Empirical Causal Asset Pricing with Trading Costs

Luca Gaegauf Vincent Wolff*

Abstract

Do stock prices of publicly listed companies respond to changes in trading costs? We document a significant and asymmetric effect of tick-size changes on prices by leveraging a novel policy framework that allows for a causal randomized control trial difference-in-differences analysis. The doubling of the tick size leads to a decrease in prices by 0.9% to 1.3%, whereas halving the tick size results in an increase of 3.3% to 3.5%. The price effect is more pronounced in smaller firms and tick-unconstrained stocks. We report substantial excess returns on the day before the tick-size change attributable to quote and price clustering and, with caution, to strategic short-term price manipulation.

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1 Introduction

The effect trading costs have on financial markets remains an open question that has piqued the interest of both academics (Gârleanu and Pedersen, 2013; Vayanos, 1998) and policymakers (European Commission, 2013; Krugman, 2009; Stiglitz, 1989). Particularly, the impact of trading costs on prices has garnered much attention (Albuquerque et al., 2020; Bogousslavsky et al., 2021; Dávila and Parlatore, 2021; Hasbrouck, 2009). However, the causal effect of trading costs on prices is an open question in all sectors of financial markets. Theoretical analyses predict that trading costs can exert either a minimal second-order effect on prices or even cause prices to increase (Buss and Dumas, 2019; Vayanos, 1998). Meanwhile, empirical studies, often leveraging cross-sectional data, have revealed

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negative or inconclusive effects of trading costs on prices (Acharya and Pedersen, 2005; Amihud and Mendelson, 1986; Bessembinder, 2003).

This paper is the first to provide empirical evidence of the causal effect of changing trading costs on prices for the European stock universe. We leverage a novel policy that introduces exogenous variation in trading costs through randomly changing tick sizes, creating a quasi-randomized control trial (RCT) setting. We uncover an asymmetric effect between tick-size increases and decreases, finding an average tick-size increase from 0.09 to 0.21 Euros reduces stock prices between 0.96% and 1.3%. In the other direction, an average tick-size reduction from 0.16 to 0.07 Euros increases stock prices between 3.3% and 3.5%. New to the literature, the effect is economically stronger for small firms than large firms, and the effect is weaker for stocks with small pre-treatment quoted spreads than stocks with large pre-treatment quoted spreads. Finally, we report substantial excess returns in absolute values for treated stocks on specific days related to the timing of treatment. On the day a tick-size change is triggered,[1] the excess returns range from 0.2% to 3.2%. These returns can be attributed to a behavioral effect from quote and price clustering and, with caution, to a strategic effect from short-term stock price manipulation.

Of the various components that make up trading costs, tick size, or the minimum price increment for asset quotes and trades, directly influences bid-ask spreads.[2] The most straightforward way tick sizes affect spreads is mechanical: increases in tick size widen spreads if they were binding, whereas decreases may narrow them if the tick size was previously the constraint. As such, the numerous global interventions demonstrate the understanding that tick size is an important tool for market regulators and policymakers. For instance, during the rise of automation in financial markets, North American and European markets reduced tick sizes to curtail market-maker profits and enhance market quality (see, *e.g.*, Bacidore, 1996; Bessembinder, 2003). As market efficiency improved following each subsequent reduction in tick size, European market makers were given the power to set their own tick sizes. This ultimately diminished overall market quality as competition for trading volume intensified in the early 2000s (Foley et al., 2023). Lessons from this "tick-size war", combined with insights from alternative markets (Dyhrberg et al., 2020) and theoretical research (Graziani and Rindi, 2023), now suggest the existence of an optimal tick size. This optimal tick size balances key market trade-offs, such as moderating competition and undercutting behavior, fostering market efficiency and transparency, and

[1]A tick-size change is triggered if an asset's price closes above (or below) a predefined price threshold on the previous trading day.

[2]Beyond influencing an investor's spread costs, tick sizes also impact, for example, execution (Bessembinder, 1997), negotiation (Van Ness et al., 2000), and speculation costs (Hau, 2006).

increasing liquidity. Consequently, efforts to establish optimal tick sizes through structured regimes (*e.g.*, MiFID II in the EU or RegNMS in the US) have since been implemented. Yet, despite this significance and the complexity of tick-size changes, a comprehensive causal analysis of how tick sizes impact markets, particularly prices, is still lacking.[3]

To conduct our analysis, we leverage the European tick size harmonization policy under the Market in Financial Instruments Directive II (MiFID II). MiFID II is a legislative framework established by the European Securities and Markets Authority (ESMA) to regulate markets, improve liquidity and market quality, and foster competition. While the legislation covers a comprehensive set of regulatory rules for financial markets, one component that came into effect in January 2018 was the standardization of tick sizes across all European markets. Under this regime, an asset's tick size is updated daily to reflect its price level and annually to reflect its liquidity. We argue that, as prices follow a random walk and as the price boundaries that determine an asset's tick size are economically arbitrary, any tick-size change triggered by the price crossing a threshold constitutes a quasi-random event.

This setting provides several distinct advantages over existing research. Most notably, policy interventions are often endogenous, and results are distorted by other simultaneous legislative changes (Angel, 2012). This is particularly true for one-time policy changes such as the move from fractional to decimal pricing and the US tick size pilot program. In the case of the pilot, which had the clearly specified and publicized intention of improving the treated markets (Wall Street Journal, 2016), being untreated was akin to receiving a negative treatment. Any subsequent comparison of the treated and untreated groups yields a biased measurement. Both Chung et al. (2020) and Rindi and Werner (2019) measure the effects of being untreated in this context, observing a liquidity migration from control to treated stocks. The MiFID II regulation, in contrast, allowing for random tick-size changes unpredictable in their timing, facilitates the clear identification of the average treatment effect of trading costs on prices. Furthermore, although MiFID II implemented a number of policy changes upon introduction, the existence of the tick-size regime, rather than its initial implementation, provides the environment for tick sizes to change at any time in subsequent years. This is reflected in the 5,765 tick-size increases and 7,161 decreases we observe over time, averaging 4.6 and 5.7 daily changes, respectively. The second most notable feature of the MiFID II intervention is that tick harmonization applies to all publicly traded stocks in the 27 EU member states.[4] In contrast to the US pilot program, which was limited to a

[3]The study by Albuquerque et al. (2020) is, to the best of our knowledge, the only study to date that provides a causal analysis of an event study on the effects of tick sizes on prices.

[4]The tick-size regime is also adopted by some third-country venues like Switzerland, which are not

subset of illiquid and small market cap stocks, MiFID II provides a framework that allows the study of tick-size effects across the whole European stock universe. Lastly, in contrast to the majority of research that studies one-size-fits-all tick sizes (see Verousis et al., 2018) or singular tick-size changes, MiFID II offers an environment where tick sizes are frequently updated to reflect the optimal tick size in relation to price and liquidity levels (see Graziani and Rindi, 2023), allowing for an analysis of both increases and decreases in tick sizes.

Alongside our key findings on the effect of tick sizes, our research also introduces a novel identification strategy as a significant methodological contribution. Leveraging the randomness of tick-size changes, we use stocks that do not undergo a tick-size change on a given day, but do cross a non-threshold value, as a control for those that do experience a change on that day, ensuring that both sets of stocks are subject to the same macroeconomic conditions. By collapsing these events to a common point in the time dimension, we effectively average out the market conditions experienced by all stocks during that period and the stock-specific characteristics as each stock can occur in either group. To ensure that prior tick-size adjustments do not confound the treatment effect, we impose a pre-event period free of tick-size changes in our instance selection. This methodology yields robust results, as confirmed through extensive robustness tests. Specifically, we demonstrate that our findings are not driven by major market events like the COVID-19 market crash or the Ukraine war, nor by outliers in other observable variables. Our results are not dependent on our methodological choices, such as our stratification criteria for defining the firm size and liquidity subgroups or the length of the pre-event window. Furthermore, the results survive in the investigation of the excess returns, indicating the overall robustness of our findings and methodology.

The remainder of this paper is structured as follows: Section 2 offers an overview of the relevant literature and outlines our hypothesis. Section 3 delves into the MiFID II framework, details our data collection and processing methodology, and discusses the identification strategy. In Section 4, we present the main findings of our study, including an investigation of the excess returns that occur on the day a MiFID II price threshold is crossed, and discuss the results of a series of robustness tests included in Appendix C. Section 5 concludes. Additionally, Appendix B contains a discussion on endogeneity pertinent to our study.

considered in our study as they are quoted in their respective currencies.

2 Literature review

Tick sizes, the minimum price movement in a financial market, are fundamental to its functioning, influencing liquidity, volatility, and the dynamics of market making and high-frequency trading. These minimal increments affect the price discovery process and the microstructure of markets, shaping market efficiency and stability (see Verousis et al., 2018). Historically, global tick-size regulation has undergone many changes as market dynamics and understanding have evolved and technology has advanced. Most prominently, the move from fractional to decimal pricing in North America saw tick-size quotes reduced from increments of $1/16^{\text{th}}$ USD (6.25 cents) to as small as 1 cent to curtail market-maker profits as market automation progressed. This shift enhanced market quality by improving liquidity (Furfine, 2003) and informational efficiency (Chen et al., 2015), and reducing investor's trading costs (Bacidore, 1996). However, such regulatory changes also enabled undercutting by market makers to gain market share, resulting in the so-called "tick-size wars" in Europe (Foley et al., 2023). Recognizing the nuanced impact of tick sizes, the SEC initiated a pilot program to explore the effects of increased minimum tick sizes on liquidity provision for illiquid assets.[5] This program reflects the understanding that an optimal tick size exists, which balances the interests of market makers and other market participants. Similarly, the EU's MiFID II regime offers a more fine-tuned approach to balance these interests, tailoring tick sizes to an asset's price and liquidity.[6]

Given these global developments and their varied impacts on markets, a significant body of theoretical and empirical research has emerged to examine how tick-size regulation impacts important aspects of market quality, most notably liquidity. Research consistently shows that decreasing tick sizes tends to narrow bid-ask spreads, implying increased liquidity (see Verousis et al., 2018). This effect is attributed to the mechanical effect of tick-size-induced spread constraints being reduced, which is particularly strong for stocks with large relative tick sizes (Harris, 1991, 1994) or liquid stocks (Kurov and Zabotina, 2005). However, a reduction in tick sizes also decreases market depth, indicating a trade-off in different liquidity dimensions (Bollen and Whaley, 1998; Hsieh et al., 2008) with impacts realizing asymmetrically; for example, heavily traded stocks benefit from narrower bid-ask spreads (Anderson and Peng, 2014; Hameed and Terry, 1998), while larger trades suffer due to the reduced depth (Cai et al., 2008; Chung et al., 2005).

[5]The pilot program was a regulatory initiative aimed at evaluating the impact of increasing tick sizes from 0.01 to 0.05 USD for randomly selected small-cap companies.

[6]Price, to control the relative tick size, and liquidity, to incentivize liquidity provision where necessary and allow competition where liquidity is sufficient. See Section 3.1 for a more thorough exposition of the MiFID II tick-size regime.

Beyond these immediate effects, the influence of market participants' behavior and incentives, and consequently, the composition of participants, adds another dimension to the liquidity implications of tick-size changes. Theoretical models (*e.g.*, Bernhardt and Hughson, 1996) and empirical findings (*e.g.*, Bacidore, 2001; Bollen et al., 2003; Chung and Van Ness, 2001; Stone, 2009) demonstrate that market makers are incentivized by larger tick sizes to provide liquidity, which increases their profits. Similarly, large tick sizes incentivize non-market-maker investors to contribute to liquidity by setting limit orders (Angel, 1997). The presence of high-frequency traders, who are attracted by smaller tick sizes (Chung et al., 2008; Mahmoodzadeh and Gençay, 2017), increases execution speeds, a metric indicating higher liquidity, yet has a mixed impact on overall market quality. Similarly, Amihud and Mendelson's (1986) findings suggest that large tick sizes would attract investors with longer-term horizons, who can endure higher trading costs, negatively affecting liquidity.

In addition to extensive research on the liquidity effects of tick sizes, studies have explored other market quality indicators, such as the price discovery process. Smaller tick sizes are associated with improved pricing efficiency, as found by Beaulieu et al. (2003) and Chen and Gau (2009), allowing for more precise pricing and finer order placement in response to new information. Furthermore, Easley and O'Hara (2004) highlight that heightened information risk, often resulting from a perceived disadvantage against informed traders, can lead agents who provide immediacy to widen the spread, potentially negating the benefits of reduced tick sizes. Similarly, Wolff (2023) illustrates that increased trading costs can decrease the adverse selection component of the spread (*i.e.,* enhancing liquidity) as informed traders move to cheaper products or venues.

Despite the range of natural settings available for study and the interest in tick sizes on market quality, there remains a surprising dearth of empirical research focused on the effect of tick sizes on asset prices. This is despite evidence that we should expect an effect (Ball and Chordia, 2001) and the breadth of theoretical work that studies various mechanisms. Standard economic theory suggests that additional costs on investors are compensated by reduced asset prices. That is, asset prices should fall by an amount equivalent to the present value of the costs, as experienced by the marginal investor and subsequent investors. However, more comprehensive models give rise to alternative mechanisms that can diminish, inflate, or even reverse this effect. Constantinides (1986) and Vayanos (1998) reveal that agents can diminish the impact of transaction costs on stock prices by reducing their trading frequency. Constantinides (1986) hence only measures a small second-order negative effect on prices, while the direction and size of the effect measured by Vayanos (1998) depends on the balance between the reduced purchase frequency and a prolonged holding period. In line with Constantinides (1986), however, Vayanos (1998) notes that a realistic model calibration typically results in a minimal negative effect on stock prices. Heaton and Lucas (1996) add that the magnitude of the direct effect further depends on the relative levels of transaction costs imposed on the various assets. The authors further model an indirect negative effect on prices stemming from agents' demand for larger equity premia to compensate for additional consumption volatility from trading fees. Contrarily, Buss and Dumas (2019) observe a price increase in risk-free bonds and stocks due to this additional consumption volatility. Lastly, Vayanos and Vila (1999) offer a further nuanced perspective, showing how rising transaction costs for illiquid assets not only make liquid assets more attractive and pricier but also establish a supply-dependent pricing mechanism for illiquid assets. That is, their price might decrease under limited supply but increase when supply is abundant, illustrating how transaction costs can lead to significant price changes through intricate channels.

Despite the overall dearth of empirical work, a select number of studies provide valuable insights into the price effects of trading costs. In this context, cross-sectional studies by Amihud and Mendelson (1986) and Acharya and Pedersen (2005) examine the effects of trading costs and liquidity on prices. Amihud and Mendelson (1986) investigate the effect of bid-ask spreads on asset returns, finding that high-spread assets have higher returns, partially attributed to higher spreads attracting longer-horizon clientele. Similarly, Acharya and Pedersen (2005) document a negative effect of illiquidity on returns, noting that persistent illiquidity and elevated liquidity risk lead to increased future returns. Münnix et al. (2010) explore how tick sizes influence financial return distributions and correlations, highlighting their importance in market microstructure, yet do not directly comment on price levels. Only Albuquerque et al. (2020) offer a causal investigation into the direct implications of tick sizes on asset prices.

In the context of the SEC's pilot program,[7] Albuquerque et al. (2020) find that treated stocks experience a significant stock price reduction between 1.75% and 3.2% after a singular increase in tick size compared to the control group. An effect, which is only observable for treated stocks with small pre-experiment dollar quoted spreads. Contrary to the main finding, they find no significant impact on prices when the tick size is reduced again at the end of the pilot program, which they attribute to investors anticipating the effect. They further study the factors contributing to the price reductions and find that treated stocks with small quoted spreads experience less price efficiency. This decline aligns with

[7]In the framework of the SEC's pilot, Chung et al. (2020) find that while liquidity diminishes for smaller orders, it improves for larger orders. Additionally, the program leads to improvements in pricing efficiency, an increase in trade size, and a decrease in the number of trades.

increased information risk and higher expected returns as suggested by Easley and O'Hara (2004). They also observe a clientele effect in line with Amihud and Mendelson (1986), where the proportion of long-term investors increases as they are less sensitive to the higher transaction costs. Finally, they find no evidence of liquidity risk affecting prices as predicted by Acharya and Pedersen (2005). However, the lack of the estimated effect does not necessarily indicate the absence of the effect as the results of the original paper do not survive reproduction.[8]

The Albuquerque et al. (2020) study of the tick size pilot program, while providing valuable and novel insights, faces several methodological challenges—inherent to the pilot's design rather than the author's analysis. Primarily, the pilot's focus on small-company stocks poses two limitations on the generalizability of the study's findings.[9] First, the focus on small-cap stocks reduces generalizability to large-cap stock prices. Second, as illiquid stocks often have larger non-binding spreads, the impact of tick-size changes is less pronounced unless the tick size was already binding pre-pilot. The generalizability is further detrimented by the timing of the pilot's implementation. Coinciding with the 2016 US presidential election, the setting introduces potential confounding factors, possibly inducing non-linear interactions between political risk and trading costs, thereby complicating the analysis of the pilot's effects. Moreover, the pilot program's publicized nature and the pre-announcement of treated and control stocks challenge traditional treatment and control constructs in experimental design, changing the interpretation of the results.[10] Specifically, not being treated with a tick-size increase is itself a treatment.[11] Finally, the pilot's two-year duration and well-known timeline might not have been adequate to significantly affect long-term investor behavior, especially for stocks with pre-existing high spreads. The modest

[8]Holden and Nam (2019) and Kazumori et al. (2019) show that the main results of the LCAPM by Acharya and Pedersen (2005) do not survive replication, leading Welch (2019) to conclude that while the ideas and theory of the LCAPM are convincing, the empirical results do not support their hypotheses.

[9]The pilot program selectively included approximately 1,200 small-company shares, which were chosen based on the criteria of having a market capitalization under 3 billion USD, a minimum closing price of 2.00 USD, and a consolidated average daily volume not exceeding 1 million shares.

[10]The tick size pilot program originated from a political initiative under the JOBS Act in April 2012. Subsequently, Congress mandated the SEC to implement a pilot program to assess the impact of increasing tick size. In June 2014, the SEC directed FINRA and the National Securities Exchanges to develop a program for selected small-cap stocks, which the SEC approved on May 6, 2015. Information about the goals to improve market liquidity and about group allocation had been extensively covered by media outlets (Wall Street Journal, 2016) a month before the program's start in October 2016.

[11]Both Chung et al. (2020) and Rindi and Werner (2019) find the effects of being untreated and observe a liquidity migration from the control stocks to the pilot stocks, thereby significantly overestimating its impact. Additionally, Vayanos and Vila's (1999) theoretical insights suggest that changes in costs for a significant portion of assets can disproportionately affect the remaining assets not subject to the change. As the ratio of treated to untreated stocks in the pilot program is 50%, the effect was likely overestimated.

increase in the investment horizon for institutional investors in stocks with small quoted spreads, as reported by Albuquerque et al. (2020), is likely more reflective of the pilot's limited duration rather than a substantive shift in investment strategies.

2.1 Hypothesis

As stated, standard theory suggests a negative effect on prices for increasing costs. That is, if one of the previously discussed mechanisms imposes an additional cost on investors, asset prices should adjust downwards to reflect these costs. Therefore, in line with Amihud and Mendelson (1986) who find that tick sizes represent a direct cost, and Van Ness et al. (2000) who find that tick sizes have a positive relation to trading costs, we expect a negative relation between tick sizes and asset prices. Concretely, our main hypothesis posits that an increase (decrease) in tick sizes leads to a decrease (increase) in stock prices. However, according to back-of-the-envelope calculations by Foucault et al. (2013) and Albuquerque et al. (2020), not all observed price variations can be explained by this direct effect, implying the presence of an indirect effect.

In asset pricing, the CAPM posits that expected returns account for trading costs, including those from liquidity. Higher illiquidity or the potential for future illiquidity typically demands greater returns as investors seek compensation for increased trading costs and associated risks. This aligns with Acharya and Pedersen (2005) and Pástor and Stambaugh (2003), who found that illiquidity and its future risk elevate required returns. Therefore, by impacting market liquidity, tick-size changes indirectly influence asset prices through adjustments of the liquidity premium. The stock price, as the present value of future cash flows, is discounted at a rate incorporating these higher liquidity costs or risks, leading to indirect price effects from tick-size changes. However, the precise impact of these changes on prices, given the complex relationship between tick size and market liquidity, remains unclear.[12]

3 Identification strategy and data

Our study employs a difference-in-differences (DiD) methodology to identify the causal impact of tick-size changes on asset prices within an RCT framework. We aim to measure

[12]Despite the complex relationship between tick sizes and liquidity and consequently asset prices, we do not expect the indirect effect of illiquidity to counteract the direct effect of trading costs. Thus, while the indirect effect is significant, it is unlikely to reverse the overall direction of the price change predicted by our hypothesis.

how increases and decreases in tick size affect asset prices. This is achieved by comparing the price development of stocks undergoing a tick-size change with that of those that do not experience a tick-size change within a designated timeframe. RCTs, regarded as the gold standard in causal inference, offer a robust means to explore the cause-effect relationship between an intervention (the change in tick sizes) and its impact (a change in asset prices). Randomization in this context ensures a balance in both observed and unobserved characteristics across treatment and control groups, allowing for a reliable attribution of any observed differences in outcomes to the intervention.

Subsequently, we detail the regulatory environment and the tick-size regime we leverage for our DiD analysis. We outline the strengths of our approach and justify the randomness of the tick-size changes, thereby validating the use of an RCT design. We then describe the data collection process and the methodology employed to generate the treatment and control groups before formally defining our identification strategy.

3.1 The MiFID II tick-size regime

Table 1: MiFID II tick-size table

	Average Daily Number of Transactions					
Price ranges	< 10	< 80	< 600	$< 2{,}000$	$< 9{,}000$	$9{,}000 \leq$
$0 \leq$ price < 0.1	0.0005	0.0002	0.0001	0.0001	0.0001	0.0001
$0.1 \leq$ price < 0.2	0.001	0.0005	0.0002	0.0001	0.0001	0.0001
$0.2 \leq$ price < 0.5	0.002	0.001	0.0005	0.0002	0.0001	0.0001
$0.5 \leq$ price < 1	0.005	0.002	0.001	0.0005	0.0002	0.0001
$1 \leq$ price < 2	0.01	0.005	0.002	0.001	0.0005	0.0002
$2 \leq$ price < 5	0.02	0.01	0.005	0.002	0.001	0.0005
$5 \leq$ price < 10	0.05	0.02	0.01	0.005	0.002	0.001
$10 \leq$ price < 20	0.1	0.05	0.02	0.01	0.005	0.002
$20 \leq$ price < 50	0.2	0.1	0.05	0.02	0.01	0.005
$50 \leq$ price < 100	0.5	0.2	0.1	0.05	0.02	0.01
$100 \leq$ price < 200	1	0.5	0.2	0.1	0.05	0.02
$200 \leq$ price < 500	2	1	0.5	0.2	0.1	0.05
$500 \leq$ price $< 1{,}000$	5	2	1	0.5	0.2	0.1
$1{,}000 \leq$ price $< 2{,}000$	10	5	2	1	0.5	0.2
$2{,}000 \leq$ price $< 5{,}000$	20	10	5	2	1	0.5
$5{,}000 \leq$ price $< 10{,}000$	50	20	10	5	2	1
$10{,}000 \leq$ price $< 20{,}000$	100	50	20	10	5	2
$20{,}000 \leq$ price $< 50{,}000$	200	100	50	20	10	5
$50{,}000 \leq$ price	500	200	100	50	20	10

NOTES: The tick size harmonization table introduced by MiFID II in 2018 sets the minimum price increment at which a stock can be traded for all European venues and is the uncompromising lower bound for the bid-ask spread. Tick sizes are set as a function of an asset's daily closing price (in Euros) and the previous year's average daily number of transactions.

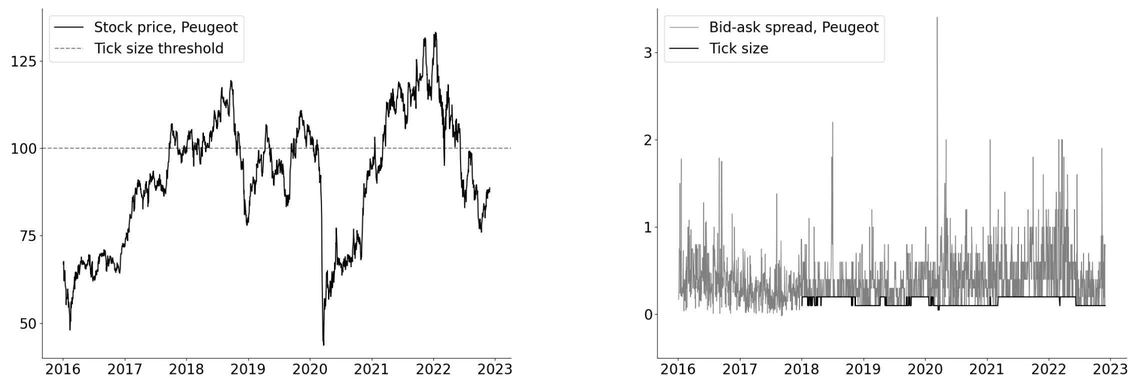
MiFID II is a legislative framework established by ESMA to regulate markets, improve market liquidity and quality, and foster competition. While the legislation covers a comprehensive set of regulatory rules for financial markets, one component was the standardization of tick sizes across all European markets. Acknowledging tick sizes as a critical determinant of market liquidity (see Sec. 2), the legislation aimed to standardize tick sizes across trading venues, thereby mitigating competitive undercutting and ensuring orderly market functioning (ESMA, 2023). Starting in January 2018, trading venues in the EU were required to follow a tick-size regime outlined in Article 49 of MiFID II and RTS 11. This regime specified minimum tick sizes for orders in shares and depository receipts, aiming to set a more optimal tick size as a function of liquidity and price. On the liquidity dimension, illiquid stocks are treated with a large tick size to attract market makers with potentially sizeable profits incentivizing liquidity provision (O'Hara et al., 2019). Conversely, liquid stocks are treated with a small tick size to unburden liquidity demanders from excessive costs. On the price dimension, penny stocks require a small tick size, so their smallest incremental price change is not the price itself. As the stock price rises, so does the tick size. MiFID II sets tick sizes relative to their traded prices to prevent low-priced stocks from incurring disproportionate costs. Otherwise, the tick size relative to the stock price would decrease with a stock's price, creating adverse incentives to trade higher-priced stocks at a relative discount. Within a liquidity bucket, relative tick sizes are approximately preserved.

Table 1 outlines the tick-size regime used by the trading venues. The tick size of an asset is determined by its price, given by the daily closing price, and its liquidity, given by the average daily number of transactions (ADNTE).[13] The closing price dictates a daily adjustment in tick size. For example, if an asset's price p_{t-2} is less than a defined threshold $\bar{p}$ on day $t-2$, with a corresponding tick size TS_{t-2}, and then closes at or above $\bar{p}$ on $t-1$, the tick size for day t adjusts upwards: $TS_t \geq TS_{t-1} = TS_{t-2}$. Conversely, if the price falls below a lower threshold $\underline{p}$, the tick size adjusts downwards, with the lower boundary included in the old tick size's price range. The liquidity component dictates an annual adjustment of the tick size, calculated based on the ADNTE from January 1[st] and December 31[st] each year. Based on the prior year's ADNTE, the tick-size is then adjusted from April 1st in the subsequent year.[14]

For illustrative purposes, consider a stock with 450 ADNTEs in 2018 and a closing price

[13]More precisely, ESMA computes the tick size using the ADNTE on the most relevant market in terms of liquidity (MRMTL). For simplicity, we refer to the ADNTE on MRMTL as the ADNTE throughout.

[14]Exceptions to this rule are made at the beginning of the program or for newly listed stocks. Initially, an estimate of the asset's liquidity is calculated without historical ADNTE data for the first six weeks. Subsequently, data from the first four weeks is used to re-estimate liquidity. This revised estimate is then employed until the next annual reassessment of ADNTE values.



NOTES: The left panel displays Peugeot's 2016–2022 stock prices (black) with multiple instances of the price crossing the 100 Euro price threshold (grey dotted line). The right panel displays Peugeot's bid-ask spread (grey) with the corresponding minimum tick size (black). The tick-size harmonization in Europe, starting in 2018, sets a lower bound for the stock's bid-ask spread. Whenever the stock price crosses the threshold of 100 Euros from below, the tick size increases from 0.1 to 0.2 Euros, and vice versa.

Figure 1: Tick-size changes triggered by threshold crossings

of 98.70 Euros on October 10$^{\text{th}}$, 2019. Positioned in the "< 600 ADNTE" liquidity bucket and the "50–100 Euro" price bucket according to Table 1, this stock is traded with a tick size of 0.1 Euros on October 11$^{\text{th}}$. If its share price crosses and closes at or above 100 Euros that day, the tick size on October 12$^{\text{th}}$ increases to 0.2 Euros. This change increases the relative tick size from approximately 0.0005 before crossing the threshold to 0.001 after. Figure 1 illustrates this scenario graphically using Peugeot's stock price. The left panel plots Peugeot's prices from 2016 to 2022, depicting instances of the stock price crossing the 100 Euro threshold (indicated by a grey dotted line). Each upward crossing increases the minimum tick size from 0.1 to 0.2 Euros and vice versa. In the right panel, the minimum tick size (represented by the black line), which sets a lower bound on the bid-ask spread (represented by the grey line), adjusts whenever a threshold is crossed.[15]

3.1.1 MiFID II as a setting to study the effect of tick-size changes

The key feature we leverage in our study is the dynamic adjustment of tick sizes driven by an asset's price fluctuations in the context of the MiFID II tick-size regime. As asset prices naturally vary over time, they trigger changes in tick sizes when crossing the predetermined thresholds. We posit that the tick-size changes induced by Table 1 are empirically random in terms of both timing and entity. This randomness is crucial, as it allows us to credibly

[15]Before MiFID II's implementation in 2018, there was no standardized lower limit on the bid-ask spread, as trading venues could set their tick sizes.

attribute observed differences between treated and untreated assets directly to the tick-size change, enabling a robust estimation of the causal average treatment effect.

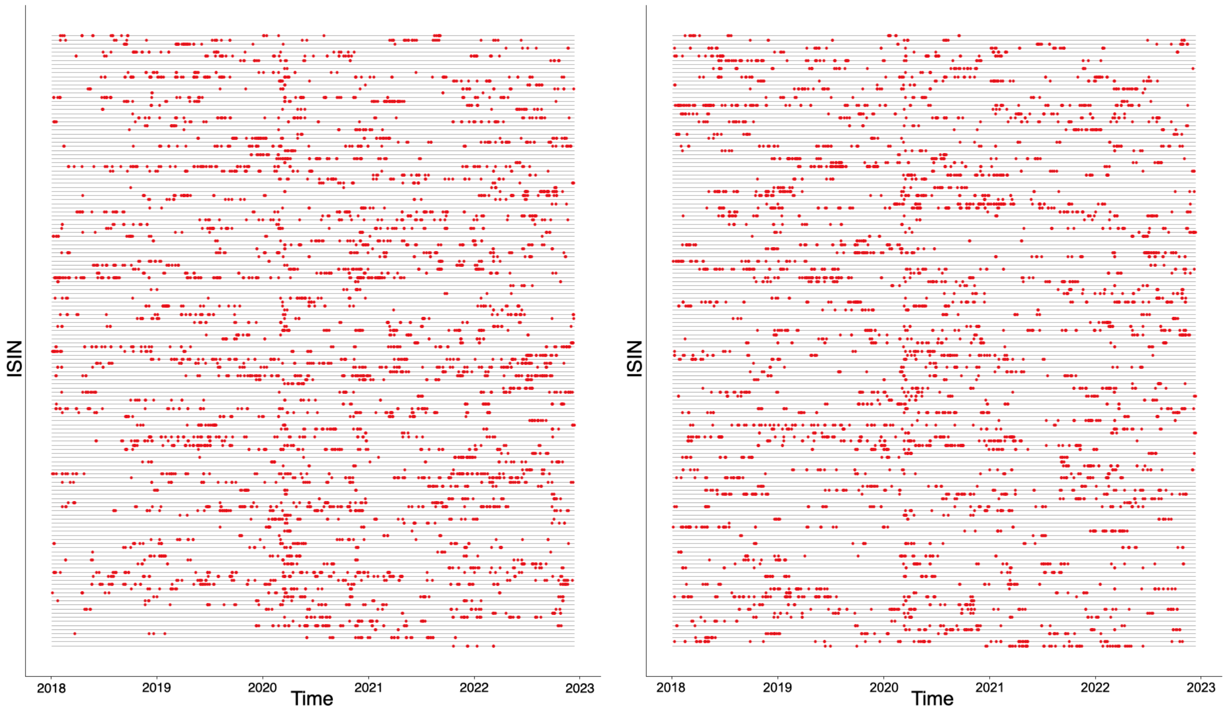
The foundation of our argument that the tick-size table induces a quasi-random phenomenon lies in the arbitrariness of the set thresholds. While economic justifications exist for adjusting tick sizes relative to stock prices (as detailed in Sec. 2), the specific levels at which thresholds are set appear economically arbitrary. For instance, there is no compelling economic reason to prefer a threshold of 20 Euros over 22 or 25 Euros. In the absence of these thresholds, we would expect liquidity and trader composition to remain consistent just above and below any given threshold. Additionally, while stock prices might be influenced post-tick-size change, for example, via the liquidity channel (Acharya and Pedersen, 2005), there is no clear economic rationale suggesting differential stock price behavior when approaching these thresholds. That is, the behavior of an asset crossing the 20 Euro threshold should not intrinsically differ from crossing a non-threshold, like 30 Euros, in the absence of the induced tick-size change. Consequently, we argue that the price-driven dynamic adjustment of tick sizes under MiFID II can be viewed as randomized treatment.[16]

This unique setting offers two distinct advantages over competing research. Firstly, as highlighted by Angel (2012), most legislative changes, including those related to tick sizes, are often endogenous and coincide with other legal amendments. This overlap typically introduces noise in assessing the impact of a policy change on an outcome due to potential interference from other legislation and seasonal effects (as noted by Heston and Sadka, 2008). Such challenges are particularly pertinent to initiatives like the US pilot program. In contrast, our identification strategy focuses on random, recurring changes in tick size triggered by price-threshold crossings, which may occur at any time following the introduction of the tick-size table (Tab. 1). This offers an opportunity for a more robust analysis compared to settings with a one-time, market-wide intervention.[17] Secondly, the repetitive nature of tick-size changes in the policy's context provides numerous instances of both tick-size increases and decreases for analysis. This contrasts sharply with the decimalization literature, where only tick-size decreases could be studied. Thirdly, stocks can cross thresholds at any time after the introduction of MiFID II. This offers the flexibility to study the impacts of varying tick sizes on the same stock at different times. Furthermore, this flexibility permits the categorization of stocks into the tick-size increase or decrease treatment groups or into the control group, depending on its price movement, which enhances

[16]This assertion of randomness is further discussed in Appendix B.

[17]Note, however, that single-event tick-size studies might be better suited for examining effects that heavily depend on current conditions (Shkilko and Sokolov, 2020), and allow for a deeper exploration of confounding macroeconomic conditions, market mechanisms and design features, *etc.*

Figure 2: The distribution of tick-size changes across assets and over time



NOTES: This figure plots the universe of French stocks, each stock represented individually by a horizontal line. Each red point represents a price-induced tick-size change for a stock between 2018 and 2022.

the robustness of our analysis by providing a comprehensive representation of stock-specific characteristics across the different study groups.

3.2 Data description

For our analysis, we require two data points to compute an asset's tick size on a given day: its daily closing prices and ADNTE. To this end, we collected the unadjusted daily closing prices of equities from Refinitiv Eikon for Austria, Belgium, France, Germany, Ireland, Italy, Luxembourg, Netherlands, and Portugal, spanning from January 3rd, 2018, to December 13th, 2022. This period captures a significant amount of market heterogeneity across the largest European markets.[18] Our initial collection comprised 1,988 unique ISIN numbers, containing 9,363 ISIN-year combinations and totaling 2,363,605 observations.

To determine the tick sizes used by trading venues, we additionally collected annual ADNTE data from the ESMA database of transparency calculation results.[19] Specifically,

[18]Refer to Table 8 in Appendix A for statistics on the representation of the different European markets.

[19]The transparency calculation results are provided at `https://registers.esma.europa.eu/publication/searchRegister?core=esma_registers_fitrs_equities`, accessed in November, 2022.

for each ISIN in our price dataset, we retrieved the corresponding historical ADNTE values.[20] To match these two datasets, it is crucial to recall that tick sizes are subject to daily price- and annual liquidity-driven adjustments. The ADNTE is computed annually for each equity but is only applied to set the tick size from April 1$^{\text{st}}$ of the succeeding year, extending to March 31$^{\text{st}}$ of the year following. Thus, our matching process allocated the ADNTE of the Y^{th} year with pricing observations from April 1$^{\text{st}}$, $Y+1$ to March 31$^{\text{st}}$, $Y+2$. Employing the criteria from Table 1, we then determined the active tick sizes using the preceding day's closing price. After matching the two datasets, our refined dataset included 1,988 unique ISIN numbers and 8,622 ISIN-year combinations, totaling 2,175,049 observations.

We define a change in tick size whenever the tick size of a given day differs from that of the previous day. Our analysis focuses exclusively on price-driven tick-size changes, excluding any changes driven by volume adjustments.[21] Therefore, a tick-size change in our context is initiated when the closing price of a stock crosses a price threshold either upwards or downwards relative to the previous day's closing price. The change in tick size becomes effective on the trading day immediately following the threshold crossing.

Figure 2 presents an overview of all price-driven tick-size changes for French stocks between 2018 and 2022, depicting each stock as a horizontal line in the figure arranged without specific order. Red dots denote individual tick-size changes for each stock. While these changes generally appear uniformly distributed across firms and over time, certain patterns emerge upon closer inspection. For instance, tick-size changes often cluster, indicating oscillations around specific thresholds. The impact of the initial COVID-19 market crash in Europe is also visible in this dataset. We observe similar trends to those displayed in Figure 2 across all European stocks. Regarding the frequency of events, we observe that a subset of stocks crosses one of the MiFID II price thresholds on any given day within our timeframe. This regular occurrence of threshold crossings translates to an average of 37 tick-size changes per day, implying that 1,950 out of the 1,988 stocks do not experience a tick-size change on average. This difference creates a natural division between treatment group candidates—stocks undergoing tick-size changes—and control group candidates for our DiD analysis. Notably, each stock has the potential to be categorized in either group across different periods.

[20] Again, the variable of interest is the ADNTE on MRMTL; however, for brevity, we refer to this as the ADNTE.

[21] Additionally, instances where a concurrent volume-driven change counteracts a price-driven tick-size change are omitted, as they do not result in an observable change in tick size for market participants. Notably, this exclusion led to excluding only three occurrences from our dataset.

Table 2: Total number of tick-size increases by price and liquidity

Price threshold	Average Daily Number of Transactions < 10	< 80	< 600	< 2000	< 9000	9000 ≤	Total
0.1	238	188	128	0	0	0	554
0.2	300	216	161	16	0	0	693
0.5	455	328	242	29	27	0	1081
1.0	814	715	326	81	51	2	1989
2.0	1019	947	520	154	64	32	2736
5.0	1136	963	774	288	187	19	3367
10.0	1024	906	820	392	419	79	3640
20.0	704	811	1080	678	397	48	3718
50.0	323	448	705	448	457	149	2530
100.0	321	244	303	283	411	106	1668
200.0	61	32	109	47	101	40	390
500.0	79	22	15	22	19	29	186
1000.0	29	15	16	0	5	0	65
2000.0	0	8	1	2	1	5	17
5000.0	12	0	7	1	0	0	20
Total	6515	5843	5207	2441	2139	509	22654

NOTES: A heatmap of the total number of tick-size increases across MiFID II price thresholds and volume buckets (corresponding to Tab. 1). The intensity of the color indicates the frequency of crossings, with darker shades representing higher values. An equivalent table for tick size decreases is provided in Appendix A, Table 7.

Our dataset captures 46,073 instances of price thresholds being crossed, with an almost equal split between 22,654 increases and 23,419 decreases. Table 2 offers an overview of the distribution of tick-size increases across different price and liquidity buckets.[22] The majority of these events occur within the 1 to 100 Euros price range, reflecting the stock price distribution in Europe. Regarding the liquidity distribution, each liquidity bin records between 500 and nearly 7,000 daily treatments for both tick-size increases and decreases.

3.2.1 Difference-in-differences data processing

Next, we outline the processing steps to transform our dataset to isolate the effect of tick-size changes on asset prices. The primary consideration is to create comparable counterfactuals. To motivate our approach, consider two hypothetical stocks: A and B. Stock A crosses an economically arbitrary price threshold of 20 Euros from below, closing with a price above this threshold. On the same day, stock B crosses another economically arbitrary threshold of 30 Euros but, unlike stock A, does not experience a tick-size change as per the European tick-size table (Tab. 1). In this scenario, stock B is a natural control for stock A. The rationale is that, in the absence of the tick-size change, there is no reason to expect

[22]Given the nearly equal distribution of treatment between increases and decreases, Table 7, which presents the distribution heatmap for tick-size decreases, is included in Appendix A.

Table 3: Number of tick-size increases by price and liquidity in treated group

Price threshold	Average Daily Number of Transactions < 10	< 80	< 600	< 2000	< 9000	9000 ≤	Total
0.1	21	31	21	0	0	0	73
0.2	33	38	29	3	0	0	103
0.5	38	74	42	6	8	0	168
1.0	129	158	64	19	13	0	383
2.0	195	229	145	50	17	10	646
5.0	316	247	220	71	54	2	910
10.0	253	231	228	110	131	27	980
20.0	211	228	290	189	114	15	1047
50.0	89	136	210	127	145	40	747
100.0	86	65	95	97	123	22	488
200.0	24	7	31	16	30	10	118
500.0	24	3	9	7	8	11	62
1000.0	8	3	7	0	3	0	21
2000.0	0	2	0	0	1	3	6
5000.0	8	0	4	1	0	0	13
Total	1435	1452	1395	696	647	140	5765

NOTES: A heatmap of the total number of tick-size increases across MiFID II price thresholds and volume buckets (corresponding to Tab. 1) for the treated group. The intensity of the color indicates the frequency of crossings, with darker shades representing higher values. An equivalent table for tick-size decreases is provided in Appendix A, Table 9.

divergent behavior between these two stocks upon crossing their respective price thresholds. We impose a tick-size change-free window before a price threshold can be crossed to isolate the effect of the tick-size change being considered. Post-threshold crossing, we refrain from enforcing restrictions and adopt an intention-to-treat (ITT) approach. That is, we neither enforce treated assets to stay treated nor control assets to stay untreated, which allows us to measure the effect of a tick-size change without conditions that may skew the outcomes.

Given the frequency of tick-size changes in our dataset, ensuring that any observed effects on prices are solely attributable to a single change in tick size is essential. Therefore, we first processed the previously collected dataset to isolate the effect. To this end, we eliminate from our dataset any tick-size change preceded by another tick-size change up to two weeks prior.[23] This exclusion criterion is vital as it allows sufficient time for asset prices to stabilize from any preceding adjustments, effectively "washing out" the influence of prior treatments. Subsequently, we track the price movements for two weeks following each tick-size change. Notably, during this post-change phase, we do not restrict price movements. Prices are allowed to re-cross thresholds, leading to further tick-size changes.

[23]The choice of a two-week window aligns with the approach used in Albuquerque et al. (2020). This timeframe has further been empirically observed to be sufficient for ensuring that the effects of previous changes have dissipated (see Fig. 5 in App. A), thereby allowing for a more accurate attribution of any subsequent price movements to the tick-size change in question.

These subsequent changes are regarded as partly a result of the initial tick-size adjustment at $t = 0$. Our methodology thus aligns with an ITT approach, where we actively avoid enforcing conditions that might introduce biases, such as a post-period price drift. For instance, if a tick-size increase leads to a price drop, enforcing that the stock must remain "treated" could attenuate or nullify the observable effect of the treatment. This four-week window surrounding a treatment event—a tick-size change—thus enables us to link a post-event average price change directly to the tick-size change being considered rather than to any previous changes.

Figure 3 provides a visual representation of normalized price movements within our treated group for both tick-size increases (Panel A) and decreases (Panel B). The mean and median prices are represented by solid and dashed blue lines, respectively. The shaded areas depict various percentile ranges: 20–80, 10–90, and 5–95 percentiles. Leading up to the crossing on $t = -1$, prices converge towards but do not exceed the threshold, represented by a black horizontal line. On $t = -1$, the closing price, determined by an end-of-day auction, crosses this threshold, triggering a change in tick size, which is effective the subsequent trading day ($t = 0$). We impose no additional constraints on price movements post-threshold crossing; hence, prices move freely above and below the previously crossed threshold.[24] Consistent with existing literature, we anticipate that the unconditional expectation of returns is zero. Consequently, both the mean and median prices remain stable over time post-tick-size change ($t \geq 0$).

To accurately measure the treatment effect of tick-size changes and obtain an unbiased estimate, it is necessary to construct a valid counterfactual. That is, we require a control group that mirrors the pre-event price behavior observed in the treatment group. Specifically, our choice of imposing a tick-size-change-free window pre-event introduces a drift into our treated group (see Fig. 3), which must be preserved in the control group. To replicate this drift in the control group, we introduce *synthetic price thresholds*: non-threshold integers, such as 30 Euros, that do not trigger a tick-size change. The choice of these synthetic thresholds is grounded in the understanding that the economic significance of specific price levels, like 20 Euros, is arbitrary. Thus, creating synthetic thresholds at integer levels, with finer discretizations (1-cent, 5-cent, and 10-cent) for lower-priced stocks, allows us to simulate similar market conditions as those faced by the stock-instances in the treatment group. To align the conditions for the control group with those of the treatment group,

[24]Note that, while the triggering event (price crossing the threshold on $t = -1$) and the treatment event (tick size change on $t = 0$) are chronologically distinct, they are practically simultaneous. That is, the triggering event, marked by the day's closing price, and the activation of the new tick size at the next day's market opening are separated by the period when markets are closed.

we impose the same restrictions: no crossing of the chosen synthetic or a real threshold pre-event and mandatory crossing of the synthetic threshold on $t = -1$. We allow prices to freely fluctuate post $t = -1$, including the possibility of crossing synthetic or real thresholds, in line with our ITT approach. Restricting price movement in this post-event period could create a ceiling or floor on prices, leading to biased results. This methodological choice ensures the control group accurately reflects the treatment group's pre-event drift, providing a robust counterfactual for our analysis.[25] As a final processing step, we excluded days with returns exceeding 25% and event windows with missing data from our treated and control datasets.

Table 3 provides an overview of the distribution of treatment events across various MiFID II price thresholds and liquidity categories, aligning closely with the full dataset as shown in Table 2, thereby maintaining a consistent representation of the liquidity buckets. However, there is a slight disproportion in the representation of the highest and lowest volume buckets within the treatment group compared to the full sample. Specifically, the lowest volume bucket accounts for 28.7% of the total tick-size changes and 24.9% within the treated subset, whereas the highest volume bucket comprises only 0.2% of the total tick-size changes but increases to 2.4% in the treated group.[26] Most events concentrate within price thresholds ranging from 1 to 100 Euros. The total number of treatment events comprises 5,765 tick-size increases and 7,162 tick-size decreases.[27] Furthermore, to ensure the comparability of our control group, we rounded the synthetic control thresholds to the nearest real price threshold to generate a table comparable to Table 3.[28] In total, we obtained a comprehensive control group encompassing over 100,000 events, divided between 46,248 tick-size increases and 65,184 tick-size decreases. Visual inspection of the heatmaps in the Appendix confirms that the distribution of events in both the control and treatment groups is consistent across the spectrum of price and liquidity categories.

Additional statistics included in Appendix A further validate our data processing. In Table 8, we provide an overview of the representation of countries in each group relative to the overall population, revealing that the proportions were maintained, ensuring geographic consistency in our dataset. Moreover, an analysis of the distribution of events over

[25] Figure 7 in Appendix A provides a plot of the normalized prices for the control group. Similar to Figure 3, the plots demonstrate a pre-event price drift approaching the chosen synthetic threshold and constant returns post-event.

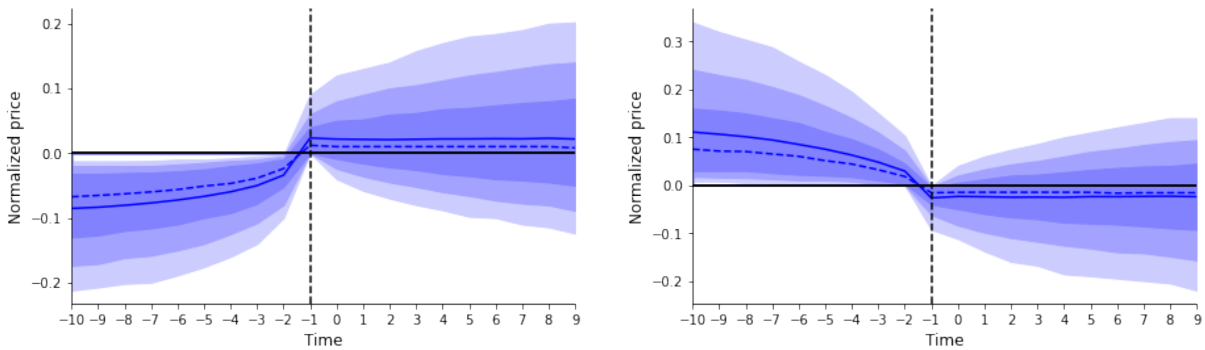
[26] The distribution among the other volume buckets is relatively balanced, with percentages in the total sample and treated sample being 25.8% and 25.2%, 23.0% and 24.2%, 10.8% and 12.1%, and 9.4% and 11.2%, respectively, for each ascending liquidity category.

[27] The distribution heatmap for tick-size decreases is detailed in Table 9 in Appendix A.

[28] Table 10 in Appendix A presents the heatmaps for tick-size increases and decreases within the control group.

(A) Tick-size increase

(B) Tick-size decrease



Notes: Normalized prices over the 4-week window for the treated group. Panels A and B present the results for tick-size increases and decreases, respectively. The price mean and median are represented by the solid and dashed lines, respectively. From inside to outside, the shaded areas represent the 20–80, 10–90, and 5–95 percentile ranges. The black horizontal line represents the normalized price threshold. Prices stay bound by the thresholds before crossing and can move freely after crossing. All prices cross the threshold at $t = -1$.

Figure 3: Normalized prices of the treated group for tick-size increases and decreases

time (see Fig. 6) suggests that our dataset is representative of financial events over time, with individual days being neither disproportionately overrepresented nor underrepresented through our data processing.[29] The treated group exhibits a strong correlation in the series of events over time when compared with the full population and with the control group, suggesting that, on this dimension, the treated group is representative.[30] The consistency across various dimensions of the dataset fortifies the credibility of our analysis, ensuring that it encompasses a comprehensive and unbiased view of the market dynamics under study.

3.3 Identification strategy

In contrast to many DiD applications that rely on a single observation before and after the event (*e.g.*, Card and Krueger, 1993) or on a single event (*e.g.*, Albuquerque et al., 2020), our approach offers multiple events across time for analysis. That is, the policy under consideration and the available data enable us to study a large set of arbitrary

[29]However, the correlations suggest that days with high market-wide variation are slightly underrepresented, as dropping them from both the full and processed data, increases the correlation between the groups.

[30]Pearson correlation coefficients of 0.56 and 0.88 are observed between the treated group and the full sample for tick-size increases and decreases, respectively. Correlations of 0.74 and 0.79 are observed between the treated and control groups for tick-size increases and decreases, respectively.

price-threshold crossings, which are geographically dispersed across European markets and distributed evenly throughout the sample period. Unlike the pilot program, which was conducted during the 2016 presidential election period (Albuquerque et al., 2020), this allows us to circumvent strong seasonalities and other external temporal and geographical factors that may distort our analysis. Hence, we can leverage the exogenous and random changes in tick sizes to estimate the causal effects of changing tick sizes on prices. Furthermore, our study's abundance and variety of data facilitate a large and representative control group and enable us to conduct several robustness checks to validate our findings.

Following Colliard and Hoffmann (2017), we adopt time-flexible dummies to differentially estimate the effect over time and account for varying adaptation speeds. The baseline estimation period covers ten trading days before and after the event. The flexible framework allows us to capture potential differences in the treatment effect between the first and second weeks following the change in tick size. Formally, the assumption underlying our approach is that for each stock i and day t,[31] the variable of interest, $y_{i,t}$, satisfies:

$$E(y_{i,t} \mid i,t) = \alpha_i + \gamma_t + \beta^{Cross} D_{i,t}^{Cross} + \beta^{Week1} D_{i,t}^{Week1} + \beta^{Week2} D_{i,t}^{Week2}. \tag{1}$$

where $D_{i,t}^{X}$ is a vector of dummy variables that take the value of 1 for treated stocks on the day the price threshold is crossed ($Cross$), the first week ($Week1$), and the second week ($Week2$) of treatment, respectively, and 0 otherwise.[32] α_i and γ_t are stock and day fixed effects, respectively. In our analysis, $y_{i,t}$ is the raw return.[33] To account for autocorrelation and company-specific idiosyncrasies, we adjust standard errors by clustering on firm and time following Thompson (2011). Equation (1) relies on the standard common trends assumption that the dependent variable for both groups of stocks should co-move closely absent any treatment. While this assumption cannot be tested formally, we follow the common argumentation and evaluate the time series visually in Figure 4.

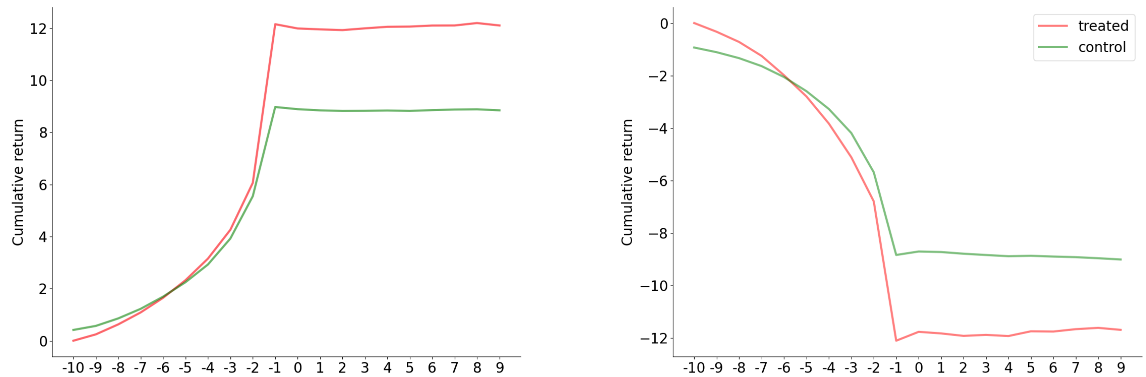
In Panels A and B of Figure 4, we present the cumulative returns for both the treatment and control groups for tick-size increases and decreases, respectively. The red line represents the returns of the treated stocks, while the green line corresponds to the control group. Several observations can be made from these plots. Initially, pre-threshold-crossing on

[31] The time index t indicates the relative distance from the treatment event at $t = 0$.

[32] We control for the effect on the day of the crossing as we observe excess returns (discussed in Sec. 4). Again, while the triggering event (price crossing threshold) and the treatment event (tick size changing) are two chronologically distinct events, the former occurs at the end of the day at market closing. Therefore, practically, we argue that the events occur simultaneously.

[33] We use raw returns and not risk-adjusted as we believe that the fixed effects account for part of the company-specific market risk and that market betas average across the sample. Moreover, Albuquerque et al. (2020) report no difference in their findings when comparing raw returns with risk-adjusted returns.

(A) Tick-size increase (B) Tick-size decrease



Notes: This figure plots the average cumulative returns for the treated and control groups over a four-week window focusing on the common trends in returns pre-event. The treatment occurs at time $t = 0$. Panels A and B present the results for tick-size increases and decreases, respectively.

Figure 4: Pre-event common trends in cumulative returns

$t = -1$, there is a strong co-movement in cumulative returns between the groups, with the treated group exhibiting a marginally higher drift.[34] Still, these co-moving trends prior to treatment lend substantial support to the common trends assumption underlying our DiD approach. Additionally, excess returns for the treated group are observable on $t = -1$ compared to the control group. Although we contend that the triggering event and the subsequent treatment event occur practically simultaneously, the observed phenomenon on $t = -1$ still warrants further investigation. Therefore, in Section 4, we explore possible explanations for this observation, including both a behavioral response to crossing round-number thresholds and strategic effects, attempting to decompose the observed effect into its constituent parts.

4 Results

In this section, we highlight two main findings. First, we document how exogenous random tick size changes impact publicly traded companies' stock prices. The estimated causal treatment effects, derived from the DiD analysis following Equation (1), are detailed in Table 4 in Section 4.1. We assess the impact of increasing and decreasing tick sizes for the universe of European stocks under consideration. Additionally, a further analysis distin-

[34]We provide a deeper analysis of the pre-treatment return distributions, including summary statistics and corresponding hypothesis tests in Appendix B.1.

guishes between tick-size constrained and unconstrained stocks and examines the variances in outcomes for larger firms compared to smaller ones. Second, we document a statistically and economically significant reaction of the treated stocks crossing a price threshold compared to the control group. This outcome may arise from two factors: round numbers such as 20, 50, and 100 may act as psychological barriers, and crossing them demonstrates points of support or resistance. Alternatively, the excess returns could stem from strategic trading by market participants aiming to force a change in tick size. In Section 4.2, we test these hypotheses and report the results in Tables 5 and 6.

4.1 Causal impact of changing tick sizes on stock prices

Table 4 presents the main findings of this study. Panel A details the outcomes associated with tick-size increases, while Panel B focuses on decreases. The first three columns quantify the impact of tick-size changes on returns for the day of the price-threshold crossing (β^{Cross}), the first week (β^{Week1}), and the second week (β^{Week2}) following the change, respectively (see Eq. (1)). The subsequent two columns include the number of treated instances used in the regressions and the total number of observations.[35] The results are organized in the following order across both panels: the first row examines the entire sample; the second and third rows present results for tick-constrained and unconstrained stocks, respectively, determined by ranking stocks based on the frequency of pre-event constrained days and selecting the top and bottom 20%; the fourth and fifth rows analyze the smallest and largest stocks in terms of market capitalization, determined by ranking stocks based on their average pre-event market cap and selecting the top and bottom 20%.[36]

Our main findings reveal a consistent inverse price effect following a tick-size change over two weeks, confirming our hypothesis (see Sec. 2.1). Specifically, tick-size increases lead to a significant decrease in stock returns, with daily returns declining by approximately 13 basis points (bps) in the first (β^{Week1}) and second week (β^{Week2}) in the treatment period (Tab. 4, Panel A, "Full sample" row). Conversely, tick-size reductions are associated with increased returns, observed as rises of 32 and 35 bps in the first and second weeks, respectively. These results translate into a 1.3% decrease and a 3.35% increase in prices over the two weeks following the respective tick-size changes,[37] suggesting an asymmetric impact where the

[35] The number of control instances can be deduced from these values, given that each treated and control instance comprises 20 observations.

[36] The results of these subgroup analyses are not sensitive to various methods of determining the group classification nor the timeframe used for computing these values.

[37] The two-week price adjustments are computed as 13 bps $\times$ 5 days + 13 bps $\times$ 5 days = 1.3% and 32 bps $\times$ 5 days + 35 bps $\times$ 5 days = 3.35%, respectively.

Table 4: Causal impact of tick-size changes on stock prices

(A) Tick-size increase	β^{Cross}	β^{Week1}	β^{Week2}	# Treated	# Obs.
Full sample	2.530***	-0.128***	-0.127***	5'765	1'022'940
	(0.000)	(0.000)	(0.000)		
Tick constrained	1.545***	-0.028	-0.149***	1'124	202'500
	(0.000)	(0.525)	(0.000)		
Tick unconstrained	3.258***	-0.209***	-0.142*	1'177	212'400
	(0.000)	(0.000)	(0.083)		
Market cap, smallest 20%	2.650***	-0.238**	-0.186**	1'150	204'400
	(0.000)	(0.037)	(0.012)		
Market cap, top 20%	1.752***	-0.111**	-0.128***	1'206	214'580
	(0.000)	(0.018)	(0.002)		
(B) Tick-size decrease					
Full sample	-1.884***	0.320***	0.346***	7'161	1'422'920
	(0.000)	(0.000)	(0.000)		
Tick constrained	-1.299***	0.081	0.195**	1'394	278'500
	(0.000)	(0.119)	(0.011)		
Tick unconstrained	-2.779***	0.570***	0.626***	1'462	292'180
	(0.000)	(0.000)	(0.000)		
Market cap, smallest 20%	-2.147***	0.518***	0.286***	1'429	283'980
	(0.000)	(0.000)	(0.000)		
Market cap, top 20%	-2.017***	0.202***	0.385***	1'500	298'160
	(0.000)	(0.006)	(0.000)		

NOTES: This table presents estimates for the β coefficients, which correspond to Equation (1), with the stock return as the dependent variable. Panels A and B present the results for tick-size increases and decreases, respectively. p-values, based on stock and time-clustered standard errors, are given in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

effect of tick-size reduction on prices is more substantial than that of an increase.

The robustness of these findings, substantiated by the sensitivity analysis in Appendix C, indicates the following: the choice of control thresholds introduces slight variations, yet the directional effect within the experiment is monotonic. Loosening synthetic thresholds by considering any price increment as a threshold increases the effect size, while employing reduced controls, such as round behavioral thresholds, reduces the effect.[38] Further analysis confirms that our main results are not driven by days with extreme events or outliers in observable variables. Excluding days with a high frequency of events does not significantly alter the outcomes for tick-size increases. For tick-size decreases, however, there is a modest sensitivity to outliers, with the findings maintaining their direction and statistical significance, albeit with a marginally lesser magnitude after excluding these outliers. The reduction in magnitude in the second treatment week is more pronounced than

[38] The latter effect, attributed to a behavioral response, is explored in Section 4.2.

in the first.[39] An analysis of the pre-event window length shows variation in effect magnitudes but nevertheless supports the main findings of an inverse price effect.[40] Despite the variations observed, the measured effects' directionality, magnitude, and significance consistently align with our primary findings.

The consistency of the results across different sensitivity analyses substantiates the robustness of the inverse price effect following a tick-size change and provides strong evidence for internal validity. The direction and significance of the observed effects, both for tick-size increases and decreases, survive even when accounting for potential outliers. From the sensitivity analyses, we estimate the magnitude of the effect to be between $[-7, -13]$ bps in the first week and $[-6, -13]$ bps in the second week after a tick-size increase; and between $[19, 32]$ bps in the first week and $[17, 35]$ bps in the second week following a tick-size decrease.[41] This translates into a 0.65% to 1.3% decrease and a 1.8% to 3.35% increase in prices over the two weeks following the respective tick-size change.

While our study demonstrates robust external validity, it is important to recognize certain limitations. Our dataset ensures generalizability as it encompasses all major European markets, spans a wide range of price and liquidity levels across all MiFID II tick-size categories, and captures a comprehensive representation of events across time. Additionally, our results are not contingent on including specific stock or time characteristics, reinforcing our dataset's representation of the broader European financial market. However, the nature of MiFID II tick sizes poses challenges to the external applicability of our results. While it is understood that optimal tick sizes exist (see, *e.g.*, Graziani and Rindi, 2023), little is known about the exact shape of the relationship between tick sizes and, *e.g.*, market efficiency. Research suggests that efficiency losses occur on either side of an optimal tick size (Dyhrberg et al., 2020). Assuming MiFID II was successful in determining optimal tick sizes, our results would then be reflective of tick-size improvements that occur close to an optimum rather than representing the impact of a universal tick-size change.[42] More likely to be true is that the effect we measure is an average of tick-size changes on either side of the optimum and moving in either direction, towards or away, from the optimum, in an unknown proportion. However, as we cannot assume that the effect is constant relative

[39] After dropping 47 of the 1288 days in our dataset, the first and second-week results are 27 and 18 bps, respectively (see results in Tab. 15, Panel B.I, "Main result controls" row).

[40] Details on the robustness tests are available in Appendix C, including the impact of control thresholds (Tab. 12), the effect of outliers (App. C.3) including extreme day outliers (Tab. 15).

[41] These lower bounds are taken from the robustness tests that excluded days with a high frequency of events (see Tab. 15).

[42] However, this critique applies to any tick-size policy study. Literature on both decimalization and the tick size pilot program lack generalizability as they examine scenarios where initial tick sizes were "very suboptimal" and were updated towards "more optimal" levels.

to the distance a tick size is away from its optimal level, the generalizability of our results suffers.

Our secondary results provide preliminary insights into this aspect. Although MiFID II accounts for liquidity levels when determining tick sizes, there may still be inaccuracies in their specification due to the annual frequency of liquidity adjustments and the inadequacy of ADNTE as a reliable liquidity measure. We analyze the price effects for tick-constrained and unconstrained stocks as a proxy for daily liquidity levels. For tick-constrained stocks, we observe no significant effect from an increase in tick size during the first week but a notable decline of 15 bps in the second week (Tab. 4, Panel A, "Tick constrained" row), aligning with the magnitude of the full sample effect. Conversely, a reduction in tick size initially shows no impact in the first week yet results in a 20 bps increase in the second week. In contrast, tick-unconstrained stocks respond more notably to an increase in tick size, with a 21 bps decline in the first week and an effect within a similar range of the full sample in the second week. When the tick size decreases, these stocks experience a significant rise, with returns increasing by 57 bps in the first week and 63 bps in the second week. While these findings are preliminary, they underscore the presence of asymmetric effects across different liquidity profiles and tentatively suggest that the effects may exhibit variability in magnitude as they diverge from the optimal tick size.

Comparing our findings with existing research, we note an alignment in the direction of our results with Albuquerque et al. (2020), yet find significant differences in the magnitude of the effects. However, a direct comparison is challenging due to the previously discussed likelihood that the relationship between proximity to optimal tick size levels and their effects is non-linear. Specifically, for tick-size increases, our main results are approximately a third of those measured by Albuquerque et al. (2020). Conversely, we find large positive effects on returns for tick-size decreases, where Albuquerque et al. (2020) observes no significant effect, attributing this to anticipatory behavior by market participants. The discrepancy in results for tick-size increases may stem from several factors in Albuquerque et al.'s study: a considerable portion of their stocks are untreated, potentially causing spill-over effects as highlighted by Vayanos and Vila (1999); their analysis pertains to a one-time policy change; and their subject is low priced, small-cap stocks with low liquidity. In our subgroup analyses, estimates are higher: for smaller market-cap firms, we find decreases of 24 bps and 19 bps in the first and second weeks (2.15% in total), respectively, following a tick-size increase (Tab. 4, Panel A, "Market cap, smallest 20%" row); our illiquid subgroup shows decreases of 21 bps and 14 bps in the respective weeks (1.75% in total) after a tick-size increase (Tab. 4, Panel A, "Tick unconstrained" row); the analysis of low-priced stocks does not yield significant deviations from the main results (see Tab. 14 in App. C.2). For tick-size decreases, the difference between our results and those in Albuquerque et al. (2020) can largely be attributed to our study's lack of anticipatory actions, where market participants cannot predict tick-size changes and hence cannot adapt early. However, this difference is not surprising, as many studies have documented the impacts of tick-size decreases on various market variables in the absence of anticipatory behavior (see Sec. 2).

In addition to the treatment effect, our findings reveal a notable response between the treated and control groups on the day the price threshold is crossed (β^{Cross})—a consistent pattern across all examined specifications and subgroups. We observe that an increase in tick size leads to excess returns experienced by the treated stocks relative to the control group by an average of 2.5%. Conversely, a reduction in tick size results in the treated stocks exceeding the control group's performance by 1.9% in absolute terms. We discuss this divergence in the following section and offer two theoretical explanations for the observed effect.

4.2 Behavioral versus strategic trading

The observed first-day effect, characterized by its substantial size, persists across all analyses in this study. The subgroup analysis within the main results (Tab. 4) reveals variations in the magnitude of this effect across the different categories. Regardless of the subgroup, the effect size remains considerable, ranging from an increase of 1.5% to 3.3% on the day for tick-size increases and a decrease of −1.8% to −2.8% for tick-size decreases. The effect demonstrates a notable resilience to extreme day fluctuations, particularly for tick-size increases. While there is some sensitivity for tick-size decreases, the effect never fully attenuates. To account for this pronounced first-day effect, we propose two explanatory hypotheses—behavioral and strategic—which are well-established in the literature. The behavioral hypothesis is linked to prices nearing "round-number" thresholds, potentially heightening traders' responsiveness to specific price levels (see, *e.g.*, Bhattacharya et al., 2012; Johnson et al., 2008). The strategic hypothesis, on the other hand, posits that market participants may be motivated to push the stock price beyond a threshold at market closing, driven by the implications of the higher or lower trading costs the subsequent day (see, *e.g.*, Aggarwal and Wu, 2003; Allen and Gale, 1992). While we find evidence supporting both hypotheses, quantifying the strategic effect is challenging. In the following experiment, the strategic effect is defined as an exclusion category for the portion of the effect not explained by the behavioral hypothesis. Consequently, this approach necessitates a careful interpretation of the preliminary findings.

4.2.1 The behavioral effect of round prices

From an economic standpoint, a distinct return behavior should not be anticipated when breaking price levels of 50 versus 52. However, behavioral or psychological considerations surrounding such "round numbers" may account for the pronounced performance on the day the price crosses a threshold for the treated group compared to the control group. The price's proximity to round numbers may heighten traders' sensitivity to the price level, rendering these numbers as either support or resistance. Price and quote clustering around round numbers has been documented by Bhattacharya et al. (2012) and Johnson et al. (2008). In our main analysis, we compare stocks crossing round numbers like 50 to stocks crossing synthetic thresholds like 52 that offer less support or resistance, potentially partially explaining the observed excess returns. For this reason, in a subsequent experiment, we control for the behavioral pattern by narrowing the control group to only round-number synthetic thresholds that do not initiate a tick-size change.[43] Table 5 presents the causal treatment effects derived from the DiD analysis with the reduced control group comprising behavioral synthetic thresholds.

The format of Table 5 follows that of the main results (Tab. 4) to enable direct comparison. That is, Panels A and B present outcomes for tick-size increases and decreases, respectively. The initial three columns measure the impact on returns on the threshold crossing day (β^{Cross}), and during the first (β^{Week1}) and second weeks (β^{Week2}) of treatment (see Eq. (1)). The following two columns show the count of treated instances and total observations. The results are ordered as follows: the first row for the full sample; the second and third for tick-constrained and unconstrained stocks; and the fourth and fifth for the smallest and largest stocks by market cap. Our findings indicate that controlling for the behavioral impact of round numbers leads to lower excess returns on the day a threshold is crossed (β^{Cross}), with the returns ranging from 0.6% to 1.5% for tick-size increases and -0.5% to -1.0% for decreases. This reduction suggests that the behavioral effect contributes to approximately half to two-thirds of the excess returns observed in the main results.[44] Despite the reduced statistical significance partially due to excluding three-

[43] The reduced set of synthetic thresholds to control for the behavioral effect include: 0.15, 0.3, 0.4, 0.6, 0.7, 0.8, 0.9, 3, 4, 6, 7, 8, 9, 15, 30, 40, 60, 70, 80, 90, 125, 150, 175, 300, 400, 600, 700, 800, 900, 3,000, 4,000, 6,000, 7,000, 8,000, and 9,000. This aligns with the findings of Cellier and Bourghelle (2007), who observed that investors tend towards round numbers when placing limit orders for European stocks, leading to a concentration of limit order prices on whole numbers, halves, and tenths.

[44] The discrepancy in the first-day effects observed in Tables 4 and 5 can be attributed to the behavioral effect. For instance, in the full sample results for tick-size increases, the difference between the effects ($2.530\% - 1.336\% = 1.194\%$) represents roughly half (47%) of the first-day effect reported in the main results.

Table 5: Causal impact of tick-size changes on stock prices, behavioral control group

(A) Tick-size increase	β^{Cross}	β^{Week1}	β^{Week2}	# Treated	# Obs.
Full sample	1.336***	-0.080**	-0.076**	5'765	311'680
	(0.000)	(0.022)	(0.020)		
Tick constrained	0.617***	-0.052	-0.075	1'124	67'720
	(0.000)	(0.405)	(0.151)		
Tick unconstrained	1.049***	-0.161***	-0.072	1'177	63'960
	(0.000)	(0.009)	(0.122)		
Market cap, smallest 20%	1.507***	-0.094	-0.180***	1'150	62'240
	(0.000)	(0.178)	(0.005)		
Market cap, top 20%	0.574***	-0.013	-0.047	1'206	65'300
	(0.000)	(0.849)	(0.446)		
(B) Tick-size decrease					
Full sample	-0.551***	0.251***	0.306***	7'161	424'520
	(0.000)	(0.000)	(0.000)		
Tick constrained	-0.329***	0.070	0.128*	1'394	85'920
	(0.000)	(0.101)	(0.060)		
Tick unconstrained	-0.949***	0.503***	0.572***	1'462	90'160
	(0.000)	(0.000)	(0.000)		
Market cap, smallest 20%	-0.486***	0.396***	0.291***	1'429	88'600
	(0.000)	(0.000)	(0.000)		
Market cap, top 20%	-0.814***	0.174**	0.273***	1'500	93'000
	(0.000)	(0.014)	(0.000)		

NOTES: This table presents estimates for the β coefficients, which correspond to Equation (1) with the stock return as the dependent variable, using the reduced set on controls. That is, the treatment effect is estimated relative to behavioral synthetic thresholds. Panels A and B present the results for tick-size increases and decreases, respectively. p-values, based on stock and time-clustered standard errors, are given in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

quarters of the control group, the direction and magnitude of the remaining treatment effects, β^{Week1} and β^{Week2}, mirror the main results (*cf.* Tab. 4).

4.2.2 The strategic trading incentives around tick-size thresholds

The alternative hypothesis we propose is that certain market participants might engage in strategic trading with the objective of inducing a tick-size change. Specifically, the market-making sector[45] earns, on average, the equivalent of the spread and benefits from a large tick size. If a stock price nears a tick-size threshold, these participants may be incentivized to push and maintain the price above this threshold until markets close, thereby triggering an increase in tick size. On average, this strategy increases the returns the

[45]Following Nagel (2012), our definition of market makers in equity markets includes not only officially designated market makers but also algorithmic traders and quantitative investors (Hendershott et al., 2011).

market makers derive from each transaction the following day. The opposite applies to participants wanting to decrease the spread: institutional investors trading large quantities in close proximity to a threshold experience an incentive to drive the price below said threshold. If successful, this strategy leads to a tick-size reduction and a corresponding decrease in the spread the following trading day. The foundational work by Allen and Gale (1992) shows that an uninformed manipulator can make profits in a rational expectations framework by simply buying and selling shares. This occurs as other utility-maximizing market participants interpret the manipulator's order flow as potentially originating from an informed trader. Empirically, Aggarwal and Wu (2003) provide evidence suggesting that informed traders, such as corporate insiders, brokers, large shareholders, and market makers, are most likely to manipulate stock prices. Khwaja and Mian (2005) provide compelling evidence of collusion among market makers to inflate prices and attract positive-feedback traders.

To quantify the magnitude of the strategic effect, we compare the treated group to a control group from before the introduction of MiFID II in January 2018. Specifically, we use a control group that consists of instances where stock prices crossed behavioral thresholds pre-MiFID II. This analysis compares the treated group, whose agents have the incentive to manipulate the price to prompt a tick-size change, with an untreated group from a period when crossing price levels did not trigger tick-size changes. However, we cannot use this control group to systematically study the effect of tick-size changes. Before the tick-size harmonization, tick sizes were set by the individual exchanges and were used strategically to attract trading volume (Foley et al., 2023). Consequently, tick sizes were much lower on average, although not directly observable. Still, since the control group is void of any strategic trading-cost incentives and by controlling for behavioral effects, a remaining first-day effect can be attributed to strategic incentives from trading costs. If a strategic effect exists, we anticipate positive excess returns on threshold-crossing days (β^{Cross}) relative to the pre-MiFID II behavioral control group. However, given the substantial differences between pre- and post-MiFID II markets, the portion of the excess returns attributed to the strategic effect cannot be estimated relative to the main results. Therefore, we calculate a benchmark first-day treatment effect by applying the same control thresholds from the main results but using pre-MiFID II data. This approach offers a rough parallel to the main results while controlling for other market design differences, making it a more appropriate comparison for analyzing the strategic results.

Table 6 presents the experiments' results designed to assess the strategic effect. Panels I and II report the estimated impact of MiFID II tick-size changes on stock prices compared to pre-MiFID II control groups. Panel I presents results with the full set of synthetic control thresholds, including integer values and smaller increments for lower-priced stocks. In contrast, Panel II, using behavioral thresholds, controls for the behavioral component. Consequently, Panel I serves as a benchmark for evaluating the first-day effects of Panel II. The format of both panels is similar to the previous results. Panels A and B present outcomes for tick-size increases and decreases, respectively. The initial three columns measure the impact on returns on the threshold crossing day (β^{Cross}), and during the first (β^{Week1}) and second weeks (β^{Week2}) of treatment (see Eq. (1)). The next three columns show the count of treated instances, control instances, and total observations. As tick-sizes pre-MiFID II are unobservable, preventing the computation of tick-constrained subgroups, our analysis is limited to assessing the general effect and performing a subgroup analysis for small and large firms. The results are presented with the first row showing the full sample, and the second and third rows detailing the smallest and largest stocks by market capitalization, respectively.

Our findings provide evidence of the presence of a strategic trading incentive. Relative to the first-day effects in Panel I, the Panel II effects are all consistently smaller. That is, in Panel I, using the full set of synthetic thresholds, excess returns range from 0.8% to 1.5% for tick-size increases and from -0.4% to -1.6% for decreases. In contrast, in Panel II, excess returns range from 0.4% to 0.6% for tick-size increases and from -0.2% to -0.8% for decreases. These values suggest that, in the full sample, up to approximately one-third of the first-day effects can be attributed to a strategic effect.[46] Regarding the first and second-week treatment effects, most results in both Panel I and II align with the main findings (*cf.* Tab. 4). While the magnitudes in Panel I differ from the main results, their direction remains consistent. Similarly, the sign of the coefficients in Panel II also aligns with the main results; those that deviate are either small in magnitude or statistically insignificant. Except for the small market-cap subgroup for tick-size decreases, all two-week effects in both Panel I and II follow the same direction as in the main results. Still, an in-depth interpretation of the β^{Week1} and β^{Week2} coefficients is cautioned against due to the differing market conditions between the treated and control groups, the unobservable control tick sizes, and the experiment being limited by a relatively small number of control instances, derived from just two years of pre-MiFID data.

[46] The subgroup analyses for tick-size decreases suggest that up to 100% of the first-day effect could be attributed to the strategic effect.

Table 6: Causal impact of changing trading costs on stock prices, pre-MiFID control group

(I) pre-MiFID II, full sample control group

(A) Tick-size increase	β^{Cross}	β^{Week1}	β^{Week2}	# Treated	# Control	#Obs.
Full sample	1.454***	-0.444***	-0.491***	5'765	6'386	243'020
	(0.000)	(0.000)	(0.000)			
Market cap, smallest 20%	1.520***	-0.331***	-0.472***	1'149	1'275	48'480
	(0.000)	(0.002)	(0.000)			
Market cap, top 20%	0.882***	-0.395***	-0.435***	1'205	1'338	50'860
	(0.000)	(0.000)	(0.000)			
(B) Tick-size decrease						
Full sample	-1.061***	0.394***	0.515***	7'161	5'661	256'440
	(0.000)	(0.000)	(0.000)			
Market cap, smallest 20%	-0.424***	0.439***	0.401***	1'429	1'128	51'160
	(0.000)	(0.000)	(0.000)			
Market cap, top 20%	-1.558***	0.333***	0.603***	1'500	1'184	53'680
	(0.000)	(0.001)	(0.000)			

(II) pre-MiFID II, behavioral threshold control group

(A) Tick-size increase	β^{Cross}	β^{Week1}	β^{Week2}	# Treated	# Control	# Obs.
Full sample	0.575***	-0.097***	-0.164***	5'765	1'308	141'500
	(0.000)	(0.001)	(0.001)			
Market cap, smallest 20%	0.413***	0.027***	-0.284***	1'150	262	28'240
	(0.001)	(0.020)	(0.023)			
Market cap, top 20%	0.404***	-0.101*	-0.211***	1'206	273	29'600
	(0.000)	(0.053)	(0.000)			
(B) Tick-size decrease						
Full sample	-0.243***	-0.029	0.204***	7'161	1'050	164'200
	(0.000)	(0.687)	(0.002)			
Market cap, smallest 20%	-0.397***	0.009	0.006	1'429	210	32'780
	(0.005)	(0.944)	(0.971)			
Market cap, top 20%	-0.838***	-0.070	0.320*	1'500	218	34'380
	(0.000)	(0.544)	(0.094)			

NOTES: This table presents estimates for the β coefficients which correspond to Equation (1) with the stock return as the dependent variable. The control groups of Panel I and II use data from before the introduction of MiFID II, including the years 2016–2017. The synthetic thresholds from Panel I correspond to integer numbers in accordance with Table 4. The synthetic thresholds from Panel II correspond to integer numbers in accordance with Table 5. Panels A and B present the results for tick-size increases and decreases, respectively. p-values, based on stock and time-clustered standard errors, are given in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

5 Conclusion

This study analyzes the causal impact of tick sizes on asset prices within equity markets. As tick sizes represent costs to investors (see Amihud and Mendelson, 1986; Van Ness et al., 2000), our work contributes to both the extensive literature on the specific role of tick sizes and the broader role of trading costs on financial markets. Our research, grounded in the European tick size harmonization policy under MiFID II, sheds light on several key ways tick sizes influence asset prices. Our novel identification strategy leverages the random-walk nature of asset prices and the resulting random tick-size changes in an RCT-style framework to isolate the effects of individual tick-size increases and decreases. Further solidified by comprehensive robustness checks that account for outliers and extreme market events, our findings consistently demonstrate an inverse price effect in response to tick-size changes.

Our findings demonstrate that tick-size changes significantly impact stock prices, with notable varying effects. To the best of our knowledge, this research is the first to find asymmetric impacts of tick-size increases and decreases on stock prices. We found that an approximate doubling of tick sizes leads to a price decrease of between 0.9% and 1.3% over two weeks, whereas an approximate halving of tick sizes results in a price increase of 3.3% to 3.5%. These effects are more pronounced in smaller firms and less pronounced in stocks with smaller pre-treatment quoted spreads. Additionally, we identified substantial excess returns coinciding with tick-size changes; approximately half to two-thirds of these returns are attributable to behavioral responses to round numbers and up to one-third to strategic trading incentives. The robustness of our findings is supported by a rigorous methodology, including a novel identification strategy and a careful selection of control stocks, ensuring that major market events like the COVID-19 market crash or the Ukraine war or anomalies in other observable variables do not confound our findings. The comprehensive sensitivity analyses reinforce our study's internal validity, verifying our results' reliability. While primarily reflecting the effects of tick-size adjustments near MiFID II's "optimal" levels, our findings offer valuable insights into the broader market dynamics in response to regulatory tick-size changes.

Several unanswered questions arise from this study, presenting avenues for future research. As our study is the first to investigate tick-size increases and decreases in the same context, the asymmetric effect of different directions of tick-size adjustment on markets is novel and warrants further research. Additionally, given the probable role of liquidity in our observed effects, a more nuanced examination of liquidity would provide valuable insights. One approach involves analyzing stocks that transitioned between MiFID II liquidity buckets and, hence, may have been traded with suboptimal tick sizes due to the annual calculation frequency of the ADNTE. In another approach, intra-day data could enable the computation of more precise liquidity metrics (see, *e.g.*, Holden and Jacobsen, 2014), allowing for the examination of these components in an environment less affected by the noise typical of event studies (*e.g.*, decimalization or the pilot program). Additionally, by studying intra-day prices pre-market-closing, we could evaluate the assertion that the triggering and treatment events of tick-size changes are indeed simultaneous. Finally, due to our two-week exclusion restriction, the effect of price oscillations around thresholds on asset prices remains unexplored. These oscillations, particularly in light of negative price effects following tick-size changes, could indicate that behavioral influences are amplified under the European tick-size regulation. Exploring these topics would build on this study's foundation, potentially enriching our understanding of complex market dynamics.

In conclusion, our research contributes to the ongoing discourse on the optimal regulation of tick sizes in financial markets by providing empirical evidence on the causal effects of tick-size changes. Our findings underscore the significance of tick size as a regulatory tool and its strong impact on financial markets.

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A Supplementary figures and tables

Table 7 presents the distribution of price-driven tick-size changes across the MiFID II price and liquidity categories. Before processing, tick-size increases and decreases are approximately equally distributed, with 22,645 instances in the former and 23,419 instances in the latter. In both directions, most observations occur between 1 and 100 Euros. Each liquidity bucket is well represented in the dataset. Visual observation between the two heatmaps indicates a high similarity between the joint distributions for tick-size increases and decreases.

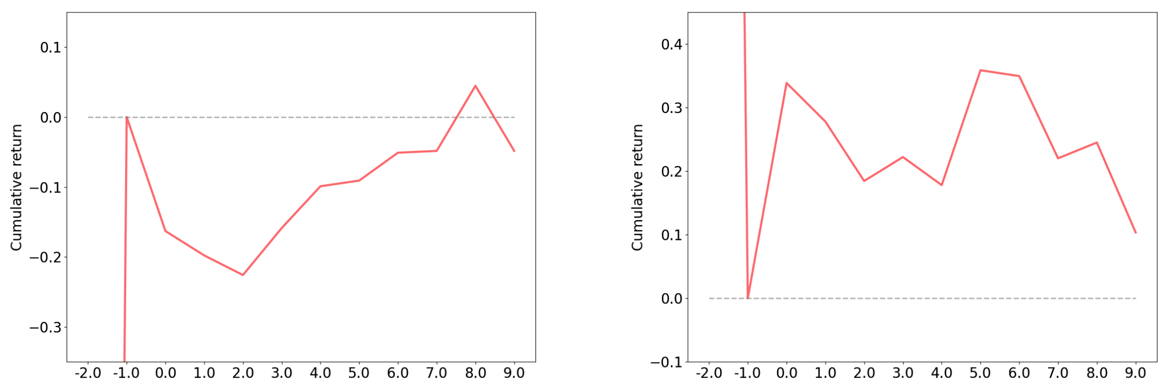
Table 7: Total number of tick-size changes by price and liquidity

Direction	Price threshold	Average Daily Number of Transactions < 10	< 80	< 600	< 2000	< 9000	9000 ≤	Total
(A) Tick-size increase	0.1	238	188	128	0	0	0	554
	0.2	300	216	161	16	0	0	693
	0.5	455	328	242	29	27	0	1081
	1.0	814	715	326	81	51	2	1989
	2.0	1019	947	520	154	64	32	2736
	5.0	1136	963	774	288	187	19	3367
	10.0	1024	906	820	392	419	79	3640
	20.0	704	811	1080	678	397	48	3718
	50.0	323	448	705	448	457	149	2530
	100.0	321	244	303	283	411	106	1668
	200.0	61	32	109	47	101	40	390
	500.0	79	22	15	22	19	29	186
	1000.0	29	15	16	0	5	0	65
	2000.0	0	8	1	2	1	5	17
	5000.0	12	0	7	1	0	0	20
	Total	6515	5843	5207	2441	2139	509	22654
(B) Tick-size decrease	0.1	249	204	146	0	0	0	599
	0.2	316	237	186	22	0	0	761
	0.5	462	360	273	41	29	0	1165
	1.0	829	757	361	98	54	3	2102
	2.0	1030	993	573	172	68	34	2870
	5.0	1126	1001	831	311	193	21	3483
	10.0	1007	921	874	433	436	83	3754
	20.0	691	823	1126	710	418	48	3816
	50.0	322	434	709	468	475	155	2563
	100.0	314	242	297	284	409	107	1653
	200.0	57	30	105	44	98	39	373
	500.0	76	22	14	21	17	27	177
	1000.0	29	17	16	0	3	0	65
	2000.0	0	9	1	2	1	5	18
	5000.0	13	0	6	1	0	0	20
	Total	6521	6050	5518	2607	2201	522	23419

NOTES: A heatmap of the total number of tick-size increases and decreases across MiFID II price thresholds and liquidity buckets (corresponding to Tab. 1). Panels A and B present the results for tick-size increases and decreases, respectively. The intensity of the color indicates the frequency of crossings, with darker shades representing higher values.

Figure 5 plots the average cumulative returns for the treated group over the 2-week window after treatment. Panels A and B present the tick-size increase and decrease results, respectively. Both panels show the cumulative returns reverting to mean zero in the second week after treatment. For tick-size increases, the cumulative returns revert to zero after approximately nine days, while the returns corresponding to tick-size decreases revert after approximately eleven days. In line with Albuquerque et al. (2020), this observation justifies our decision to use a 2-week window pre-event to allow for the effects of previous tick-size changes to dissipate.

(A) Tick-size increase (B) Tick-size decrease



Notes: This figure plots the average cumulative returns for the treated group over the 2-week window after the treatment event. Panels A and B present the results for tick-size increases and decreases, respectively.

Figure 5: Post-event 2-week convergence of cumulative returns

Table 8 presents the country representation in the various study groups, suggesting that the processing did not inadvertently over or underrepresent a specific country.

Table 8: Number of ISINs per country

Country	Raw	Treated	Control
Austria	62	56	60
Belgium	115	106	115
France	594	553	581
Germany	622	571	599
Ireland	28	27	25
Italy	377	336	341
Luxembourg	30	28	28
Netherlands	126	108	117
Portugal	34	31	33

NOTE: This table presents the country representation in the various groups.

Table 9 presents the distribution of price-driven tick-size changes across the MiFID II price and liquidity categories for the treated group. As with the full population (*cf.* Tab. 7), tick-size increases and decreases are approximately equally distributed with 5,765 and 7,161 instances, respectively. Again, most observations occur between 1 and 100 Euros, and the liquidity buckets are represented well in both groups.

Table 9: Number of tick-size changes by price and liquidity in treated group

Direction	Price threshold	Average Daily Number of Transactions < 10	< 80	< 600	< 2000	< 9000	9000 ≤	Total
(A) Tick-size increase	0.1	21	31	21	0	0	0	73
	0.2	33	38	29	3	0	0	103
	0.5	38	74	42	6	8	0	168
	1.0	129	158	64	19	13	0	383
	2.0	195	229	145	50	17	10	646
	5.0	316	247	220	71	54	2	910
	10.0	253	231	228	110	131	27	980
	20.0	211	228	290	189	114	15	1047
	50.0	89	136	210	127	145	40	747
	100.0	86	65	95	97	123	22	488
	200.0	24	7	31	16	30	10	118
	500.0	24	3	9	7	8	11	62
	1000.0	8	3	7	0	3	0	21
	2000.0	0	2	0	0	1	3	6
	5000.0	8	0	4	1	0	0	13
	Total	1435	1452	1395	696	647	140	5765
(B) Tick-size decrease	0.1	45	47	43	0	0	0	135
	0.2	59	59	58	6	0	0	182
	0.5	88	105	92	14	9	0	308
	1.0	195	242	130	34	14	0	615
	2.0	248	316	195	73	33	10	875
	5.0	281	313	285	119	58	6	1062
	10.0	285	307	294	156	138	41	1221
	20.0	197	266	402	244	153	22	1284
	50.0	81	121	228	164	158	60	812
	100.0	79	61	89	92	132	34	487
	200.0	12	11	26	14	23	15	101
	500.0	20	9	3	7	4	9	52
	1000.0	5	3	2	0	1	0	11
	2000.0	0	4	0	1	0	1	6
	5000.0	8	0	2	0	0	0	10
	Total	1603	1864	1849	924	723	198	7161

NOTES: A heatmap of the total number of tick-size increases and decreases across MiFID II price thresholds and liquidity buckets (corresponding to Tab. 1) for the treated group. Panels A and B present the results for tick-size increases and decreases, respectively. The intensity of the color indicates the frequency of crossings, with darker shades representing higher values.

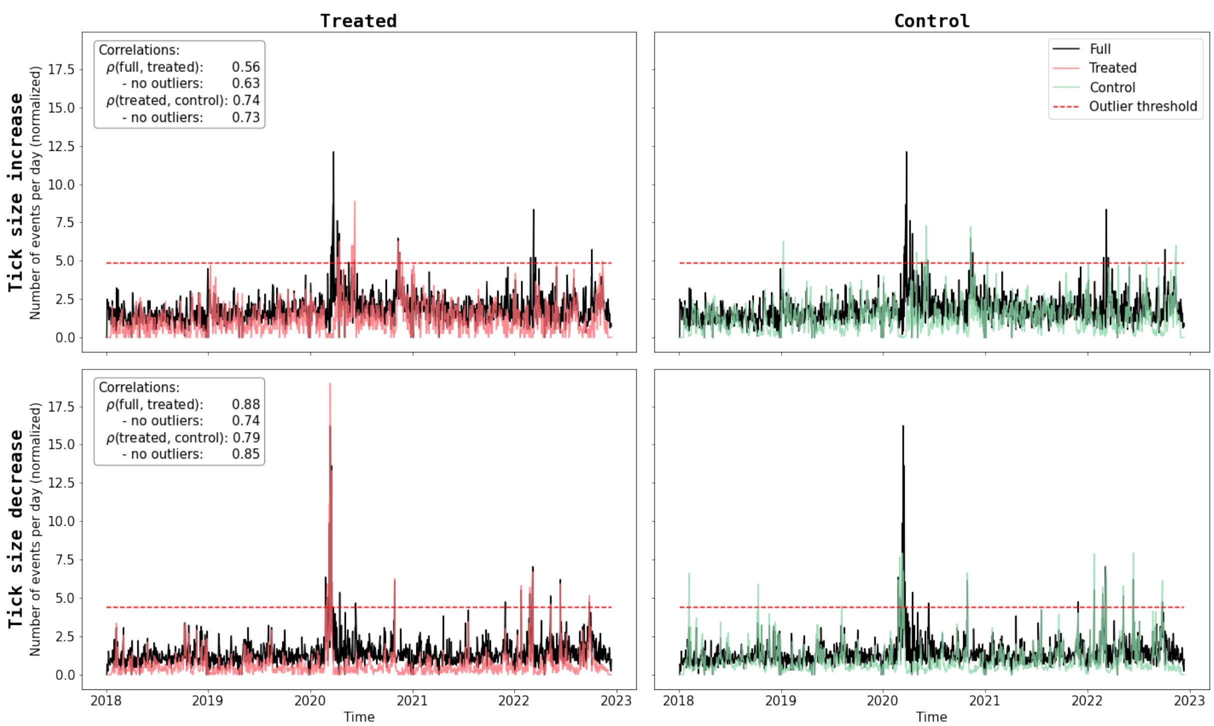
Table 10 presents the distribution of synthetic price threshold crossings, rounded to the nearest MiFID II price thresholds to create the table. The size of the control groups for tick-size increases and decreases are 46,248 and 65,184 instances, respectively. Following the distribution of the full data sample (*cf.* Tab. 7) and the treated group (*cf.* Tab. 9), the majority of the observations occur between 1 and 100 Euros. While relative to the treated group, there is a slight underrepresentation of 1 to 5 Euro stocks; these differences do not influence the results as relative tick sizes are preserved between price ranges (see Sec. 3.1). Liquidity buckets are represented well in both groups, with most control instances falling in the lower range of ADNTE buckets.

Table 10: Synthetic threshold crossings by price and liquidity in the control group

Direction	Price threshold	< 10	< 80	< 600	< 2000	< 9000	9000 ≤	Total
		Average Daily Number of Transactions						
(A) Tick-size increase	0.0	110	47	48	6	0	3	214
	0.1	109	187	128	45	0	0	469
	0.2	135	279	157	31	21	5	628
	0.5	177	268	178	49	32	6	710
	1.0	163	176	145	35	26	0	545
	2.0	229	243	175	66	20	3	736
	5.0	652	594	608	298	132	37	2321
	10.0	917	1143	1494	844	597	169	5164
	20.0	1163	2496	3676	2542	2122	345	12344
	50.0	514	1678	3013	2841	3246	748	12040
	100.0	146	509	1441	2331	2879	687	7993
	200.0	5	46	424	606	854	385	2320
	500.0	8	0	45	133	234	233	653
	1000.0	0	0	0	1	67	11	79
	2000.0	0	0	0	0	8	24	32
	5000.0	0	0	0	0	0	0	0
	Total	4328	7666	11532	9828	10238	2656	46248
(B) Tick-size decrease	0.0	231	120	105	20	0	6	482
	0.1	332	396	257	121	9	3	1118
	0.2	341	484	259	55	51	17	1207
	0.5	376	425	319	69	38	14	1241
	1.0	276	220	142	33	25	0	696
	2.0	384	387	279	102	35	5	1192
	5.0	1058	981	871	425	167	45	3547
	10.0	1647	1869	2167	1150	740	178	7751
	20.0	2234	3910	5396	3067	2523	379	17509
	50.0	1490	2714	3802	3110	3411	904	15431
	100.0	1261	1424	1860	2106	2437	650	9738
	200.0	749	379	637	480	522	278	3045
	500.0	456	211	63	186	118	145	1179
	1000.0	136	176	61	1	58	18	450
	2000.0	97	191	9	10	8	23	338
	5000.0	199	10	26	25	0	0	260
	Total	11267	13897	16253	10960	10142	2665	65184

Notes: A heatmap of the total number of synthetic threshold crossings (rounded to the nearest MiFID II price thresholds) with liquidity buckets corresponding to Tab. 1. Panels A and B present the results for tick-size increases and decreases, respectively. The intensity of the color indicates the frequency of crossings, with darker shades representing higher values.

Figure 6 plots the distribution of events over time for both tick-size increases and decreases in the top and bottom rows, respectively. The columns present the distributions for the treated and control groups, respectively. The red and green lines plot the number of events per day for the respective group, while the black line plots the number of events per day for the full unprocessed dataset. The correlation statistics show that the treated group exhibits a high correlation with both the full dataset and the control group, demonstrating that the treated group is representative of both the full dataset and is represented by the control group in the time dimension. The red dashed line in each panel depicts the 3σ-outlier threshold computed on the full sample. Dropping days with more observations than this threshold improves the correlation statistics, suggesting that extreme days are underrepresented. This subset is used for robustness tests to evaluate the result's sensitivity to extreme days (see Tab. 15).

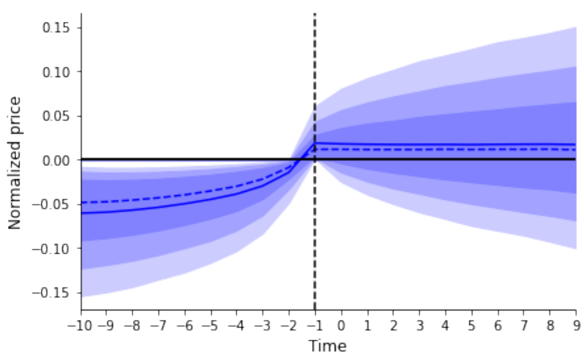


NOTES: This figure illustrates the temporal distribution of events—price-driven tick-size changes. The columns represent the treated and control groups, while rows plot the events related to tick-size increases and decreases, respectively. The black, blue, and red lines represent the normalized number of events in the complete raw dataset, treatment group, and control group, respectively. The red dashed horizontal line marks the outlier threshold applied in robustness analyses, with days exceeding 3σ events—where σ denotes the standard deviation of event counts in the full dataset—being excluded. The annotated correlations indicate a consistent representation across the samples.

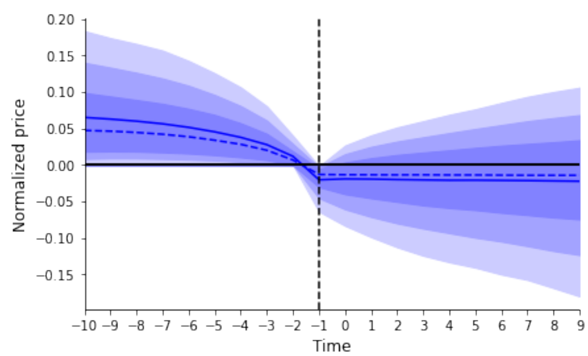
Figure 6: Temporal distribution of events

Figure 7 plots the normalized prices over time for the control group for both tick-size increases (Panel A) and decreases (Panel B). This figure corresponds to Figure 3, which plots the normalized prices over time for the treated group. The mean and median prices are represented by solid and dashed blue lines, respectively. The shaded areas depict various percentile ranges: 20–80, 10–90, and 5–95 percentiles. Like with the treated group, prices converge towards but do not exceed the synthetic price thresholds on $t = -1$, represented by a black horizontal line. On $t = -1$, the closing price crosses the respective threshold. Prices are unrestricted post-threshold cross and can move above and below the threshold. Prices have an upward drift pre-event similar to the drift observed in the treated group (*cf.* Fig. 3) and stabilize during the treatment period, as is visualized by the flattening of the mean and median values.

(A) Tick-size increase



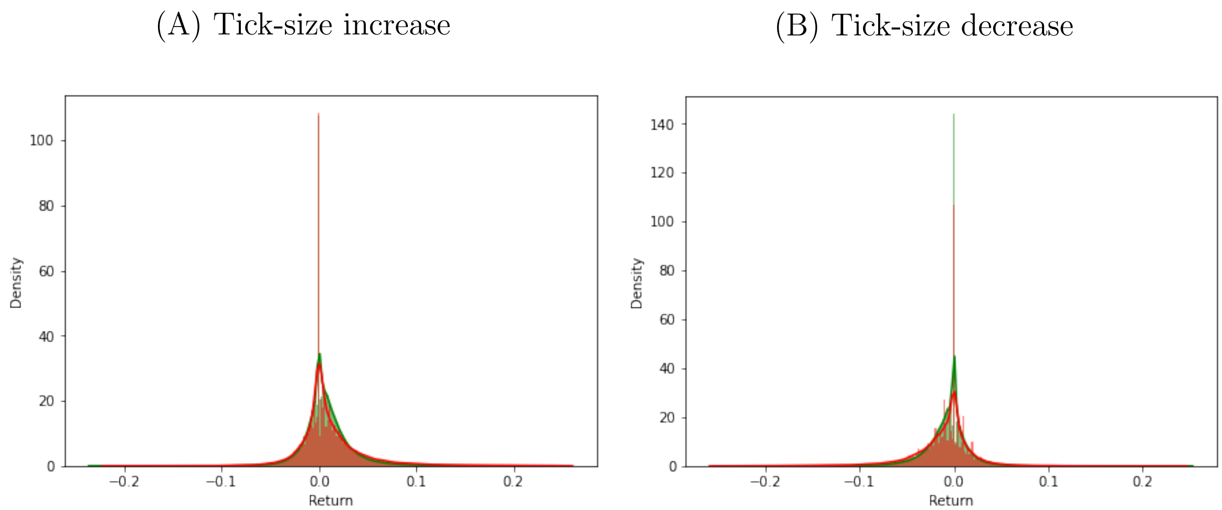
(B) Tick-size decrease



Notes: Normalized prices over the 4-week window for the control group. Panels A and B present the results for tick-size increases and decreases, respectively. The price mean and median are represented by the solid and dashed lines, respectively. From inside to outside, the shaded areas represent the 20–80, 10–90, and 5–95 percentile ranges. The black horizontal line represents the normalized synthetic price threshold. Prices stay bound by the thresholds before crossing and can move freely after crossing. All prices cross the threshold at $t = -1$.

Figure 7: Normalized prices of the control group for tick-size increases and decreases

Figure 8 plots the pre-event return distributions for tick-size increases and decreases in Panels A and B, respectively. The red and green histograms plot the treated and control groups, respectively. Due to issues with discretization induced by the tick sizes (see Sec. B.1), the distributions demonstrate a heaping of zero-return days, causing the kernel estimates of the distributions (red and green lines) to form a peak. This heaping is also visible at other return levels and more strongly in the treated group. Still, the kernel estimates suggest that the return distributions are very similar (see App. B.1 for more information on the statistical similarity between the two distributions).



NOTES: This figure plots the pre-event return distribution (for $t < 0$). Panels A and B present the results for tick-size increases and decreases, respectively. The red and green histogram and kernel density estimations (line) correspond to the treatment and control groups, respectively. A notable clustering of returns around zero is observed, attributable to the tick-size-induced discretization. Corresponding summary statistics are reported in Table 11.

Figure 8: Pre-event return distributions

B A note on the endogeneity

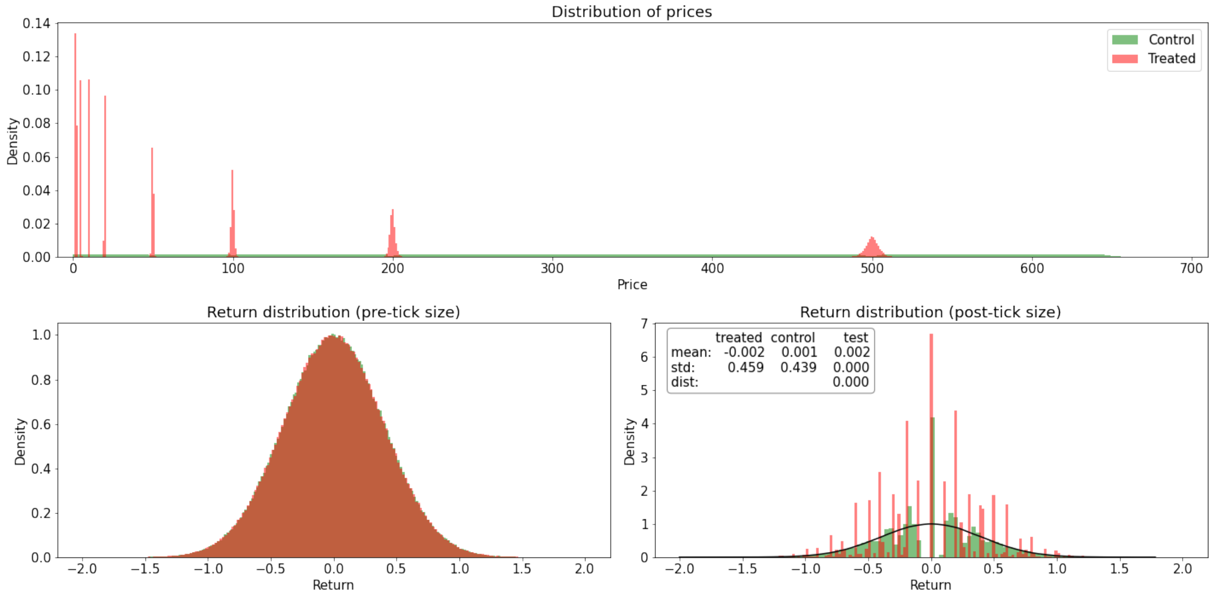
Endogeneity presents a valid concern in our study due to the evident bidirectional relationship between the independent and dependent variables. Specifically, the MiFID II tick-size table directly dictates tick sizes based on price levels, which, as hypothesized, influence prices. This cycle can be summarized as follows: price dictates tick size, change of tick size defines the treatment, and treatment impacts the price. However, this bidirectional relationship would be disrupted if randomization were successful, meaning that changes in tick size—and consequently the treatment—would be random occurrences rather than systematic. This key aspect of randomization is crucial for breaking the cyclical link in our analysis.

Unlike Albuquerque et al. (2020), who benefit from an external entity that randomizes stocks into groups with balanced observable characteristics, our study lacks such centralized control. Instead, the "entity" responsible for randomization in our scenario is comprised of the market participants whose buying and selling activities influence stock prices. While the resultant prices are generally accepted in financial research to follow a random walk, this assumption is often not a critical underpinning of a result.[47] Therefore, our subsequent analysis aims to persuade the reader that the progression of stock prices is sufficiently random, ensuring the effective randomization of stocks into their respective groups.

Our first step to this end involves analyzing the return distribution, which serves as an indicator of potential discrepancies between treatment and control groups. As the distributions summarize the probability of prices, differences between these distributions might suggest a failure in randomization. However, the nature of our data complicates this analysis. Specifically, the discretization caused by tick sizes distorts these distributions, making direct comparison challenging without a carefully tailored prior. The difficulty is discerning whether observed differences in return distributions are attributable to inherent disparities or merely the result of mechanical distortions due to discretization. Conventional hypothesis tests that evaluate the equality of two distributions may fail, not necessarily due to actual differences in the underlying distributions, but due to these distortions. While possible, developing a test that accounts for such distortions poses a risk of overfitting—a test may indicate equality, but this could result from an over-engineered test rather than a true reflection of underlying similarities. Although these distortions also have implications for the representativeness of our groups, our primary focus here is to motivate the approach we propose to navigate these complexities. Specifically, we aim to demonstrate the effectiveness of randomization by predicting the treatment status of a two-week series of prices. This method is designed to circumvent the challenges posed by distributional distortions while targeting the relevant issue: whether agents can predict price-threshold crosses. We will first discuss the expected distortions to the return distribution in Section B.1 and subsequently outline our strategy for predicting treatment, along with the results, in Section B.2.

[47]While the randomness of prices is generally accepted, we acknowledge that this alone may not fully address concerns about endogeneity. Yet, we do contend that prices are sufficiently random, especially for minor price changes. Arguing that an asset's precise value is, for instance, 199 Euros instead of 200 Euros, or 4.95 Euros instead of 5.10 Euros, is difficult. As these minor variations are crucial for our treatment allocation, we maintain that the economic story is sufficient to claim randomness. We recognize that we could substantiate this claim empirically with intra-day data.

B.1 An analysis of the return distribution



NOTES: This figure demonstrates the impact of tick sizes on return distributions using simulated data. The top panel displays the distribution of 160,000 simulated 10-day price trajectories, each generated with normally distributed i.i.d. returns (mean: 0.0; standard deviation: 0.4). These sequences are divided into two groups of 80,000 each: treatment (price trajectories crossing a threshold) and control. Different tick sizes apply between consecutive price thresholds (taken from the "< 10 ADNTE" liquidity bucket from Tab. 1). The bottom left panel shows the return distribution before the tick-size application, while the bottom right panel shows the distribution after rounding to the nearest tick size. Treated and control groups are represented by red and green histograms, respectively, with the original kernel density estimation of returns shown as a black line for comparison. The key in the bottom right includes summary statistics and p-values for hypothesis tests: t-test (means), Levene test (standard deviation), and Kolmogorov-Smirnov test (distribution).

Figure 9: Tick-size induced distortion of return distribution

This section examines the pre-event return distributions for both treated and control groups, emphasizing the difficulties in using these distributions to substantiate claims. The challenge stems from the mechanical distortion in return distributions due to price discretization into tick-size increments. Such distortions complicate the interpretation of hypothesis tests, as they entangle true differences in the underlying distributions with the mechanical distortions introduced by the discretization process. While the primary goal of this analysis is to motivate our approach for demonstrating randomness (as detailed in Sec. B.2), we also mention the implications of these findings for the overall validity of our experimental design.

Table 11: Pre-treatment return distribution statistics

		(A) Tick size increase			(B) Tick size decrease		
		Treated	Control	Hypothesis test	Treated	Control	Hypothesis test
	Mean	1.242	1.098	0.000	-1.252	-1.009	0.000
	Std	3.668	3.127	0.000	3.488	2.907	0.000
	Min	-21.212	-24.681		-25.000	-25.000	
Quartiles	25	-0.407	-0.358		-2.381	-2.000	
Quartiles	50	0.328	0.518	0.000	-0.526	-0.462	0.000
Quartiles	75	2.139	2.113		0.196	0.126	
	Max	25.000	25.000		23.810	24.481	

NOTES: This table presents statistics for return distributions for tick-size increases (Panel A) and decreases (Panel B), along with hypothesis test results. The p-values from the t-test, Levene test, and Kolmogorov-Smirnov test assess the equality of means, standard deviations (std), and overall distributions between treated and control groups. The null hypothesis, positing equal distribution statistics for both groups, is rejected at p-values below a set significance level. Refer to Appendix A, Figure 8 for the related return distributions.

The distortion induced in return distributions by tick-size discretization is tightly linked to the MiFID II tick-size table (see Tab. 1). As prices increase within a given liquidity bucket and price range, the relative tick size decreases. Upon crossing a price threshold, the relative tick size jumps, reverting approximately to the largest relative size in the previous price range. Consequently, in the treatment group, where prices are more likely to cluster near these thresholds due to the sorting, the relative tick sizes are more concentrated compared to the control group. The control group's relative tick sizes are less restricted, allowing for a broader fluctuation across the entire price range. Although the differences in relative tick sizes between the two groups are not economically significant,[48] they nonetheless have a pronounced effect on the return distributions. In essence, the treatment group's clustering around price thresholds results in a narrower distribution of relative tick sizes, in contrast to the control group. This leads to the observed distortions in the return distributions.

To illustrate the aforementioned effect, we conducted a stylized Monte Carlo simulation. We generated uniformly distributed prices from 0 to 650 Euros and normally distributed i.i.d. returns (mean 0, standard deviation 0.4) to create 10-day price series. These series

[48]Should these differences be substantial, the effectiveness of the tick size harmonization program could be questioned, as the tick sizes would not correspond to "optimal" levels. Additionally, robustness tests in Table 13 indicate that relative tick-size clustering does not significantly alter our results. These tests use a subset of synthetic controls near real price thresholds (within 10% or 20%). While this threshold selection criterion introduces certain biases, the consistent results offer supportive evidence for our claim.

were then classified as treated or control, based on whether they crossed a reduced set of predefined price thresholds, with 80,000 series simulated for each group. The top panel of Figure 9 displays the price distribution for the treated (red bars) and control (green bars) groups. Given their identical setup, both groups exhibit identical return distributions, as shown in the bottom left panel of Figure 9. Following this, we applied the tick sizes from the "< 10 ADNTE" liquidity bucket (column 1) of Table 1 to each price, rounding the prices to the nearest tick-size increment. The return distribution of these adjusted prices is depicted in the bottom right panel of Figure 9, where the treated group demonstrates notably higher return clustering, attributable to the clustering in relative tick sizes. This visual representation underscores the distortions caused by relative tick-size clustering in prices. Although this example represents an extreme case, with real data yielding a less pronounced distortion, the underlying effect remains. Consequently, this leads to a challenge: despite being generated from identical distributions, the summary statistics (mean, variance, percentiles, *etc.*) differ, and hypothesis tests conclude, with confidence, that these statistics are derived from different distributions (see summary statistics in the key in the bottom right panel of Fig. 9).

We provide a detailed summary of the return distributions for our treated and control groups in Table 11.[49] This table provides summary statistics, including mean, median, standard deviation (std), minimum and maximum values, and quartiles for both groups for tick-size increases (Panel A) and decreases (Panel B). Statistics for the treatment and control groups are displayed in the first and second columns of each panel, respectively. The third column contains p-values from hypothesis tests aimed at assessing the similarity of these statistics. We use the t-test for the mean, the Levene test for the standard deviation, and the Kolmogorov-Smirnov test for the distribution comparisons, with the null hypothesis of statistical equality being rejected for p-values below a certain significance threshold. The return distributions for both groups demonstrate skewness and drift, attributable to the criteria for sorting them into treatment and control groups (*i.e.*, the two-week period where prices cannot cross a threshold). Although Figure 4 suggests minor visual differences in the drifts between groups, our statistical tests do not affirm equivalence in return distributions.[50] Consequently, we cannot conclusively assert that the control group is representative of the treatment group prior to treatment and, hence, cannot statistically claim

[49] The corresponding figure depicting these distributions is available in Appendix A as Figure 8.

[50] It is important to note that the hypothesis tests employed are designed to detect differences rather than confirm equivalence. Establishing equivalence would necessitate an equivalence test with a predefined equivalence margin, which must consider the mechanical distortions. This approach, however, brings up the challenge of overfitting, as previously discussed.

that randomization was successful.

B.2 Predicting treatment status

Evaluating the randomness of an event commonly involves employing a randomness test, which compares the realized outcome to that expected given a truly random process. Such tests require a well-defined expectation regarding the random process or its outcomes. For example, in empirical randomization studies, where comparing observable variables between groups is standard, the implicit assumption is equality. In other words, successful randomization is expected to produce groups that are, on average, equal in observable characteristics. This constitutes an expectation of a random outcome. Other random processes may require different prior expectations. For example, one could investigate whether the event distribution over time, as illustrated in Figure 2, is random, thereby supporting the notion of random treatment in the time dimension. However, observable trends like clustering and market-wide fluctuations, as discussed in Section 3.2, do not invalidate randomness but rather necessitate a different prior expectation. Conventional tests, such as runs tests,[51] are not suitable in these contexts as they lead to the erroneous conclusion that the process is non-random due to miscalibrated prior expectations. The difficulty in accurately defining the prior expectation mirrors the issues discussed in the context of return distributions (see Sec. B.1). The lack of clarity in specifying prior expectations leads to challenges in distinguishing between a failed test resulting from non-randomness and one resulting from a misspecified prior expectation. Equivalently, challenges in distinguishing between a successful test resulting from randomness and one resulting from an over-fit prior expectation are also present.

In an alternative approach to assessing the randomness of treatment, we endeavor to use a highly non-linear machine learning method to predict treatment status; *i.e.*, to forecast whether a stock's price will cross a MiFID II threshold on a subsequent day. Specifically, for a given company i at time t, the goal is to predict if the price, following a 9-day sequence of prices, will close above or below a predefined price threshold. This task is formulated as:

$$p_{i,t} = f(p_{i,t-1}, ..., p_{i,t-9}) + \varepsilon,$$

where $p_{i,t}$ represents the price on day t for company i, and the function f represents the unknown dynamics of past prices $p_{i,k}$ for $k \in [t-1, ..., t-9]$. Although this type of prediction

[51]Runs tests, as described by (Bradley, 1960), evaluate the randomness of sequences of positive or negative outcomes (runs).

is typically suited for time-series machine learning methods, our approach employs a dense feedforward neural network.[52] Our method, while not a conventional time-series algorithm, resembles an attention-based approach (Vaswani et al., 2017). Despite its deviation from typical models, this approach effectively considers all input variables in its predictions. By approximating f with a neural network, we use its outputs to predict tick-size changes.

We employed several strategies to optimize the neural network's ability to predict price threshold crossings accurately. First, we over-parameterize the neural network with 12 hidden layers, totaling approximately 4,000,000 trainable parameters to enhance the network's predictive capacity. Second, we implement a multitask learning approach (Caruana, 1997), which improves the network's ability to learn patterns across multiple output variables by requiring the neural network to predict both a point estimate of the next period's closing price and the occurrence of a threshold crossing; *i.e.*, a price range. Specifically, the algorithm must predict future prices and a dummy variable representing whether the price will cross a threshold on the following day:

$$\mathcal{NN} \ : \ [p_{i,t-1}, ..., p_{i,t-9}]^\top \to [p_{i,t}, z_{i,t}]^\top,$$

where $z_{i,t}$ is 1 if $p_{i,t}$ closes on the opposite side of a threshold compared to $p_{i,t-1}$, and 0 otherwise. For positive prediction classification, we define a tick-size increase prediction if either the price $p_{i,t-1}$ is below a threshold $\bar{p}$ and the predicted price $\hat{p}_{i,t}$ equals or exceeds $\bar{p}$, or if $\hat{z}_{i,t} = 1$. Similarly, we define a tick-size decrease prediction if the price $p_{i,t-1}$ is at or above a threshold $\underline{p}$ and the predicted price $\hat{p}_{i,t}$ falls below $\underline{p}$, or if $\hat{z}_{i,t} = 1$. Third, we standardize each 9-day sequence of prices by scaling the data between 0 and 1. The bounds for scaling are set to $p_{i,t-1}$'s nearest MiFID II price thresholds. This standardization provides the algorithm with contextual information on how close prices are to a threshold and their relative movement toward it. Fourth, we oversample treated price sequences in our training loop to increase their representation in the dataset.

We created the dataset by segmenting the equities dataset (from Sec. 3.2) into 10-day price sequences. The initial nine observations in each sequence serve as the "features" (*i.e.*, independent variables), while the tenth-day price is used as the "target" variable (*i.e.*, dependent variable). The secondary target variable, $z_{t,i}$, is assigned a value of 1 if $p_{i,t-1}$ and $p_{i,t}$ are on either side of a MiFID II price threshold, and 0 otherwise. Our dataset consists

[52] As the details of this exercise are not a core component of this study, we refrain from providing a comprehensive introduction to neural networks, which can be found in, for example, Goodfellow et al. (2016). Importantly, neural networks are universal approximators (Hornik et al., 1989) that are a popular choice of method for a wide range of applications due to their flexibility and ability to handle large datasets.

of 2,155,169 price series, with 45,879 instances of MiFID II price-thresholds crosses—22,520 tick-size increases and 23,359 decreases. We trained our 12-layer neural network[53] for up to 10 epochs, using a batch size of 128 and dividing the data into an 80:20 ratio for training and test sets. Subsequently, we evaluated the performance of networks that outperformed the benchmark, which always predicts no threshold crossing ($\hat{p}_{i,t} = p_{i,t-1}$), calculating the out-of-sample percentage of accurately classified observations. This experiment was repeated 100 times for both tick-size increases and decreases, where training and test sets were resampled in each iteration.

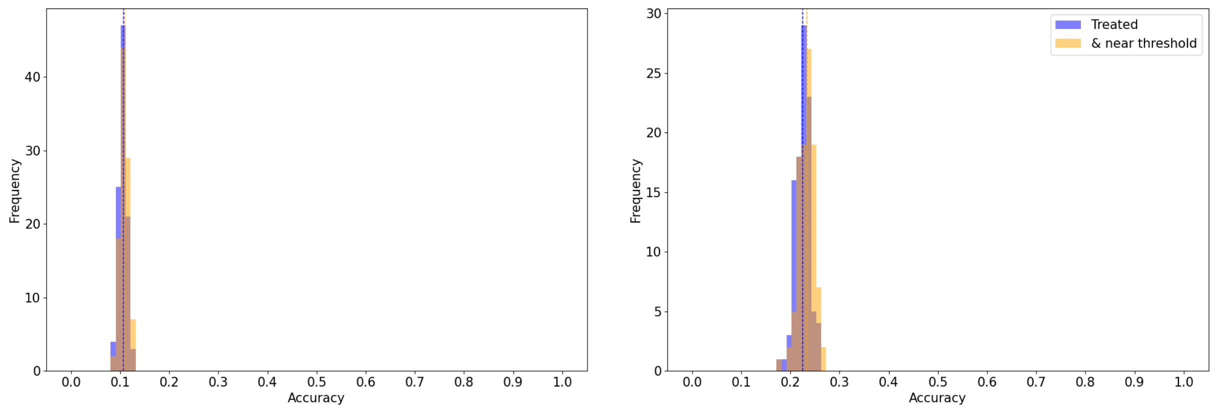
We present the results in Figure 10, where Panel A displays the prediction performance for tick-size increases and Panel B for decreases. Specifically, we present the results for price series in our treated sample (refer to Sec. 3.2.1) and those within 20% of a MiFID II threshold, as the latter is presumably easier to predict. The analysis shows that, across 100 experiments, the neural network successfully predicts approximately 10% of the tick-size increases and 20% of the decreases. We expect the drift, created by the tick-size change-free window pre-event, to signal the possibility of a threshold crossing, especially for stocks near a threshold. Nevertheless, the neural network generally fails to predict the day of the price-threshold crossing precisely and, hence, cannot consistently forecast tick-size changes, providing validity to the claim that treatment is random. We acknowledge that this analysis does not provide particularly strong evidence of the claim as it is likely that better performance could be achieved with more expertise and dedication. Still, these results reemphasize the known difficulty associated with predicting prices at a specific time in the future.[54]

[53] The hidden layers consist of 1024, 1024, 1024, 1024, 512, 256, 128, 64, 32, 16, 8, and 4 ReLU-activated nodes, respectively.

[54] This discussion is related to the broader discourse on price prediction and asset pricing (see, *e.g.*, Welch and Goyal, 2008), which we do not intend to contribute to. Furthermore, this analysis raises the critique that price series sufficiently far from thresholds are more easily predicted to be control instances. For these stocks, random treatment allocation would, therefore, be more difficult to justify. The robustness of our results under this consideration is demonstrated in Table 13 in Appendix C.1, where we limit controls to those within a maximum distance from MiFID II thresholds.

(A) Tick-size increase

(B) Tick-size decrease



Notes: This figure illustrates the accuracy distribution of neural networks in predicting treatment. The blue histogram represents the prediction performance for the treated group, with the blue dashed line indicating the average. The orange histogram shows the prediction accuracy for treated instances within 20% of a MiFID II price threshold, marked by the orange dashed line for the mean. Panels A and B present the results for tick-size increases and decreases, respectively.

Figure 10: Treatment prediction performance of the treated group

C Robustness and sensitivity analysis

C.1 Choice of threshold

Table 12: The impact of the choice of control thresholds

(A) Tick-size increase	β^{Cross}	β^{Week1}	β^{Week2}	# Treated	# Control	# Obs.
Every price increment	2.597***	$-0.795^{\times}$	$-0.809^{\times}$	5,765	228,754	4,690,380
	(0.000)	(0.135)	(0.128)			
Main result thresholds	2.530***	-0.128***	-0.127***	5,765	45,382	1,022,940
	(0.000)	(0.000)	(0.000)			
Behavioral thresholds	1.336***	-0.080**	-0.076**	5’765	25,403	311’680
	(0.000)	(0.023)	(0.020)			
(B) Tick-size decrease						
Every price increment	-1.872***	$0.950^{\times}$	1.028*	7,161	247,943	5,102,080
	(0.002)	(0.108)	(0.082)			
Main result thresholds	-1.884***	0.320***	0.346***	7,161	63,985	1,422,920
	(0.000)	(0.000)	(0.000)			
Behavioral thresholds	-0.551***	0.251***	0.306***	7’161	35,291	424’520
	(0.000)	(0.000)	(0.000)			

NOTES: This table presents estimates for the β coefficients that correspond to Equation (1), with the stock return as the dependent variable, for various choices of control groups. Panels A and B present the results for tick-size increases and decreases, respectively. “Every price increment” uses any price increment as a potential synthetic threshold; “Main results thresholds” replicates the main results from Table 4, which use integer values (and 1, 5, and 10-cent increments for lower priced stocks) as synthetic thresholds; “Behavioral thresholds” replicates the results from Table 5, which uses round integer values as synthetic thresholds. p-values, based on stock and time-clustered standard errors, are given in parentheses. ***, **, *, and $^{\times}$ denote statistical significance at the 1%, 5%, 10%, and 15% level, respectively. The results demonstrate consistent effect direction between the control thresholds and a monotonic trend in magnitude from the most relaxed choice of thresholds to the most reduced.

Table 13: Preserving relative tick sizes in treated and control groups

(A) Tick-size increase	β^{Cross}	β^{Week1}	β^{Week2}	# Treated	# Control	# Obs.
thresholds within ±10%	2.901***	-0.190***	-0.174***	5,765	9,236	300,020
	(0.000)	(0.000)	(0.000)			
thresholds within ±20%	2.520***	-0.163***	-0.152***	5,765	23,470	584,700
	(0.000)	(0.000)	(0.000)			
(B) Tick-size decrease						
thresholds within ±10%	-2.206***	0.407***	0.436***	7,161	12,176	386,740
	(0.000)	(0.000)	(0.000)			
thresholds within ±20%	-1.944***	0.354***	0.385***	7,161	32,495	793,120
	(0.000)	(0.000)	(0.000)			

Notes: This table presents estimates for the β coefficients that correspond to Equation (1), with the stock return as the dependent variable, using control instances where relative tick sizes closely align with the treated group (within a range of 0.9 to 1.1, or ±10%, and 0.8 to 1.2, or ±20%, of MiFID II price thresholds). Panels A and B present the results for tick-size increases and decreases, respectively. p-values, based on stock and time-clustered standard errors, are given in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. These results highlight two findings: first, the clustering of relative tick sizes does not influence the main results (see App. B.1); second, stocks with prices far from MiFID II thresholds are more easily identified as controls; thus, if differences between controls and treated were to be expected, these stocks would likely differ more. However, our focus on stocks near thresholds confirms that such biases do not drive our main results (see App. B.2).

C.2 Price-stratified effect of tick size changes

Table 14: The price-stratified impact of tick-size changes on stock prices

Tick size increase	$\beta_1^{crossing}$	$\beta_2^{1st\ week}$	$\beta_3^{2nd\ week}$	# Treated	# Obs.
Third highest priced stocks	1.943***	-0.007	0.046	1’190	358’000
	(0.000)	(0.914)	(0.472)		
Middle third priced stocks	2.019***	-0.121***	-0.115**	1’287	358’040
	(0.000)	(0.002)	(0.021)		
Third lowest priced stocks	1.076***	-0.119***	-0.134***	3’288	306’900
	(0.000)	(0.007)	(0.001)		
Tick size decrease					
Third highest priced stocks	-2.413***	0.207**	0.233***	1’288	498’020
	(0.000)	(0.010)	(0.004)		
Middle third priced stocks	-1.218***	0.225***	0.320***	1’475	498’020
	(0.000)	(0.000)	(0.000)		
Third lowest priced stocks	-0.772***	0.306***	0.346***	4’398	426’880
	(0.000)	(0.000)	(0.000)		

NOTES: This table presents estimates for the β coefficients that correspond to Equation (1), with the stock return as the dependent variable, for price-level subgroups. Panels A and B present the results for tick-size increases and decreases, respectively. The rows, from top to bottom, record the results for the highest, middle, and lowest third of prices in our dataset. p-values, based on stock and time-clustered standard errors, are given in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. The results show no significant differences in the effects on low-priced assets, thus offering limited insights for contrasting our findings with those of Albuquerque et al. (2020).

C.3 Outlier analysis

C.3.1 The effect of days with strong market-wide fluctuations

Table 15: Excluding days with strong market-wide fluctuations

			(I) 2σ					(II) 3σ		
(A) Tick-size increase	β^{Cross}	β^{Week1}	β^{Week2}	# Treated	# Obs.	β^{Cross}	β^{Week1}	β^{Week2}	# Treated	# Obs.
Main result controls	2.551***	-0.120***	-0.105***	5,193	943,420	2.530***	-0.116***	-0.114***	5,559	995,920
	(0.000)	(0.000)	(0.000)			(0.000)	(0.000)	(0.000)		
Behavioral controls	1.349***	-0.070**	-0.059**	5,193	261,340	1.342***	-0.067**	-0.064**	5,559	278,320
	(0.000)	(0.014)	(0.034)			(0.000)	(0.027)	(0.027)		
(B) Tick-size decrease										
Main result controls	-1.621***	0.268***	0.181***	5,835	1,249,400	-1.630***	0.281***	0.199***	6,108	1,291,760
	(0.000)	(0.000)	(0.000)			(0.000)	(0.000)	(0.000)		
Behavioral controls	-0.362***	0.192***	0.167***	5,835	357,220	-0.369***	0.203***	0.187***	6,108	371,580
	(0.000)	(0.000)	(0.000)			(0.000)	(0.000)	(0.000)		

NOTES: This table presents estimates for the β coefficients that correspond to Equation (1), with the stock return as the dependent variable, for data where days with strong market-wide fluctuations were excluded. Using the full dataset, the total number of tick-size changes per day was counted for tick-size increases and decreases. The standard deviation σ of the number of events per day across the timeframe was used to clip days with too many events. Specifically, we dropped every day with more than 2σ or 3σ events per day. Panels A and B present the results for tick-size increases and decreases, respectively. p-values, based on stock and time-clustered standard errors, are given in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. These results indicate that tick-size increase effects are not influenced by days with significant market-wide fluctuations. In contrast, tick-size decrease effects initially show sensitivity to such exclusions but stabilize once the most extreme outliers are removed, showing negligible differences between the 2σ and 3σ results. The new results suggest that extreme market conditions largely drive excess returns on crossing days, and post-change effects align more with tick-size increases when such days are excluded.

C.3.2 The effect of high-return days

Table 16: Excluding instances with high returns days

(A) Tick-size increase	β^{Cross}	β^{Week1}	β^{Week2}	# Treated	# Control	# Obs.
< 25% returns	2.530***	-0.128***	-0.127***	5,765	45,382	1,022,940
	(0.000)	(0.000)	(0.000)			
< 20% returns	2.066***	-0.118***	-0.124***	5,573	45,149	1,014,440
	(0.000)	(0.000)	(0.000)			
< 15% returns	1.607***	-0.105***	-0.121***	5,289	44,569	997,160
	(0.000)	(0.000)	(0.000)			
< 10% returns	1.042***	-0.071***	-0.099***	4,723	43,144	957,340
	(0.000)	(0.003)	(0.001)			
(B) Tick-size decrease						
< 25% returns	-1.884***	0.320***	0.346***	7,161	63,985	1,422,920
	(0.000)	(0.000)	(0.000)			
< 20% returns	-1.649***	0.307***	0.339***	7,046	63,850	1,417,920
	(0.000)	(0.000)	(0.000)			
< 15% returns	-1.277***	0.308***	0.307***	6,771	63,341	1,4022,40
	(0.000)	(0.000)	(0.000)			
< 10% returns	-0.834***	0.258***	0.252***	6,220	61,901	1,362,420
	(0.000)	(0.000)	(0.000)			

NOTES: This table presents estimates for the β coefficients that correspond to Equation (1), with the stock return as the dependent variable, for data where high return days were excluded. That is, from top to bottom, the rows present the results for data where events with returns higher than 25%, 20%, 15%, and 10% were excluded, respectively. Panels A and B present the results for tick-size increases and decreases, respectively. p-values, based on stock and time-clustered standard errors, are given in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. These results assess how our data-cleaning criteria impact our main findings, particularly our exclusion of days with returns exceeding 25%. While tightening this threshold mechanically reduces the effects, the overall economic narrative remains consistent.

C.3.3 Enforcing similarity in treatment and control by truncating data

Table 17: Enforcing similarity between treated and control groups

(A) Tick-size increase	β^{Cross}	β^{Week1}	β^{Week2}	# Treated	# Control	# Obs.
< 90^{th} percentile	2.385***	-0.122***	-0.101***	4,786	31,948	734,680
	(0.000)	(0.000)	(0.002)			
< 85^{th} percentile	2.179***	-0.100***	-0.096***	4,383	26,915	625,960
	(0.000)	(0.005)	(0.009)			
< 80^{th} percentile	2.061***	-0.095***	-0.101***	3,975	22,505	529,600
	(0.000)	(0.009)	(0.010)			
< 75^{th} percentile	1.966***	-0.091**	-0.099**	3,560	19,080	452,800
	(0.000)	(0.013)	(0.014)			
(B) Tick-size decrease						
< 90^{th} percentile	-1.799***	0.318***	0.329***	5,956	49,292	1,104,960
	(0.000)	(0.000)	(0.000)			
< 85^{th} percentile	-1.772***	0.323***	0.322***	5,444	43,542	979,720
	(0.000)	(0.000)	(0.000)			
< 80^{th} percentile	-1.718***	0.334***	0.329***	4,954	37,809	855,260
	(0.000)	(0.000)	(0.000)			
< 75^{th} percentile	-1.680***	0.345***	0.331***	4,429	32,487	738,320
	(0.000)	(0.000)	(0.000)			

NOTES: This table presents estimates for the β coefficients that correspond to Equation (1), with the stock return as the dependent variable, using treated and control sets whose similarity are enforced. That is, we truncate outliers in all observable variables. From top to bottom, the rows present the results using data where the 10^{th}, 15^{th}, 20^{th}, and 25^{th}-percentiles were truncated, respectively. Panels A and B present the results for tick-size increases and decreases, respectively. p-values, based on stock and time-clustered standard errors, are given in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. These results demonstrate that outliers in observable characteristics do not drive the results.