Information in Prices

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Abstract

Theory suggests that informational frictions lead to mispricing and resource misallocation. We empirically test how an exogenous upgrade to the public quote signal impacts price informativeness. We exploit the SEC's Rule 600(b) amendment that made round-lot size price-contingent, rendering some previously odd-lot quotes NBBO-eligible. Using an event-study design with synthetic difference-in-differences, we find that the improvement in the quote signal increases price informativeness and reduces noise in the price. Our results show how even minor enhancements in information dissemination can lead to meaningful improvements in market quality.

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JEL Classification: G10, G12, G14

A central theme in modern information economics is that frictions in the acquisition and dissemination of information can have first-order real effects. David et al. (2016) formalize this idea by showing how firms making decisions under imperfect, noisy signals can generate misallocation and lower aggregate productivity. Farboodi and Veldkamp (2020) push the logic further by treating improvements in financial data technology as an equilibrium force that changes what information is produced, transmitted, and ultimately embedded in market signals. This perspective updates Hayek's classic insight that the price system is valuable as it compresses dispersed, locally held information into a publicly observable signal that coordinates decentralized decisions. Motivated by these theories, we test how the public signal impacts the informational content of asset prices.

Over the past few decades, equity market design has changed in ways that impact what information is visible, to whom, and when. Trading has moved to electronic limit order books; latency and execution costs have fallen; and a growing share of activity now arrives in small pieces. A simple but consequential example of the piecemeal nature of modern markets is the rise of odd-lot orders. Odd-lots are orders for fewer than 100 shares, which now account for a large fraction of trades in U.S. equities (Bartlett, 2021). Odd-lots are not just "small." They are often the natural unit of execution for algorithmic order slicing, for retail traders, and for high-priced stocks where 100 shares is economically large. Yet, despite their prevalence, odd-lot quotes historically have not been fully incorporated into the National Best Bid and Offer (NBBO) disseminated through the SIP. Instead, full odd-lot quoting interest is primarily visible to traders who purchase proprietary exchange data feeds. The result is a two-tier information environment: some participants observe a richer quote signal than others.

On November 3, 2025, the U.S. Securities and Exchange Commission (SEC) implemented an amendment to Rule 600(b) that changed round-lot sizes (SEC, 2025). Recognizing the increase in odd-lot trading, the SEC aimed to enhance market participants'

information set and enable trading at better prices. The SEC introduced a tiered round-lot structure: 100 shares for stocks below $250.01, 40 shares for stocks with prices above $250, 10 shares for stocks with prices above $1,000 and 1 share for stocks with prices above $10,000. For high-priced stocks, the NBBO incorporates smaller quotes that had previously been odd-lot quotes into the best price and disseminates them to the market. Consequently, the reduction in lot sizes for these stocks changes the composition of the NBBO and thereby arguably improves pre-trade information available to a broader audience.

In this paper, we investigate the empirical relationship between transparency and price informativeness. The question is of first-order importance because efficient prices play a crucial role in the allocation of scarce resources (Hayek, 1945). Thus, the information conveyed in prices affects the real economy by improving the decision of capital allocators (Leland, 1992; Dow and Gorton, 1997; Dow and Rahi, 2003; Goldstein and Guembel, 2008). To establish causal effects of transparency on price informativeness, we study the Rule 600(b) amendment as an exogenous shock to public information. After the event, a fraction of odd-lot quotes have become fully eligible to set the best price and thus have become transparent to a broader market audience.

Following an event-study design, we classify stocks where the amendment changed the round-lot size as treated and those unaffected as control stocks. We sample control stocks based on market capitalization strata and estimate the average treatment effect with a symmetric event window of ten trading days. Because the price-delay measure violates the parallel trend assumption, we use synthetic difference-in-differences (SynthDiD) as an identification strategy. SynthDiD is a weighted difference-in-differences estimator that reweights observations by unit weights to improve parallel trends and time weights to reduce estimation bias from dissimilar periods (Arkhangelsky et al., 2021).

After the reduction in round-lot sizes, we find that prices have become more infor-

mative for high-priced stocks. We examine Hasbrouck (1993)'s pricing error to capture deviations of the efficient price from a random walk and document that the pricing error ratio declines by 14.7%. This result suggests that observed prices are closer to the efficient price and less distorted by market frictions, such as the inability to process available information. We further strengthen the results by documenting a reduction in the noise share of the treated stocks using the information decomposition by Brogaard et al. (2021). The noise share - the component that is neither attributed to information nor changes in the discount rate (drift) - decreases by 21.9%. Finally, we find that the speed of information incorporation increases. By constructing a price-delay measure in the spirit of Hou and Moskowitz (2005), we document that the price delay decreases by 3.2%. The result suggests that a higher fraction of the return variation is attributed to contemporaneous returns and thus market participants incorporate information more quickly.

We next ask whether the decline in noise is accompanied by changes in other information components. We follow Brogaard et al. (2021) to decompose the return variance into information contribution from the market, the firm-specific, and the noise component. We find that treated stocks exhibit a pronounced reallocation towards firm-specific information. The firm-specific private and public information increase by 11.5% and by 37.1%, respectively. The substantial increase in contribution by firm-specific public information is consistent with a positive public information shock. Following the event, more information is incorporated by quotes. In addition, we document that the increase in the public signal is also associated with an increase in the private information component. The result indicates that the improvement is not accompanied by a crowding-out of private information acquisition.

Documenting that the contribution of firm-specific information increases, we next examine whether that increase comes at the cost of liquidity providers. Research on the effects of transparency on adverse selection is mixed (Boulatov and George, 2013; Harris,

1996; Glosten, 1999; Baruch, 2005). We find that liquidity providers face less adverse selection and lower order flow toxicity. The relative price impact decreases by 2.5% and the volume-synchronized probability of informed trading (VPIN) by Easley et al. (2012) decreases by 15%. These results are consistent with Glosten (1999) who argues that an increase in transparency reduces adverse selection by greater commonality of information.

The contribution of our paper is threefold. First, we study how market frictions affect price informativeness. We use the Rule 600(b) amendment as an information shock to publicly available information. By examining price informativeness, we document that market frictions have strong effects on information in prices. Next, we provide timely evidence on the effect of the Rule 600(b) amendment. As such, we provide feedback to the regulator and input into their evaluation of the reform. Third, we contribute to the literature on transparency. Whereas prior studies typically examine venue-specific transparency changes, the amendment affects the full market. As it alters which quotes are protected and eligible for NBBO, it provides a clean setting to study how broader dissemination of previously excluded but informative odd-lot quotes affect price informativeness.

The remainder of the paper is structured as follows. Section I discusses the institutional details. While Section II presents the data and sample, Section III provides our identification strategy, main results, and robustness checks. Section IV further explores changes in the information environment whereas Section V concludes.

I Institutional Details

This section describes the institutional details, the Rule 600(b) amendment, and our theoretical predictions. We first explain how odd-lot quotes are treated in the pre-event NBBO and argue how the Rule 600(b) amendment changes the composition of the NBBO

for high-priced stocks. We then provide evidence that the implementation has indeed changed the composition and size distribution of displayed quotes and trades, rather than aggregate market activity. Motivated by these institutional details, we provide our theoretical prediction on the effect on price formation, while leaving the net effect on informativeness ex ante ambiguous because greater transparency may both improve information aggregation but also alter strategic trading incentives.

A NBBO and Odd-Lot Quotes

The National Market System (NMS) was established in 1975 and provides the national best bid and offer price (NBBO) across exchanges. This consolidated limit order book is collected and disseminated through two Securities Information Processors (SIP).[1] Importantly, it consists of round-lot quotes. Although odd-lot orders may be priced more aggressively than the currently displayed quote, exchanges can post only a bundle of odd-lot orders that sums to a round lot. The exchange posts the least aggressive price of that bundle as their best price. For example, if an exchange's top-three asks are 20 shares priced at \$10.30, 70 shares at \$10.20, and 10 shares at \$10.10, the exchange can display the best price for 100 shares at \$10.30. The same logic also applies to bids. For a more detailed discussion on the institutional details of odd-lots, we refer the reader to Bartlett (2021) or Bartlett et al. (2025).

Interestingly, the full content of odd-lot quotes is not disseminated to all market participants. The current market structure creates an information asymmetry between market participants. Market participants that subscribe to the proprietary data feed of the respective exchange can observe odd-lot quotes. Because these quotes have reduced transparency, informed traders might use this property because they have an incentive to conceal their information from the market. As such, market participants that learn

[1]For more information, we refer to https://www.finra.org/rules-guidance/guidance/national-market-system-plans.

about the odd-lot quotes can use them to infer their expectation of fundamental value of the stock and its full liquidity. Consequently, the information and liquidity of odd-lot quotes arguably contain value.

B The Rule 600(b) Amendment

In September 2024, the SEC adopted the final rules for several amendments: *Regulation NMS: Minimum Pricing Increments, Access Fees, and Transparency of Better* (SEC, 2024). Among these amendments, the Rule 600(b) amendment changed the traditional round-lot definition for high-priced stocks. Under revised Rule 600(b)(93), the historical 100-share round-lot has been replaced by a tiered schedule that depends on the stock's average closing price on its primary listing exchange during the prior evaluation period. The evaluation periods, defined under 600(b)(93)(iii), consist of all trading days in March (September) for enforcement on the first business day of June (November). Hence, the round-lot sizes are reassessed on a semiannual basis.

The new round-lot sizes are as follows:

$$RoundLots_i = \begin{cases} 100, & \text{if } \overline{p_i} \leq \$250.00 \\ 40, & \text{if } \$1{,}000.00 \geq \overline{p_i} \geq \$250.01 \\ 10, & \text{if } \$10{,}000.00 \geq \overline{p_i} \geq \$1{,}000.01 \\ 1, & \text{if } \overline{p_i} \geq \$10{,}000.01 \end{cases} \tag{1}$$

where $\overline{p_i}$ is the average closing price for stock, i, during the evaluation period. These rule changes are designed to increase transparency and trading activity by allowing market participants to trade at better prices. Hence, the round-lot size for stocks with a closing price below \$250.01 is not affected. The resulting NBBO has a minimum lot of \$10,000 for stocks at the price threshold. This is a substantial reduction when considering high-

priced stocks. For example, a stock with an average closing price of $1,000.01 had a minimum lot of about $100,000 before.

While the change to the round-lot sizes under Rule 600(b)(93) became effective on the first business day of November 2025, the SEC granted a temporary exemptive relief that postponed compliance on the remaining amendments (SEC, 2025). This postponement creates a clean event without confounding changes in other market design dimensions. The event is an exogenous shock to the public information set and altered the composition of the consolidated limit order book.[2]

[**Figure 1** here]

Figure 1 shows the development of the NBBO depths, trading volume and odd-lot trading volume around the Rule 600(b) amendment. The top row reports aggregated market activity by the total NBBO depth, total trading volume, and total odd-lot volume. The bottom row reports the corresponding average measures. The vertical line marks the enforcement date. In the top row, we observe a sharp and persistent decline in total odd-lot volume. Market participants trade against smaller quantities than before and seem not to increase their frequency of odd-lot trading to offset any loss in volume. By contrast, the total trading volume and the total NBBO depth remain similar, highlighting that the aggregate level of market activity has not been affected.

On the other hand, the average measures show a substantial decline. Average odd-lot volume declines due to lower size thresholds for odd-lot classification. The average NBBO depth also drops substantially. While at first glance counterintuitive, the reduction in average NBBO depth is reasonable. For example, TSLA at roughly $400 had a minimum depth of $400,000 ($400 * 100) before the event and a minimum depth of at least $160,000 ($400 * 40) after the event. As such, the average NBBO depth is reduced unless the

[2]We are not aware of any other major regulatory or market-structure change affecting U.S. equities during this 20-day window.

new top-level shares exceed 100 shares. It shows that the total NBBO depth is sliced into smaller depths per trading instance. Consequently, the figure underscores that the event changed the composition and size distribution of orders and trades rather than the aggregated market activity.

C Theoretical Hypotheses

After the Rule 600(b) amendment, many odd-lot quotes become NBBO-eligible. Bartlett et al. (2025) document that odd-lot quotes convey information about future NBBO midpoint changes and O'Hara et al. (2014) show that odd-lot trades contribute about 30% of the efficient price. Consistent with the idea that informed traders use odd-lot sizes as part of their execution strategy, Roseman et al. (2018) document that odd-lot trades are used by stealth traders.

The event has the potential to alter price formation by expanding the set of informative quotes. The new composition is disseminated through the SIP to a wider market audience. Hence, more market participants can use this information to update their expectations about the fundamental value and quote on these expectations with lower capital requirements. In this sense, the event lowers the cost of accessing quote information. Theoretical work suggests that lower information acquisition costs can improve the incorporation of information into prices (Verrecchia, 1982; Hellwig, 1980). In addition, improving one information channel can stimulate information acquisition and strengthen price efficiency (Veldkamp, 2006; Goldstein and Yang, 2015).

At the same time, the effect of transparency on price informativeness is theoretically and empirically ambiguous. On the one hand, broader dissemination of quote and order-flow information improves the ability to infer fundamental value and thereby enhances the information in prices (Biais, 1993; Pagano and Röell, 1996; Madhavan, 1995; Madhavan, 1996). Consistent with this view, Bloomfield and O'Hara (1999) show experimentally

that trade disclosure speeds the convergence of prices to fundamentals, while Boehmer et al. (2005) and Hendershott and Jones (2005) find that greater quote transparency improves informational efficiency.

On the other hand, transparency can alter strategic behavior by exposing trading interest of liquidity providers. This may weaken liquidity provision and thus affect the price efficiency (Harris, 1996; Madhavan et al., 2005). Moreover, the informativeness of prices also depends on how effectively order flow reveals private information in equilibrium (Kyle, 1985; Admati and Pfleiderer, 1988). Gozluklu et al. (2015) document improved market quality without a corresponding improvement in informational efficiency when Borsa Italiana changed its minimum lot to one unit across all stocks. In our setting, the event may reduce the informational advantage of traders who previously relied on proprietary feeds. This may lead to a crowding-out of private information production and changes in how market participants coordinate on information (Morris and Shin, 2002; Amador and Weill, 2010) which could negatively affect price informativeness.

II Data

This section describes the sample, data, and variable construction. We combine intraday quote and trade data from DTAQ with stock-level information from CRSP to form treated and control samples around the Rule 600(b) amendment. After applying standard data filters, we construct measures of price informativeness and adverse selection, including pricing errors, relative information shares, price delay, price impact, and order flow toxicity. These measures provide the empirical foundation for our analysis of how the event affects price formation and information-related trading costs.

A Data Sources and Cleaning

Our initial sample of stocks consists of all common stocks that are present in the Daily TAQ (DTAQ) master file within 10 trading days around November 3, 2025. We restrict our sample to all stocks listed in the CRSP database as of December 31, 2024, with a closing price exceeding \$5.00. To keep the full sample traceable, we calculate market capitalization strata and randomly select 250 stocks per stratum for the control sample. We then exclude any stock that is not available during our event window.

To classify our sample into treatment and control groups, we calculate the monthly average closing price during the evaluation period of September 2025. Stocks with an average closing price above (below) \$250 are assigned to the treatment (control) group. Our sample consists of 164 treated stocks and 742 control stocks. For this sample, we access intraday quote- and trade-level information from DTAQ and stock-level information from CRSP using the WRDS API based on tidyfinance (Scheuch et al., 2023). We clean the DTAQ data following Holden and Jacobsen (2014) and Holden et al. (2023) and construct price informativeness measures based on the respective NBBO and trade price series.

B Informativeness Measures

The objective of our paper is to assess changes in price informativeness following the Rule 600(b) amendment. As such, we rely on several measures that have been proposed in the literature.

First, we decompose observed prices into an efficient price component and a pricing error following Hasbrouck (1993). The magnitude of the pricing error, measured by its variance, is an indicator of informational efficiency. We obtain the pricing error by estimating a VAR model on intraday midpoint returns corresponding to trades and trade

characteristics. For a meaningful comparison, we scale the standard deviation of the pricing error by the standard deviation of the logarithm of price.

$$Pricing\ Error\ Ratio = \frac{\sigma_s}{\sigma_p} \tag{2}$$

where σ_s is the standard deviation of the pricing error and σ_p is the standard deviation of the log price. A high pricing error ratio suggests less efficient prices.

Next, our second measure for price informativeness follows Brogaard et al. (2021). We estimate a structural VAR with market midpoint return, signed dollar volume, and stock midpoint return to obtain relative information contribution from various sources of information. The model assumes that the stock return variance can be fully decomposed into four distinct sources of variation corresponding to variance driven by market-wide information (market midpoint return), private firm-specific information revealed through trading (signed volume), public firm-specific information (stock midpoint return), and noise (pricing error). The variance components are normalized to 100% to express variance shares:

$$Info\ Share = \frac{\theta_C^2 \sigma_{\epsilon_C}^2}{\sum_{j \neq C} \theta_j^2 \sigma_j^2 + \theta_C^2 \sigma_C^2} \tag{3}$$

where $\theta_C^2 \sigma_C^2$ denotes the relative variance share by C and C is an element of {Market, Private, Public, Noise}. The intuition is that temporary shifts with subsequent reversion reflect the temporary component and price stabilization above its initial level reflects permanent components. The transient component can be interpreted as deviations from equilibrium prices that arise from sources such as overreaction (underreaction) to information, imperfect liquidity, and microstructure frictions.

Finally, we estimate the price delay specified by Hou and Moskowitz (2005) to capture delayed incorporation of information. We estimate two time-series models with stock and market returns and their lags. The price delay is measured as the fraction of variation of

contemporaneous individual stock returns explained by lagged stock and market returns:

$$Price\ Delay = 1 - \frac{R^2_{restricted|\beta_{t-h}=0}}{R^2_{unrestricted}} \tag{4}$$

The measure can be interpreted as the additional fit attributed to information based on lagged coefficients. Unlike the other measures, the price delay indicates delayed information incorporation as another dimension to price informativeness.

Each measure is estimated intraday. To allow for stable estimates, we split the trading day into ten-minute intervals. For each estimation, we include five lags to account for serial correlation based on short-term momentum and reversal, persistence in order flow, and non-synchronous trading. Finally, we aggregate the coefficients to daily averages.

C Adverse Selection Measures

Since previous research provides a link between changes in transparency and adverse selection, we examine this relationship by analyzing the price impact component of the relative effective spread and order flow toxicity by Easley et al. (2012).

First, the relative effective spread measures the transaction costs associated with a trade. These costs can be decomposed into the realized spread (profit for liquidity providers) and the price impact (adverse selection). The price impact quantifies how much the price moves against the liquidity provider within a given time window. Since transitory effects associated with order-processing cost and inventory management are short-lived, the residual effect is associated with adverse selection. We measure the price impact as the relative change in the fundamental value 5 minutes after the trade:

$$Price\ Impact = 2 \sum w_i d_i \frac{M_{t+h} - M_t}{M_t} \tag{5}$$

where M_t is the fundamental value (midpoint) at the time of trade, h is five minutes after

the trade, w_i is the respective volume weight, and d_i is the trade direction. It measures how much the fundamental value changed after the trade to quantify how much liquidity providers are adversely selected by informed traders.

Next, we estimate order flow toxicity by Easley et al. (2012).[3] Order flow toxicity is measured by volume-synchronized probability of informed trading (VPIN) where we split the trading day into volume buckets to i) mimic the speed at which information arrives, and ii) to be based on a comparable amount of information. These specifications are important because volume can be very imbalanced during the trading day and updating in clock-time leads to heterogeneous amounts of information. The assumption is that signed volume correlates with order toxicity and an imbalance to buy (sell) volume suggests that toxicity arises from good (bad) news. To construct the measure, we set the equal-sized volume buckets to one-fiftieth of the average daily volume to approximate the expected trade imbalance. We then calculate the sum of the absolute order imbalance in each volume-bucket and divide it by the average daily volume:

$$VPIN = \frac{\alpha\mu}{\alpha\mu + 2\epsilon} \approx \frac{\sum_{\tau=1}^{50} |V_\tau^S - V_\tau^B|}{50 \times V} \tag{6}$$

where $\alpha\mu$ is the arrival rate of informed orders and 2ϵ is the arrival rate for uninformed orders. Easley et al. (2012) show that this approach yields the probability of informed trading where a high (low) VPIN is associated with a high (low) probability of informed trading.

[3]Different from Easley et al. (2012), we calculate the VPIN on a daily basis and do not create a rolling measure.

III The Role of Limit Orders in Information Aggregation

This section examines whether the increase in public quote transparency affects price informativeness and market quality. We first describe the identification strategy and motivate our use of synthetic difference-in-differences to address potential non-parallel trends between treated and control stocks in the price delay measure. We then show that the amendment is associated with a significant improvement in price informativeness: treated stocks exhibit lower pricing errors, lower noise shares, and lower price delay after the event relative to the synthetic control group. The cross-sectional and robustness results point in the same direction, with stronger effects for stocks more exposed to the rule change. Finally, we show that these improvements do not come at the expense of liquidity provision, as adverse-selection costs and trading costs also decline.

A Identification Strategy

To examine the effects of changes in the NBBO composition on price informativeness, we follow an event-study design using ten trading days around the event. In event studies, researchers commonly apply traditional difference-in-differences (DiD) to infer the treatment effect. The DiD is a double difference estimator that allocates the treatment effect based on the difference in the outcome of the treatment group after adjusting for the difference in the outcome from the control group. However, the method relies on a parallel trend assumption to establish the control group as the counterfactual for how the outcome would have evolved in the absence of treatment.

[**Figure 2** here]

Figure 2 presents the visual inspection of parallel trends between the treatment and

control group. The top panel is the *Noise Share*, the middle panel is the *Pricing Error Ratio*, and the bottom panel is the *Price Delay*. We match the control group to the treated group based on market capitalization and the average relative quoted spread two months prior to the event. Figure 2 shows strong parallel trends for the *Noise Share* and the *Pricing Error Ratio* because the control and treatment group move in tandem. For the *Price Delay*, however, the parallel trends seem to be violated. This violation makes inference based on traditional DiD questionable for the price-delay measure.

To have the same methodology across price informativeness measures, we opt for synthetic difference-in-differences (SynthDiD). This method is designed for settings in which traditional DiD is compromised by non-parallel trends (Arkhangelsky et al., 2021). SynthDiD combines key properties of DiD and synthetic control. Similar to DiD, it uses double differencing to absorb time-invariant differences between treated and control units. Similar to synthetic control, it uses unit and time weights to improve balance in pre-treatment outcome levels and reduce bias from dissimilar control units or periods. Accordingly, SynthDiD can be interpreted as a weighted two-way fixed-effects estimator that localizes identification to more comparable units and time periods. Recent studies use SynthDiD to estimate treatment effects in related settings (Brogaard et al., 2025, Do et al., 2024). For detailed discussions, see Arkhangelsky et al. (2021), Clarke et al. (2023), and Porreca (2022).

SynthDiD can be written as a standard two-way fixed-effects DiD which is estimated via weighted least squares with (i) unit weights on the controls and (ii) time weights on the pre-event period. We use our full sample of 164 treated and 742 control stocks to estimate the SynthDiD according to the following specification:

$$Y_{it} = \alpha_i + \delta_t + \tau Treat_i \times Post_t + \beta X + \epsilon_{it} \tag{7}$$

where the index i refers to stocks, and t refers to the trading day. $Treat_i$ is a dummy

that is one if a stock had an average closing price above \$250 in September 2025. $Post_t$ is a dummy that is one if the trading day is at or past November 3, 2025. α_i and δ_t are stock- and day fixed effects which control for unobserved characteristics. X denotes control covariates: the total log dollar turnover and intraday volatility.

The coefficient of interest is τ. It identifies the average treatment effect of stocks subject to the round-lot change. Intuitively, τ measures the post-event change in price informativeness for treated stocks relative to (i) their own pre-event level and (ii) the change measured in the synthetic control.

B Summary Statistics

We present our summary statistics of the treated stocks and the synthetic control group. Since SynthDiD applies unit and time-weights to create a counterfactual control group, we calculate the average statistics for the controls using these exact weights. Table 1 presents the summary statistics of our sample in the pre-event and post-event period.

[**Table 1** here]

The *Pricing Error Ratio* is 0.05 before and 0.041 after the event for the treated stocks compared to 0.05 before and 0.049 after the event for the control stocks. In contrast to the moderate decline in the price error ratio with a significant at the 10%, the difference between the pre- and post period for treatment stocks is highly significant and of a larger magnitude. The *Noise Share* shows a similar pattern. Before the event, it was about 0.56 for the treated stocks and 0.38 for the control stocks. After the event both show a reduction in the *Noise Share* but importantly, the change for the treated stocks is of a magnitude larger than for the control stocks, indicating likely event effects. Finally, the *Price Delay* is 0.834 for the treated stocks and 0.8215 for the control group. The treatment stocks show an improvement in price delay that is not observed in the control stocks.

C Main Results

Following the summary statistics, we now turn to the results of the DiD and SynthDiD. Although SynthDiD is our main identification method, we provide our findings on traditional DiD. Since the parallel trends for the *Pricing Error Ratio* and the *Noise Share* have been intact, we assume that the SynthDiD and DiD recover similar average treatment effects.

[**Table 2** here]

Table 2 presents the results on price informativeness based on Equation (7). Panel A illustrates the baseline DiD results of a matched sample where we match control to treated stocks based on market capitalization and the relative quoted spread two months before the event. Panel B presents the SynthDiD results which improve the pre-treatment outcome dynamics by reducing bias arising from dissimilar control units or periods.

As shown in Panel A, the *Pricing Error Ratio* and the *Noise Share* are highly significant and negative. *Pricing Error Ratio* and *Noise Share* decrease by 16.1% ($\frac{-0.0088}{0.05}$) and 22.6% ($\frac{-0.1302}{0.58}$), respectively. The results show a substantial decline in noise in the prices of high-priced stocks. Although the *Price Delay* is also negative, it is statistically insignificant. The control variables for *Turnover* and *Volatility* are in line with our expectations. *Turnover* is negative and significant. It suggests that more trading activity reduces the noise in the efficient price. Since order flow is informative to some degree, liquidity providers learn from the aggregated flows and improve their expectation of the efficient price. On the other hand, *Volatility* is positive and only significant for *Pricing Error Ratio*. A higher intraday volatility is in line with a more difficult setting to extract the fundamental value of the stock.[4]

[4] A concern is that the control variables might be affected by the event itself. We note that the results do not change qualitatively when excluding them from the analysis.

Turning to Panel B, the estimates for the *Pricing Error Ratio*, *Noise Share*, and *Price Delay* are all negative and statistically significant. Prices in the treated group have become more informative. The *Pricing Error Ratio* declines by 14.7% ($\frac{-0.008}{0.05}$), the *Noise Share* by 21.9% ($\frac{-0.1262}{0.58}$), and *Price Delay* by 3.2% ($\frac{-0.0274}{0.87}$).[5] These results confirm the reduction in the level of transitory noise in prices and the contribution of noise to return variation seen in Panel A. Hence, the observed prices track the efficient price more closely after the reform. Further, we find an acceleration of the incorporation of information into prices. The decline in *Price Delay* indicates that less of the variation in current returns is explained by lagged market information. It is strong in both magnitude and statistical significance compared to Panel A.

Our findings are consistent with the empirical evidence in Boehmer et al. (2005) and Hendershott and Jones (2005), both of which show that greater quote transparency can improve the informational efficiency. They also complement Bartlett et al. (2025) by demonstrating that incorporating semi-hidden quotes into the protected quotation set and disseminating them to the broad market has measurable effects on price informativeness. Moreover, the results support theoretical and experimental work arguing that greater transparency can strengthen the public signal embedded in prices and facilitate more efficient price discovery (Biais, 1993; Pagano and Röell, 1996; Bloomfield and O'Hara, 1999). Furthermore, to the extent that more informative prices improve the allocation of financial capital, these effects may matter beyond secondary-market trading. Consistent with the broader literature linking price informativeness to real decision-making, the improved price signal could arguably support capital allocation by investors, managers, and other economic agents (Leland, 1992; Dow and Gorton, 1997; Goldstein and Guembel, 2008; Bond et al., 2012).

[5]The denominator is the treated sample mean in the pre-event period.

D Robustness

Following our main results, we now provide robustness tests to strengthen our findings. In particular, we show that our results are not sensitive to a different identification strategy and specification choices in the estimation of the SynthDiD.

First, we complement our main identification strategy by exploiting the discontinuity in treatment assignment at the $250 price threshold. We use a regression discontinuity design (RDD) to compare stocks with an average closing price above $250 during the evaluation period to stocks just below the cutoff which remain at the 100-share round-lot size. It allows us to compare firms that are likely to be similar in underlying characteristics but differ in treatment status because of smaller differences in their pre-event price level. By focusing on a narrow window around the cutoff, the design isolates the local effect of the rule change and provides a useful corroborating test of the main results.

[**Figure 3** here]

Figure 3 presents the regression discontinuity results around the treatment threshold of $250. Specifically, we compute stock-level pre- versus post-event changes in *Pricing Error Ratio*, *Noise Share*, and *Price Delay*, and estimate a local-linear RD using a triangular kernel and heteroskedasticity-robust inference within a $50 bandwidth around the cutoff. This local comparison consists of 48 treated stocks and 34 control stocks. The annotated coefficient in each panel is the bias-corrected RD estimate and measures the discontinuous jump at the threshold. As such, it is the difference in the change of the outcome for stocks just above the cutoff relative to otherwise similar stocks just below it.

The figure shows negative discontinuities in *Pricing Error Ratio* and *Noise Share*. It indicates that the reform improved price informativeness where the treatment binds most directly. The coefficients for *Pricing Error Ratio* and *Noise Share* are highly significant and negative, at -0.15 and -0.41, respectively. By contrast, the estimate for *Price Delay* is

small and statistically indistinguishable from zero. This weaker result mirrors the earlier DiD evidence, where *Price Delay* also proved less robust than the other informativeness measures. It suggests that the effect on the speed of information incorporation is smaller or more heterogeneous across treated stocks than the effect of the other measures. Consistent with this interpretation, widening the bandwidth alters the result for *Price Delay* as it turns negative and significant, whereas the inference for *Pricing Error Ratio* and *Noise Share* remains unchanged.

Overall, these findings strongly corroborate an improvement in price informativeness near the regulatory threshold, while the evidence for faster incorporation of information seems to be more specification-sensitive.

Following the RDD results, we now examine the treatment intensity to assess whether the effects are concentrated among stocks that are more exposed to the treatment. We define treatment intensity as the ratio of the relevant price threshold to the stock's average closing price in September 2025. It captures how close a stock is to the threshold that determines its round-lot category and the extent to which the reform lowers the capital required to trade at the NBBO. Although the amended rule assigns a fixed round-lot size within each price bucket, the notional value of that round-lot still rises substantially. As a result, the reform should be more consequential for stocks closer to the lower bound of a bucket, where the newly protected lot size is economically easier to reach and a larger share of previously odd-lot quoting becomes NBBO-eligible.

[**Figure 4** here]

Figure 4 presents the results across treatment-intensity quintiles. We estimate Synth-DiD separately within each treatment-intensity quintile using the full set of control stocks as the donor pool. The figure shows that the improvement in price informativeness is generally stronger in higher treatment-intensity quintiles, consistent with the idea that the reform has larger effects when the reduction in the capital required to quote or trade

at the NBBO is largest. At the same time, the standard errors widen across quintiles and this pattern could be suggestive of greater heterogeneity in treatment effects among stocks.

Following the RDD and cross-sectional results, we now examine the effect of improved price informativeness on adverse selection. After the event, liquidity providers may be exposed to greater adverse selection when transacting with informed order flow since the public signal has become stronger. At the same time, the transparency literature offers mixed predictions.

While greater quote visibility may signal trading interest and weaken liquidity provision (Harris, 1996; Boulatov and George, 2013), it may also increase the commonality of information and thus reduce adverse selection (Glosten, 1999; Baruch, 2005). Boulatov and George (2013) conclude that informed traders opt for market orders and increase the price impact of trades whereas Baruch (2005) shows liquidity improves by lower price impacts as these market orders become smaller.

[**Table 5** here]

Table 5 presents the results for *Price Impact* and *VPIN*. The estimates show that market quality generally improves. The *Price Impact* decreases by 2.5% ($\frac{-0.2203}{8.74}$) and the toxicity of the order flow (*VPIN*) decreases by 15% ($\frac{-0.0464}{0.31}$). These are strong reductions that suggest that liquidity providers are less exposed to adverse selection from informed traders and simultaneously face lower order flow toxicity. In contrast to Boulatov and George (2013), the price impact has not increased. Therefore, our findings are in line with the prediction of Glosten (1999) that an improvement in information reduces adverse selection. It underscores that the increase in price informativeness is not at the expense of liquidity providers. Lastly, Table 5 also provides results on market quality. We show that the *Quoted Spread* and the *Effective Spread* decline by 9.5% ($\frac{6.17}{31.66}$) and 14.7% ($\frac{2.15}{14.65}$),

respectively. These improvements in market quality are similar in magnitude to the effects documented by Mantel (2025) on the same event.

Following the results on adverse selection, we now provide evidence that our findings are not sensitive to specification choices in the SynthDiD estimation. We show that different choices of standard errors, different donor pools, and different event windows do not alter our findings.

[**Table 3** here]

First, Table 3 provides evidence on different standard errors. We report bootstrap, jackknife, and placebo standard errors (Arkhangelsky et al., 2021). Jackknife standard errors are estimated using a leave-one-unit-out procedure: after obtaining the baseline SynthDiD estimate, we re-estimate the treatment effect N times, each time omitting one unit. Bootstrap standard errors, on the other hand, are estimated by repeatedly resampling units with replacement, re-estimating the SynthDiD treatment effect in each bootstrap sample. The placebo standard errors are estimated by repeatedly assigning placebo treatment to units in the donor pool, re-estimating the SynthDiD treatment effect under these placebo assignments. We estimate the respective standard errors by computing the variance of the resulting estimates, respectively. Finally, Table 3 shows that our results are not sensitive to the choice of standard errors.

[**Figure 5** here]

Next, Figure 5 illustrates donor-pool robustness to assess whether the SynthDiD estimates are sensitive to the composition of the control group. In particular, we address potential concerns that the estimator may place disproportionate weight on a narrow subset of donor units. This could lead to idiosyncratic features of controls driving the results rather than a stable treatment effect. The figure presents the treatment effect with its confidence intervals across donor pools for our price informativeness measures. We

document that the estimates are stable across the choice of donor pool and the estimates do not change in sign or significance. This indicates that our results are not driven by a specific subset of control stocks and that the treatment effect is stable and significant across donor pools.

[**Table 4** here]

Finally, we test alternative event windows to assess whether the SynthDiD estimates are sensitive to the choice of pre- and post-treatment periods. This is important because SynthDiD assigns unit and time weights based on the estimation sample, so changing the event window also changes the weighting structure underlying the estimator. Varying the event window therefore provides a check on whether the baseline results are driven by a particular sample period rather than by a stable treatment effect. Table 4 shows that the inferences remain unchanged when we use one-day and five-day event windows. This pattern indicates that the estimated treatment effects are robust to changes in the weighting structure induced by alternative event-window choices.

Overall, the cross-sectional and robustness results strengthen the interpretation of the baseline findings. The effects are larger for stocks more strongly exposed to the rule change and they remain stable across alternative standard-error procedures, donor pools, and event windows.

IV Changes in the Information Environment

This section examines how the Rule 600(b) amendment altered the information environment. Using an information decomposition, we show that the post-event improvement in price informativeness reflects a shift from the noise component toward firm-specific information, especially firm-specific public information. Importantly, we document that

a substantial improvement in the public signal is not associated with a decline in private-information acquisition. We then turn to analyst-based measures and find lower forecast dispersion and smaller forecast errors after the event, providing complementary evidence that the reform improved the public signal embedded in prices.

A Information Decomposition

Documenting improved price informativeness, we now turn to examining the main source of this improvement. We follow Brogaard et al. (2021) and decompose stock return variance into market-wide information (*Market InfoShare*), firm-specific public information (*Public InfoShare*), and firm-specific private information (*Private InfoShare*). This allows us to assess whether the reform changes the composition of information incorporated into prices.

[**Table 6** here]

Table 6 shows a substantial reallocation of return variance towards firm-specific information. The coefficients on *Public InfoShare* and *Private InfoShare* are both positive and statistically significant, implying that treated stocks incorporate more firm-level information. In particular, the contribution of firm-specific public information increases by 37.1% ($\frac{0.1153}{0.31}$), while the contribution of firm-specific private information also increases by 11.5% ($\frac{0.0088}{0.08}$). The increase in *Public InfoShare* indicates that most of the reduction in *Noise Share* is attributed to firm-specific public information in prices.

These results are consistent with the intention of the reform. By changing the composition of the NBBO, the reform broadens the audience and improves the public signal embedded in prices. This interpretation aligns with the broader transparency literature, which argues that greater dissemination of trading-relevant information can improve the efficiency of price formation (Biais, 1993; Pagano and Röell, 1996; Boehmer et al., 2005).

Importantly, the large increase in *Public InfoShare* is not accompanied by a crowding-out of private information production. The results show that *Private InfoShare* increases despite the improvement in the public information signal. As such, the evidence is more consistent with complementarities in information acquisition (Veldkamp, 2006; Goldstein and Yang, 2015).

In conclusion, the decomposition indicates that the reform improved price informativeness by reallocating noise to firm-specific information. The largest increase in the public-information component is consistent with a stronger public signal.

B Analyst Forecasts

The previous section establishes that the public information component substantially increased as the main source of improvement of price informativeness. We now turn to analysts' forecast divergence and accuracy to examine whether the improved public signal affected analysts' ability to extract information.

First, we access information on the price level per analyst from LSEG Workspace before the event (Q3 2025) and after the event (Q4 2025). Then we calculate our measures of divergence as (1) relative dispersion of the price target, (2) the median absolute deviation of the normalized price target, and (3) the standard deviation of the normalized price target across analysts. The normalized price target is the price target divided by the closing price on the day of the broker forecast release. For the divergence measure, we exclude any stock that only has a single analyst. Finally, we extract absolute relative earnings surprises for consensus analysts' accuracy.

[**Table 7** here]

Table 7 presents the results on analyst divergence for 12 month-ahead target price forecasts (1-6) and absolute relative earnings surprises (7-8). Across all three measures

of analyst divergence, the interaction term *Treat* × *Post* is negative and significant at 5% for the relative dispersion and at 10% for the other two measures. The results hold when controlling for unobserved stock characteristics by stock fixed effects. The estimates suggest that analyst forecasts seem to converge after the reform. It supports the view that the reform is associated with an improvement in the public signal.[6]

Moreover, we document a negative and significant interaction term *Treat* × *Post* for the absolute relative earnings surprises. The coefficient shows that the event is associated with an improvement in consensus earnings forecast accuracy. It suggests that analysts can use the improved public signal to increase the accuracy of their forecast. After the event, the difference between the earnings forecast and the realization significantly decreases for high-priced stocks.

Overall, these results provide complementary evidence that the reform improved the public information environment. While the restricted broker-forecast sample warrants caution, the evidence is nevertheless consistent with the main results that the reform strengthened the public signal incorporated into prices.

V Conclusion

In this paper, we analyze how market design affects price informativeness. In particular, the SEC's Rule 600(b) amendment changed publicly available information for high-priced stocks by reducing their round-lot sizes. A lower round-lot threshold reduces odd-lot quotes and translates into more granular consolidation of the National Best Bid and Offer. These quotes are disseminated to a wider market audience. More market participants can trade their interest in smaller units protected from trade-through and use the new composition of the NBBO to improve their expectation about the fundamental value of

[6]We caution against overinterpretation of the results since the broker-forecast sample is highly restrictive, covering only 34 treated and 90 control stocks and only one quarter before and after the event.

the stocks.

Using synthetic difference-in-differences regressions, we show that price informativeness increases substantially for the treatment group. The pricing error ratio and the noise share decrease by 14.7% and 21.9% for treated stocks. Further, price delay decreases by 3.2%, indicating less reliance on lagged prices. To understand the source of the effects, we decompose the contribution of information into market-wide and firm-specific information. Consistent with more publicly available information, we find that the public-information component increases by 37.1%. Importantly, the substantial increase is not accompanied by a crowding-out of private information production. The private information component also increases by 11.5%, highlighting evidence more consistent with complementarities in information acquisition.

By documenting improved price informativeness, we show that market frictions affect price discovery. Because the information embedded in prices is central to capital allocation in the real economy, the amendment to Rule 600(b) may have effects that extend beyond secondary markets. More broadly, our findings suggest that market frictions matter not only for trading outcomes, but also for the informational efficiency of financial markets and, potentially, for real economic decisions.

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Table 1
Descriptive Statistics

This table presents the descriptive statistics of our sample. Our sample consists of 164 treated stocks and 742 control stocks. *Pre* indicates the sample mean in the ten trading days prior to the event; *Post* indicates the sample mean in the ten trading days after the event; *VPIN* is the volume-synchronized probability of informed trading (Easley et al., 2012); *Pricing Error Ratio* is the standard deviation of the pricing error to the standard deviation of the log price (Hasbrouck, 1993); *Price Delay* is delay in information incorporation (Hou and Moskowitz, 2005); *Noise Share*, *Market InfoShare*, *Private InfoShare*, and *Public InfoShare* are the relative contribution of market-wide, private firm-specific, public firm-specific information and noise to the stock variance (Brogaard et al., 2021); *Turnover* is the natural log of the daily turnover; *Intraday Volatility* is the variance of realized one-second midpoint returns based on quotes. The averages for the Synthetic Control are the respective sample characteristics adjusted for unit and time weights based on the SynthDiD estimations. We apply Student's t-test to test the difference in the means. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Treatment			Synthetic Control		
	Pre	Post	Difference	Pre	Post	Difference
Price Informativeness						
Pricing Error Ratio	0.0503	0.0409	-0.0094***	0.0501	0.0491	-0.0010*
Price Delay	0.8341	0.8076	-0.0264**	0.8215	0.8253	0.0039
Noise Share	0.5561	0.4188	-0.1372***	0.3796	0.3661	-0.0135**
Market InfoShare	0.0402	0.0422	0.0020	0.0401	0.0400	-0.0001
Public InfoShare	0.3283	0.4490	0.1206***	0.4956	0.5051	0.0095
Private InfoShare	0.0764	0.0899	0.0135***	0.0802	0.0847	0.0045**
Market Quality						
Quoted Spread (bps)	31.6637	23.6795	-7.9842***	28.3450	29.8723	1.5273
Effective Spread (bps)	14.6560	10.4984	-4.1576**	11.3370	11.6632	0.3262
Price Impact (bps)	7.9019	6.8539	-1.0480**	6.4136	6.9368	0.5233
VPIN	0.2987	0.2482	-0.0505***	0.2846	0.2788	-0.0058**
Controls						
Turnover ($'000,000)	19.2212	19.2040	-0.0172***	17.6022	17.6638	0.0616
Intraday Volatility	0.0051	0.0029	-0.0023***	0.0029	0.0033	0.0004

Table 2
Effect of Rule 600(b) amendment on Price Informativeness

This table presents the results on price informativeness. Our event window is symmetrically ten trading days around the event. Our main sample consists of 164 treated and 742 control stocks. *Treat* is a dummy that is one if a stock had an average closing price above $250 in September 2025. *Post* is a dummy that is one if the trading day is at or past the November 3, 2025. *Pricing Error Ratio* is the standard deviation of the pricing error to the standard deviation of the log price (Hasbrouck, 1993); *Noise Share* is the contribution of noise to the innovation in prices (Brogaard et al., 2021); *Price Delay* is delay in information incorporation (Hou and Moskowitz, 2005). We estimate these measures in ten-minute intervals with five lags and aggregate them to daily averages. We control for the *Turnover* and Intraday *Volatility* and winsorize at the 1% and 99% within treatment and pre-post period. Panel A illustrates the DiD of a matched sample where 114 control stocks are matched to 114 treated stocks based on market capitalization and the relative quoted spread two months before the event. Panel B presents the SynthDiD results following Arkhangelsky et al. (2021). *N0* presents the average control stocks used; *N1* the average treatment stocks; *T0* the average pre-event period; *T1* the average post-event period. The standard errors of DiD are clustered by stock and date; the standard errors of SynthDiD are bootstrapped with 500 replications. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Diff-in-Diff	Pricing Error Ratio		Noise Share		Price Delay	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post	-0.0104***	-0.0095***	-0.1227***	-0.1247***	-0.0068	-0.0056
	(0.0004)	(0.0014)	(0.0045)	(0.0072)	(0.0075)	(0.0094)
Turnover		-0.0111***		-0.0630***		0.0191***
		(0.0010)		(0.0041)		(0.0026)
Volatility		1.4489***		0.4623		-0.2396
		(0.3699)		(0.6222)		(0.2111)
Observations	4,335	4,334	4,321	4,320	4,520	4,520
Adj. R^2	0.041	0.762	0.329	0.858	0.059	0.846
FE	No	Yes	No	Yes	No	Yes

Panel B: Synth Diff-in-Diff	Pricing Error Ratio		Noise Share		Price Delay	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post	-0.0071***	-0.008***	-0.121***	-0.1262***	-0.0306***	-0.0274***
	(0.0010)	(0.0009)	(0.0061)	(0.0058)	(0.0039)	(0.0039)
Controls	No	Yes	No	Yes	No	Yes
N0 (N1)	522.5 (125)	521.6 (125)	595.9 (153)	601.2 (153)	677.6 (164)	684.4 (164)
T0 (T1)	5.7 (10)	5.9 (10)	8.7 (10)	8.8 (10)	6.2 (10)	6.3 (10)

Table 3
Changes in Standard Error Estimation

This table focuses on different estimation approaches for the standard errors to check for robust inference. τ is the treatment effect; *Panel A* presents the standard errors estimated by jackknife; *Panel B* presents standard errors estimated by placebo; *Panel C* presents the standard errors estimated by bootstrapping. The jackknife standard errors use a leave-one-unit-out procedure; the bootstrap standard errors repeatedly resample units with replacement; the placebo standard errors repeatedly assign placebo treatment to units in the donor pool. We control for unobserved characteristics with stock and day fixed effects, and we control for the *Turnover* and *Volatility*. Each regression is run with controls. The variables are winsorized at the 1% and 99% for each stock and pre-post period. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Jackknife			
	Pricing Error Ratio	Noise Share	Price Delay
Treat x Post	-0.008***	-0.1262***	-0.0274***
	(0.001)	(0.006)	(0.004)
Controls	Yes	Yes	Yes
N0 (N1)	521.6 (125)	601.2 (153)	684.4 (164)
T0 (T1)	5.9 (10)	8.8 (10)	6.3 (10)
Panel B: Placebo			
	Pricing Error Ratio	Noise Share	Price Delay
Treat x Post	-0.008***	-0.1262***	-0.0274***
	(0.001)	(0.004)	(0.002)
Controls	Yes	Yes	Yes
N0 (N1)	521.6 (125)	601.2 (153)	684.4 (164)
T0 (T1)	5.9 (10)	8.8 (10)	6.3 (10)
Panel C: Bootstrap			
	Pricing Error Ratio	Noise Share	Price Delay
Treat x Post	-0.008***	-0.1262***	-0.0274***
	(0.001)	(0.006)	(0.004)
Controls	Yes	Yes	Yes
N0 (N1)	521.6 (125)	601.2 (153)	684.4 (164)
T0 (T1)	5.9 (10)	8.8 (10)	6.3 (10)

Table 4
Changes in Event Window

This table focuses on restrictions to the event window to check for robust inference. Our original event window is ten trading days before and after the November 3, 2025. *Panel A* restricts the event window to one day around the event, October 30, 2025 and November 3, 2025; *Panel B* restricts the event window to one trading week around the event, October 27, 2025 and November 7, 2025; *Panel C* is our original event window and represents the results of Table 2. We control for unobserved characteristics with stock and day fixed effects, and we control for the *Turnover* which is the natural log of the daily turnover and the Intraday *Volatility* which is the variance of realized one-second midpoint returns based on quotes. Each regression is run with the controls. The variables are winsorized at the 1% and 99% for each stock and pre-post period. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: 1 day			
	Pricing Error Ratio	Noise Share	Price Delay
Treat x Post	-0.0093***	-0.1165***	-0.0187***
	(0.002)	(0.008)	(0.005)
Panel B: 5 days			
	Pricing Error Ratio	Noise Share	Price Delay
Treat x Post	-0.0094***	-0.1215***	-0.0259***
	(0.001)	(0.007)	(0.004)
Panel C: 10 days			
	Pricing Error Ratio	Noise Share	Price Delay
Treat x Post	-0.008***	-0.1264***	-0.0274***
	(0.001)	(0.006)	(0.004)
Controls	Yes	Yes	Yes
N0 (N1)	521.6 (125)	601.2 (153)	684.4 (164)
T0 (T1)	5.9 (10)	8.8 (10)	6.3 (10)

Table 5
Effect of Rule 600(b) amendment on Adverse Selection

This table presents the SynthDiD results on adverse selection following Arkhangelsky et al. (2021). Our sample consists of 164 treated stocks and 742 control stocks. Our event window is ten trading days before and after the event. *Treat* is a dummy that is one if a stock had an average closing price above $250 in September 2025. *Post* is a dummy that is one if the trading day is at or past the November 3, 2025. *Price Impact* is the adverse selection component of the effective spread following 300 seconds after the time of trade. *Quoted Spread* is the time-weighted proportional bid-ask spread, *Effective Spread* is the volume-weighted proportional effective spread; *Price Impact* is adverse selection component of the effective spread following 300 seconds after the time of trade. We estimate volume-synchronized probability of informed trading *VPIN* by Easley et al. (2012). We control for the *Turnover* as the natural log of the daily turnover and the Intraday *Volatility* as the variance of realized one-second midquote returns. The variables are winsorized at the 1% and 99% for each stock and pre-post period. *N0* presents the average control stocks used, *N1* the average treatment stocks, *T0* the average pre-event period and, *T1* the average post-event period. Standard errors are bootstrapped with 500 replications. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Price Impact		VPIN		QuotedSpread		EffectiveSpread	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat x Post	-1.64***	-0.2203***	-0.0435***	-0.0464***	-10.11***	-6.17***	-4.23***	-2.15***
	(0.3089)	(0.2465)	(0.0045)	(0.0045)	(0.8042)	(0.6667)	(0.2828)	(0.2201)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N0 (N1)	694.5 (164)	709.9 (164)	719.7 (164)	719.9 (164)	723.9 (164)	714.7 (164)	705.2 (164)	714.5 (164)
T0 (T1)	5.5 (10)	5.7 (10)	7.7 (10)	7.7 (10)	5.6 (10)	5 (10)	4.1 (10)	4.6 (10)

Table 6
Information Decomposition

This table presents the results of the information decomposition. Our event window is ten trading days before and after the event. Our sample consists of 164 treated stocks and 742 control stocks. *Treat* is a dummy that is one if a stock had an average closing price above $250 in September 2025. *Post* is a dummy that is one if the trading day is at or past the November 3, 2025. We follow Brogaard et al. (2021) to decompose the stock return variance into market-wide information (*Market InfoShare*), private firm-specific (*Private InfoShare*) information, public firm-specific information (*Public InfoShare*), and noise (*Noise Share*). We estimate these measures in ten-minute intervals with five lags and aggregate them to daily averages. We control for the *Turnover* as the natural log of the daily turnover and the Intraday *Volatility* as the variance of realized one-second midquote returns. The variables are winsorized at the 1% and 99% for each stock and pre-post period. *N0* presents the average control stocks used, *N1* the average treatment stocks, *T0* the average pre-event period and, *T1* the average post-event period. Standard errors are bootstrapped with 500 replications. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Market InfoShare		Public InfoShare		Private InfoShare	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post	0.0015	0.0005	0.1093***	0.1153***	0.0082***	0.0088***
	(0.0014)	(0.0013)	(0.0053)	(0.0050)	(0.0016)	(0.0016)
Controls	No	Yes	No	Yes	No	Yes
N0 (N1)	610.4 (153)	616.6 (153)	594.7 (153)	603.3 (153)	630.6 (153)	630.5 (153)
T0 (T1)	8 (10)	8.4 (10)	8.6 (10)	8.6 (10)	9.3 (10)	9.4 (10)

Table 7
The Effect of Rule 600(b) Amendment on Analyst Forecasts

This table presents the effect of Rule 600(b) Amendment on analyst forecasts. We access information on 12-month-ahead price levels forecast per analyst and earnings surprises from LSEG Workspace before the event (Q3 2025) and after the event (Q4 2025). First, we calculate the normalized price target as the price target divided by the closing price on the day of the broker forecast release. Then, we calculate the absolute earnings surprise as the relative difference between the forecast and the realization. Column (1-2) is the relative dispersion of the price target, column (3-4) the median absolute deviation of the normalized price target, column (5-6) the standard deviation of the normalized price target across analysts and column (7-8) the absolute earnings surprise. Column (1) to (6) quantify analyst forecast divergence whereas column (7-8) quantifies the accuracy of the forecasts. For columns (1) to (6), we filter stocks that do not have multiple analysts. We only include stocks with observations before and after the event. This yields a sample of 34 treated and 90 control stocks for column (1) to (6) and of 135 treated and 600 control stocks for column (8). We winsorize the absolute earnings surprise at the 2.5% and 97.5% within treatment and pre-post period to account for outliers. We further include stock fixed effects to control for unobserved stock characteristics that could contaminate these effects. The standard errors are clustered by stock. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Intercept)	0.0966***		0.0677***		0.1173***		0.2475***	
	(0.0109)		(0.0124)		(0.0148)		(0.0160)	
Treat	0.0001		0.0127		-0.0103		-0.1553***	
	(0.0173)		(0.0196)		(0.0196)		(0.0177)	
Post	0.0305*	0.0262	0.0565**	0.0608***	0.0564	0.0543	0.0843***	0.0843***
	(0.0158)	(0.0186)	(0.0217)	(0.0217)	(0.0347)	(0.0343)	(0.0269)	(0.0269)
Treat x Post	-0.0573**	-0.0624*	-0.0560*	-0.0695**	-0.0784*	-0.0869*	-0.0718**	-0.0718**
	(0.0266)	(0.0319)	(0.0317)	(0.0337)	(0.0422)	(0.0439)	(0.0287)	(0.0287)
Observations	224	224	239	239	202	202	1,462	1,462
Adj. R^2	0.016	0.301	0.017	0.407	0.016	0.326	0.023	0.389
Stock-FE	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*

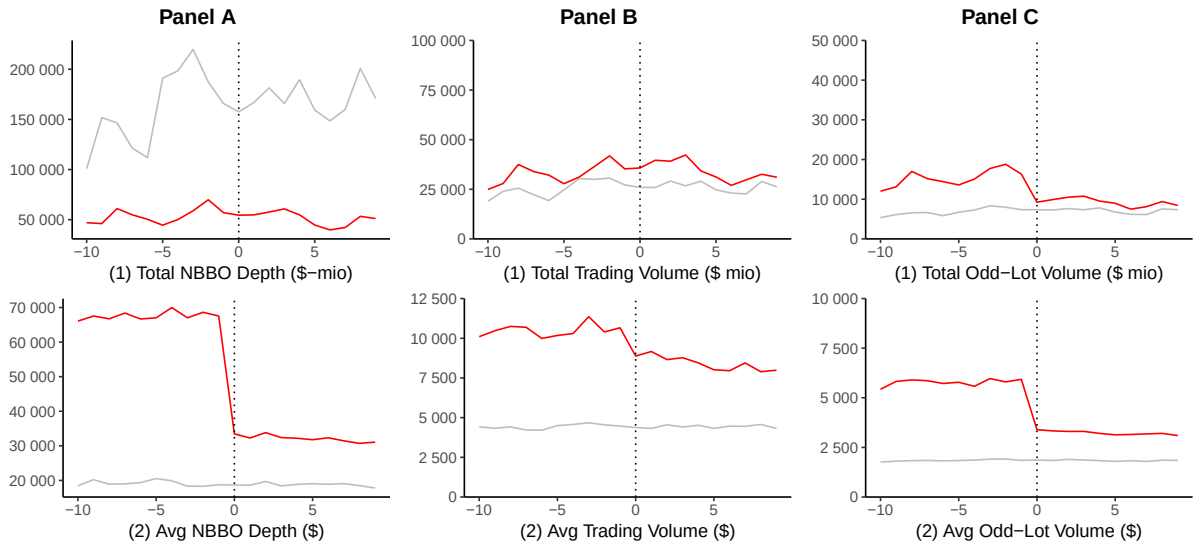


Figure 1. Changes in NBBO Depths and Trading Volumes

This figure shows changes in the market activity after the reform. Panel A shows the NBBO Depth as the total NBBO depth (1) and the average NBBO depth (2); *Total NBBO Depth* is the total sum of the NBBO Bid depth and Ask depth in $-billion in a given day whereas *Avg NBBO Depth* is the mean of NBBO Bid depth and Ask depth in $. Panel B shows the Trading Volume as the total trading volume (1) and the average trading volume (2). *Total Trading Volume* is the total sum of volume transacted in $-billion in a given day whereas *Avg Trading Volume* is the mean of volume transacted in $. Panel C shows the Odd-Lot Trading Volume as the total odd-lot trading volume (1) and the average odd-lot trading volume (2). We identify odd-lot trades conditional on the size being lower than the active round-lot threshold (100 shares before and the respective share tier after). *Total Odd-lot Trading Volume* is the total sum of odd-lot volume transacted in $-billion in a given day whereas *Avg Odd-Lot Trading Volume* is the mean of odd-lot volume transacted in $. These are cross-sectional averages of the control and treatment group in our event window of ten days before and after the compliance date of Rule 600(b) amendment. The control group has been matched to the treated group based on market capitalization and the average relative quoted spread two months prior to the event. The treatment group is indicated by the color red, the control group is indicated by the color gray. The vertical line represents the compliance date on November 3, 2025.

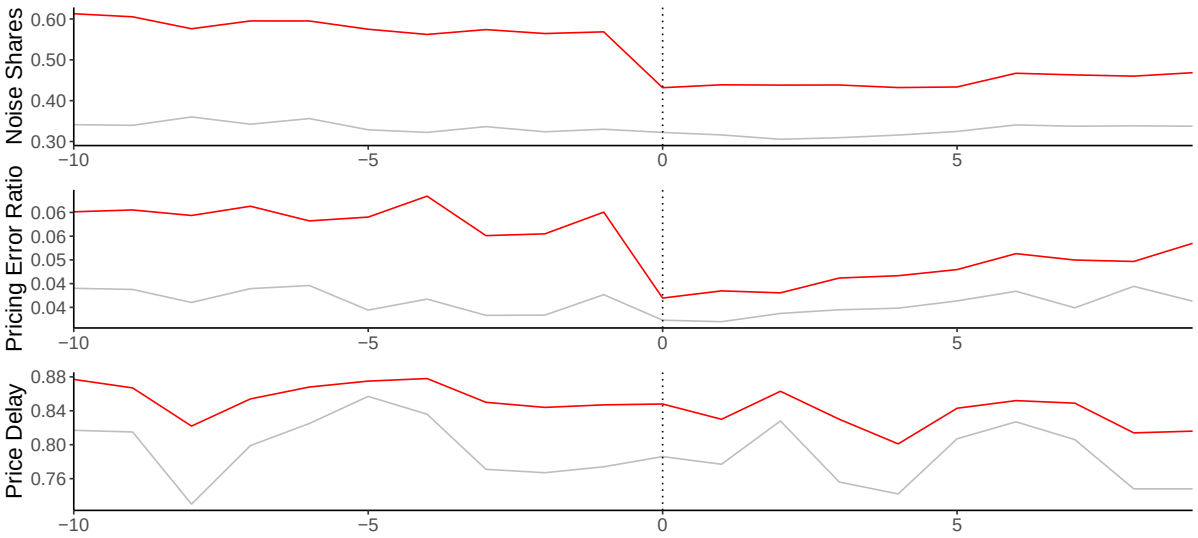


Figure 2. Parallel Trends

This figure shows the *Pricing Error Ratio*, *Price Delay*, and the *Noise Share* as defined in Table 2. These are cross-sectional averages of the control and treatment group in our event window of ten days before and after the compliance date of Rule 600(b) amendment. The control group has been matched to the treated group based on market capitalization and the average relative quoted spread two months prior to the event. The treatment group is indicated by the color red, the control group is indicated by the color gray. The vertical line represents the compliance date on November 3, 2025.

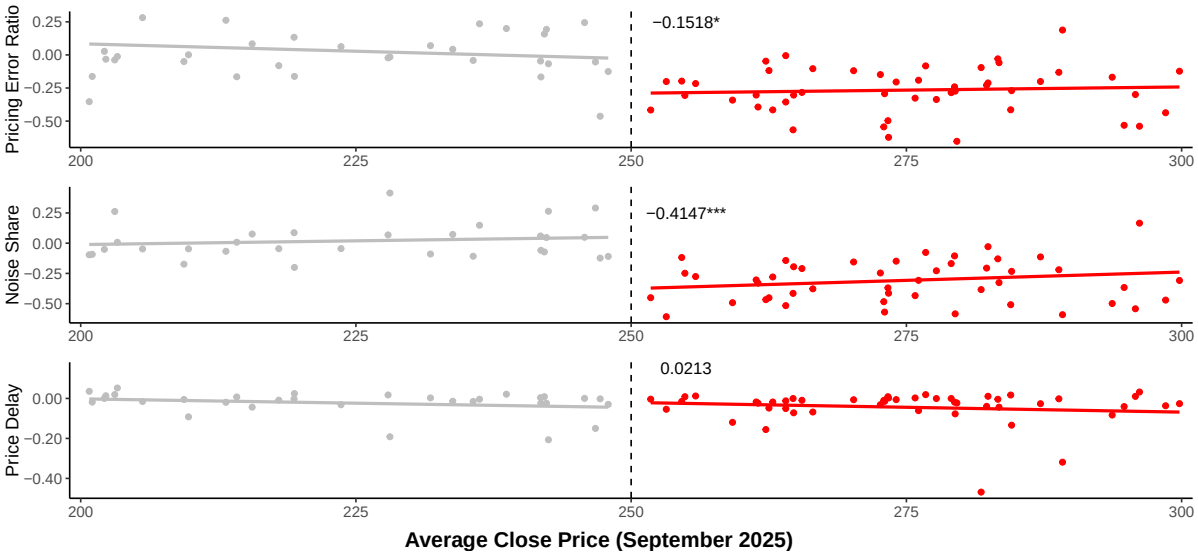


Figure 3. Regression Discontinuity Design

This figure presents the results on price informativeness using a Regression Discontinuity Design. *Pricing Error Ratio*, *Price Delay*, and *Noise Share* are defined as in Table 2. We estimate the regression discontinuity with a triangular kernel and use heteroskedasticity-robust standard errors. We use a bandwidth of $50 around the regulatory threshold of $250 to identify the treatment effect. The sample consists of 48 treated and 34 control stocks. The y-axis illustrates the change in pre-event and post-event levels and the x-axis is the average closing price in September 2025. The Rule 600(b) amendment determined the new round-lot size based on the average closing price during the evaluation period, here September. Further, we annotate the point estimate in the plot. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The treatment group is indicated by the color red, the control group is indicated by the color gray. The vertical line represents the regulatory cutoff of $250.

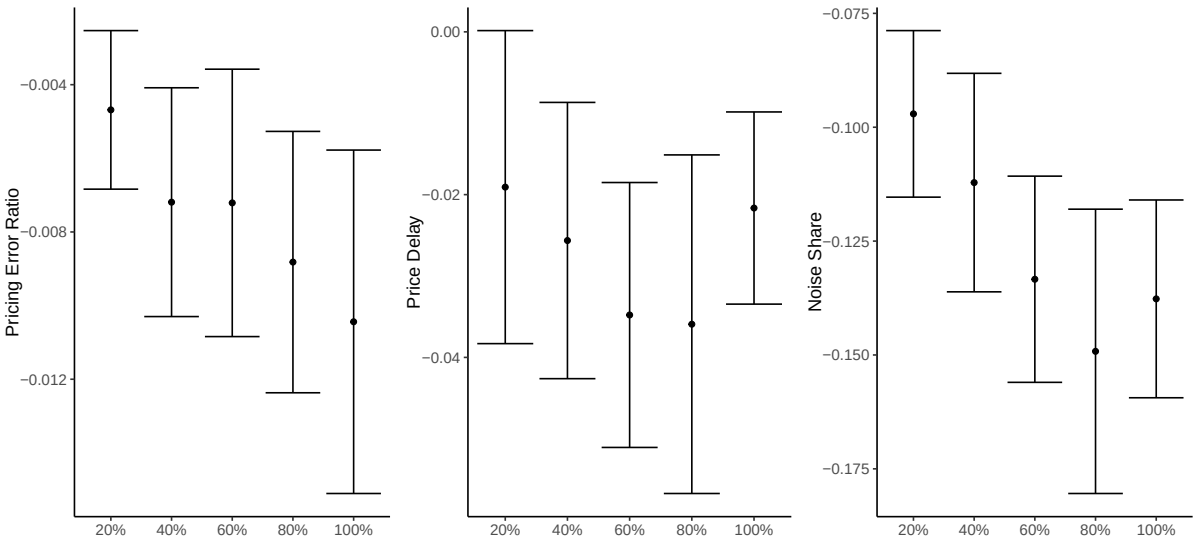


Figure 4. Treatment Intensity

This figure shows the *Pricing Error Ratio*, *Price Delay*, and the *Noise Share* sorted into treatment intensity quintiles. Our price informativeness measures are defined as in Table 2. Our event window is symmetrically ten trading days around the event. Our main sample consists of 164 treated stocks. Treatment intensity is defined as the ratio of the relevant price threshold to the stock's average closing price in September 2025. It therefore captures how close a stock is to the threshold that determines its round-lot category and, in turn, the extent to which the reform lowers the capital required to trade at the NBBO. The y-axis is the estimate from the SynthDiD based on the respective treatment intensity quintile. We apply confidence intervals to visually guide the reader on significance. We sort the treated stocks into quintiles and run the SynthDiD on a sample containing the treated stocks of the respective quintile and all potential control stocks. We estimate the standard error using the Jackknife method.

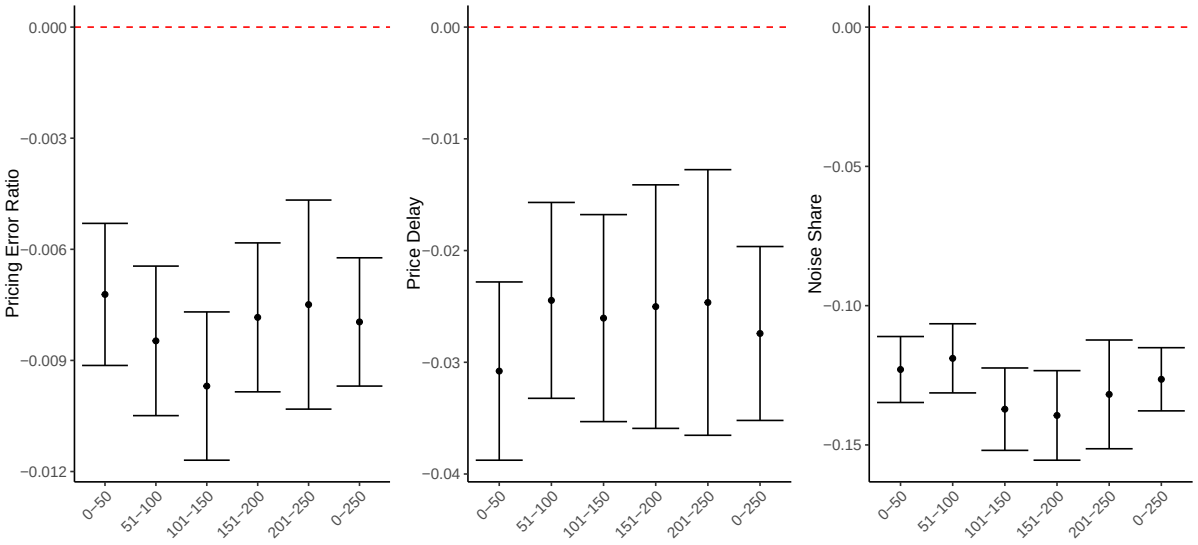


Figure 5. Donor-Pool Restrictions

This figure focuses on restrictions to the donor pools (control stocks) to check for robust inference. Our event window is ten trading days before and after the event. The y-axis is the estimate from the SynthDiD based on the respective set of potential donors. The red horizontal line marks zero to indicate whether donor pools are insignificant. *(0-50)* presents a restriction to the SynthDiD to only use control stocks with average closing price below $50; *(51-100)* presents a restriction to the SynthDiD to only use control stocks with average closing price above $50 and below $100; *(101-150)* presents a restriction to the SynthDiD to only use control stocks with average closing price above $100 and below $150; *(151-200)* presents a restriction to the SynthDiD to only use control stocks with average closing price above $151 and below $200; *(201-250)* presents a restriction to the SynthDiD to only use control stocks with average closing price above $201 and below $250. We estimate the standard error using the Jackknife method.