investor trading costs? Because of underlying mechanisms that govern the execution of retail trades, the answer is unclear. Using data surrounding the widespread adoption of commission-free trading, our estimates indicate that retail investors in aggregate benefitted through a reduction in total trading costs, primarily driven by the reduction in commission. Even accounting for increased turnover after the industry's elimination of commissions in the Fall of 2019, aggregate dollar trading expenditures declined.

These findings are timely in light of much industry debate and the SEC's proposed rule to completely rewrite order routing policy. Our results suggest policymakers have no basis to make such a dramatic change solely on the assertion that retail traders are getting an unfair deal on executions. Contemporaneous research by Battalio and Jennings (2022) and Dyhrberg et al. (2022) only bolsters this view. Of course, our conclusions rely on exchange trades as a counterfactual. A more appropriate counterfactual would capture execution costs under a proposed alternative policy, but such an alternative does not exist. Ernst et al. (2022) argue on theoretical grounds that forcing retail orders to auctions, which is the centerpiece of the SEC proposal, would increase execution costs. In their model, the auctions would allocate orders to the lowest cost venues, but less competition would ultimately lead to less aggressive bidding and more expensive executions.

Our findings also cohere with stock market reactions to brokers' announced commission cuts and the surrounding commentary. 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."[28] Additional news and industry experts largely recognized this move as unexpected and the next big step in the price war among retail brokerages. Thus, the zero-commission shift represents a reduction in deadweight transactions costs and, hence, signifies a potential wealth transfer from brokers to retail clients.

More empirical work is needed. The substantial drop in retail transactions costs combined with the practical ease with which individuals can trade stocks appropriately prompts a resurgence in the analysis of retail trading behavior and performance. Given the growing popularity of options trading among retail investors and the risks therein, researchers should extend this analysis beyond the equities space. Interestingly, brokers also route option orders to PFOF venues, and payments for option orders far exceed those for equities. While our results indicate retail traders do not bear substantial costs stemming from agency problems in equity order routing arrangements, contemporaneous work by Ernst and Spatt (2022) suggest a very different outcome in options order routing.

[27] 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.

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

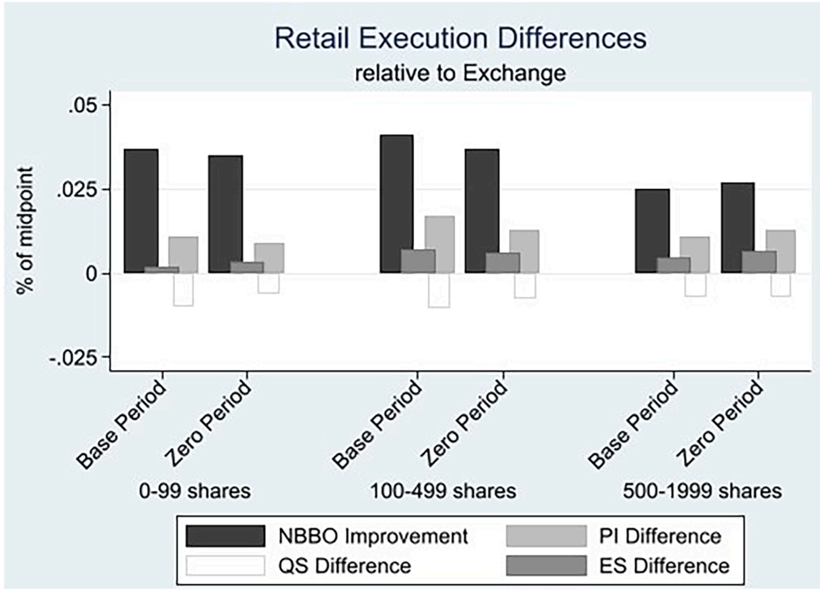


Fig. 7. Execution differences.
This Figure shows the difference between retail and exchange execution quality. NBBO Improvement shows the total NBBO price improvement for retail trades. QS Difference shows half the difference in quoted spreads between retail and exchange trades. ES Difference shows half 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 2 and 4.

CRediT authorship contribution statement

Samuel W. Adams: Writing – review & editing, Writing – original draft, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Connor Kasten:** Writing – review & editing, Writing – original draft, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Eric K. Kelley:** Writing – review & editing, Writing – original draft, Supervision, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

None.

Data availability

Data will be made available on request.

Appendix

Recent Payment for Order Flow 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 payment for order flow (PFOF) model succinctly summarize the disparate viewpoints. For example, Dennis Kelleher, CEO of Better Markets, emphasized the conflicts of interest between a broker's duty to seek best execution and their duty to maximize profits for shareholders that we discuss above.[29] Other concerns were also raised about PFOF. First, PFOF models entrench dominant High Frequency Trading Firms (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.[30] 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.[31]

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

[29] "Robinhood business model under fire at GameStop hearing in Congress" by Chris Matthews, MarketWatch, March 17, 2021.
[30] 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.
[31] *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.

free trading, which encourages investor participation in the market.[32] 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.[33] From a market quality standpoint, proponents argued that PFOF models lower costs to retail investors. Not only are upfront commission costs eliminated, but market makers are more confidently able to execute trades without the 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.[34]

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).[35] More recently, SEC Chairman Gary Gensler has stated publicly that the possibility of banning payment for order flow is "on the table".[36]

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 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 a 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 3), 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 et al. (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 et al. (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.[37]

Research that considers how PFOF arrangements create externalities affecting overall market quality offers mixed messages as well. Easley et al. (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 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 et al. (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.

Appendix II: 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.

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

[33] *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.

[34] *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.

[35] *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.

[36] "SEC Chairman Says Banning Payment for Order Flow is 'On the Table' by Avi Salzman, Barron's, August 30, 2021.

[37] 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.

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

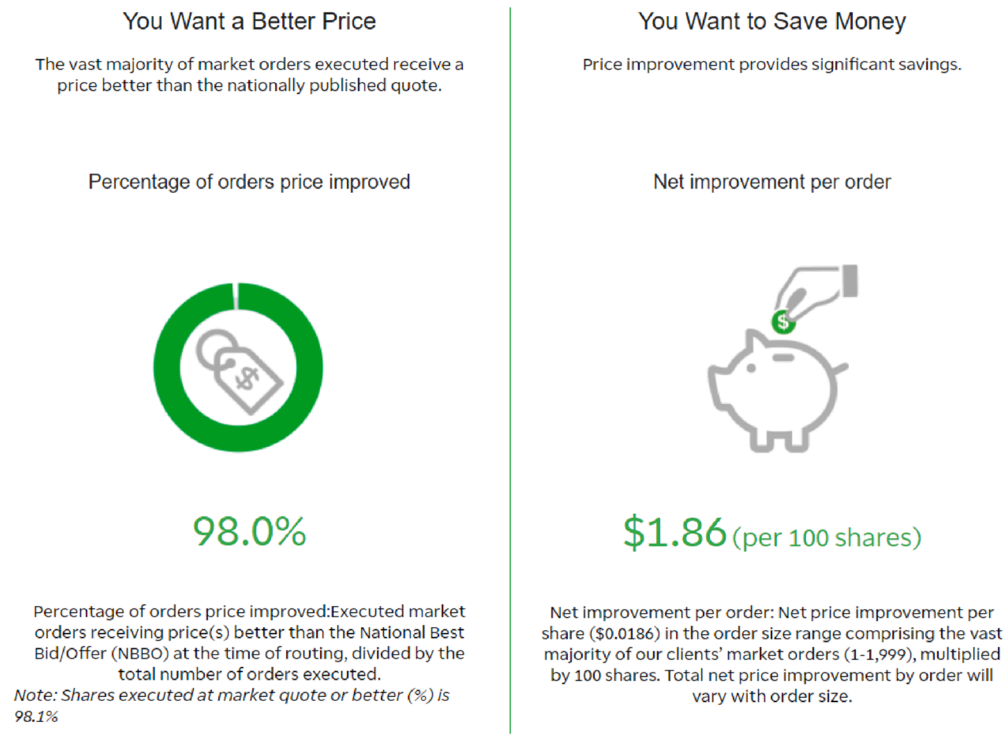
Effective spread
Average effective spread:

$0.0121

How it's measured

Source: Fidelity Q2 2021 statistics.

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

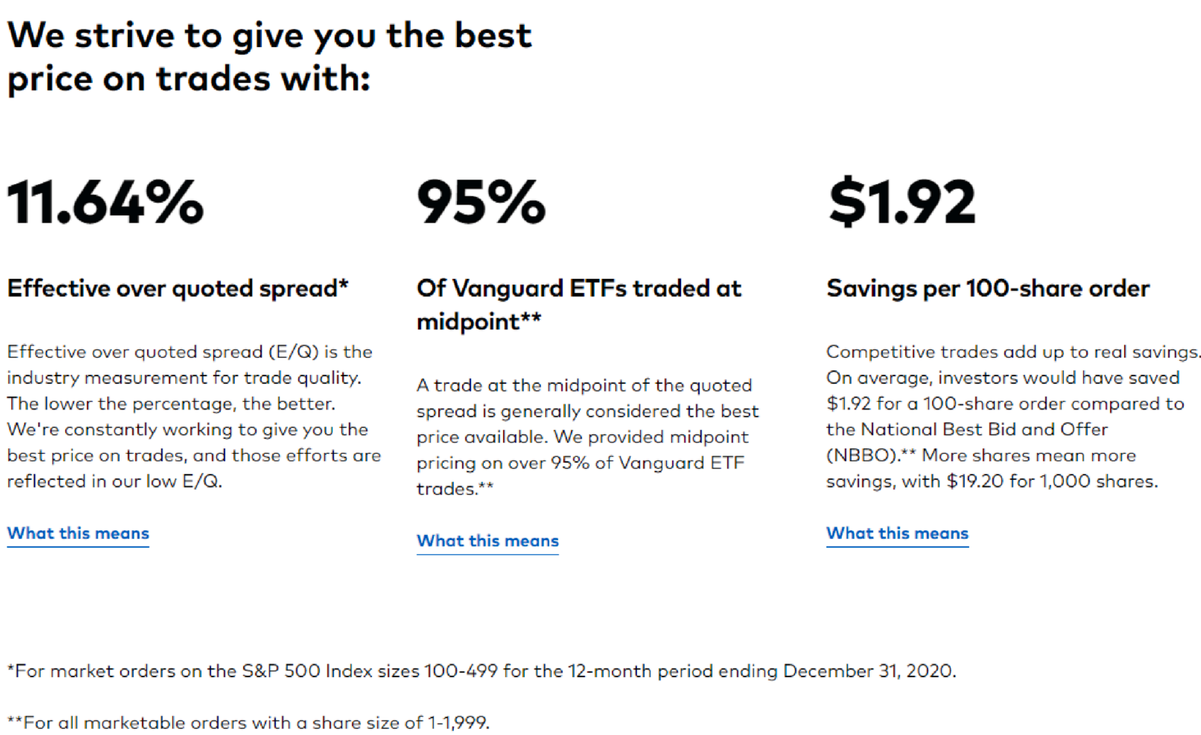
Price improvement provides significant savings.

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.

. (*continued*).

Appendix III: 605 Price Improvement for Market Makers

	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 %

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 to September 2019, which covers the period before the commission cut in October 2019. Panel B shows November to December 2019, which covers the period immediately after the commission cut. Panel C shows March to April 2020, which covers the beginning of the pandemic. 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.

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