Odd Lots & Optics: Joint Strategic Operational and Disclosure Responses to Scrutiny*

Chuck Downing
Massachusetts Institute of Technology

Bradford Levy
University of Chicago

Matthew A. Phillips
Northwestern University

Eric C. So
Massachusetts Institute of Technology

June 2026

Abstract

We show firms can strategically align superficial operational changes with tailored voluntary disclosures to combat public scrutiny. Specifically, we study the trade execution behavior of retail brokerages and market makers upon the onset of a voluntary disclosure program that emphasized price improvement (PI) rates in response to regulatory and public scrutiny. We show that the rate of retail orders receiving *any* price improvement jumped discontinuously at the outset of the program. This jump was driven by trivially small amounts of PI given to a larger fraction of orders, enabling market makers and brokers to tout higher PI rates without significant cost. In a controlled experiment, we find that investors systematically prefer brokers with higher reported PI rates even at the expense of actual cost savings, supporting the notion that favorable PI rates would reduce the regulatory attention that unfavorable statistics would otherwise invite. Our findings document a distinct strategic response to scrutiny in which operational changes serve primarily as raw material for targeted voluntary disclosures rather than genuine improvement.

JEL Classifications: G10, G11, G18, G24, G32

*Direct correspondence to Eric So (eso@mit.edu). We thank Sam Anderson, Tim de Silva, Elia Ferracuti, Anthony Joffre (discussant), David Kim, S.P. Kothari, Zoe Han, Alina Lerman (discussant), Bokyung Park (discussant), K. Ramesh, Andrew Sutherland, Rodrigo Verdi, Gabe Voelcker, Joe Weber, Chloe Xie, and participants at the University of Alabama, 2023 BASS Conference, Emerging Scholars Brownbag Series, 2024 FARS Midyear Meeting, Fordham University, 2025 Hawaii Accounting Research Conference, 2024 INSEAD Accounting Symposium, University of Minnesota, Rice University, the U.S. Securities and Exchange Commission, and MIT Sloan for helpful comments and suggestions. This study has benefited from discussions with individuals at the Financial Information Forum (FIF) as well as at FIF member firms. Generative artificial intelligence was used to minimize typographical and programming errors. All remaining errors are our own.

1. Introduction

Firms facing heightened public scrutiny must decide how to respond to pressure that threatens their reputation, profitability, and regulatory standing. One response is to implement meaningful operational changes that actually address the underlying concern. While effective, such changes are typically costly (Dyreng et al., 2016; Tomar, 2023). Alternatively, firms can pursue "symbolic" disclosures designed to manage perceptions without requiring substantive operational changes (see, e.g., "greenwashing" in the corporate sustainability literature (Marquis et al., 2016; Kim and Lyon, 2011)). Such responses rely on an important precondition: the metrics under scrutiny must be ambiguous or costly to verify, so that firms can craft favorable narratives which are not immediately unwound. When performance metrics rest on auditable data, firms cannot credibly claim improvement without some basis in operational reality. In this paper, we document a distinct strategic response to scrutiny whereby nearly costless operational tweaks are paired with voluntary disclosure highlighting the improvements.

We study this response in the context of trade execution quality for small equity orders. This setting satisfies the conditions under which the strategy becomes feasible: verifiable metrics, a salient disclosure statistic, a low-cost operational lever, and heightened scrutiny. An investor placing a small equity order typically transmits it to a broker, who routes the order to a market maker for execution off-exchange. A common measure of execution quality is whether the order is executed at a price better than the prevailing National Best Bid or Offer (NBBO). When it is, the investor is said to have received "price improvement."[1] All else equal, more price improvement is better for investors. Broker-dealers, however, face an inherent conflict of interest: they owe clients the best price possible, yet providing greater price improvement reduces their own profit margins.

[1]The NBBO is based on non-odd lot quotes, where prior research has found that odd lots frequently have better quotes available (Bartlett et al., 2025).

This conflict of interest prompted the SEC in November 2000 to mandate that brokers and market makers publicly disclose trade execution quality metrics, including the percent of shares receiving price improvement. Trades of fewer than 100 shares, known as "odd lots," were exempted from these disclosure requirements, keeping execution quality for smaller trades opaque. Despite this exemption, odd lots grew into a substantial share of retail trading activity, accounting for over 50% of all trade orders by 2020 (Bartlett et al., 2025). This growth drew increased attention to odd lot execution. In 2014 and 2015, the release of Michael Lewis's *Flash Boys* and subsequent regulatory probes from the SEC and FINRA placed mounting pressure on brokerages and market makers to justify their execution practices for odd lot trades. In response, members of the Financial Information Forum (FIF), an industry trade group, introduced a program to voluntarily disclose execution quality statistics for odd lot trades.[2] The stated goal was "to provide more visibility into execution quality measures" for off-exchange trades.

Several features of the setting are worth emphasizing. First, trade execution metrics are grounded in verifiable transaction data available to the SEC,[3] which constrains firms' ability to claim better performance without a factual basis. Second, the pre-existing mandatory disclosures under SEC Rule 605 for non-odd lot trades established a set of recognized execution quality metrics, limiting firms' ability to construct novel statistics that might cast performance in a more favorable light. Third, implementing meaningful operational improvements would be costly, as it would directly compress the trading spreads from which market makers earn their profits. Together, these features produce a tension: firms cannot rely on purely symbolic responses because the metrics are verifiable, yet substantive improvement in execution quality would cut into profitability.

[2]Participants included prominent brokerages like Charles Schwab, Fidelity, and Scottrade, alongside major market makers such as Citadel, KCG (now Virtu), Two Sigma, and UBS.

[3]This information is, however, costly for the SEC to process. From the MIDAS webpage: "Most institutional investors, retail investors, and academics, generally do not consume this data – it is extremely voluminous, challenging to process correctly, and requires specialized data expertise."

The metric headlined in the FIF disclosures, however, has a feature that creates scope for strategic behavior despite this verifiability. A trade is classified as "price improved" whenever it receives *any* price better than the NBBO, even by a trivially small amount (e.g., \$0.0001 per share). The disclosed statistic is therefore binary at the trade level and does not reflect the magnitude of improvement actually provided to investors. This gap between what the metric communicates and what it captures about the underlying outcome means that market makers can move the statistic at negligible cost by shifting orders from zero to de minimis price improvement. The operational change and the disclosure thereby become mutually reinforcing: the operational change is undertaken to enable the disclosure, and the disclosure draws attention to the result of the operational change in a favorable light.

We employ a difference-in-differences (DiD) design around the 100-share odd lot threshold to test whether market makers altered their execution practices upon adoption of the voluntary disclosure regime. Using Trade and Quote (TAQ) data, we examine changes in price improvement rates for odd lot trades (1–99 shares), using both all non-odd lot trades (100+ shares) and "mixed lot" trades (101–199 shares) as control groups. Mixed lots provide the most economically comparable trades around the 100-share reporting threshold, since trades just above 100 shares were already subject to mandatory disclosure under Rule 605. We focus on changes at the onset of the FIF's data collection window on May 6, 2015, which reveals whether FIF members changed their behavior to align with the forthcoming disclosure. A notable feature of our setting is that we observe these changes in execution quality before the disclosed statistics became publicly available in June 2015. This timing mitigates the concern that market participants changed behavior in response to the information content of the disclosure rather than to the initiation of the collection window.

Studying price improvement rates in isolation is potentially misleading because a trade qualifies as "price improved" even when the improvement is trivially small. We therefore pair our tests of price improvement rates with tests of effective spreads. Effective spreads capture the magnitude of improvement delivered per order, and therefore provide a more informative

measure of overall execution quality. The two metrics together yield competing predictions. If market makers meaningfully improved execution in alignment with the FIF disclosure, we would expect both an increase in price improvement rates and a decrease in effective spreads. If instead market makers used the disclosure to highlight a less informative signal of execution quality, we would expect price improvement rates to rise with no corresponding change in effective spreads.

Our first result is that the daily percentage of odd lot shares receiving price improvement jumps discontinuously by approximately 7 percentage points, relative to a pre-period base rate of 50%, at the onset of the FIF data collection window on May 6, 2015. This jump brought odd lot rates into line with those of mixed lot trades already reported under Rule 605. The timing and magnitude are consistent with market makers acting to close the gap that would otherwise invite continued or worsening scrutiny. Our second result is that there is no corresponding change in effective spreads for these odd lot trades. Together, these findings suggest that odd lot investors received no material improvement in execution quality despite the jump in the disclosed statistic. Falsification tests and pre-trend analyses further support the interpretation that the shift in odd lot execution is directly attributable to the onset of the disclosure program.

Further analyses identify the specific mechanism behind the jump in price improvement rates. Rather than reflecting lower execution costs, the increase was driven by a larger fraction of orders receiving trivially small amounts of price improvement. In particular, many orders that previously executed without price improvement began to receive exactly \$0.0001 per share, the smallest allowable increment. This reallocation is concentrated where adverse selection is lower, including for smaller-value trades and stocks of larger firms. In addition, we find that the increase in de minimis price improvement is largest for securities with lower pre-period price improvement rates, consistent with market makers concentrating their efforts in areas that would have potentially invited more scrutiny. This bunching at the de minimis threshold is the mechanism by which market makers satisfied the binary

disclosure metric at minimal cost, consistent with a joint operational-disclosure strategy rather than a meaningful improvement in execution quality.

Our next set of analyses more directly links the joint operational and disclosure behavior we observe to the scrutiny that motivated it. First, our main findings concentrate in stocks where FIF members handle a greater share of total order flow. Second, the effects concentrate further among FIF members that were under SEC investigation for order execution prior to the onset of the FIF program. This pattern is consistent with market makers under the most intense scrutiny having stronger incentives to enact the strategic response. Although we cannot directly discern which brokers and market makers execute each individual trade, the concentration of our findings in stocks routed more heavily to FIF members mitigates alternative explanations. Finally, we examine outcomes around the date the statistics first became public. We document no systematic changes in execution quality or average odd lot order size around the date of the first set of FIF disclosures, and a small increase in trading volume for odd lot orders around the disclosure date, especially for securities whose order flow is most frequently handled by FIF participant market makers.

Our final set of tests attempts to evaluate whether the gamed metric is salient to stakeholders. We conduct an experiment in which participants with self-reported investing experience choose between pairs of brokerages that differ in price improvement rates and average savings per order (ASPO), the two main metrics in the FIF disclosures. The brokerage with higher price improvement always delivers lower average savings in our design, a stricter condition than the reality in our empirical setting. We replicate the exact disclosure made available during the FIF voluntary disclosure regime and find that participants systematically favor the higher price improvement brokerage even when doing so means accepting lower average savings. This pattern persists among finance-literate participants and those who pass comprehension and attention checks. These results are consistent with retail investors overweighting price improvement rates relative to average savings when evaluating execution quality, despite the latter more accurately capturing the economic benefit to in-

vestors. This finding ties directly to our empirical results: the bunching at the de minimis threshold raised the price improvement rate without improving (or reducing) average savings, and this low-cost operational change may have plausibly influenced public and media narratives around execution quality, which in turn affect regulatory scrutiny. Together, the empirical and experimental evidence supports the view that scrutiny paired with verifiable but coarse metrics can induce a strategic response that is truthful yet misleading.

Our study contributes to the literature by identifying a distinct strategic response to scrutiny available to firms constrained by performance metrics grounded in verifiable data. The bunching at the de minimis price improvement threshold, combined with no change in effective spreads, shows that the increase in the disclosed statistic was achieved through a specific, low-cost strategy that exploits the coarseness of the disclosure metric rather than through meaningful changes in execution quality. This finding extends the literature on real activities manipulation (Roychowdhury, 2006; Cohen et al., 2008; Kanodia and Sapra, 2016) by showing that firms can target the boundary of a binary or threshold-based metric to generate favorable disclosure data at negligible cost. Whereas prior work documents how regulatory scrutiny can shift firms from one form of manipulation to another (Blackburne et al., 2020), we show that verifiability constraints can channel firms toward a distinct strategy of operational adjustments that are real but economically trivial, undertaken specifically to satisfy a disclosure statistic. The mechanism we document is not limited to our setting. Binary and threshold-based disclosure metrics are common across regulatory and voluntary reporting regimes, including pass/fail compliance indicators, percentage-based sustainability targets, and star-rating systems in which small changes near a rounding threshold can shift the reported outcome (Leuz and Wysocki, 2016). In each of these settings, the coarseness of the metric creates a gap between what the disclosed statistic communicates and what it captures about the underlying outcome. When a low-cost operational action also exists near the threshold, strategic gaming becomes a feasible response to scrutiny.

We also show that this strategy succeeds because the gamed metric is salient to stakehold-

ers who cannot easily distinguish it from the underlying outcome. Our experiment demonstrates that participants overweight the price improvement percentage relative to average savings per order when evaluating broker quality, even though the latter more accurately captures the economic benefit to investors. The operational change and the disclosure thus become mutually reinforcing: the operational change enables the disclosure, and the disclosure draws attention to the result of the operational change. More broadly, our findings show that scrutiny paired with verifiable but gameable metrics can induce a strategic response that is truthful yet misleading. This result complicates the view that monitoring and verifiability alone are sufficient to discipline firm behavior (Hanlon et al., 2014; Kubick et al., 2016). Whether verifiability produces meaningful improvement depends on whether the salient metric and the underlying outcome it is supposed to measure are distinguishable to the audience that motivates regulatory scrutiny, including the media, public discourse, and the individual decision-makers who respond to the disclosed information.

The remainder of the paper proceeds as follows. Section 2 describes the related literature and institutional background, and develops our theoretical framework. Section 3 describes our data. Section 4 presents our main design and results. Sections 5 and 6 contain additional analyses and the experimental setting. Section 7 concludes.

2. Related Literature, Institutional Details, and Hypothesis Development

2.1. Related Literature

Our study is related to overlapping streams of literature examining firm responses to public scrutiny, the interplay between voluntary disclosure and real activities, and strategic communication. When facing heightened public scrutiny, firms navigate threats to their reputation, profitability, and regulatory standing (e.g., Beyer et al., 2010; Leuz and Wysocki, 2016). Such scrutiny intensifies monitoring but can also increase the market's focus on specific performance metrics, potentially creating complex incentives around reporting and

operational choices (Samuels et al., 2021). Previous research identifies a spectrum of strategic responses firms adopt under scrutiny. These responses range from resistance or minimal compliance (Belnap, 2023), to substantive operational adjustments aimed at addressing the underlying issues (Dyreng et al., 2016; Tomar, 2023; Fiechter et al., 2022), to symbolic disclosures or actions (labeled "greenwashing" in a sustainability context), designed to manage perceptions without significant operational change (Marquis et al., 2016; Kim and Lyon, 2011; Cho et al., 2015).[4]

For instance, Dyreng et al. (2016) document meaningful operational tax strategy changes by firms facing public scrutiny over tax avoidance, while Kubick et al. (2016) show that SEC comment letters on tax issues lead firms to reduce tax avoidance. Similarly, She (2022) and Fiechter et al. (2022) find that firms adjust real activities (like supply chain audits or CSR initiatives) following new mandatory non-financial disclosure requirements. Regulatory scrutiny can also induce shifts in reporting behavior, such as reducing discretionary accruals while potentially increasing real activities management, as managers perceive different detection risks for different types of manipulation (Blackburne et al., 2020). Conversely, some studies find responses are merely symbolic, fulfilling compliance obligations superficially (Belnap, 2023), or involve selective reporting where firms highlight positive aspects while obscuring negative ones, a behavior observed in voluntary environmental disclosures (Kim and Lyon, 2011; Marquis et al., 2016). The effectiveness and nature of responses can also depend on firm characteristics and the type of stakeholder pressure, such as whether activists are long-term institutional investors (Flammer et al., 2021).

A related literature examines how firms strategically alter operational decisions to achieve desired financial reporting outcomes or manage stakeholder perceptions. Roychowdhury

[4]There is also evidence of intentional misreporting of sustainability information. Gipper et al. (2025) find that firms opportunistically under-report sustainability information when the information would lead to negative perceptions of the firm. The paper also finds that firms selectively comply with reporting standards in ways that create positive trends in sustainability metrics.

(2006) documents that firms manipulate sales, production levels, and discretionary spending to avoid reporting losses and Cohen et al. (2008) find that firms manage real activities around seasoned equity offerings. More broadly, Kanodia and Sapra (2016) highlight firms' incentives to manage narratives through economically suboptimal decisions. Blackburne et al. (2020) find that regulatory scrutiny can alter the perceived risks of different influence tactics, potentially motivating managers to favor changes in real activities over accrual adjustments, suggesting these actions are substitutive. However, disclosure and investment/operational decisions are often interdependent. Beyer and Guttman (2012) show that managers may manipulate voluntary reporting through suboptimal real activities to influence financing terms or market perceptions of performance. A motivation of our paper is to demonstrate a setting where there is evidence of this joint strategy of simultaneously choosing a voluntary disclosure strategy that complements real actions intended to portray a company in the most favorable light.

Therefore, we also relate to the literature on disclosure salience and strategic communication, which emphasizes managerial incentives to selectively highlight information or frame performance contextually to shape stakeholder perceptions, particularly under pressure. For example, Li (2008) finds that firms strategically obfuscate their disclosure using narrative complexity to mask declining performance. In an Australian setting, Kays (2022) finds that firms that are forced to disclose unfavorable information in mandatory disclosures complement these disclosures with more favorable voluntary disclosures. Packard et al. (2023) show that firms increase discussion of their performance within their CD&A disclosures when their executive compensation relative to performance is high relative to their peers. In each of these cases, firms exercise discretion over how they present or frame existing information. Our setting differs in that the operational change and the disclosure are jointly determined: firms alter the underlying metric to enable a more favorable disclosure. This distinction is sharpened by the verifiability of the data in our context.

Unlike typical "greenwashing" contexts involving environmental or social performance,

where metrics can be ambiguous, self-selected, and costly to verify (e.g., Marquis et al., 2016; Kim and Lyon, 2011; Cho et al., 2015; Li and Wu, 2020), the relatively clear and standardized nature of execution quality metrics introduces a critical constraint. Firms cannot easily fabricate favorable disclosures without risking detection and significant penalties, similar to how tax authorities' monitoring can enhance financial reporting quality (Hanlon et al., 2014) or how SEC scrutiny impacts tax reporting (Kubick et al., 2016). This verifiability distinguishes our setting from those where firms might engage in "symbolic disclosure" maintaining a gap between talk and action due to conflicting stakeholder pressures (Cho et al., 2015). Our study explicitly links this selective disclosure to preceding real operational adjustments undertaken specifically to enable the disclosure strategy in a context of high verifiability. Our experimental evidence that participants overweight the price improvement percentage relative to average savings per order highlights the salience of the disclosed metric, showing that the operational change is targeted at the specific statistic that the disclosure highlights.

2.2. Retail Market Access and Trade Execution

Central to our paper and identification strategy is the life cycle of an odd lot order to buy or sell a stock. When an investor with a small trade size (e.g., retail investor) wishes to buy or sell a stock, they often transmit an order to a broker (see, e.g., Odean (1999)). This broker then chooses where to route the order for execution. Most commonly, the broker routes the order to a wholesale market maker, an entity specialized in providing liquidity (Easley et al., 2012).[5] Critically, these routing arrangements operate at the brokerage level rather than the stock level: a brokerage routes its entire book of retail orders to wholesaler counterparties under standing payment-for-order-flow agreements, so the brokerage and the wholesaler are two ends of the same pipeline for any given order. Because retail flow is concentrated among a small number of wholesalers who receive orders from essentially all major retail brokerages,

[5]Alternatively, the broker may choose to fill the order from their own inventory, a practice known as internalization. This is significantly less common for retail trade execution.

separating brokerages and market makers is empirically challenging. The receiving market maker has the right but not the obligation to fill the order. If the market maker does not wish to fill the order then the order may be directed to a network of market makers. If the order is still unfilled at this stage, then it is directed to an exchange where it is executed against the standing limit orders on-exchange. In this sense, an order can be filled, i.e., "executed," by the market maker(s), on-exchange, or by the broker themselves. Where the order is ultimately executed, and at what price, is then reported to a "trade reporting facility" for broad dissemination.

A natural question is whether the order received "good" or "bad" execution relative to an appropriate benchmark. Execution costs are measured against a benchmark of market conditions when the order was received. The most common benchmark is the National Best Bid and Offer (NBBO) which represents the best available quoted prices aggregated across all exchanges. In that sense, the NBBO represents the price an order likely would have received had it been executed on-exchange rather than executed by market makers and represents a legal lower bound on acceptable execution quality. The NBBO is measured at the time the order was received (see Rule 17 CFR § 242.611).

When an order is executed at a price better than the NBBO, it is said to have received "price improvement." Orders may receive price improvement for a variety of reasons; however, one of the most common justifications is that market makers expect the order to be comparatively less informed than the average order on an exchange, of which the NBBO is representative, and therefore are willing to execute it at a better price than the NBBO. There are several ways in which the *extent* of price improvement is measured in practice. For example, the FIF disclosures reported average savings per order, defined as the total dollar amount of price improvement divided by the number of orders executed. According to a recent study, retail investors typically receive between $20 and $30 million per month in price improvement, making it an economically meaningful outcome for retail traders in

aggregate (Ernst and Spatt, 2022).[6]

2.3. Trade Execution Disclosure and the FIF's Voluntary Disclosure Program

Trade execution disclosure regulation has evolved over time with the aim of increasing transparency for investors. In 2000, the SEC adopted Rules 605 and 606 requiring market centers and broker-dealers to publish monthly reports detailing execution quality statistics for customer orders. Rule 605 requires that market centers disclose execution price, speed, and size information for market orders at the stock level. Rule 606 requires broker-dealers to publish quarterly reports on the venues where they route customer orders. Prior evidence shows a significant decline in effective spreads following the introduction of Rule 605 (Zhao and Chung, 2007), consistent with improvements in market quality following this mandatory disclosure reform. However, Rule 605 exempts odd lot orders from these disclosure requirements. Until 2013, odd lot orders were not even reported to the consolidated trade tape, suggesting an opaque information environment where investors had limited knowledge of how their small trade orders would be executed by brokerages and market centers.

Over the past decade, the information environment of odd lot trade execution has come under greater scrutiny, both by regulators and the general public, coinciding with a significant increase in odd lot orders (Bartlett et al., 2025). In 2012, odd lots accounted for approximately 12% of all trade orders whereas in 2020 odd lot orders accounted for over 50% of all trade orders and nearly 20% of total market volume.[7] In 2015, there were no odd lot execution quality disclosure requirements for brokerages or market makers. Around this time, Michael Lewis' popular book *Flash Boys* was published, characterizing the capital markets as rigged against the retail investor. This increased pressure on market makers, such as Citadel, to provide disclosures aimed at the retail investor.

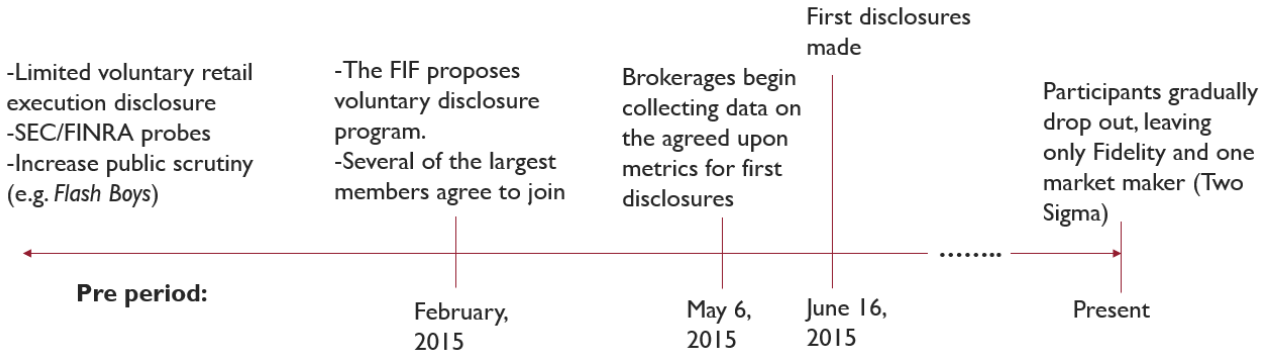
[6] In a comment letter to the SEC, Virtu Financial makes a more aggressive estimate of price improvement of $4 billion per year across the industry. See https://www.sec.gov/rules/petitions/2021/petn4-775.pdf

[7] https://www.tradersmagazine.com/am/the-growth-in-odd lot-trading/

In response to the increased discourse surrounding trade execution quality for retail investors and small trades, the Financial Information Forum (FIF) initiated a voluntary disclosure program aimed at enhancing transparency into odd lot execution quality. The FIF is an industry trade group consisting of over 100 broker-dealers, exchanges, and financial technology vendors. They seek to address and resolve open implementation issues that impact the securities industry across the order life cycle through the use of working group initiatives.

In 2015, the FIF began a working group aimed at providing more visibility into retail trade execution quality. Several FIF member firms agreed to produce a report each quarter that is "meaningful and understandable" to retail investors, through disclosures of simple measures of execution quality across different trade sizes, with data collection beginning in May of 2015. This working group is still active today, although it currently contains fewer member participants than at the outset of the program. Below is a timeline of the FIF's retail execution disclosure program, and the dates relevant for our setting.

Figure 1. **Timeline of the FIF's Disclosure Program**



The stated goal of the working group was to provide retail investors more visibility into key metrics, such as price improvement rates, and many press outlets claimed that the program was also in response to increased regulatory and public scrutiny around the

execution quality of these trades.[8] While not every brokerage or market maker joined the FIF disclosure program, many of the largest member firms who handle the vast majority of retail order flow did join. At the onset of the program, participants included three prominent brokerages (Charles Schwab, Fidelity, and Scottrade) and four of the largest market makers (Citadel, KCG (now part of Virtu), Two Sigma, and UBS). While we cannot directly observe off-exchange order routing during this window, Ernst and Spatt (2022) use proprietary data and document that the four prominent market makers that joined the FIF program in 2015 handled 82.7% of off-exchange retail order flow from 2019-2021.[9] Our paper examines the operational response of these prominent market makers in combination with this disclosure program and its impact on actual execution quality received by retail investors.

The FIF disclosure program involves each participant disclosing quarterly summary statistics for key execution metrics of several order size categories, most notably including the first standardized set of disclosures for odd lot trades. An example of these new FIF disclosures is included in Appendix B. For each order size category, participating firms disclosed execution statistics including average order size, the percentage of shares executed at the National Best Bid or Offer (NBBO) or better, the percentage of shares receiving price improvement, the average savings per order, and average trade execution speed.

The percentage of shares executed at the NBBO or better statistics exhibits relatively little variation due to regulatory requirements for best execution discussed earlier. Thus, the percentage of shares receiving PI was the statistic more heavily emphasized by market participants because it reflected an easy statistic for investors and the media to understand, but which is also a statistic more subject to influence. Indeed, media outlets covering the initial

[8] https://www.reuters.com/article/uk-usa-brokerage-regulations-exclusive/exclusive-u-s-retail-brokers-to-avoid-regulators-wrath-disclose-more-idUKKBN0L60CH20150202

[9] During this time, Ernst and Spatt (2022) show that an additional 12% of retail order flow was handled by G1 execution, which is similarly constructed as Citadel, Virtu, and Two Sigma but did not exist at the outset of the FIF disclosure program.

release of these reports focused on PI% and Average Savings.[10] Examining the disclosure from left to right, it was also the first statistic that reflected discretionary trade execution, where higher rates of PI meant the market maker was giving prices better than they were otherwise obligated to provide. An investor could use this PI% number to estimate the probability that they would receive a better price than the NBBO. The next statistic, Average Savings Per Order, by contrast, is difficult for a trader to use to estimate their own savings in dollar terms because the statistic is not scaled by share price and has no clear reference point for quality, even though trades of securities with higher prices generally receive greater absolute savings.[11]

Because retail investors typically trade in odd lots, the FIF's disclosure program was the first standardized information about the quality of trade execution for odd lot trades available to retail investors. Importantly, the SEC Rule 605 disclosures contain this information and even more comprehensive statistics for all trades of over 100 shares. Thus, the only new information included in these disclosures was for odd lot trades, which are likely to be of the most interest to retail investors (e.g., O'Hara et al., 2014). Articles covering the initial announcement of the disclosure program suggest that prominent intermediaries, including Barron's, may have used these disclosures to guide their brokerage rankings.[12] Today, only one brokerage and one market maker are still releasing disclosures in the format provided by the FIF. However, it is now commonplace for brokerages and market makers to voluntarily disclose tailored odd lot execution statistics of their own choosing outside of the FIF program.

[10] "Most interesting to us is the disparity of Price Improvement and Average Savings per Order given to Odd Lot trades.": https://tabbforum.com/opinions/enhanced-retail-execution-statistics-raise-best-ex-concerns/

[11] The calculation the trader would need to make to estimate their true savings is: Expected $ Savings = Average Savings Per Order / (Order Size x Price Per Share). Another way to think of the expected dollar savings would be that it reflects the amount of price improvement in dollar terms.

[12] "Our next report could make a sharper ranking of brokers if they all begin disclosing under execution-quality standards being developed by the Financial Information Forum, a Wall Street trade group. It's likely that some brokers will start talking up the FIF's new execution measures on their websites and in ads": https://perma.cc/2YLX-FNSX

2.4. Hypothesis Development

Firms facing heightened public scrutiny often confront a strategic dilemma: how to respond to external pressure that threatens their reputation, profitability, and regulatory standing (e.g., Beyer et al., 2010). Such scrutiny increases the expected costs associated with maintaining the status quo, stemming from potential regulatory sanctions, customer retaliation, or damaged public image. The potential for these adverse outcomes forces companies to evaluate strategic responses (Marquis et al., 2016; Dyreng et al., 2016). Broadly, firms can choose to: (1) resist, making no changes if the perceived risks or costs of responding outweigh the benefits (Belnap, 2023); (2) implement meaningful operational changes to address the underlying issues that attracted scrutiny (Dyreng et al., 2016; Tomar, 2023); or (3) engage in symbolic communication or disclosure tactics, akin to "greenwashing," aimed at placating stakeholders without substantial operational adjustments (Marquis et al., 2016). Our study examines strategic responses to scrutiny in the context of brokerage firms and market makers facing scrutiny over the execution quality of odd lot trades.

The choice between resistance, substantive change, and symbolic action is consequential, as these strategies represent distinct approaches to resource allocation, risk mitigation, and stakeholder management. Resistance accepts ongoing risk to avoid immediate costs, substantive change requires significant investment to address root causes directly, and symbolic tactics prioritize managing perception at lower initial cost but with potential long-term credibility risks. Each option therefore carries unique implications for the firm's operational commitments and its relationship with external observers.

Prior to the period of heightened attention surrounding odd lot execution, evidence suggests market makers may have benefited from the opacity in this market. Studies indicate that odd lot trades systematically received worse execution quality compared to reported trades of similar size, consistent with rent extraction arising from information asymmetries due to their exemption from disclosures under SEC Rule 605 (Bartlett, 2021; Bartlett et al.,

2025). The increased focus of the public and the media intensified the risk of regulatory intervention (Wood and Waterman, 1991), making inaction potentially costly. Specifically, market makers faced the threat of new mandatory disclosure rules for odd lots, mirroring the existing Rule 605 framework, or potential changes to the calculation of the National Best Bid and Offer (NBBO) to include odd lot quotes. Both outcomes threatened to increase competition and reduce profitability by eliminating information advantages and setting stricter execution benchmarks (Lipson, 2003; Zhao and Chung, 2007; Boehmer et al., 2007).

One strategic option available to market makers was to make "greenwashing-like" symbolic responses in response to the scrutiny. However, unlike areas like environmental or social impact where performance metrics can be ambiguous, chosen ad-hoc, and difficult to verify (Marquis et al., 2016), trade execution quality metrics, particularly those related to price improvement, draw upon established concepts from existing regulations (like SEC Rule 605) and are reflected in transaction data. This data is verifiable, at a cost, by resource-constrained regulators through the SEC's Market Information Data and Analytics System (MIDAS). This possibility of verification imposes a crucial constraint: participant firms cannot simply claim better performance through disclosure without risking detection if the underlying reality differs significantly. Fabricating execution quality data carries substantial regulatory and reputational penalties. Moreover, the relative standardization of execution quality metrics, guided by Rule 605 precedents, offered limited freedom for market makers to select obscure or self-serving measures.

Given this verifiability constraint, market making firms seeking to manage public perception and mitigate regulatory threats could not rely solely on crafting favorable narratives detached from operational reality. If they wanted to signal improvement convincingly, they needed some factual basis for their claims, specifically demonstrating that odd lot execution performs similar to comparable mixed lot orders on established execution quality metrics. However, implementing meaningful operational changes to substantially improve performance across the board on these metrics (e.g., consistently providing significantly larger

price improvements leading to better effective spreads) would be costly, directly impacting profitability derived from trading spreads and order flow arrangements.

These institutional details and theoretical arguments lead to the central premise of our study: faced with scrutiny in a domain where claims were verifiable and meaningful change was expensive, market makers in our setting adopted a strategy coupling minor operational adjustments with strategic voluntary disclosure. Specifically, we propose that the market makers made low-cost (i.e., de minimis) tweaks to their execution practices primarily designed to generate favorable data points on highly visible, easily digestible metrics: the percentage of shares receiving any price improvement. These operational adjustments, while having little impact on overall execution quality experienced by investors (e.g., average effective spreads), produced factually accurate data showing "improvement" on the selected metric.

Having generated this data, these market makers could then leverage it in their voluntary disclosures (the FIF program). The disclosure served to highlight the acceptable execution performance achieved through the operational tweak, framing it as evidence of responsiveness and acceptable performance. The operational change provided the necessary verifiable data point, and the disclosure provided the platform to communicate this data point effectively through public narratives and media coverage, reducing the pressure on regulators to pursue further action even if the underlying economic benefit to investors was negligible. The operational and disclosure changes thus become mutually reinforcing: the operational change is undertaken partly to enable the disclosure, and the disclosure draws attention to and interprets the result of the operational change in a favorable light.

This perspective suggests that firms' responses to scrutiny may not simply reflect a choice between "real change" and "symbolic talk." Instead, firms can strategically use minimal real changes as a foundation for their communication with stakeholders, particularly when facing verifiable metrics. The viability of this strategy depends on a specific feature of the disclosure

regime: the coarseness of the disclosed metric. When a binary or threshold-based metric classifies performance into discrete categories, firms can move from one category to another at negligible cost without materially affecting the underlying outcome. In our setting, the percentage of shares receiving price improvement classifies any execution better than the NBBO as "price improvement" regardless of the magnitude. This coarseness creates the operational lever that enables the joint strategy.

These considerations lead to two testable predictions. First, if market makers adopt a joint operational-disclosure strategy, we would expect the percentage of shares receiving price improvement to increase for odd lot trades at the onset of the FIF disclosure program, with no corresponding change in effective spreads. Second, if this strategy is targeted at a metric that is salient to the audience of the disclosure, we would expect the increase to be concentrated at the de minimis threshold ($0.0001 per share), where the cost of satisfying the binary metric is lowest.

3. Data Description and Descriptive Results

3.1. Data and Sample Construction

This study utilizes trade and quote data from the NYSE TAQ database to examine changes in execution quality for odd lot and non-odd lot trades following the introduction of the FIF's voluntary disclosure program window in May 2015. The main sample focuses on a two month window surrounding May 6, 2015, the window in which the FIF's disclosure regime began taking effect, but notably before the first set of disclosures were actually released. The advantage of this approach is that it isolates the effects of the firms collecting data for the disclosed metrics prior to any informational effects of the actual disclosures.

To focus our attention on trades subject to the FIF disclosures, we collect all trades of exchange-listed stocks executed off exchange (TAQ exchange code D) from April 6, 2015 to June 6, 2015. The sample only includes trades executed during normal trading hours, and

we calculate the NBBO using a combination of the TAQ NBBO and Quote files following Holden and Jacobsen (2014). We drop trades with locked or crossed NBBOs and those with missing data. We identify trade direction by unanimous vote of the Lee and Ready (1991), Ellis et al. (2000), and Chakrabarty et al. (2007) algorithms, dropping any trades with disagreement between them, though our results are not sensitive to this design choice. A traded share is defined as receiving price improvement (PI) if it is executed at a price inside of the NBBO, and defined as receiving de minimis price improvement (DMPI) if it receives price improvement of \$0.0001 on a per-share basis. Table 1 Panel A provides summary statistics on the key variables in our study, split between odd and non-odd lot trades. About 44% of all non-odd lot trades receive price improvement, compared to 54% of odd lot trades. Odd lots are much more likely to receive de minimis price improvement, with over 24% of executed shares in odd lot trades receiving \$0.0001 of price improvement, compared to only 8.8% of non-odd lot trades. Effective spreads are on average smaller for odd lot trades, and odd lots are more frequently traded in higher priced stocks and larger firms.

In our main analyses, we aggregate our sample of individual trades to the firm-day-type level, where trades are grouped into type based on whether they are odd or non-odd lot trades. This firm-day panel dataset contains aggregated statistics for many of the key variables that are disclosed by the participating brokerages and market makers under the FIF's disclosure program, as well as other relevant additional and control variables. Table 1 Panel B provides descriptive statistics corresponding to this firm-day panel. The sample consists of 600,003 firm-day-trade type pairs, of which 304,249 firm-days are aggregated statistics for non-odd lot trades, and 295,754 correspond to odd lot firm-days. The difference is due to some firm-days containing no odd lot trades. The firm and day fixed effects used in the main regressions reduce the concern that these observations would be driving the results.

Panel B shows similar statistics relative to Panel A. However, at the firm-day panel level, non-odd lot shares are more likely to receive both price improvement and de minimis price improvement compared to at the trade level. This difference reflects the fact that

a small proportion of non-odd lot firm-days have very high trading volume, predominantly stemming from institutional order flow. Since institutional orders typically receive little price improvement compared to retail orders, equal-weighting firm-days for non-odd lot trades overweights days with higher price improvement.[13] Finally, we see that non-odd lot days contain on average 6.35 times as many trades, for over 27 times as much dollar volume, reflecting institutional and mechanical differences between the two trade groups.

3.2. Descriptive Visuals of Main Results

Prior to running any regressions, we first look visually at the data surrounding May 6, 2015. To provide a comprehensive picture, we start by using the entire sample of non-exchange trades. In Figure 2, we plot the aggregate percentage of all shares that receive price improvement, and average effective spreads, each day in our sample window for odd and non-odd lot trades that are executed between 1-199 shares, excluding 100. This comparison group of non-odd lot trades is meant to be the most economically similar to the odd lot trades of interest in our sample. Panel A of Figure 3 plots the aggregate percentage of all shares that receive price improvement each day in our sample window for *all* odd and non-odd lot trades.

In both figures, we document a large and economically significant jump in the percentage of shares that receive price improvement for odd lot trades, with non-odd lot rates remaining constant. In Figure 2, the odd lot trades jump up to almost exactly remediate the gap between them and the comparison group, in line with the idea that market makers may have wanted to remediate any perceived poor execution quality prior to the first round of disclosures. The jump in Panel A of Figure 3 is approximately 7 percentage points and can be compared to the pre-period base rate of around 50%. Panel B of Figure 3 plots effective spreads, and shows no clear changes between the pre- and post-periods for odd or non-odd

[13]The inclusion of firm and day fixed effects and controls for volume in our main specification mitigate concerns that this design choice affects our analysis and results. Random sub-sample testing using trade-level regressions indicates our results are not sensitive to this design choice.

lot trades. This null result suggests that while price improvement rates jumped following the initiation of the disclosure program, this jump was not accompanied by a noticeable change in aggregate savings given to odd lot investors.

Given that we see a jump in price improvement but no corresponding jump in aggregate execution quality, we investigate how these two findings can be reconciled by considering de minimis price improvement. Figure 4 documents the aggregate percentage of shares that receive DMPI each day on odd and non-odd lot trades. We document a large jump in DMPI for odd lot trades on May 6, 2015 that corresponds almost exactly to the jump in the percentage of shares receiving price improvement. The jump is around 7.5 percentage points relative to a pre-period base rate of around 20%, suggesting that the shares that newly received price improvement around the disclosure program were filled with de minimis levels of price improvement. Taken together, the descriptive evidence presented in our summary statistics and figures is consistent with market makers jointly determining an operating and disclosure strategy that allowed them to publicly tout higher execution statistics in response to public scrutiny without fundamentally altering their overall execution quality. The next sections investigate these findings econometrically, supporting the visual analysis and examining the cross-sectional features of these changes in trade execution.

4. Empirical Design and Results

We now test whether the onset of the FIF disclosure program coincides with evidence of the joint operational-disclosure strategy outlined previously. Our empirical tests focus on three key dimensions. First, we examine whether the onset of the FIF disclosure program coincides with a discontinuous increase in the percentage of shares receiving price improvement for odd lot trades. Second, we assess whether this increase reflects genuine improvements in execution quality by analyzing changes in effective spreads. Finally, we investigate the nature of the price improvement being delivered, specifically, whether the observed increase is driven

by a rise in de minimis improvements that would inflate headline statistics while preserving profitability. This empirical design allows us to test whether brokerage firms strategically adjusted operations in ways that align with the incentives created by verifiable but coarse disclosure metrics. We use a difference-in-differences (DiD) design focused on understanding the market maker's decision to give price improvement to shares surrounding the FIF's voluntary disclosure program. We use firm-day execution statistics to evaluate whether market makers strategically changed their execution to improve their disclosed statistics, estimating the following specification:

$$Outcome_{itg} = \beta_0 + \beta_1 OddLot_{itg} + \beta_2 OddLot_{itg} * Post_{itg} + \lambda_t + \alpha_i + Controls_{itg} + \varepsilon_{itg}, \quad (1)$$

where $\%Outcome_{itg}$ represents one of four execution-based outcome variables (the percent of traded shares of exchange-listed firm i on trading day t by trade type g that receives a filled price better than the NBBO ($PI\%_{itg}$); the average effective spread ($EffectiveSpread_{itg}$); the average savings per order ($AvgSavings_{itg}$); and the percent of traded shares receiving de minimis price improvement of \$0.0001 per share ($DMPI\%_{itg}$)). *OddLot* identifies the treated sample, defined as shares belonging to trades of fewer than 100 shares. The control sample comprises trades of 100 or more shares (non-odd lots), which already had detailed execution information disclosed by market makers under SEC Rule 605. *Post* equals one for trading days on or after May 6, 2015, the date on which participating brokerages and market makers began tracking the specific metrics to be included in the FIF disclosures.[14] The initial specification of (1) includes no control variables, since odd lots and non-odd lots are traded for the same firm on the same day; subsequent specifications add controls for differences in traded volume between the two groups. We include firm and day fixed effects throughout, unless otherwise noted, to absorb time-invariant firm characteristics and market-wide daily

[14]This date is not explicitly stated in the disclosures themselves. We confirmed it through conversations with the FIF and FIF member firms.

shocks.

4.1. Price Improvement

Table 2 shows the relation between the percentage of shares that receive price improvement for odd lot and non-odd lot trades between the pre- and post-periods, using the specification described in (1). Column (1) estimates equation (1) with firm and day fixed effects. Column (2) estimates the same specification with the addition of controls for dollar volume, as well as changes in volume in the post period. In both columns, the coefficient on $OddLot \times Post$ is statistically and economically significant, suggesting that about 6.8 percentage points more shares of odd lot trades receive price improvement relative to a pre-period base rate of around 50%, corresponding to a 13.6% relative increase in the likelihood of a share receiving price improvement.

The estimated increase in price improvement rates is consistent with our main argument that market makers strategically adjusted their execution practices to shape a highly visible and verifiable disclosure metric. The shift begins precisely at the onset of the data collection window, consistent with a coordinated response to scrutiny rather than a gradual improvement in execution quality. The targeting of $PI\%$ specifically—rather than other dimensions of execution quality—foreshadows our subsequent finding that effective spreads remain unchanged over the same window.

4.2. Effective Spreads

The next test examines overall execution quality during the same window by testing for changes in average effective spreads and average savings per order received each firm-day for odd and non-odd lot trades. Effective spreads are a robust measure of the savings investors receive on their trades, as it takes into account both the NBBO and the actual filled price of the shares. Average savings, while a less robust metric, is directly disclosed in the FIF disclosures. To test for differences in effective spreads and average savings, we re-estimate

equation (1) using firm and day fixed effects and the same design as Table 2, with a different set of dependent variables. Table 3 shows that overall execution quality, measured using effective spreads and average savings, does not change significantly after the beginning of data collection for the voluntary disclosure program. We estimate these changes both with and without controls. Column (1) regresses the daily share-weighted average effective spread on the *OddLot* and *Post* variables without any controls other than firm and day fixed effects, and column (2) presents the same specification with the addition of controls for volume and changes in volume in the post period. In both columns, we see no significant changes in the average effective spread for odd lot trades, suggesting that there are no benefits being delivered to odd lot investors due to this disclosure program despite the change in execution documented in Table 2. Next, columns (3) and (4) run the same regressions as (1) and (2), but with average savings per order ($) as the dependent variable. Once again, we document no significant change in overall execution quality.

These results, combined with the increase in price improvement, suggest an increase in the headline FIF disclosure metric despite no detectable change in the underlying execution quality given to retail investors. The gap between the disclosed statistic and the underlying outcome is consistent with the type of strategic response that becomes feasible when a coarse, binary metric is the basis for voluntary disclosure under scrutiny.

4.3. de minimis Price Improvement

The final main test examines how the market makers achieve the documented changes in price improvement rates without changing average effective spreads. Participants could increase price improvement rates while preserving their profitability by shifting the dollar amount of per-share price improvement to provide more shares with lower levels of price improvement. Specifically, we examine whether market makers provide some shares that previously would not have received price improvement with de minimis levels of price improvement ($0.0001 per share).

Table 4 provides evidence of a corresponding jump in de minimis price improvement following the beginning of the disclosure window. Column (1) regresses the percentage of shares that receive per-share price improvement of exactly \$0.0001 on the *OddLot* and *Post* variables in a difference-in-differences design, with no controls other than firm and day fixed effects. Column (2) provides results for the same specification, including controls for volume and changes in volume between the pre- and post-periods. Both columns show similar results, documenting a jump in de minimis price improvement of around 7% of the shares each day for odd lot trades. Relative to a pre-period base rate around 20%, this jump corresponds to an increase in the likelihood of an odd lot share receiving de minimis price improvement of around 35% following the beginning of the disclosure window. The magnitude of the increase shown for DMPI in Table 4 corresponds almost exactly to the increase in price improvement documented in Table 2, lending further support to the market makers providing these newly improved shares almost exclusively with de minimis price improvement that is unlikely to be economically meaningful.

Given that the jump in price improvement rates we document in Table 2 seems to be driven entirely by trades executed with de minimis amounts of price improvement, we argue that this change in price improvement is not economically meaningful even if the market makers did not compensate for this increase by decreasing price improvement elsewhere. A simple back-of-the-envelope calculation underscores this point. Combining our 6.8 percentage point estimate (Table 2, Column 1) with the mean odd lot trade size of 30.59 shares and a per-share price improvement of \$0.0001, the implied benefit is approximately \$0.0002 per odd lot trade. Scaled across the 18,859,263 off-exchange odd lot trades in our one-month post-period, the aggregate cost to market makers is approximately \$3,923, or roughly 0.1% of all price improvement given to these trades. The trivial per-trade magnitude reinforces that the documented jump in headline PI rates does not reflect meaningful savings to retail investors.

Taken together, our main results show both descriptively and empirically that the exe-

cution of odd lot trades significantly changed around the FIF voluntary disclosure program, but that this change did not coincide with improvements in overall execution quality. These changes are most consistent with FIF participants enacting a joint operational and disclosure strategy to reduce scrutiny of odd lot trade execution by increasing PI rates for odd lots without providing odd lot traders any meaningful improvement in economic benefits. These operational changes generated real, and favorable, data points for participant market makers to subsequently disclose, plausibly providing the appearance of higher execution quality to reduce scrutiny at a low cost.[15]

5. Additional Analysis and Robustness

5.1. Narrowing the Trade Size Window

Our main empirical tests rely on trades over 100 shares as a control group for trades under 100 shares. However, these two trade groups have differences in execution due in part to adverse selection, since trades of over 100 shares are more likely to be institutional trades. To alleviate concerns that non-odd lot trades do not serve as a valid control group for odd lot trades in an individual firm-day, we take a few steps. First, in Figures 3 and 4, we can see that pre-period trends are almost identical for odd and non-odd lot trades, suggesting a valid set of comparisons. Next, Figure 5 reports event-time coefficients and broadly finds no distinct trend in any outcome variables in the pre-period.

While it is possible that odd lot and non-odd lot trades may have significant differences in execution due to the differences in how market makers handle institutional and retail order flow, it is less likely to be true for smaller windows around the 100 share size threshold, and for mixed lots. These trades are likely more economically comparable to odd lots than trades for "round lots" (multiples of 100), which are more likely to be institutional order flow.

[15]We note that it is impossible to completely rule out that these findings are driven by a single market maker given the anonymity of execution in the TAQ data.

We conduct additional regression analyses using the narrower sample windows to reduce concerns of institutional differences driving our results. Table 5 re-estimates the specifications from Tables 2 and 4 using narrower trade size windows around the 100 share threshold. Column (1) uses a sample of trades of sizes between 70 and 110, column (2) uses trade sizes between 90 and 110, column (3) compares all odd lot trades to trades between 101 and 199 shares, and column (4) compares trades of 90-99 shares to those between 101-110 shares. The last two columns try to avoid the round-lot orders of exactly 100 shares that are more likely to be institutional orders, thus providing a potentially more relevant control group. Table 5 provides evidence for both PI and DMPI that shows robust jumps that are statistically and economically significant. The magnitudes are generally smaller than in Tables 2 and 4, which we explain with corroborating evidence in additional cross-sectional analyses.

5.2. Market Maker Disclosure and Scrutiny Order Flow Validation Tests

One potential concern in our analyses is that these results cannot be conclusively tied to the disclosing market makers, but could instead reflect a broader market-wide effect. Although we cannot completely rule out this explanation because we do not observe which market maker executed each trade, Table 6 takes steps to reduce this concern. We collect SEC Rule 605 disclosures for June 2016, the farthest back period for which we were able to obtain disclosures, and link these disclosures to execution behavior in our sample. Specifically, we create MM_pct as the ratio of shares in 100–499-share trades of a security executed by the four FIF participant market makers to the total number of off-exchange shares traded in that security. We calculate this ratio for the 100–499-share order-size category because it is the closest disclosed trade-size category to odd-lot trades.

The assumption in these tests is that if the four disclosing market makers drive the results in our paper, then the effects should concentrate in securities for which these market makers handle a larger fraction of order flow compared to the rest of the market. Table 6 Panel A shows that the increases in both price improvement and de minimis price improve-

ment are more prominent in securities more frequently executed by the disclosing market makers. However, consistent with our main results, there is no significant difference in effective spreads for odd-lot trades around the disclosure program for securities more frequently handled by the disclosing market makers. To the extent that market share in June 2016 is similar to market share in May 2015 for these market makers, and that 100–499-share orders are handled similarly to odd-lot orders, these results provide evidence that the disclosing market makers are driving the main results of this paper.

In Table 6 Panel B, we further refine this analysis to focus on the two private market makers, Citadel and Two Sigma, that were investigated by the SEC prior to the initiation of the FIF disclosure program and were also affiliated with major hedge fund arms at the center of the post-*Flash Boys* public debate over conflicts between retail order-flow handling and affiliated proprietary trading.[16] As such, these two market makers represent those most likely to feel public scrutiny. We recalculate order-flow exposure within the FIF participants and create Scrutinized_MM_Pct as the relative share of a given security executed by these two scrutinized participant market makers, Citadel and Two Sigma, compared to the shares executed by the other two FIF participant market makers, KCG and UBS.[17] Consistent with our arguments, we find that the increases in PI and DMPI rates are relatively more concentrated in securities handled by the two scrutinized market makers compared to the other FIF participants. This result further validates the relevance of scrutiny in the joint operational and disclosure decisions by these market makers to positively influence perceptions about their execution quality.

[16]See Reuters reporting on SEC investigations of market-making operations involving Citadel and KCG and a related high-frequency trading investigation.

[17]The correlation between Scrutinized_MM_Pct and MM_Pct is 0.175.

5.3. Execution Differences Across Trade and Firm Size

We next show that our main findings reflect a cross-sectional reallocation of price improvement at the outset of the FIF program window toward smaller value trades, where the risk of adverse selection is lower. Table 7 presents cross-sectional evidence across different trade sizes within a given firm-day. These regressions focus exclusively on odd lot trades, and use a variable called $TradeSizeGroup$, which is equal to 1 for trades between 1-10 shares, 2 for trades between 11-20 shares, and so on up to 10. The coefficient of interest is the interaction $TradeSizeGroup \times Post$, which captures how the post-period change in execution quality varies with trade size. We see in column (1) that this interaction is negative and significant, indicating that smaller odd lot trades experienced larger post-period increases in the percentage of shares receiving price improvement. Notably, the main effect of $TradeSizeGroup$ in column (1) is positive, meaning that prior to the program window, larger odd lot trades were the ones more likely to receive price improvement; the negative interaction thus reflects a reallocation of price improvement toward smaller trades rather than a continuation of pre-existing level differences. Column (2) shows that this differential increase appears for the percentage of shares receiving de minimis price improvement as well, with an almost identical economic magnitude between the two columns. This finding suggests that the post-period gains in price improvement among smaller trades were delivered by way of de minimis price improvement. The reason market makers chose these trades when shifting their execution is likely due to the lower risk of adverse selection in the smaller trades. Column (3) documents no significant change in average effective spreads across any trade size group, consistent with our main findings.

Examining across traded firms, we similarly observe that increases in PI are concentrated in large-cap stocks, where adverse selection risk is also likely lower. Table 8 provides results showing execution differences between traded firms of different sizes. To estimate this, we use a triple-difference design that moderates our main design using a $MarketCap$ variable, equal

to the natural log of the traded firm's market capitalization. In column (1), we document that odd lot trades in the post period for larger firms are more likely to receive price improvement, and column (2) shows a corresponding increase in the likelihood of receiving DMPI for odd lot trades of larger firms in the post period. Finally, we show no differences in effective spreads for firms of different sizes in column (3). These cross-sectional effects help characterize the strategy that FIF members used to boost execution quality statistics, suggesting that market makers concentrated their efforts to improve specific execution quality statistics by favoring trades and traded firms where it was least costly in terms of adverse selection.[18]

5.4. Changes in Execution and Trading Responses After the FIF Disclosures

An important feature of our research setting is that we are able to observe changes in execution quality *before* they were publicly observable, which mitigates concerns that market participants changed behavior due to the arrival of information rather than the initiation of the information collection window. In our final empirical tests, shown in Table 9, we assess whether there are clear differences in execution quality or trading responses by odd-lot investors following the first set of disclosures made by FIF participants on June 16, 2015.

Panel A examines execution outcomes using June 16, 2015 as an alternative treatment date, several weeks after the May 6, 2015 rule change. We find no evidence of significant changes in PI or DMPI around this date. The increase in price improvement is small and insignificant relative to the initial response around May 6, and there is also no detectable change in the rate of de minimis price improvement. We do document a small increase in effective spreads, which signals a small *deterioration* in execution quality after the disclosures. This effect, around 2% of the sample mean of effective spreads, still suggests that market makers did not meaningfully improve execution quality around the release of these statistics.

Panel B examines whether odd-lot trading behavior changed around either the May 6

[18]In additional untabulated analyses, we also find the largest increases in rates for both PI and DMPI to be concentrated in the securities previously receiving *lower* rates.

rule change or the June 16 disclosure date. Columns (1) and (3) test whether changes in odd-lot dollar volume are larger in securities with greater FIF market-maker order-flow exposure, using the May 6 and June 16 dates, respectively. Columns (2) and (4) estimate the corresponding baseline odd-lot volume responses around the May 6 and June 16 dates. The main finding is that the interaction with MM_pct is not significant around the May 6 rule-change date, but is positive and significant around the June 16 disclosure date. This pattern provides suggestive evidence that retail investors and/or brokerages may have responded to the first round of public disclosures, particularly in securities where FIF participant market makers handled a larger share of order flow. Importantly, this response occurs after the execution-quality changes documented in our main tests and is therefore distinct from the regulatory mechanism that drove market makers' initial operational changes at the May 6 data collection date, which preceded any public disclosure by six weeks.

6. Experimental Evidence - Trader Responses

Our archival analyses demonstrate that, under increased scrutiny, market makers engaged in operational and disclosure strategies that highlighted price improvement rates while providing minimal actual cost savings. These findings raise the question: is it possible that the strategically emphasized metrics influence investor perceptions of execution quality? To examine this question, we conduct a randomized controlled experiment that directly tests whether disclosure-driven changes in price improvement rates shape investor perceptions and decision-making.

Participants were recruited from Prolific and were informed that they would be evaluating disclosures from hypothetical brokerages.[19] We incorporate an attention check that

[19]Participants had to be at least 18 years old, reside in the US, speak English fluently, have completed at least 10 previous Prolific studies with an approval rate of at least 85%, and have previously made an investment in the common stock of a publicly traded company.

instructed participants to select a particular brokerage from a multiple-choice question after a prompt; 80% of participants passed this check. Upon consenting and starting the task, participants were presented with execution quality statistics from two brokerages, labeled A and B, displayed in a single table. The table included each brokerage's price improvement rate, average savings per order, and other execution quality metrics, with definitions provided alongside the table. The disclosures matched the FIF disclosures exactly, including the table presentation and definitions of order execution metrics. Participants were then asked which brokerage they would choose to send a 50-share order to, followed by two comprehension check questions testing their understanding of the Price Improvement (%) and Average Savings Per Order ($) metrics. Approximately 50% of participants got one of the two comprehension check questions correct, highlighting the difficulty retail investors have in understanding order execution quality based on the disclosure.

To elicit preferences between the PI(%) and average savings per order (ASPO)[20] metrics, we randomly assign Brokerage A's execution statistics within realistic ranges over a uniform distribution and then draw random adjustments to Brokerage B's statistics relative to A, with the average gap in PI% closely matching the magnitudes we observed empirically (approximately 7 percentage points on average). Although we observe no significant change in average savings per order in the archival setting following the FIF disclosures, we also introduce a random gap in ASPO between the brokerages (in the opposite direction of the gap in PI%) to avoid one brokerage dominating the other. We draw the two gaps in opposite directions but with random and independent magnitudes, reducing concerns that any findings are driven by a mechanical correlation between the two gap variables. In expectation, investor welfare is highest by choosing the brokerage with the higher ASPO, meaning the welfare-maximizing choice is the brokerage with higher average savings. As such, eliciting

[20]ASPO is defined as the average dollar amount saved per order from receiving an execution price better than the best available quoted price.

a trade-off *between* PI% and ASPO is a stricter design than the reality in the field, where market makers increased PI% without any loss in ASPO. A complete description of the survey design, participant screening, attention check, comprehension checks, and experimental instrument is provided in Appendix C.

We model each participant's decision to select the brokerage with the higher PI% using a logistic regression, focusing on two key explanatory variables: *PriceImprovementGap* and *SavingsGap*, which represent the standardized differences in PI rates and ASPO between Brokerage A and Brokerage B, respectively. To distinguish the effects of relative differences from absolute quality levels, we also include *PILevel* and *SavingsLevel*, which reflect the standardized PI and ASPO levels for Brokerage A.

Table 10 presents our results. A striking feature of the raw data is that 70% of participants chose the brokerage with the higher price improvement rate despite the brokerage having lower average savings per order. This descriptive evidence by itself suggests that participants anchor on the price improvement metric as a salient statistic for execution quality. In the more stringent regression specifications in Panel A, a larger *PriceImprovementGap* consistently and significantly increases the probability of a participant choosing the higher-PI brokerage across all specifications. *SavingsGap* is also statistically significant in most specifications, indicating that participants are still sensitive to the savings tradeoff. Taken together, the results indicate that participants respond strongly to differences in price improvement rates, consistent with the notion that the price improvement percentage receives disproportionate weight relative to the metric (ASPO) that more accurately captures the economic benefit to investors. This pattern persists in subsamples of participants who (1) passed the attention check, (2) passed both comprehension checks and the attention check, (3) are finance-literate and (4) are finance-literate and passed the attention check, as reported in their respective columns of Panel B of Table 10.

In sum, the experimental evidence is consistent with our archival findings. Individual

decision-makers appear disproportionately influenced by differences in price improvement rates, even when those differences translate into only trivial financial benefits. The finding that price improvement percentage draws disproportionate weight relative to average savings per order even among finance-literate participants who passed attention and comprehension checks indicates that PI% is the salient metric in the disclosure. This finding helps explain why market makers chose to target PI% specifically because a disclosure presenting favorable headline statistics on a metric that individual decision-makers weight heavily reduces the regulatory attention that unfavorable statistics would otherwise invite. More broadly, this evidence illustrates why the coarseness of a disclosed metric matters because when firms can satisfy a binary statistic at minimal cost and present the result favorably, they can influence the information environment that affects their regulatory standing.

7. Conclusion

This study examines how brokerage firms and market makers responded to public scrutiny of odd lot trade execution quality. We focus on a 2015 voluntary disclosure program aimed at enhancing transparency into odd lot execution. We find that the percentage of odd lot shares that received price improvement jumped discontinuously upon adoption of the voluntary disclosure program. However, this increase was driven by allocating trivial amounts of price improvement to more shares. These changes resulted in no material difference in overall execution quality but generated favorable data for market makers on the disclosed metric. Our experimental evidence indicates that this metric is salient to individual decision-makers, suggesting that the operational change of giving trivial price improvement was targeted at the specific statistic that the disclosure highlights.

Our findings have implications for evaluating the effectiveness of disclosure regimes. Studying only the disclosure might suggest genuine improvement, while examining only aggregate operational outcomes might miss the strategic shift toward optimizing specific,

reportable metrics. By analyzing both together, we show that scrutiny paired with verifiable but coarse disclosure metrics can induce a strategic response that is truthful yet misleading. This mechanism is not unique to our setting. Binary and threshold-based disclosure metrics are common across regulatory and voluntary reporting regimes, and in each case, the coarseness of the metric can create scope for firms to satisfy it at minimal cost. Whether verifiability produces meaningful improvement depends on whether the metric and the underlying outcome it is supposed to measure are distinguishable to the audience that motivates regulatory scrutiny, including the media, public discourse, and the individual decision-makers who respond to disclosed information.

References

Bartlett, R. P. (2021). Modernizing Odd Lot Trading. *Columbia Business Law Review*, page 520.

Bartlett, R. P., McCrary, J., and O'Hara, M. (2025). The Market Inside the Market: Odd-lot Quotes. *The Review of Financial Studies*, 38(3):661–711.

Belnap, A. (2023). The Effect of Intermediary Coverage on Disclosure: Evidence from a Randomized Field Experiment. *Journal of Accounting and Economics*, 75(1):101522.

Beyer, A., Cohen, D. A., Lys, T. Z., and Walther, B. R. (2010). The Financial Reporting Environment: Review of the Recent Literature. *Journal of Accounting and Economics*, 50(2-3):296–343.

Beyer, A. and Guttman, I. (2012). Voluntary Disclosure, Manipulation, and Real Effects. *Journal of Accounting Research*, 50(5):1141–1177.

Blackburne, T., Bozanic, Z., Johnson, B. A., and Roulstone, D. T. (2020). The Regulatory Observer Effect: Evidence from SEC Investigations. *Available at SSRN 3514915*.

Boehmer, E., Jennings, R., and Wei, L. (2007). Public Disclosure and Private Decisions: Equity Market Execution Quality and Order Routing. *The Review of Financial Studies*, 20(2):315–358.

Chakrabarty, B., Li, B., Nguyen, V., and Van Ness, R. A. (2007). Trade Classification Algorithms for Electronic Communications Network Trades. *Journal of Banking & Finance*, 31(12):3806–3821.

Cho, C. H., Laine, M., Roberts, R. W., and Rodrigue, M. (2015). Organized Hypocrisy, Organizational Façades, and Sustainability Reporting. *Accounting, Organizations and Society*, 40:78–94.

Cohen, D. A., Dey, A., and Lys, T. Z. (2008). Real and Accrual-based Earnings Management in the Pre-and Post-Sarbanes-Oxley Periods. *The Accounting Review*, 83(3):757–787.

Dyreng, S. D., Hoopes, J. L., and Wilde, J. H. (2016). Public Pressure and Corporate Tax

Behavior. *Journal of Accounting Research*, 54(1):147–186.

Easley, D., López de Prado, M. M., and O'Hara, M. (2012). Flow Toxicity and Liquidity in a High-frequency World. *The Review of Financial Studies*, 25(5):1457–1493.

Ellis, K., Michaely, R., and O'Hara, M. (2000). The Accuracy of Trade Classification Rules: Evidence from Nasdaq. *Journal of Financial and Quantitative Analysis*, 35(4):529–551.

Ernst, T. and Spatt, C. S. (2022). Payment for Order Flow and Asset Choice. Technical report, National Bureau of Economic Research.

Fiechter, P., Hitz, J.-M., and Lehmann, N. (2022). Real Effects of a Widespread CSR Reporting Mandate: Evidence from the European Union's CSR Directive. *Journal of Accounting Research*, 60(4):1499–1549.

Flammer, C., Toffel, M. W., and Viswanathan, K. (2021). Shareholder Activism and Firms' Voluntary Disclosure of Climate Change Risks. *Strategic Management Journal*, 42(10):1850–1879.

Gipper, B., Sampson, C., and Shi, S. X. (2025). Do firms manipulate their carbon emissions reporting? *Available at SSRN 5959895*.

Hanlon, M., Hoopes, J. L., and Shroff, N. (2014). The Effect of Tax Authority Monitoring and Enforcement on Financial Reporting Quality. *The Journal of the American Taxation Association*, 36(2):137–170.

Holden, C. W. and Jacobsen, S. (2014). Liquidity Measurement Problems in Fast, Competitive Markets: Expensive and Cheap Solutions. *The Journal of Finance*, 69(4):1747–1785.

Kanodia, C. and Sapra, H. (2016). A Real Effects Perspective to Accounting Measurement and Disclosure: Implications and Insights for Future Research. *Journal of Accounting Research*, 54(2):623–676.

Kays, A. (2022). Voluntary Disclosure Responses to Mandated Disclosure: Evidence from Australian Corporate Tax Transparency. *The Accounting Review*, 97(4):317–344.

Kim, E.-H. and Lyon, T. P. (2011). Strategic Environmental Disclosure: Evidence from the DOE's Voluntary Greenhouse Gas Registry. *Journal of Environmental Economics and*

Management, 61(3):311–326.

Kubick, T. R., Lynch, D. P., Mayberry, M. A., and Omer, T. C. (2016). The Effects of Regulatory Scrutiny on Tax Avoidance: An Examination of SEC Comment Letters. *The Accounting Review*, 91(6):1751–1780.

Lee, C. M. and Ready, M. J. (1991). Inferring Trade Direction from Intraday Data. *The Journal of Finance*, 46(2):733–746.

Leuz, C. and Wysocki, P. D. (2016). The Economics of Disclosure and Financial Reporting Regulation: Evidence and Suggestions for Future Research. *Journal of Accounting Research*, 54(2):525–622.

Li, F. (2008). Annual Report Readability, Current Earnings, and Earnings Persistence. *Journal of Accounting and Economics*, 45(2-3):221–247.

Li, J. and Wu, D. (2020). Do Corporate Social Responsibility Engagements Lead to Real Environmental, Social, and Governance Impact? *Management Science*, 66(6):2564–2588.

Lipson, M. L. (2003). Competition Among Market Centers. *Available at SSRN 470882*.

Marquis, C., Toffel, M. W., and Zhou, Y. (2016). Scrutiny, Norms, and Selective Disclosure: A Global Study of Greenwashing. *Organization Science*, 27(2):483–504.

Odean, T. (1999). Do Investors Trade Too Much? *American Economic Review*, 89(5):1279–1298.

O'Hara, M., Yao, C., and Ye, M. (2014). What's Not There: Odd Lots and Market Data. *The Journal of Finance*, 69(5):2199–2236.

Packard, H. A., Pawliczek, A., and Skinner, A. N. (2023). Voluntary Performance Disclosures in the CD&A. *The Accounting Review*, 98(6):435–466.

Roychowdhury, S. (2006). Earnings Management through Real Activities Manipulation. *Journal of Accounting and Economics*, 42(3):335–370.

Samuels, D., Taylor, D. J., and Verrecchia, R. E. (2021). The Economics of Misreporting and the Role of Public Scrutiny. *Journal of Accounting and Economics*, 71(1):101340.

She, G. (2022). The Real Effects of Mandatory Nonfinancial Disclosure: Evidence from

Supply Chain Transparency. *The Accounting Review*, 97(5):399–425.

Tomar, S. (2023). Greenhouse Gas Disclosure and Emissions Benchmarking. *Journal of Accounting Research*, 61(2):451–492.

Wood, B. D. and Waterman, R. W. (1991). The Dynamics of Political Control of the Bureaucracy. *American Political Science Review*, 85(3):801–828.

Zhao, X. and Chung, K. H. (2007). Information Disclosure and Market Quality: The Effect of SEC Rule 605 on Trading Costs. *Journal of Financial and Quantitative Analysis*, 42(3):657–682.

Appendix A: Variable Definitions

Variable	Definition	Source
OddLot	Indicator equal to 1 for executed trades on the consolidated tape for less than 100 shares.	TAQ
Non-Odd Lot	Indicator equal to 1 for executed trades on the consolidated tape for over 100 shares.	TAQ
PI	Indicator equal to 1 for individual trades that have received a price better than the national best bid or offer (NBBO).	TAQ
NBBO	The national best bid or offer. This is the best available quoted price across all exchanges and quotes. Calculated using the Daily TAQ NBBO file and the Daily TAQ Quotes file Holden and Jacobsen (2014).	TAQ
DMPI	de minimis price improvement. Indicator equal to 1 for all trades that receive price improvement (PI=1), but the per-share dollar value of price improvement is exactly \$0.0001.	TAQ
Effective Spread	Two times the difference between the filled price of a trade and the prevailing NBBO midpoint, scaled by the midpoint share price of the security. Measures the actual price the investor is paying when trading.	TAQ
Trade Size	The number of shares traded during an individual trade.	TAQ
Savings	The per-share level of price improvement (the difference between the NBBO and the filled price), multiplied by the trade size.	TAQ
Share Price	The price at which the trade order was executed.	TAQ
Volume(\$)	The natural logarithm of total dollars traded during an individual trade. Defined as Trade Size multiplied by Share Price.	TAQ
Market Cap	Total value in millions of dollars of the traded firm. Calculated as shares outstanding multiplied by CRSP share price.	CRSP
Post	Indicator equal to 1 for trades executed on or after May 6, 2015.	TAQ
% Shares PI	This is the percentage of shares for each group/period of interest that have received price improvement. This is defined as the share-weighted average of *PI*.	TAQ
% Shares DMPI	This is the percentage of shares for each group/period of interest that have received de minimis price improvement. This is defined as the share-weighted average of *DMPI*.	TAQ

Appendix A (Continued): Variable Definitions

Variable	Definition	Source
AverageEffectiveSpread	The share-weighted average effective spread for each firm-day-trade group, winsorized at the 1% level. This measure aggregates the effective spread for all trades in each firm-day across odd/non-odd lot trades, and averages these spreads, weighted by the number of shares in each trade. Also shown as AvgEffSpread.	TAQ
Average Savings	Average savings per order is the average dollar value of "savings" (total price improvement) for individual trades. For an individual trade, savings is defined as the per-share level of price improvement (the difference between the NBBO and the filled price), multiplied by the trade size. Average savings per order is then the unweighted average of savings per order, averaged across firm-day-type groups. Also shown as AvgSavings.	TAQ
TradeSizeGroup	Factor variable with integer values ranging from 1-10. This is a separate aggregation measure used to focus within odd lot trades. Equal to 1 for trade sizes between 1-10 shares, 2 for trades between 11-20, 3 for 21-30, 4 for 31-40, 5 for 41-50, up to 10.	TAQ
PostDisclosure	Indicator equal to 1 for trades executed on or after June 16, 2015, when the first set of FIF disclosures were released.	TAQ
MM_pct	The ratio between the number of shares (of trades between 100-499 shares) of a security executed by the four FIF participant market makers (per SEC Rule 605 disclosures), divided by the total number of executed off-exchange shares traded for that security, for June of 2016.	TAQ
# Trades Per Day	Average number of trades executed on a given day for a given security and trade type.	TAQ
PrePI	Pre-period security-level average odd-lot price improvement rate. Computed as the mean of *% Shares PI* for odd-lot observations during the pre-period from April 6, 2015 through May 5, 2015.	TAQ
PostJune16	Indicator equal to 1 for trades executed after June 16, 2015.	TAQ
Scrutinized_MM_Pct	Security-level measure of exposure to the FIF participant market makers receiving greater scrutiny. Constructed using the log volume shares of Citadel and Two Sigma relative to the log volume shares of KCG and UBS, where each market maker's share is based on 100–499-share off-exchange shares executed in June 2016. Winsorized at the 1st and 99th percentiles.	TAQ

Appendix A (Continued): Experimental Variable Definitions

Variable	Definition	Source
PriceImprovementGap	Absolute difference in *Price Improvement* (%) between Brokerage A and Brokerage B presented to experimental participants. Standardized except in specifications marked as using raw levels.	Experiment
SavingsGap	Absolute difference in *Average Savings Per Order* ($) between Brokerage A and Brokerage B presented to experimental participants. Standardized except in specifications marked as using raw levels.	Experiment
PILevel_std	Level of *Price Improvement* (%) for Brokerage A presented to experimental participants, standardized.	Experiment
SavingsLevel_std	Level of *Average Savings Per Order* ($) for Brokerage A presented to experimental participants, standardized.	Experiment
PicksHigherPIBrokerage	Indicator equal to 1 if the experimental participant selected the brokerage offering the higher price improvement rate, and 0 otherwise.	Experiment
AttentionCheck	Indicator equal to 1 if the experimental participant passed the attention check question concerning brokerages.	Experiment
ComprehensionChecks	Indicator equal to 1 if the experimental participant passed both post-survey comprehension check questions concerning the definitions of PI% and ASPO.	Experiment
FinanceLiterate	Indicator equal to 1 if the experimental participant has prior finance coursework or finance-related work experience, defined as a positive response to either having taken any finance courses or to having worked in finance.	Experiment

Appendix B: Example FIF Disclosure - Fidelity's First Set of Disclosures

This figure shows the first set of disclosures made by Fidelity Investments, one of the voluntary participants in the FIF Retail Execution Quality working group. This disclosure was made on June 16, 2015 and was linked from the front page of the FIF's website, along with the disclosures of the other participants. Most participants also featured this disclosure on their own websites near the links to their 605 disclosures.

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	35	96.158%	73.413%	$0.49	0.2285
100 - 499	188	95.822%	86.536%	$2.01	0.2518
500 - 1,999	825	93.904%	82.782%	$6.68	0.2612
2,000 - 4,999	2,634	90.316%	75.409%	$13.23	0.3173
5,000 - 9,999	6,200	85.043%	66.675%	$14.76	0.3085

Other Exchange-Listed 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	34	97.614%	72.757%	$0.66	0.2424
100 - 499	204	97.082%	86.271%	$3.23	0.3177
500 - 1,999	859	95.187%	81.455%	$8.74	0.3789
2,000 - 4,999	2,677	92.188%	74.973%	$15.84	0.4406
5,000 - 9,999	6,026	88.915%	69.166%	$19.35	0.5017

Appendix B (Continued): Variable Definitions Included in the FIF's Disclosures

FIF disclosures consist of standard variable definitions that are included in each individual set of disclosures, with the design of making standard definitions that are easily understood by retail investors. Below are the definitions of each column of the disclosures.

Definitions:

- **Order Size Range (Shares)** – Orders have been grouped into different order size ranges as follows 1 – 99 shares, 100- 499 shares, 500 – 1,999 shares, 2000 – 4,999 shares and 5,000-9,999 shares.
- **Average Order Size (Shares):** Measured as the average order size within each order size range.
- **Shares Executed at Current Market Quote or Better (%):** Measured as the percentage of shares that receive price improvement (an execution priced better than the National Best Bid/Offer at the time of order routing) plus the percentage of shares executed at the National Best Bid/Offer at the time of order routing.
- **Price Improvement (%):** Measured as the percentage of shares that receive price improvement. Price improvement is defined as an execution priced better than the National Best Bid/Offer at the time of order routing.
- **Average Savings per Order ($):** Measured as the net notional dollar value of price improvement per share multiplied by the number of executed shares given to clients divided by the total number of executed orders: [(Net price improvement per Share * Executed Shares) / Executed Orders].
- **Average Execution Speed (Seconds):** Measured from the order route time to execution time.[1]

Appendix C: Experimental Instrument

This appendix documents the experimental instrument in full. Section C.1 describes recruitment and logistics. Section C.2 walks through the complete survey in the order participants encounter it, including the screening questions, the attention check, the main choice task, and the post-task comprehension checks. Section C.3 details the construction of the trade execution tables shown to participants and the randomization protocol underlying the treatment. The full Qualtrics instrument is available at this link.

C.1 Recruitment and Logistics

We recruit participants through Prolific. To qualify, a participant must (1) be at least 18 years old, (2) reside in the United States, (3) speak English fluently, (4) have completed at least 10 prior studies on Prolific, and (5) hold a Prolific approval rate of at least 85%. We balance the sample on gender. We additionally require, via Prolific's pre-screening, that participants have previously invested in the common stock of a publicly traded company. We impose this last restriction because such participants more closely represent the marginal user of trade execution quality disclosures.

The study is posted on Prolific under the short title "Academic Survey" with the longer description: "In this study you will be presented with some disclosures from stock brokerages regarding how well they are doing in executing trades. You will be asked to make decisions based on the provided information." Our sample contains 500 completed responses. Compensation is administered through Prolific upon completion.

C.2 Survey Instrument

The instrument proceeds in four blocks, presented in the order below: (i) background and trading experience (Section C.2.1), (ii) an attention check (Section C.2.2), (iii) the main brokerage choice task (Section C.2.3), and (iv) two comprehension checks (Section C.2.4).

After completing block (iv), participants are returned to Prolific for compensation.

C.2.1 Background and Trading Experience

Participants begin with eight questions establishing their trading experience and financial background:

1. Have you ever set up an online brokerage account? (Yes / No)
2. Have you ever traded a company's stock using an online brokerage? (Yes / No)
3. How often do you trade stocks? (Weekly / Monthly / Annually)
4. Have you ever estimated transaction costs for a stock trade you've made or considered making? (Yes / No)
5. Have you ever requested order routing information from your brokerage? (Yes / No)
6. Have you ever been provided trade execution quality disclosures by your brokerage? (Yes / No / I don't know)
7. Approximately how many finance courses (college-level or higher) have you completed in your life? (Open-ended numeric response)
8. Please indicate the number of years of finance-related work experience that you have. (Open-ended numeric response)

C.2.2 Attention Check

Immediately before the main task, participants complete an attention check that we use to flag inattentive respondents. The full prompt reads:

> *Before we begin, we'd like to understand your familiarity with online brokerages. Many investors use platforms like Fidelity, Charles Schwab, or Robinhood to execute their trades. When selecting a brokerage, investors often consider factors*

> *such as fees, execution quality, and the range of available investment products. Some brokerages also provide detailed reports on how well they execute orders on behalf of their customers. We want to make sure you are reading these instructions carefully, so rather than answering the question below, please select "TD Ameritrade" to continue.*
>
> *Which of the following brokerages would you be most likely to recommend to a friend who has never invested before?*

The response options are TD Ameritrade, Fidelity, Charles Schwab, E*TRADE, and Robinhood. The order in which these options are displayed is randomized across participants. Participants who select any option other than "TD Ameritrade" are flagged as failing the attention check.

C.2.3 Main Choice Task

The main treatment presents participants with a side-by-side execution quality table for two hypothetical brokerages and elicits a binary choice between them. The instructions read:

> *Below are two tables containing execution quality statistics from two different online brokerages, with Brokerage A on the top and Brokerage B on the bottom. These tables present key metrics related to the performance of stock trade executions, such as price improvement rates, average savings, and execution speed. You will use this information to decide which brokerage you would prefer to send your trade to. After reviewing both tables, continue below where you will find definitions for each variable to assist with your decisions or calculations.*

A representative realization of the table appears in the figure below. The numerical values in the two focal columns—Price Improvement (%) and Average Savings Per Order ($)—are drawn independently for each participant according to the protocol in Section C.3.

Brokerage	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)
Brokerage A	1-99	37	98.621%	66.890%	$0.39	0.2040
Brokerage B	1-99	37	98.621%	60.944%	$0.44	0.2040

Figure: Representative realization of the execution quality table shown to participants.

Beneath the table, participants are shown variable definitions taken verbatim from the FIF disclosures. The two definitions most relevant to the choice task—Price Improvement (%) and Average Savings Per Order ($)—appear in the figure below.

Definitions:

Order Size Range (Shares) - Orders have been grouped into different order size ranges as follows 1-99 shares, 100-499 shares, 500-1,999 shares, 2,000-4,999 shares and 5,000-9,999 shares.

Average Order Size (Shares) - Measured as the average order size within each order size range.

Shares Executed at Current Market Quote or Better (%) - Measured as the percentage of shares that receive price improvement (an execution priced better than the National Best Bid/Offer at the time of order routing) plus the percentage of shares executed at the National Best Bid/Offer at the time of order routing.

Price Improvement (%) - Measured as the percentage of shares that receive price improvement. Price improvement is defined as an execution priced better than the National Best Bid/Offer at the time of order routing.

Average Savings Per Order ($) - Measured as the net notional dollar value of price improvement per share multiplied by the number of executed shares given to clients divided by the total number of executed orders: [(Net price improvement per Share * Executed Shares)/Executed Orders].

Average Execution Speed (seconds) - Measured from the order route time to execution time.

Figure: Variable definitions shown to participants beneath the execution quality table. Definitions are reproduced verbatim from the Financial Information Forum (FIF) disclosure templates.

After reviewing the table and definitions, participants make the central choice in our experiment:

> *Presented above are two brokerages. If you were an investor looking to place an order for 50 shares of stock, which brokerage would you choose to send your order to?*
>
> ◯ *Brokerage A (top)* ◯ *Brokerage B (bottom)*

C.2.4 Comprehension Checks

Immediately after the choice task, participants answer two comprehension questions that assess their understanding of the two focal disclosure variables, Price Improvement (%) and Average Savings Per Order ($). For both questions, the order of response options is randomized across participants. We mark the correct response below with a dagger (†); the dagger does not appear in the participant-facing instrument.

1. Which of the following best describes what "Price Improvement (%)" measures?

    - The percentage of total dollar savings relative to the total dollar value of all orders executed.
    - † The percentage of shares that received an execution price better than the best available quoted price at the time the order was received.
    - The percentage by which the average execution price was better than the best available quoted price.
    - The average percentage discount received on each order relative to the best available quoted price.

2. Which of the following best describes what "Average Savings Per Order ($)" measures?

    - The difference in brokerage commissions or fees charged per order relative to the industry average.
    - The dollar amount saved on each order that received price improvement.
    - The total dollar amount saved across all orders from receiving execution prices better than the best available quoted prices.
    - † The average dollar amount saved per order from receiving an execution price better than the best available quoted price.

After submitting their comprehension responses, participants are returned to Prolific for compensation.

C.3 Treatment Construction and Randomization

The execution quality table shown in the main choice task (Section C.2.3) is generated from random draws specific to each participant. We construct Brokerage A's row first, then construct Brokerage B's row as a function of Brokerage A's, so that only the two focal columns (Price Improvement (%) and Average Savings Per Order ($)) vary across the two brokerages. This holds constant all non-focal dimensions of execution quality and isolates the participant's relative weighting of the two focal metrics.

Fixed columns. The Order Size Range column is fixed at "1–99" shares for both brokerages, consistent with the hypothetical 50-share order in the choice prompt.

Brokerage A. Each of the remaining columns of Brokerage A's row is an independent random draw, with display precision matching the precision shown in the participant-facing table:

- **Average Order Size (shares):** random integer drawn uniformly from $\{20, 21, \ldots, 50\}$.
- **Shares Executed at Current Market Quote or Better (%):** continuous uniform on $[95.000, 100.000)$, displayed to three decimal places.
- **Price Improvement (%):** continuous uniform on $[40.000, 80.000)$, displayed to three decimal places.
- **Average Savings Per Order ($):** uniform draw from $\{\$0.06, \$0.07, \ldots, \$1.06\}$. This is implemented by drawing $u \sim U[0, 1)$, rounding to the nearest cent, and shifting by \$0.06 to ensure a non-zero positive value.

- **Average Execution Speed (seconds):** continuous uniform on $[0.1000, 0.4000)$, displayed to four decimal places.

Brokerage B. Brokerage B's Average Order Size, Shares Executed at Current Market Quote or Better, and Average Execution Speed columns are set equal to Brokerage A's values. Brokerage B's two focal columns are constructed by applying signed adjustments to Brokerage A's values:

- **Price Improvement (%) adjustment.** We draw an adjustment magnitude $\delta_{PI} \sim U[2.00, 10.00)$ and assign it a positive or negative sign with equal probability. The signed adjustment is added to Brokerage A's Price Improvement (%) to obtain Brokerage B's value, displayed to three decimal places.
- **Average Savings Per Order ($) adjustment.** We draw an adjustment magnitude $\delta_{ASPO} \sim U[\$0.01, \$0.05)$ and assign it the sign *opposite* that drawn for the price improvement adjustment. The signed adjustment is added to Brokerage A's Average Savings Per Order ($) and the result is rounded to the nearest cent to obtain Brokerage B's value.

By construction, Brokerage A and Brokerage B always differ on both focal metrics, and the two adjustments are of opposite signs. Each participant therefore faces a genuine tradeoff: whichever brokerage offers the higher Price Improvement (%) necessarily offers the lower Average Savings Per Order ($), and vice versa. This design isolates the participant's relative weighting of the two focal metrics from any confounding variation in the non-focal table contents.

8. Figures and Tables

Figure 2. Price Improvement Rates and Effective Spreads - 1-199 Share Trades

This figure plots rates of price improvement and average effective spreads for odd lot and non-odd lot trade groups in the 4 months surrounding the FIF program initiation. In Panel A, each observation represents the percentage of shares that received price improvement across off-exchange trades of shares between 1 and 199, excluding 100 share trades, executed for that trade type that day. In Panel B, each observation represents the per-share average effective spread across the same off-exchange trades for that trade type that day. The red line on May 6, 2015 is the beginning of the disclosure program window (i.e., treatment period).

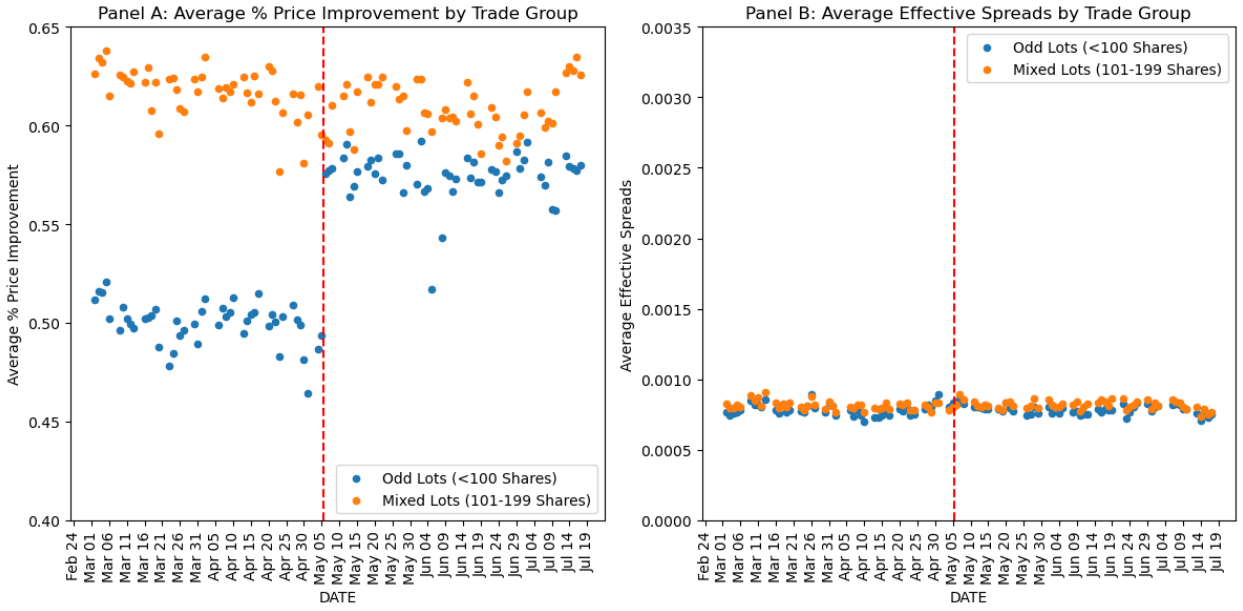
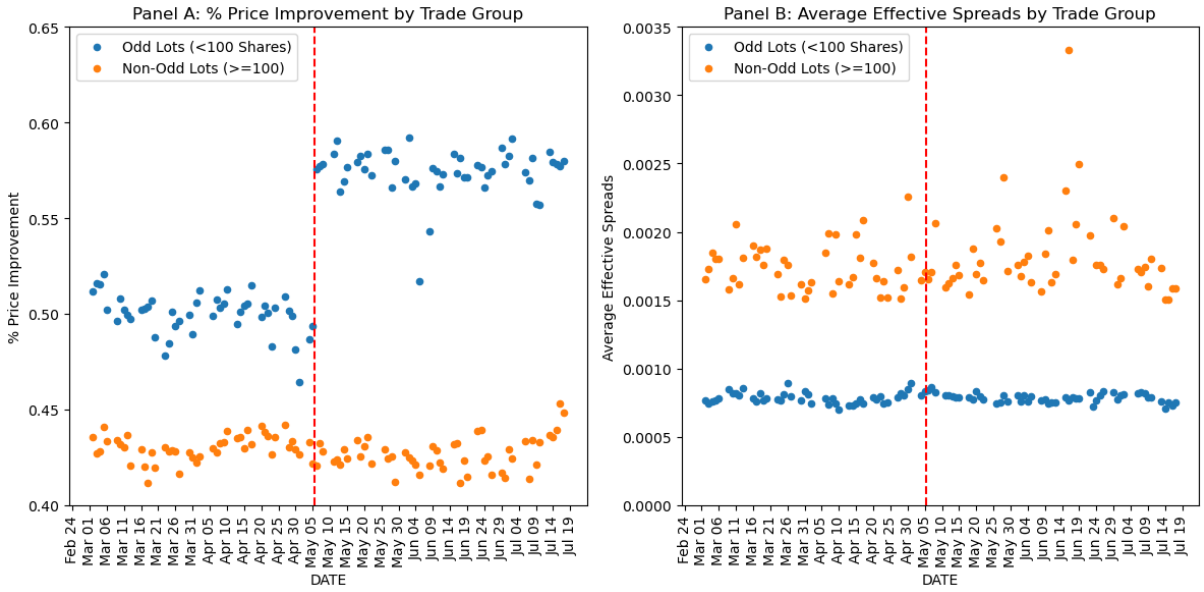


Figure 3. Price Improvement Rates and Effective Spreads for All Trades



This figure plots rates of price improvement and average effective spreads for odd lot and non-odd lot trade groups in the 4 months surrounding the FIF program initiation. In Panel A, each observation represents the percentage of shares that received price improvement across all off-exchange trades for that trade type that day. In Panel B, each observation represents the per-share average effective spread across all off-exchange trades for that trade type that day. The red line on May 6, 2015 is the beginning of the disclosure program window (i.e., treatment period).

Figure 4. de minimis Price Improvement Rates

This figure plots aggregate de minimis price improvement over the sample period for odd lot and non-odd lot trade groups. Each observation represents the percentage of shares that received price improvement of $0.0001 per share. The red line on May 6, 2015 is the beginning of the treatment period.

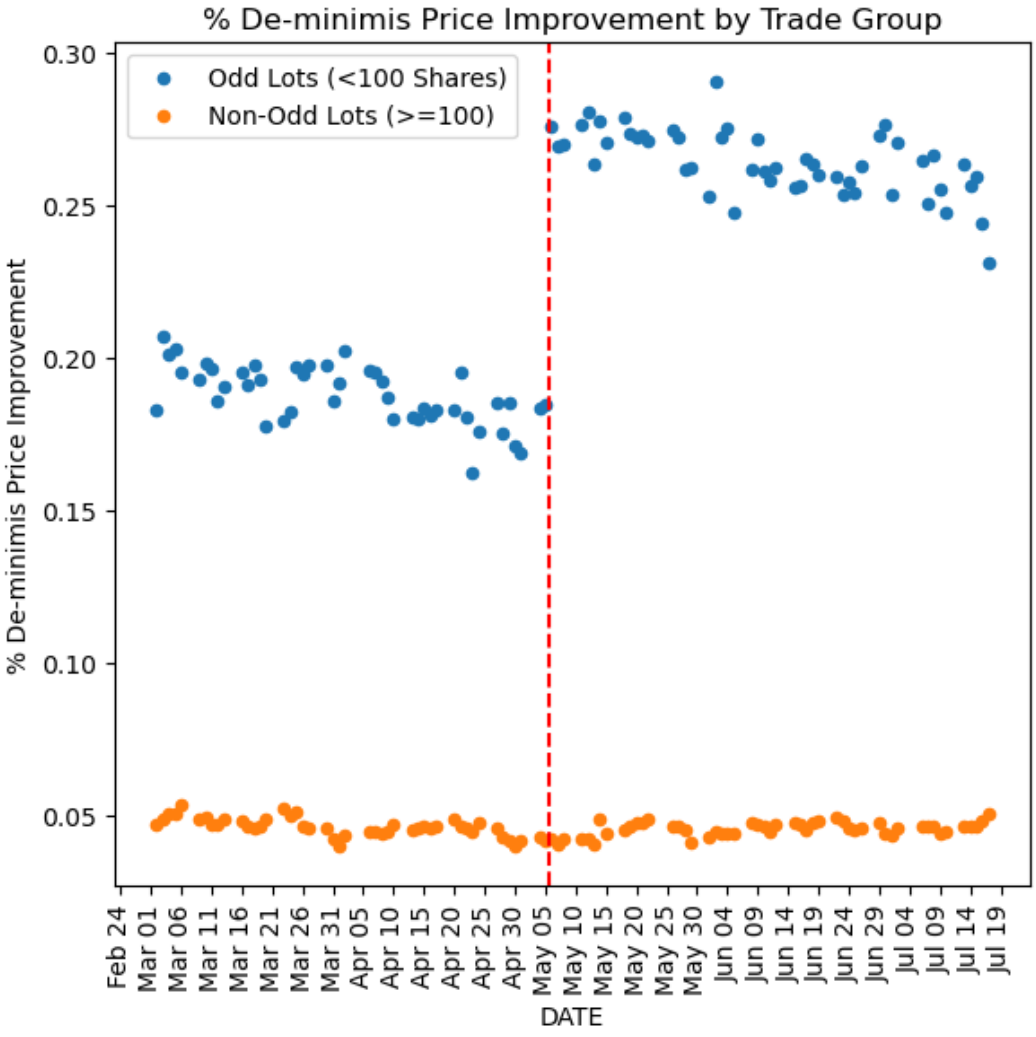
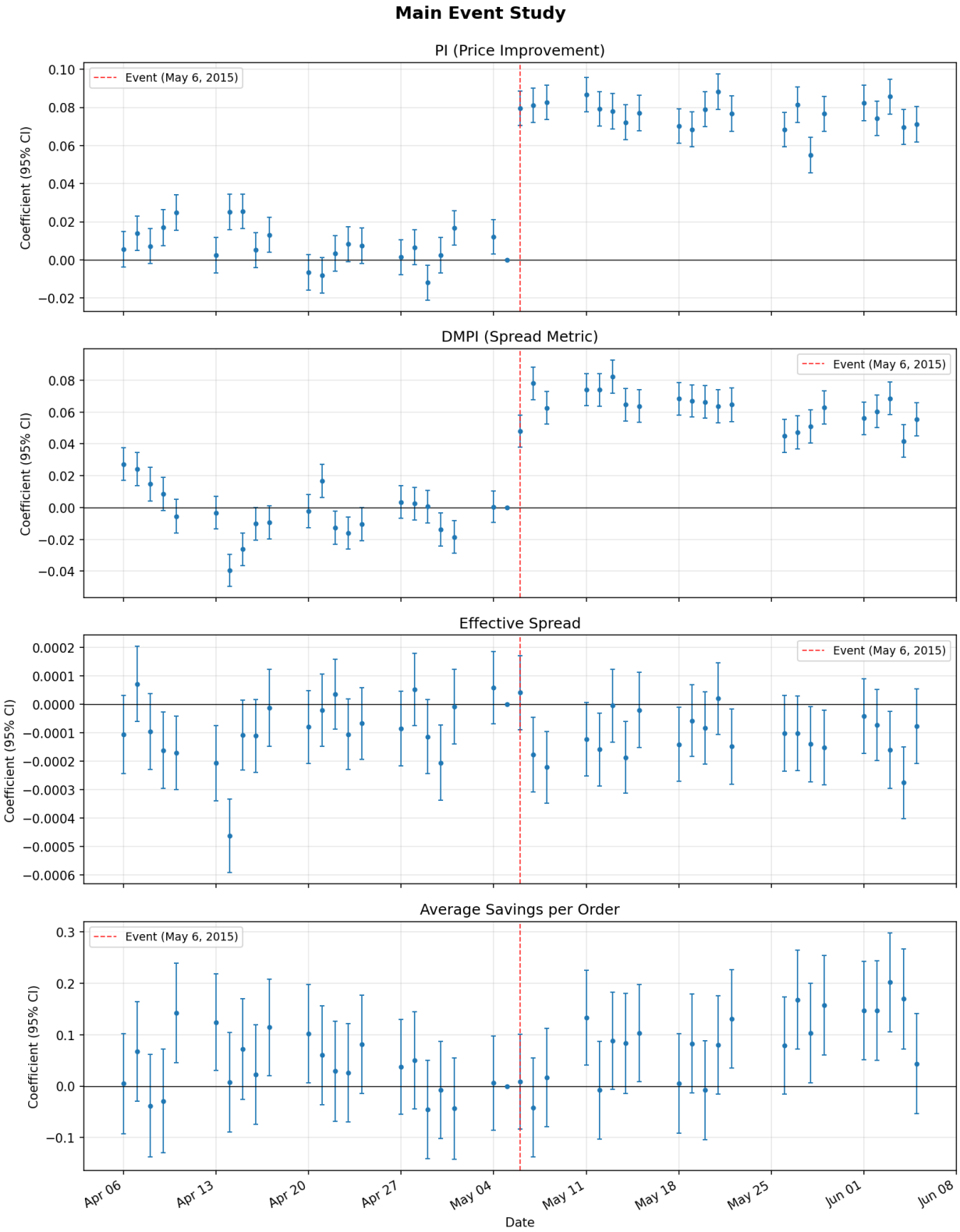
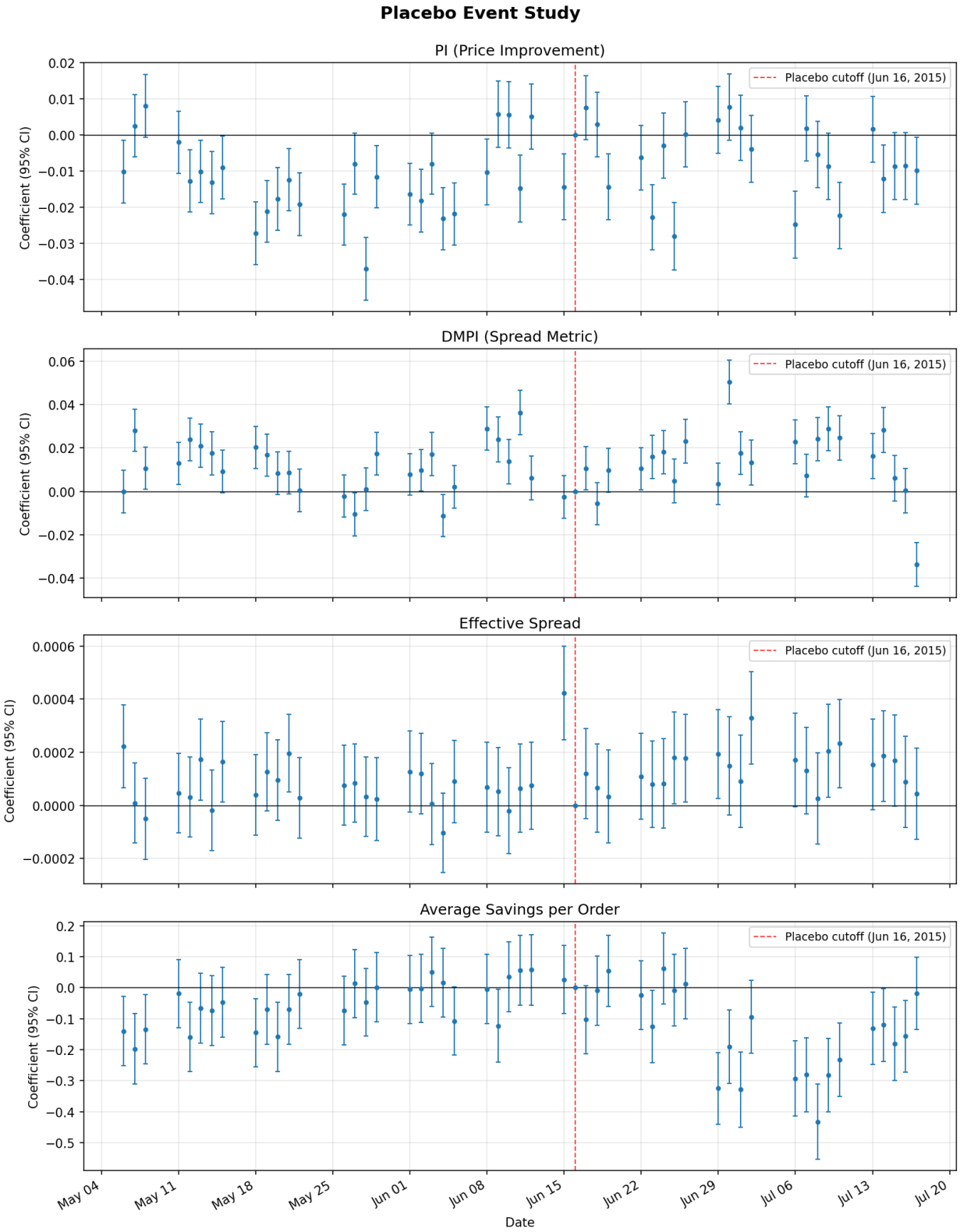


Figure 5. Main Coefficient Plots



Main Event Study Coefficients around May 6, 2015. This figure plots event-time coefficients from versions of Tables 2, 3, and 4 to assess differential outcomes for odd-lot trades, day by day, around the event. Within each panel the points are coefficients on OddLot interacted with a date indicator from a regression of the outcome on these interactions, OddLot, and a Volume control, with firm and day fixed effects. The reference day is the trading day immediately before May 6, 2015, so each coefficient is read relative to the day before the event. Vertical bars are 95% confidence intervals based on standard errors clustered by firm, and the red dashed line marks the treatment date.

Figure 6. Coefficient Plots around June 16 Disclosure Date



Event Study Coefficients around June 16 Disclosure Date. This figure plots event-time coefficients from versions of Tables 2, 3, and 4 estimated using post-May-6 data only, centered around the actual disclosure date of June 16, 2015. Within each panel the points are coefficients on OddLot interacted with a date indicator from a regression of the outcome on these interactions, OddLot, and a Volume control, with firm and day fixed effects. The reference day is the trading day immediately before June 16, 2015, so each coefficient is read relative to the day before the event. Vertical bars are 95% confidence intervals based on standard errors clustered by firm, and the red dashed line marks the treatment date.

Table 1. Summary Statistics

This table provides summary statistics for individual trades in our sample. Each trade receives a value of either 0 or 1 for whether or not it received price improvement (PI) and de minimis price improvement (DMPI), measured as trades receiving per-share price improvement of $0.0001. Effective spreads are calculated as 2 times the difference between the price and the national best bid or offer (NBBO), scaled by the midpoint. Savings is the dollar amount of total price improvement given in a trade. Trade size is the number of shares traded each trade. Share price is the average executed share price per trade. Volume is the dollar value of each individual trade. Market cap is total value in millions of dollars of public traded stock, calculated as the number of shares outstanding multiplied by share price from CRSP.

Panel A: Trade Level

	Mean	Median	Std	25th Pctl	75th Pctl
Non-Odd Lots					
% Shares PI	0.4388	0	.4949	0	1
% Shares DMPI	0.0883	0	0.2300	0	0
Effective Spread	0.0018	0.0004	0.099	0.00012	0.0013
Savings	1.59	0	63.25	0	0.655
Trade Size	357.15	100	6374.18	100	200
Share Price	45.71	32.69	51.93	15.20	59.24
Volume($)	13119.99	4573	290117.71	2107.50	9297
Market Cap	22755.48	7458.58	918152.70	1765.37	30326.38
Odd Lots					
% Shares PI	0.5379	1	0.4987	0	1
% Shares DMPI	0.2401	0	0.4369	0	0
Effective Spread	0.00078	0.00027	0.0125	0.00012	0.00068
Savings	0.127	0.0004	1.28	0	0.027
Trade Size	30.59	23	27.09	7	50
Share Price	77.18	56.97	89.72	31.24	97.28
Volume($)	2112.82	1046.52	3315.69	292.68	2716.55
Market Cap	31088.57	14211.48	417970.24	3262.35	59198.26
Non-Odd Lot Trades:	253,712,344				
Odd Lot Trades:	38,827,524				

Table 1. Summary Statistics (continued)

This table provides summary statistics for individual firm-days in our sample. Each trade receives a value of either 0 or 1 for whether or not it received price improvement (PI) and de minimis price improvement (DMPI). Effective spreads are calculated as 2 times the difference between the price and the national best bid or offer (NBBO), scaled by the midpoint. Average savings is the average dollar amount of total price improvement given per trade, averaged for each firm-day. Share price is the average executed share price per trade. Volume is the dollar value traded each firm-day in thousands. Market cap is total value in millions of dollars of public traded stock, calculated as the number of shares outstanding multiplied by share price from CRSP.

Panel B: Firm Day Level					
	Mean	Median	Std	25th Pctl	75th Pctl
Non-Odd Lots					
% Shares PI	0.576	.582	0.216	0.438	0.723
% Shares DMPI	0.149	0.099	0.162	0039	0.206
Effective Spread	0.004	0.001	0.007	0.0006	0.0036
Average Savings	3.75	1.25	69.50	0.48	2.97
# Trades Per Day	833.90	176	2319.42	38	699
Volume(Thousands)	7621.79	837.81	18205.86	143.27	4858.92
Market Cap	4124.88	510.95	11988.84	113.44	2332.48
Odd Lots					
% Shares PI	0.544	0.552	0.238	0.420	0.682
% Shares DMPI	0.247	0.216	0.211	0.103	0.332
Effective Spread	0.0037	0.0014	0.0067	0.0007	0.0035
Average Savings	0.208	0.0768	0.45	0.0274	0.209
# Trades Per Day	131.28	30	429.05	9	98
Volume(Thousands)	275.84	18.61	1539.34	3.33	106.45
Market Cap	4214.84	543.54	12105.32	124.18	2417.81
Non-Odd Lot Firm-Days:	304,249				
Odd Lot Firm-Days:	295,754				

Table 2. **Price Improvement**

This table provides results from the main difference-in-differences specification, looking at changes in the percent of shares that receive price improvement before and after the treatment window for odd lot and non-odd lot trades. OddLot is equal to 1 for trades under 100 shares, Post corresponds to trades executed on or after May 6, 2015. Volume is the natural log of total dollar volume of trades executed for each group-firm-day.

Dependent Variable:	% of Shares Receiving Price Improvement	
Model:	(1)	(2)
Variables		
OddLot × Post	0.0687***	0.0680***
	(0.0028)	(0.0024)
OddLot	-0.0654***	-0.0654***
	(0.0024)	(0.0036)
Volume		-0.00001
		(0.0009)
Volume × Post		-0.0002
		(0.0005)
Fixed-effects		
Firm	Yes	Yes
Day	Yes	Yes
Fit statistics		
Observations	600,003	600,003
R^2	0.19327	0.19327
Within R^2	0.01266	0.01266

Clustered (Firm & Day) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 3. **Effective Spreads**

This table provides results from the main difference-in-differences specification, looking at changes in effective spreads and average savings per order before and after the treatment window between odd lot and non-odd lot trades. Effective spreads are calculated as 2 times the difference between the price and the national best bid or offer (NBBO), scaled by the midpoint price. Firm-day effective spreads are the share-weighted average of effective spreads executed during each firm-day, winsorized at the 1% level. Average savings per order is the average dollar savings given to each trade, also winsorized. OddLot is equal to 1 for trades under 100 shares, Post corresponds to trades executed on or after May 6, 2015. Volume is the natural log of total dollar volume of trades executed for each group-firm-day.

Dependent Variables:	Average Effective Spread		Average Savings	
Model:	(1)	(2)	(3)	(4)
Variables				
OddLot × Post	-0.00002	-0.00005	-0.1755	0.1712
	(0.00003)	(0.00006)	(0.2704)	(0.1777)
OddLot	0.0002***	-0.00002	-3.377***	4.219**
	(0.00003)	(0.00008)	(0.2900)	(1.701)
Volume		-0.00006***		1.952***
		(0.00002)		(0.4981)
Volume × Post		-0.000007		0.0687
		(0.00001)		(0.0738)
Fixed-effects				
Firm	Yes	Yes	Yes	Yes
Day	Yes	Yes	Yes	Yes
Fit statistics				
Observations	600,003	600,003	600,003	600,003
R^2	0.74807	0.74813	0.09935	0.10069
Within R^2	0.00078	0.00103	0.00135	0.00283

Clustered (Firm & Day) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4. de minimis Price Improvement (DMPI)

This table provides results from the main difference-in-differences specification, looking at changes in the percent of shares that receive de minimis price improvement (DMPI) before and after the treatment window for odd lot and non-odd lot trades. Trades receive DMPI if they receive a price better than the national best bid or offer, and the total level of price improvement is equal to \$0.0001. OddLot is equal to 1 for trades under 100 shares, Post corresponds to trades executed on or after May 6, 2015. Volume is the natural log of total dollar volume of trades executed for each group-firm-day.

Dependent Variable:	% Shares that Receive DMPI (PI = \$0.0001)	
Model:	(1)	(2)
Variables		
OddLot × Post	0.0688***	0.0742***
	(0.0023)	(0.0027)
OddLot	0.0573***	0.0610***
	(0.0016)	(0.0025)
Volume		0.0009*
		(0.0005)
Volume × Post		0.0014***
		(0.0004)
Fixed-effects		
Firm	Yes	Yes
Day	Yes	Yes
Fit statistics		
Observations	600,003	600,003
R^2	0.21280	0.21300
Within R^2	0.09723	0.09746

Clustered (Firm & Day) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 5. Narrower Trade Size Samples

This table provides a robustness test for results from the main difference-in-differences specification, using restricted samples in narrower windows around the 100-share line. Panel A tests price improvement, and Panel B tests de minimis price improvement, using the same definitions as Tables 2 and 4, respectively. In both panels, Column (1) restricts the sample to trades between 70 and 110 shares, Column (2) restricts the sample to trades between 90 and 110 shares, Column (3) restricts the sample to trades between 1 and 199 shares while excluding exactly 100-share trades, and Column (4) restricts the sample to trades between 90 and 110 shares while excluding exactly 100-share trades. OddLot is equal to 1 for trades under 100 shares, and Post corresponds to trades executed on or after May 6, 2015. Volume is the natural log of total dollar trading volume for each firm-day. Standard errors are clustered at the firm and day level.

Panel A: Price Improvement

Dependent Variable:	% of Shares Receiving Price Improvement			
Trade Size Sample:	70-110	90-110	1-199, not 100	90-110, not 100
Variables				
OddLot × Post	0.0559***	0.0494***	0.0718***	0.0397***
	(0.0033)	(0.0034)	(0.0030)	(0.0035)
OddLot	0.0786***	0.0766***	-0.1657***	-0.1192***
	(0.0034)	(0.0038)	(0.0021)	(0.0027)
Volume	0.0040***	0.0034***	-0.0093***	-0.0316***
	(0.0011)	(0.0009)	(0.0012)	(0.0014)
Volume × Post	0.0014**	0.0010*	0.0005	0.0055***
	(0.0006)	(0.0006)	(0.0009)	(0.0010)
Fixed-effects				
Firm	Yes	Yes	Yes	Yes
Day	Yes	Yes	Yes	Yes
Fit statistics				
Observations	535,081	482,006	553,630	334,501
R^2	0.17373	0.15555	0.2347	0.14630
Within R^2	0.03694	0.02550	0.0808	0.02614

Clustered (Firm & Day) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 5. Narrower Trade Size Samples (continued)

Panel B: DMPI				
Dependent Variable:	% of Shares Receiving DMPI			
Trade Size Sample:	70-110	90-110	1-199, not 100	90-110, not 100
Variables				
OddLot × Post	0.0689***	0.0644***	0.0794***	0.0545***
	(0.0036)	(0.0037)	(0.0023)	(0.0062)
OddLot	0.1622***	0.1434***	0.0154***	0.0546***
	(0.0038)	(0.0042)	(0.0017)	(0.0051)
Volume	-0.0201***	-0.0198***	-0.0079***	0.0046**
	(0.0009)	(0.0008)	(0.0008)	(0.0021)
Volume × Post	0.0027***	0.0022***	0.0018***	0.0040***
	(0.0005)	(0.0005)	(0.0006)	(0.0014)
Fixed-effects				
Firm	Yes	Yes	Yes	Yes
Day	Yes	Yes	Yes	Yes
Fit statistics				
Observations	495,777	434,745	553,630	272,808
R^2	0.29060	0.25804	0.1674	0.09148
Within R^2	0.20280	0.16584	0.0390	0.01446

Clustered (Firm & Day) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 6. Execution Differences across Market Maker Order Flow

This table uses a triple difference-in-differences design to test whether execution differences vary with market maker order flow. Panel A uses MM_pct as the additional independent variable. MM_pct is calculated as the ratio of shares in 100–499-share trades of a security executed by the four FIF participant market makers, divided by the total number of off-exchange shares traded for that security, measured in June 2016. Panel B uses Scrutinized_MM_Pct as the additional independent variable. Scrutinized_MM_Pct is constructed using security-level volume shares for the two market makers considered most scrutinized within our sample, Citadel and Two Sigma, relative to the shares of the other two FIF participant market makers, KCG and UBS, where each market maker's contribution is the number of 100–499-share off-exchange shares executed by that FIF participant in June 2016. Post corresponds to trades executed on or after May 6, 2015. %PI and %DMPI are calculated using the same methods as Tables 2 and 4, respectively. Effective spreads are calculated as in Table 3. Volume is the natural log of total dollar trading volume for each firm-day. Standard errors are clustered at the firm level.

Panel A: FIF vs. Non-FIF Order Flow			
Dependent Variables:	% PI	% DMPI	EffectiveSpread
Model:	(1)	(2)	(3)
Variables			
OddLot × Post × MM_pct	0.0675***	0.0406***	0.0001
	(0.0082)	(0.0063)	(0.0001)
OddLot	0.0025	0.0943***	-0.0002**
	(0.0039)	(0.0026)	(0.00007)
Volume	0.0017**	0.0018***	-0.00006***
	(0.0007)	(0.0005)	(0.00002)
OddLot × Post	0.0577***	0.0674***	-0.00004
	(0.0028)	(0.0020)	(0.00005)
OddLot × MM_pct	-0.2129***	-0.1016***	0.0005***
	(0.0084)	(0.0065)	(0.0001)
Post × MM_pct	0.0407***	0.0204***	0.0002
	(0.0060)	(0.0040)	(0.0002)
Volume × Post	0.0020***	0.0025***	0.000008
	(0.0004)	(0.0003)	(0.00001)
Fixed-effects			
Firm	Yes	Yes	Yes
Day	Yes	Yes	Yes
Fit statistics			
Observations	554,377	554,377	554,377
R^2	0.19904	0.21896	0.74028
Within R^2	0.02059	0.10506	0.00143

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 6. Execution Differences across Market Maker Order Flow (continued)

Panel B: Within-FIF Differences in Scrutiny			
Dependent Variables:	% PI	% DMPI	EffectiveSpread
Model:	(1)	(2)	(3)
Variables			
OddLot × Post × Scrutinized_MM_Pct	0.0096***	0.0053*	0.0000
	(0.0035)	(0.0029)	(0.0001)
OddLot	-0.0602***	0.0654***	-0.0000
	(0.0031)	(0.0019)	(0.0001)
Volume	0.0006	0.0014***	-0.0001***
	(0.0007)	(0.0005)	(0.0000)
OddLot × Post	0.0671***	0.0738***	-0.0000
	(0.0018)	(0.0014)	(0.0000)
OddLot × Scrutinized_MM_Pct	-0.0171***	-0.0103***	0.0001*
	(0.0033)	(0.0027)	(0.0000)
Post × Scrutinized_MM_Pct	0.0025	0.0006	0.0001
	(0.0024)	(0.0016)	(0.0001)
Volume × Post	-0.0001	0.0014***	0.0000
	(0.0003)	(0.0003)	(0.0000)
Fixed-effects			
Firm	Yes	Yes	Yes
Day	Yes	Yes	Yes
Fit statistics			
Observations	554,397	554,397	554,397
R^2	0.19353	0.21671	0.74023
Within R^2	0.01378	0.10252	0.00127

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 7. Odd Lot Trade Size Execution Differences

This table examines whether execution differences vary across odd-lot trade sizes. The sample is restricted to odd-lot trades only. TradeSizeGroup is an integer from 1 to 10, where 1 corresponds to trades between 0 and 10 shares, 2 corresponds to trades between 11 and 20 shares, and so on through larger odd-lot trade-size bins. Post corresponds to trades executed on or after May 6, 2015. %PI and %DMPI are calculated using the same methods as Tables 2 and 4, respectively. Effective spreads are calculated as in Table 3. Volume is the natural log of total dollar trading volume for each firm-day. Standard errors are clustered at the firm and day level.

Dependent Variables:	% PI	% DMPI	EffectiveSpread
Model:	(1)	(2)	(3)
Variables			
TradeSizeGroup × Post	-0.0056***	-0.0054***	-0.000002
	(0.0004)	(0.0003)	(0.000003)
TradeSizeGroup	0.0018***	-0.0020***	-0.000006**
	(0.0003)	(0.0002)	(0.000003)
Volume	0.0131***	-0.0063***	-0.0002***
	(0.0013)	(0.0011)	(0.000009)
Volume × Post	0.0049***	0.0056***	-0.000007
	(0.0009)	(0.0005)	(0.000009)
Fixed-effects			
Firm	Yes	Yes	Yes
Day	Yes	Yes	Yes
Fit statistics			
Observations	2,140,744	2,140,744	2,140,744
R^2	0.09114	0.09978	0.67684
Within R^2	0.00224	0.00518	0.00485

Clustered (Firm & Day) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 8. Execution Differences across Firm Size

This table uses a triple difference-in-differences design to test whether execution differences vary with firm size. MarketCap is the additional independent variable and is defined as the natural log of market capitalization of the traded firm, calculated using price and shares outstanding. Post corresponds to trades executed on or after May 6, 2015. %PI and %DMPI are calculated using the same methods as Tables 2 and 4, respectively. Effective spreads are calculated as in Table 3. Volume is the natural log of total dollar trading volume for each firm-day. Standard errors are clustered at the firm and day level.

Dependent Variables:	% PI	% DMPI	EffectiveSpread
Model:	(1)	(2)	(3)
Variables			
OddLot × MarketCap × Post	0.0019**	0.0026***	-0.000001
	(0.0008)	(0.0007)	(0.00002)
OddLot	-0.1752***	-0.0115**	0.0003*
	(0.0066)	(0.0053)	(0.0002)
MarketCap	0.0360***	-0.0008	-0.0011***
	(0.0056)	(0.0030)	(0.0002)
Volume	-0.0044***	-0.0020***	-0.00009***
	(0.0009)	(0.0006)	(0.00002)
OddLot × Post	0.0698***	0.0631***	0.00004
	(0.0062)	(0.0063)	(0.0002)
OddLot × MarketCap	0.0159***	0.0115***	-0.00006***
	(0.0008)	(0.0006)	(0.00002)
MarketCap × Post	-0.0053***	-0.0022***	-0.00003
	(0.0008)	(0.0006)	(0.00002)
Volume × Post	0.0036***	0.0025***	0.00001
	(0.0007)	(0.0006)	(0.00002)
Fixed-effects			
Firm	Yes	Yes	Yes
Day	Yes	Yes	Yes
Fit statistics			
Observations	414,044	414,044	414,044
R^2	0.20457	0.24888	0.74860
Within R^2	0.02041	0.13357	0.00445

Clustered (Firm & Day) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 9. Execution Outcomes and Trading Responses for Odd-Lot Investors around June 16 Disclosure Date

This table reports tests of execution outcomes and trading responses for odd-lot investors around the June 16, 2015 disclosure date. Panel A replicates the design of Column (1) of Tables 2, 4, and 3, respectively, substituting June 16, 2015 as an alternative treatment date, several weeks after the May 6, 2015 rule change. The sample uses the window around the June 16, 2015 disclosure date, May 6 – July 16, 2015. Panel B reports difference-in-differences regressions for potential trading responses by odd-lot investors around the June 16, 2015 disclosure date and the May 6, 2015 rule change. In Panel B, Columns (1) and (2) use the June 16 date, and Columns (3) and (4) use the May 6 date; Columns (1) and (3) estimate baseline responses, while Columns (2) and (4) add interactions with MM_pct. OddLot is equal to 1 for trades under 100 shares. Post is equal to 1 for trades executed on or after May 6, 2015. PostJune16 is equal to 1 for trades executed on or after June 16, 2015. %PI and %DMPI are calculated using the same methods as Tables 2 and 4, respectively. Effective spreads are calculated as in Table 3. Volume is the natural log of total dollar trading volume for each odd-lot/round-lot, firm, day group; in Panel B, ln(Volume) is winsorized at the 5%/95% level within each day. Standard errors are clustered at the firm and day level.

Panel A: June 16 Outcomes			
Dependent Variables:	% PI	% DMPI	EffectiveSpread
Model:	(1)	(2)	(3)
Variables			
OddLot × PostJune16	0.0048	0.0040	0.00008***
	(0.0030)	(0.0040)	(0.00002)
OddLot	0.0556***	0.2667***	-0.00015**
	(0.0037)	(0.0037)	(0.00006)
Fixed-effects			
Firm	Yes	Yes	Yes
Day	Yes	Yes	Yes
Fit statistics			
Observations	572,674	557,015	572,674
R^2	0.25580	0.46020	0.74188
Within R^2	0.01418	0.36446	0.00146

Clustered (Firm & Day) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Execution Outcomes and Trading Responses for Odd-Lot Investors around June 16 Disclosure Date (continued)

Panel B: Trading Responses				
Dependent Variable:	Volume ($)			
Model:	(1)	(2)	(3)	(4)
Variables				
OddLot × PostJune16	0.0712***	0.0096		
	(0.0146)	(0.0226)		
OddLot × PostJune16 × MM_pct		0.3049***		
		(0.0616)		
OddLot × Post			-0.0380***	-0.0270
			(0.0117)	(0.0199)
OddLot × Post × MM_pct				-0.0477
				(0.0374)
OddLot × MM_pct		1.6972***		1.4341***
		(0.0844)		(0.0714)
PostJune16 × MM_pct		-0.3546***		
		(0.0662)		
Post × MM_pct				-0.0722*
				(0.0437)
OddLot	-3.6333***	-4.0914***	-3.6707***	-4.0689***
	(0.0172)	(0.0291)	(0.0143)	(0.0260)
Fixed-effects				
Firm	Yes	Yes	Yes	Yes
Day	Yes	Yes	Yes	Yes
Fit statistics				
Observations	586,516	545,893	599,991	554,371
R^2	0.92198	0.92547	0.91814	0.92198
Within R^2	0.82327	0.83168	0.82423	0.83061

Clustered (Firm & Day) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 10. **Experimental Results**

This table reports logistic regressions of an indicator equal to 1 if a participant chose the brokerage offering higher price improvement. PriceImprovementGap is the absolute difference between the two brokerages in price improvement, and SavingsGap is the absolute difference between the two brokerages in average savings per order. By experimental design, the higher-price-improvement brokerage always offers lower average savings, so a larger SavingsGap represents a larger savings trade-off against choosing the higher-price-improvement brokerage. PILevel and SavingsLevel are the price improvement and average savings levels for Brokerage A and control for overall magnitudes. Panel A presents four specifications on the full sample. Column (1) includes only PriceImprovementGap, Column (2) includes only SavingsGap, and Column (3) includes both gaps, with Columns (1)–(3) using standardized gap and level variables. Column (4) repeats the Column (3) specification using raw, unstandardized variables for comparability of magnitudes. Panel B applies the Panel A Column (3) specification to four subsamples: participants who passed the attention check, participants who passed the attention check and both comprehension checks, finance-literate participants, and finance-literate participants who also passed the attention check.

Panel A: Main Specifications				
Dependent Variable:	1(Picks Higher-PI Brokerage)			
Model:	(1)	(2)	(3)	(4)
Variables				
PriceImprovementGap	0.465***		0.293**	0.128**
	(0.104)		(0.118)	(0.052)
SavingsGap		-0.496***	-0.355***	-21.477***
		(0.104)	(0.118)	(7.124)
PILevel	0.198*	0.219**	0.212**	0.018**
	(0.101)	(0.101)	(0.102)	(0.009)
SavingsLevel	0.092	0.112	0.111	0.375
	(0.100)	(0.101)	(0.102)	(0.344)
Fit statistics				
Observations	500	500	500	500
Pseudo R^2	0.043	0.048	0.058	0.058
Raw variables	No	No	No	Yes

Standard errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Experimental Results (continued)

Panel B: Subsamples

Dependent Variable:	1(Picks Higher-PI Brokerage)			
Subsample:	Attention Check	Attention + Comprehension	Finance-Literate	Finance-Lit. + Att. Check
Model:	(1)	(2)	(3)	(4)
Variables				
PriceImprovementGap	0.361***	0.537**	0.299**	0.381**
	(0.135)	(0.239)	(0.139)	(0.160)
SavingsGap	-0.313**	-0.261	-0.364***	-0.308*
	(0.134)	(0.233)	(0.139)	(0.160)
PILevel	0.201*	0.318*	0.158	0.117
	(0.114)	(0.191)	(0.119)	(0.133)
SavingsLevel	0.084	-0.126	0.068	0.068
	(0.113)	(0.188)	(0.119)	(0.134)
Fit statistics				
Observations	392	148	371	287
Pseudo R^2	0.061	0.097	0.056	0.059

Standard errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*