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How did South Korea's Short-Selling Bans implemented between 2020 and 2025
impact the South Korean equity market?

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Abstract

This thesis investigates the impact of South Korea's three short-selling bans implemented between March 2020 and March 2025 on market liquidity, volatility and stock performance. Short-selling restrictions are frequently used as crisis-management tools, yet their effects on market quality remain debated. Using daily stock-level data and a difference-in-differences framework with firm and time fixed effects, the study compares treated South Korean firms to matched Japanese peers for the banning period 1 and 3 and to non-treated South Korean stocks for the second banning period.

The results show that liquidity and volatility improved during the first and third bans, although non-parallel pre-trends caution against causal interpretation. For Ban 2, where identification is clean, the partial lifting of the ban for the treatment group leads to reduced illiquidity and consistently lower volatility across all measures. Excess returns remain unaffected in all periods. The findings highlight that the market impact of short-selling regulation depends strongly on market conditions and the scope of the ban.

Overall, the thesis contributes new evidence on how short-selling regulation affected market quality during South Korea's most recent short-selling bans between 2020 and 2025 and more broadly enhances the understanding of regulatory impacts outside traditional crisis periods.

Zusammenfassung

Diese Masterarbeit untersucht die Auswirkungen der drei Short-Selling-Verbote, die Südkorea zwischen März 2020 und März 2025 eingeführt hat, auf Marktliquidität, Volatilität und Aktienperformance. Short-Selling-Restriktionen werden häufig als Kriseninstrumente eingesetzt, dennoch bleibt ihre Wirkung auf die Marktqualität umstritten. Auf Grundlage täglicher Aktien-Einzeldaten und eines Difference-in-Differences-Ansatzes mit Unternehmens- und Zeit-Fixed-Effects vergleicht die Studie behandelte südkoreanische Unternehmen mit gematchten japanischen Peers für die Banperiode 1 und 3 sowie mit nicht behandelten südkoreanischen Aktien für die Banperiode 2.

Die Ergebnisse zeigen, dass sich Liquidität und Volatilität während des ersten und dritten Verbots verbessert haben, wobei jedoch nicht-parallele Pre-Trends eine kausale Interpretation einschränken. Für Ban 2, bei dem die Identifikation robust ist, führt die teilweise Aufhebung des Verbots der Treatment-Gruppe zu geringerer Illiquidität und durchweg geringerer Volatilität über alle Messgrößen hinweg. Excess Returns bleiben in allen Perioden unbeeinflusst. Die Befunde verdeutlichen, dass die Marktwirkung von Short-Selling-Regulierungen stark von den Marktbedingungen und dem Umfang des Verbots abhängt.

Insgesamt liefert diese Arbeit neue Evidenz darüber, wie die Short-Selling-Regulierung die Marktqualität während der jüngsten südkoreanischen Short-Selling-Verbote zwischen 2020 und 2025 beeinflusst hat, und erweitert darüber hinaus das Verständnis regulatorischer Effekte außerhalb klassischer Krisenzeiten.

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List of Abbreviations

KFX - Korean Financial Index

ESMA - European Securities and Markets Authority

COVID-19 - Coronavirus Disease 2019

FSC - Financial Services Commission

MSCI - Morgan Stanley Capital International

FTSE - Financial Times Stock Exchange

KOSPI - Korea Composite Stock Price Index

KOSDAQ - Korea Securities Dealers Automated Quotations

NSDS - Naked Short Selling Detection System

FSS - Financial Supervisory Service

TARP - Troubled Asset Relief Program

Spread - Bid-Ask Spread

Amihud - Amihud Illiquidity Index

RVOL - Relative Volatility

RollV - Rolling Volatility

ParkV - Parkinson Volatility

ExcessR - Excess Return

AR(1) - Autoregressive Process of order one

TWFE - Two Way Fixed Effects

CE - Clustered Errors

1. Introduction

Short-selling bans are a recurring and controversial policy tool in modern financial markets. During periods of heightened market stress, policymakers often justify such bans as a mean to stabilize financial markets, reduce excessive downward pressure on stock prices and restore investor confidence (Spolaore & Le Moign, 2024, p. 3). At the same time, their overall effectiveness remains contested, as several studies question whether such interventions genuinely improve market functioning or instead introduce unintended frictions into the price formation process (World Federation of Exchanges, 2020; Beber & Pagano, 2013, p. 344). The debate over their effectiveness has persisted for decades and remains particularly relevant in light of the renewed regulatory actions taken in various markets after the COVID-19 crisis (Spolaore & Le Moign, 2024, p. 31).

Since the Global Financial Crisis of 2008, South Korea has repeatedly imposed and lifted short-selling bans in response to external shocks and political pressure. Most recently, three major regulatory interventions occurred between 2020 and 2025: the first in March 2020 as an immediate response to the COVID-19-induced market collapse, the second in May 2021 when the government partially lifted the ban for large-cap firms and the third in November 2023 when the Financial Services Commission reinstated a full ban due to widespread evidence of illegal short-selling practices (FSC, 2023). These repeated interventions make the Korean market a unique case to study the economic and microstructural effects of short-selling prohibitions. Understanding how these bans affected liquidity, volatility and returns provides valuable insights into whether such policies genuinely influence market quality.

The academic literature on short-selling bans is extensive but was historically focused on crisis-related interventions for example during the Global Financial Crisis of 2008. Numerous influential studies, including Beber and Pagano (2013), Boehmer et al. (2014) and Saffi & Sigurdsson (2010), document that broad-based bans typically reduce market liquidity and weaken price efficiency. More recently, however, a growing body of research has begun to examine the short-selling bans introduced during the COVID-19 market turmoil in 2020, thereby expanding the empirical foundation. One prominent example is Spolaore and Le Moign (2024), who analyse the cross-country effects of the 2020 short-selling bans across multiple European markets. Their findings indicate that volatility decreased for the affected stocks during the ban periods, suggesting that under specific market conditions, restrictions may have

had a stabilising effect. In contrast, Helmes et al. (2016) investigate the Australian ban during the Global Financial Crisis and, using an unusually long observation window of eight months before and after the intervention, document that certain volatility measures increased following the ban.

Despite this large body of work, several important research gaps remain. First, existing studies focus primarily on bans implemented during severe market distress. Much less is known about how short-selling restrictions affect markets under relatively stable conditions. Second, limited attention has been given to repeated short-selling bans within the same market, particularly to cases where different types of restrictions, such as full and partial bans, are imposed in close succession, as observed in South Korea.

This study aims to fill these gaps by examining the three major short-selling bans implemented in South Korea between 2020 and 2025. Unlike previous episodes that were primarily crisis-driven, the recent Korean bans offer a distinct setting to analyze the effects of short-selling restrictions under varying market and regulatory conditions, ranging from pandemic-related turmoil to compliance-oriented interventions.

Building on the theoretical and empirical literature, the main research question of this thesis is:

“How did the Short-Selling Bans in South Korea affect South Korean equity markets?”

To empirically evaluate these hypotheses, this study employs a difference-in-differences framework with firm and day fixed effects. This approach isolates the average treatment effect of the bans by comparing the performance of treated firms (subject to the ban) with those of control firms (not subject to the ban) before and after the intervention. For the first and third bans, Japanese peer firms matched by sector and market capitalization serve as the control group. For the second ban, when short selling was selectively reopened for large-cap firms, the still-banned Korean firms function as an internal control group. The empirical analysis relies on a comprehensive panel of daily stock-level data, incorporating various liquidity, volatility and return measures as dependent variables. Standard errors are clustered at the firm level and Driscoll–Kraay corrections are applied to account for potential autocorrelation and cross-sectional dependence.

The remainder of this thesis is structured to provide a coherent and logical progression from theoretical foundations to empirical analysis and interpretation. Chapter 2 introduces the

theoretical background, beginning with the fundamental concepts of short selling and its role in financial markets. It discusses the underlying motivations for short selling and its contribution to market efficiency. Furthermore, this chapter explains the mechanisms through which short-selling bans may influence key market parameters such as liquidity, volatility and returns. The final part of the chapter focuses on the historical context and specific regulatory developments surrounding the short-selling bans implemented in South Korea. Chapter 3 describes the methodological framework of the study. It begins with an overview of the data sources and outlines the process of constructing the final sample. The chapter then defines the independent variables used in the analysis. Finally, it details the empirical approach, introducing the difference-in-differences model that forms the core of the quantitative analysis. Chapter 4 presents the empirical results of the study. It first assesses the validity of the identification strategy by examining pre-treatment trends between the treatment and control groups. The pre ban parallel trends are analyzed through event studies, placebo tests and graphical evidence. The chapter then reports the main regression results for each of the three short-selling bans, analyzing their respective impacts on various liquidity and volatility proxies and stock returns. In addition, the analysis includes an interaction framework to examine whether the policy produced heterogeneous effects across firms with different market capitalizations, thereby allowing a more nuanced assessment of how the bans affected small-, mid-, and large-cap companies. Finally, Chapter 5 concludes the thesis by summarizing the key empirical insights and interpreting them in light of the theoretical expectations and prior literature. It also discusses the methodological and contextual limitations of the study and outlines avenues for future research.

2. Theoretical Background

This chapter provides the conceptual foundation for the analysis conducted in this study. It first introduces the concept of short selling, outlining its basic mechanism, the motivation for short selling and its role within financial markets. It then examines short selling bans, discussing their different forms, regulatory objectives, potential effects and market mechanisms, with a particular focus on the historical development of such bans in South Korea. Finally, the chapter presents the key market parameters that are used to assess the impact of short selling restrictions in the empirical analysis.

2.1 Short Selling – Definition and Mechanics

Short selling is a trading strategy that enables investors to profit from expected declines in the price of a security. The practice involves selling a security that the seller does not currently own, with the obligation to repurchase and return it to the lender at a later date. In doing so, the short seller anticipates that the repurchase price will be lower than the initial selling price, generating a profit from the price differential. Conversely, if the security's price increases after the short sale, the transaction results in a loss, as the repurchase price exceeds the sale price (Duffie, Garleanu, & Pedersen, 2002, pp. 2-3).

In most cases, short selling is facilitated through the securities lending market, where institutional investors such as insurance companies, index funds or pension funds lend securities to intermediaries, typically broker-dealers, who in turn make them available to short sellers (Duffie, Garleanu & Pedersen, 2002, p. 5). This process allows the seller to deliver the borrowed security to the buyer at the time of the short sale, ensuring the trade settles in accordance with market conventions. The short seller later closes the position, or “covers” the short, by purchasing the same security in the market and returning it to the lender. The profitability of the trade depends on the difference between the initial sale price and the eventual repurchase price, net of any borrowing fees or interest charged for the loaned securities.

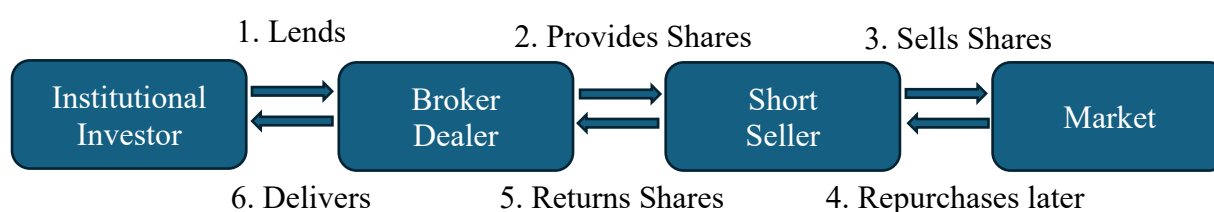


Figure 1: Short selling process

Short sales can generally be divided into covered and naked transactions, which differ in the timing and certainty of borrowing the underlying security.

In a covered short sale, the short seller has already arranged to borrow the security, or at least has secured a firm commitment from a lender, before executing the sale. This arrangement ensures that the seller can deliver the borrowed shares to the buyer at settlement, in line with market conventions. Covered short selling is the standard and widely accepted practice in modern financial markets, as it reduces the risk of settlement failures and promotes orderly market functioning (European Commission, 2012).

By contrast, a naked short sale occurs when a trader sells shares without first ensuring that the security can be borrowed and delivered. In this case, the seller has not “located” or “circled” the shares prior to execution. Naked shorting introduces the risk of settlement failure, which can undermine market integrity and distort supply-demand dynamics in the short term. For this reason, naked short selling is heavily restricted or outright prohibited in some jurisdictions (European Commission, 2012). Regulators, including the U.S. Securities and Exchange Commission, have implemented „locate“ and „close-out“ rules to prevent excessive failures to deliver and to ensure that short selling does not artificially create downward pressure on stock prices. The locate rule requires brokers to confirm that a security can reasonably be borrowed before accepting a short sale order, while the close-out rule obliges market participants to purchase and deliver securities in the event of a settlement failure within a specified timeframe. (U.S. Security and Exchange Commission, 2022)

From a market perspective, short selling serves functions beyond speculative profit. It enables the expression of negative information or opinions about a security, thereby contributing to price discovery and potentially improving market efficiency (Bris, Goetzmann & Zhu, p. 324). At the same time, the practice carries inherent risks. Because the potential loss from a short position is theoretically unlimited, given that a security’s price can rise without bound, short sellers face significant exposure if market prices move against them. Moreover, during periods of market stress, short selling may be subject to heightened scrutiny and regulatory restrictions due to concerns about its potential to exacerbate downward price pressure (Jones, Fabozzi & Larsen, p. 2) .

There are several well-documented motivations that drive investors to engage in short selling. These motivations vary across market participants, ranging from speculative trading strategies

to information-driven positions and arbitrage trading considerations. (Wan Kot, 2007, pp. 124-126)

First, trend-following strategies represent one of the most common motivations. Investors who rely on momentum trading tend to buy stocks that have recently exhibited upward price movements and short those that have recently declined. Implying that short sellers use trend signals in their trading decisions, entering short positions in response to negative short-term returns and closing them once upward trends reemerge (Wan Kot, 2007, p. 124).

Second, short selling may be motivated by perceptions of overpricing. Investors who believe that a stock's market valuation temporarily exceeds its fundamental value can use short sales to benefit from the expected price correction. Dechow et al. (2001, pp. 78–79) provide extensive evidence that short sellers systematically target firms whose fundamentals, such as earnings or book values, are low relative to market prices. These fundamental-to-price ratios, long established in the asset-pricing literature as predictors of future returns, serve as a central input to short sellers' screening process. The authors show that short sellers open positions when valuation ratios signal overpricing and subsequently cover them as these ratios revert toward their historical norms. Moreover, Dechow et al. (2001, p. 78) document that short sellers refine these strategies by avoiding stocks with high borrowing or transaction costs, incorporating additional information beyond fundamental ratios and distinguishing between low ratios driven by temporarily depressed fundamentals and those stemming from excessively high market prices.

Third, arbitrage opportunities can also motivate short selling, particularly in situations where prices of economically linked securities become temporarily misaligned. Merger arbitrage provides one of the clearest examples of this mechanism. When a stock-financed merger is announced, arbitrageurs typically short the acquirer's shares and go long the target to exploit the mechanically induced price relation between the two securities. As Mitchell et al. (2004, p. 31) document, this trading activity can generate substantial short-term price pressure: the sharp increase in short selling around merger announcements expands the effective supply of the acquirer's shares, pushing prices downward even in the absence of new information. Arbitrage-driven short selling is not limited to merger contexts. As Wan Kot (2007, p. 125) emphasizes, relative-value strategies more broadly rely on identifying mispricing between closely related instruments, whether due to corporate events, index rebalancing, or deviations from theoretical pricing relationships. In such settings, short sales are an integral component of maintaining

price coherence across connected markets. By enabling traders to exploit small inconsistencies between securities, arbitrage-motivated short selling contributes to market efficiency and ensures that prices of related assets move in line with one another.

2.2 Short Selling Bans

Short selling bans can take the form of either full or partial restrictions, depending on their scope. A full ban prohibits short sales across the entire market, whereas a partial ban restricts short selling in only certain sectors or specific securities.

The Global Financial Crisis of 2008 provides a notable example. As a consequence of rapidly deteriorating market conditions, a number of countries all around the world implemented broad restrictions on short selling. While some jurisdictions opted for comprehensive market-wide bans, many regulators chose to impose sector-specific restrictions that primarily targeted financial stocks (Beber and Pagano, 2013, p. 352).

Banks and other financial firms are systemically important and sharp declines in their stock prices can quickly undermine confidence in the stability of the financial system as a whole. Regulators feared that speculative short selling could exacerbate downward spirals in bank valuations, thereby threatening not only the solvency of individual firms but also the functioning of credit markets more broadly. By restricting short sales in financial stocks, policymakers aimed to stabilize the sector that was most vulnerable to confidence shocks and contagion, while still allowing other parts of the market to operate with fewer restrictions (Beber and Pagano, 2013, pp. 343-344).

A different form of partial ban was implemented in South Korea, where regulators imposed a full short-selling ban on March 13, 2020, in response to the COVID-19 crisis. This ban remained in place until May 3, 2021, when it was partially lifted. From that point, short selling was allowed only for companies included in the KOSPI200 and KOSDAQ150 indices, effectively restricting the practice to larger, more liquid firms while maintaining protection for smaller or less transparent stocks. This partial ban remained in force until November 6, 2023, when regulators once again imposed a full market-wide ban, citing concerns over illegal practices and retail investor protection (FSC, 2025). The full ban was finally lifted on March 31, 2025, restoring short selling across all listed securities. A more detailed discussion of these bans will be provided in later, as the empirical research of this thesis is centered on examining their effects.

In addition to distinguishing between full and partial bans, regulators also differentiate between temporary and permanent restrictions on short selling. Temporary bans are by far the most common, typically introduced during periods of acute market stress, such as the Global Financial Crisis of 2008 or the COVID-19 shock in 2020. The rationale is to provide markets with a cooling-off period when volatility spikes, thereby reducing the risk of self-reinforcing price declines. These bans are usually lifted once conditions stabilize (Beber and Pagano, 2013, p. 343). By contrast, permanent restrictions on covered short selling are exceedingly rare in developed markets. Like already outlined earlier, policymakers generally recognize that, despite its risks, short selling contributes to price efficiency, liquidity and risk management.

What is, however, permanently prohibited in some jurisdictions is naked short selling, reflecting regulators' concerns over settlement risk and artificial market pressure. In the United States, for instance, such restrictions are embedded in Regulation SHO, while in Europe they were codified in the 2012 EU Short Selling Regulation, as outlined in Chapter 2.1. While generally restricted, the SEC notes that naked short sales are permitted only under specific circumstances. (U.S. Securities and Exchange Commission, 2022; European Commission, 2012).

Thus, while temporary bans on covered short sales are used as short-term crisis-management tools, permanent bans are typically reserved for practices deemed structurally harmful, most notably naked short selling.

A number of regulatory mechanisms have been developed to limit potentially destabilizing short-selling activity. The traditional uptick rule, introduced in the United States in 1938, permitted short sales only at a price above the last executed trade. The goal was to prevent short sellers from reinforcing downward price spirals by repeatedly hitting lower bids. This rule did not eliminate short selling, it rather restricted the conditions under which short-sale orders could be executed, primarily by influencing order sequencing and execution priority. The alternative uptick rule (Rule 201), implemented in 2010, applies only when a stock experiences an intraday price decline of more than 10%. Once triggered, short-sale orders may only execute at a price above the current national best bid. This mechanism operates like a temporary circuit breaker that activates automatically in periods of unusually sharp price drops. It constrains aggressive short selling when markets are under stress but allows the practice to continue under controlled conditions (SEC, 2010).

2.3 Short Selling Bans in South Korea

The regulatory approach toward short selling in South Korea has involved repeated interventions, which have occurred more frequently and for longer durations than in many other markets. One characteristic of the Korean equity market is the relatively high participation of retail investors, who account for a large share of trading activity compared to other markets (The Korea Times, 2023). In addition, South Korea has for several years sought reclassification as a developed market by major index providers such as MSCI and FTSE. In this context, the use of short-selling bans has been noted as a factor in assessments by international observers, including credit rating agencies (Ministry of Economy and Finance, 2025).

Several major bans have been implemented since the late 1990s. During the Asian Financial Crisis of 1997, short selling was suspended to limit further downward pressure in a period of severe financial instability. In 2008, a market-wide ban was imposed during the Global Financial Crisis, in line with measures taken in other jurisdictions, though its duration in South Korea was comparatively long. In 2011, the European sovereign debt crisis prompted another suspension, this time focusing primarily on financial sector stocks.

The most recent and longest-lasting restrictions began in March 2020 at the start of the COVID-19 pandemic. Initially introduced as a temporary measure, the ban remained in place until March 2025, making it the most extensive prohibition in the modern history of the Korean market. While the first two periods are linked to market uncertainty during the pandemic and its aftermath, the last period, starting from November 2023, was driven by a program of regulatory reform aimed at strengthening oversight of short-selling practices and improving market infrastructure.

On March 13, 2020, in response to the outbreak of COVID-19 and the sharp increase in market uncertainty, the FSC announced a comprehensive ban on all short-selling activities across the Korea Exchange, effective March 16, 2020. Initially set to expire on September 16, 2020, the ban was later extended to March 15, 2021, citing continued instability. A further extension followed in February 2021, postponing the reopening to May 2, 2021. At that point, regulators indicated that only a partial ban would be lifted, whereby short selling would again be permitted exclusively for firms included in the KOSPI200 and KOSDAQ150 indices.

From May 3, 2021 onward, the ban applied to all companies not listed in the KOSPI200 or KOSDAQ150. Thus, while all these equities could again be shorted, the majority of smaller

and mid-cap firms remained under restriction. Over this period, regulators began taking enforcement actions against violations. For example, in March 2023 the Securities and Futures Commission, a sub-commission of the FSC, imposed the first administrative fines on two companies found to have engaged in unlawful short-selling practices (FSC, 2023)

The partial reopening created a complex environment in which significant amounts of naked short selling were detected, making it increasingly difficult to clearly distinguish between legal and illegal activity. This reinforced concerns among policymakers and the public that short selling was being misused, particularly by large institutional players and set the stage for renewed calls for a comprehensive market-wide ban. The situation culminated in a full reinstatement of the ban on November 6, 2023 (Chessum, 2025). At the time, South Korea lacked a real-time monitoring infrastructure to effectively track short-selling positions, while many institutions operated without robust internal controls. The structure of the South Korean equity market further amplifies potential vulnerabilities. Retail investors account for a disproportionately large share of trading activity, consistently exceeding 60% according to official figures, making the market far more retail-driven than its Western counterparts (The Korea Times, 2023). Compared to institutional investors, these retail participants typically face informational disadvantages, weaker technological infrastructures and limited risk-management capacity, which can leave them more exposed during periods of heightened market stress.

During this third ban period, regulators used the suspension not as a crisis response but as an opportunity to restructure the short-selling framework. The centerpiece of this reform was the introduction of the Naked Short-Selling Detection System (NSDS), which obliges corporations engaged in short selling, along with broker-dealers, to implement electronic compliance and monitoring systems capable of blocking naked short sales (Chessum, 2025). Corporations with net short positions above defined thresholds must now report their positions daily to the Korea Exchange and maintain detailed compliance records. Broker-dealers are further required to verify clients' systems and submit inspection results to the Financial Supervisory Service (FSS) (Korea.net - Financial Services Commission, 2025).

In parallel, the reform package also sought to level the playing field between institutional and retail investors, who had long argued that the existing short-selling framework placed them at a structural disadvantage. Effective March 31, 2025, the maximum repayment period for stock-lending agreements was standardized at 90 days (extendable up to 12 months) reducing

uncertainty around rollover conditions for all market participants. At the same time, the collateral ratio required from retail investors was lowered from 120% to 105%, bringing it in line with the terms available to institutional investors (Korea.net - Financial Services Commission, 2025).

This adjustment is economically meaningful: a collateral requirement of 120% forced individual investors to post significantly more capital upfront than their professional counterparts, making short selling disproportionately costly and often prohibitively capital-intensive for them. Reducing the ratio to 105% not only narrows this gap but also improves market accessibility by lowering the capital burden associated with borrowing securities. The reform therefore directly responds to long-standing concerns about unequal market access, aiming to ensure that retail investors can engage in short-selling strategies under comparable financial conditions as institutions.

2.4 Relevant Market Parameters

In order to assess the impact of short-selling bans, it is essential to understand the key market parameters that are most directly affected by such regulatory interventions. Among the most frequently analyzed dimensions are volatility, liquidity and stock overpricing as these variables capture both the stability and the efficiency of financial markets (Beber and Pagano, 2013, p. 345; Boehmer et al., 2014, pp. 1364-1366). This section provides an overview of these parameters, their conceptual foundations and their theoretical relationship to short selling. A more detailed empirical investigation will follow in the methodology and results chapters.

2.4.1 Volatility

Volatility refers to the degree of variation in asset prices over time and is a central measure of financial market risk. In practice, volatility can be measured in several ways. The most straightforward approach relies on the standard deviation of returns over a given period, capturing the dispersion of observed price changes. More advanced methods include realized volatility, which uses intraday data to provide a precise estimate of daily or weekly volatility and implied volatility, derived from option prices, which reflects market expectations about future uncertainty.

The theoretical link between short selling and volatility is multifaceted. On the one hand, regulators often argue that restricting short sales can reduce volatility, particularly in times of crisis. By preventing speculative downward pressure, bans are expected to limit sharp price

declines and excessive fluctuations (Spolaore & Le Moign, 2024, p. 25). On the other hand, financial theory suggests that suppressing short selling may actually increase volatility in the medium term. Empirical evidence supports this view: Helmes et al. (2016, p. 19) analyze an unusually long window of eight months before and after the Australian short-selling ban during the Global Financial Crisis and find that volatility increased following the introduction of the ban. Their extended observation period allows them to capture medium-term market dynamics rather than short-lived announcement effects.

This dual perspective highlights why volatility is one of the most critical parameters in assessing the effectiveness of short-selling bans.

2.4.2 Liquidity

Liquidity in financial markets is a multifaceted concept that cannot be captured by a single indicator. Instead, the literature distinguishes four complementary categories of liquidity measures, each highlighting a different dimension of how well a market functions (Sarr & Lybek, 2002, p. 8).

Transaction-cost measures focus on the expenses associated with executing trades, such as explicit fees or implicit costs like Bid–Ask Spreads. Because spreads reflect order-processing costs, asymmetric information and inventory risks borne by dealers, they serve as a widely used proxy for the tightness and immediacy of a market. High spreads indicate that trades are costly and that liquidity providers require compensation for the risks they assume (Sarr & Lybek, 2002, p. 9).

Volume-based measures, by contrast, evaluate liquidity through the lens of trading activity. Markets with consistently high volumes and frequent trades tend to possess both breadth and depth, enabling large orders to be executed with minimal price disruption. Frequent order flow also provides continuous information about fair prices, reducing the likelihood of abrupt price jumps and uncertainty about equilibrium values (Sarr & Lybek, p. 11).

Price-based measures examine the nature of price adjustments themselves, capturing how orderly and resilient markets are when new information arrives. In highly liquid markets, prices incorporate information smoothly, with only modest short-term deviations from their long-run equilibrium levels. In less resilient markets, by contrast, prices often exhibit transitory volatility or overshooting before stabilizing (Sarr & Lybek, 2002, p. 14).

Finally, market-impact measures isolate the degree to which observed price changes stem from liquidity rather than information. These indicators assess how sensitive prices are to trades of different sizes and seek to separate liquidity-driven movements from those caused by fundamental news. When markets have limited capacity to absorb order imbalances, even modest trading volumes can cause significant price swings, indicating low liquidity and a slow speed of price discovery (Sarr & Oybek, 2002, p. 17).

The connection between short selling and liquidity is particularly strong. Short sellers contribute to trading activity by increasing the number of transactions and by adding sell orders to the order book. From a regulatory perspective, the potential negative impact of bans on liquidity represents a significant trade-off. While the intention is to safeguard markets during periods of stress, the withdrawal of short sellers can result in thinner markets and reduced efficiency, thereby complicating the task of maintaining orderly trading. Understanding liquidity dynamics is therefore crucial to evaluating the broader consequences of short-selling restrictions.

2.4.3 Excess Returns

Mispricing poses a fundamental challenge for financial markets and the real economy. When stock prices deviate from their intrinsic values, capital is misallocated: overvalued firms attract excessive resources that later need to be reabsorbed, whereas undervalued firms may be starved of funding they would otherwise merit. Although the market eventually corrects such distortions, the realignment of labor, capital and investment decisions occurs much more slowly, creating persistent inefficiencies. Short sellers play an important role in mitigating these distortions. By identifying overpriced firms, uncovering aggressive accounting practices, or detecting outright fraud, they contribute to a more accurate reflection of fundamentals (Battalio et al., 2011, p. 2)

Concerns arise, however, when short sellers are believed to push prices below fundamental values. In practice, this is difficult to achieve profitably. Any downward pressure they exert must be reversed when covering their positions, often negating the impact on prices. Attempts to manipulate markets through false rumors or coordinated attacks are illegal, risky and empirically rare (Battalio et al., 2011, p. 3).

These dynamics illustrate the delicate balance between the beneficial role of short selling in correcting overvaluation and the potential destabilizing effects when market sentiment becomes

fragile. It is precisely this tension that motivates policymakers to intervene through short-selling regulations. When short-selling bans are imposed, the corrective and informational function of short sellers is suppressed. As a result, overvalued stocks may remain overpriced for longer, informational efficiency decreases and price discovery becomes less reliable. At the same time, bans may temporarily shield vulnerable firms from excessive downward pressure during periods of turmoil. Understanding these trade-offs is essential for assessing the empirical impact of short-selling bans and for interpreting how returns respond when a key mechanism of market discipline is suspended.

3. Method

3.1 Data

The data section provides an overview of the datasets and variables used in the empirical analysis. It includes three parts: Data Sources, describing the origin, collection and preparation of the data. Summary Statistics, presenting the main characteristics and distributions of the dependent variables across the ban periods and Sample Construction, explaining how treatment and control groups were defined and how the final firm–day panels were assembled.

3.1.1 Data Sources

The empirical analysis is based on financial market data obtained from Refinitiv Datastream. The dataset covers a period of six months before and four to six months after the respective short-selling ban introductions, depending on the ban. The analysis focuses on South Korea as the treatment market, where the bans were implemented and for the first and third ban on Japan as the control market, where no bans were in place during the sample period. Japan represents a particularly suitable control group since it is a major Asian economy with close regional and economic ties to South Korea, it did not implement short-selling bans during the COVID-19 crisis and it has a similar industrial structure with a strong focus on semiconductors and technology. In contrast to the first and third bans, the analysis of the second ban employs South Korean companies that were constituents of the KOSPI200 or KOSDAQ150 indices as the control group, since these firms were not subject to the short-selling restrictions.

The following variables were downloaded on a daily frequency for each stock:

- Price: the official exchange closing price of the security.

- Volume-Weighted Average Price (VWAP): the average trading price, weighted by the traded volume on the same day.
- Bid Price: the highest price a buyer was willing to pay at market close.
- Ask Price: the lowest price a seller was willing to accept at market close.
- Volume: Numbers of shares traded, expressed in thousands.
- Market Value: the market value of a security expressed in millions, calculated as the number of outstanding shares multiplied by the security's price
- Intraday High: the highest traded price of the security within the trading day.
- Intraday Low: the lowest traded price of the security within the trading day.
- Sector: the sector classification of the company according to Refinitiv.

To ensure comparability across the entire sample period, the dataset was restricted to equities listed on South Korean and Japanese exchanges. Only actively traded stocks that remained continuously listed and traded throughout the research horizon were retained. In addition, only companies listed in the respective local currency were included and firms from all sectors were considered in order to capture the full cross-sectional variation of the market. After applying these filtering criteria, the final sample consists of 931 South Korean firms and 1,434 Japanese firms, which serve as the basis for the subsequent analysis.

In addition to the Refinitiv Datastream data, information on the constituents of the KOSPI200 and KOSDAQ150 indices was directly retrieved from the website of the Korea Exchange (KRX). Specifically, the constituent lists as of the introduction of the second ban, as well as the index changes on 11 June 2021, which fall within the observation window of the second ban, were incorporated. A detailed overview of these companies is shown in the chapter 3.1.3 Sample Construction.

3.1.2 Summary Statistics

Tables 1–3 on the following page present summary statistics for all dependent variables across the three ban periods. The statistics are reported at the firm–day level and include the number of observations (N), mean, standard deviation, minimum and maximum values, as well as the 25th, 50th (median) and 75th percentiles. Across all periods, the variables exhibit strong right-skewness, indicating the presence of firms or trading days with unusually high values relative to the average. This is particularly visible in measures such as the Bid–Ask Spread and Relative Volatility, where the maximum values substantially exceed the mean. Moreover, it is noteworthy that during the first ban period, the level of spreads and volatility measures is

considerably higher compared to the subsequent bans, reflecting the pronounced impact of the global market turmoil and liquidity stress at the onset of the COVID-19 pandemic.

The Amihud Illiquidity ratio was cleaned to exclude undefined or infinite values (Inf), which occasionally arise when trading volume equals zero. After cleaning, the mean and maximum values fall within an economically reasonable range across all periods. Apart from this adjustment, no further smoothing or winsorization was applied to the data used in the regressions, in order to preserve genuine variation and avoid attenuating potential treatment effects.

The number of observations (N) per ban varies slightly across variables due to missing or cleaned data. In the first ban window, the cross-section is essentially balanced with 296 treated and 296 control firms, observed over 261 and 262 unique trading days respectively which implies a theoretical panel of 77,256 treated and 77,552 control firm–day observations, i.e., 153,838 in total. In the second ban window, the cross-section is more uneven, meaning 135 treated versus 172 control firms but both groups are followed for 287 trading days, yielding 38,745 treated and 49,364 control observations or 88,109 combined. In the third ban window, there are 123 treated and 111 control firms, each tracked for 306 trading days. This translates into 37,638 treated and 33,966 control observations and a combined theoretical N of 71,604 firm–days.

Summary Statistics for Ban 1:

Parameter	N	Mean	Std. Dev.	Min.	25%	Median	75%	Max.
Spread	143,850	0.371288	0.418625	0.000000	0.175135	0.282885	0.425532	16.764133
Amihud	142,933	0.000032	0.000159	0.000000	0.000002	0.000007	0.000023	0.021748
RVOL	143,966	0.037323	0.030207	0.000000	0.019461	0.029042	0.044973	0.543765
Roll. Vol.	153,798	0.032446	0.079098	0.003244	0.016453	0.022469	0.031970	1.741789
Park. Vol.	143,966	0.000835	0.002235	0.000000	0.000136	0.000304	0.000729	0.113743
Ex. Return	153,838	0.001011	0.076304	-0.740268	-0.013277	-0.001709	0.009950	2.668584

Table 1: Summary Statistics Ban 1

Summary Statistics for Ban 2:

Parameter	N	Mean	Std. Dev	Min.	25%	Median	75%	Max
Spread	81,455	0.0023	0.0015	0.0002	0.0013	0.0018	0.0030	0.0410
Amihud	81,528	0.0002	0.0010	0.0000	0.0000	0.0001	0.0001	0.0605
RVOL	81,528	0.0388	0.0280	0.0000	0.0220	0.0317	0.0464	0.4820
Roll. Vol.	80,938	0.0176	0.0124	0.0000	0.0105	0.0146	0.0203	0.2342
Park. Vol.	81,528	0.0008	0.0020	0.0000	0.0002	0.0004	0.0008	0.0811
Ex. Return	82,082	-0.0011	0.0229	-0.0406	-0.0137	-0.0066	0.0045	0.9375

Table 2: Summary Statistics Ban 2

Summary Statistics for Ban 3:

Parameter	N	Mean	Std. Dev	Min.	25%	Median	75%	Max
Spread	66,983	0.0023	0.0022	0.0000	0.0012	0.0018	0.0027	0.1201
Amihud	66,791	0.000011	0.00005	0.00000	7.900000e-07	0.000003	0.000009	0.007250
RVOL	67,024	0.0281	0.0202	0.0000	0.0158	0.0228	0.0338	0.4019
Roll. Vol.	70,434	0.0201	0.0116	0.0016	0.0126	0.0172	0.0243	0.2008
Park. Vol.	67,024	0.0004	0.0011	0.0000	0.0001	0.0002	0.0004	0.0571
Ex. Return	71,370	-0.0002	0.0228	-0.3071	-0.0104	-0.0004	0.0084	0.3103

Table 3: Summary Statistics Ban 3

3.1.3 Sample Construction

The construction of the treatment and control groups varies across the three short-selling bans, reflecting differences in the institutional setup and data availability during each period.

For the first ban, the initial sample was defined based on firm-level market capitalization and industry classification to ensure comparability between the two markets. Using these characteristics, South Korean and Japanese firms were matched and from this universe, around 300 best matches (296 pairs) were retained for further analysis. Figure 2 displays the deviations in log-market capitalization between Korean firms and their Japanese matching partners for the 296 best matches in the sample, sorted by the magnitude of the deviation.

Throughout all empirical analysis related to Ban 1, the South Korean firms constitute the treatment group, as the short-selling ban was implemented in Korea, while the matched Japanese firms serve as the control group.

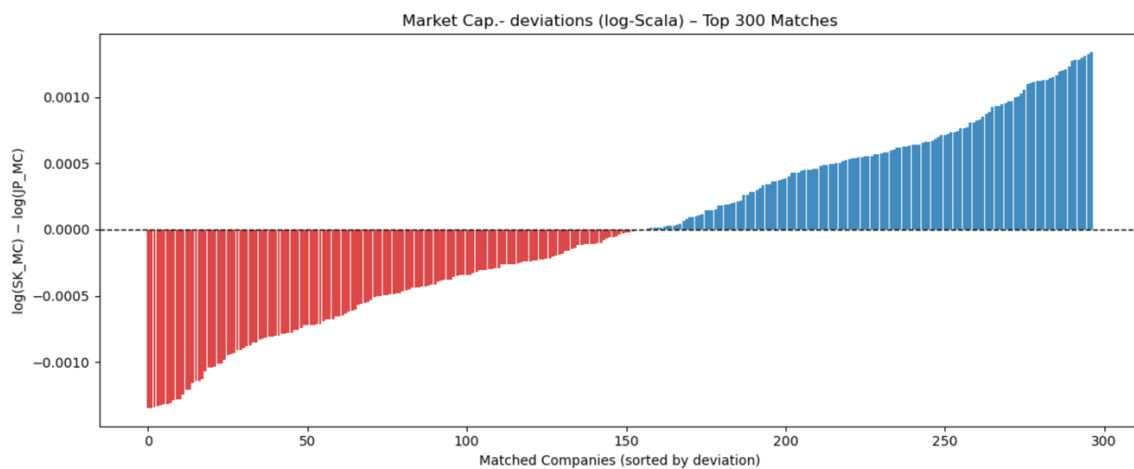


Figure 2: Top 300 matches Ban 1

For the second ban, the setting differs as the ban was lifted only for a subset of South Korean firms, which were part of the KOSPI200 or KOSDAQ150 indices. Accordingly, the treatment group consists of those Korean firms for which the short-selling restriction was removed, whereas the control group includes Korean firms that remained subject to the ban. The treatment group was identified by cross-referencing the list of index constituents with the prior selected firm universe and selecting the firms included in the index at the time of the policy change. Since the index composition changed dynamically over time, the treatment group was adjusted to account for firms that entered or exited the indices during the observation window. Table 4 shows all index constituents within the sample that were added to or removed from the indices during the study period. “In” indicates additions, whereas “Out” indicates removals (Kim and Kim, 2024, p. 419).

Company	Status	Date	Company	Status	Date
Aju ib invest	In	11.06.2021	Aekyung ind.	Out	11.06.2021
Devsisters	In	11.06.2021	Bing-grae	Out	11.06.2021
Dongwon ind.	In	11.06.2021	Clio cosmetics	Out	11.06.2021
Doosan tesna	In	11.06.2021	Easyholdings	Out	11.06.2021
Hana materials	In	11.06.2021	Golfzon	Out	11.06.2021
HS hoysung adv. materials	In	11.06.2021	Hanil hyundai cement	Out	11.06.2021
Hyosung tnc	In	11.06.2021	Saramin	Out	11.06.2021
Park systems	In	11.06.2021	Spc samlip	Out	11.06.2021
Simmtech	In	11.06.2021	Vitzrocell	Out	11.06.2021
Sungwoo hitech	In	11.06.2021			
Taihan electric wire	In	11.06.2021			
Woori technology inv.	In	11.06.2021			

Table 4: Indices constituents that changed on 11.06.2021

For the third ban, short-selling was once again suspended for all Korean firms. Therefore, the treatment group for Ban 3 builds upon the treatment firms identified for Ban 2 and was updated based on the available firm universe downloaded from Refinitiv Datastream. To establish a suitable control group, the Korean treatment firms were matched with Japanese firms based on market capitalization and industry classification. These Japanese firms, unaffected by the regulatory intervention, serve as the control group for the third ban.

3.2 Outcome variables

This section introduces the dependent variables used throughout the analysis. Each variable is briefly described, highlighting its economic interpretation and potential statistical challenges.

To provide an initial intuition about their behavior, graphical representations (figures 3 - 20) are presented for each outcome variable, illustrating their average evolution before and after the respective short-selling bans. These visualizations serve as a first overview of the variables' dynamics, offering insights into whether treatment and control groups exhibited similar developments prior to the ban, as well as how the variables evolved following the implementation of the short-selling restrictions.

In the figures, the vertical dotted line indicates the respective ban date. Dashed horizontal lines represent the average level within the pre-ban period (to the left of the vertical line) and the post-ban period (to the right) for each group. All values are displayed on a logarithmic scale, allowing movements to be interpreted approximately as percentage changes. For improved readability, the time series are smoothed using a short moving average. These visualizations are intended to provide an initial descriptive overview, the formal statistical assessment of these relationships is conducted in the subsequent empirical analysis.

3.2.1 Relative Bid-Ask Spread

The Bid–Ask Spread reflects the cost of immediacy: tighter spreads indicate greater market depth and competition among traders, while wider spreads suggest thinner markets and reduced liquidity (Beber & Pagano, 2013, p. 346). As such, the spread is sensitive to market stress and regulatory interventions, making it a particularly suitable measure for evaluating the consequences of short-selling bans.

The relative bid–ask spread is defined as follows:

$$Relative\ Bid - Ask\ Spread_{i,t} = \frac{Ask_{i,t} - Bid_{i,t}}{\frac{1}{2}(Ask_{i,t} + Bid_{i,t})}$$

Equation 1: Relative Bid-Ask Spread

where $Ask_{i,t}$ is the lowest price offered by sellers (ask quote) for stock i at the end of day t and $Bid_{i,t}$ is the highest price buyers are willing to pay (bid quote) at the same point in time. The denominator represents the midquote, ensuring that the spread is normalized by the price level. This normalization makes spreads comparable across firms with different absolute price levels.

The spread was calculated for each stock and each trading day in the sample. Observations with missing or invalid bid and ask quotes, such as non-positive values or absent trading data, were

excluded from the computation. The resulting series provides a firm-day level measure of transaction costs across both the treatment and control groups.

A lower relative Bid–Ask Spread indicates higher liquidity and investors can trade at lower transaction costs and with less market impact. Conversely, a higher spread suggests reduced liquidity and more costly trading. From a regulatory perspective, short-selling bans are expected to widen spreads: with short sellers excluded from the market, the supply of sell orders diminishes, reducing market depth and increasing the cost of immediacy. This effect is of particular interest in assessing whether bans achieved stability at the expense of liquidity.

Graphical Overview

Figure 3 plots the time series of the daily cross-sectional mean log Bid–Ask Spread for the treatment market (blue) and the control market (orange) from August 2019 to July 2020. Visually, both markets exhibit relatively stable spreads before March 2020 with short-lived fluctuations. Around the ban date, the control market displays a sharp, temporary widening of spreads, after which levels remain higher than their pre-ban average. The treatment market also shows a brief uptick around the event but then reverts and trends close to, or slightly below, its pre-ban average. The dashed lines indicate that the post-ban average decreased for the treatment group, whereas the post-ban average increased for the control group.

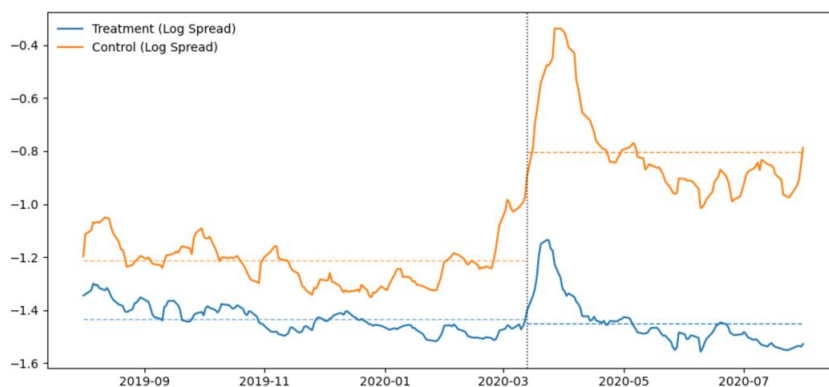


Figure 3: *Log(Spread) Ban 1*

Following the second ban, the average spread for both groups (see figure 4) decreased slightly compared to the pre-ban period. During the first half of the post-ban period, both groups exhibited a mild downward trend, which later reversed into an upward movement toward the second half of the post-ban observation window.

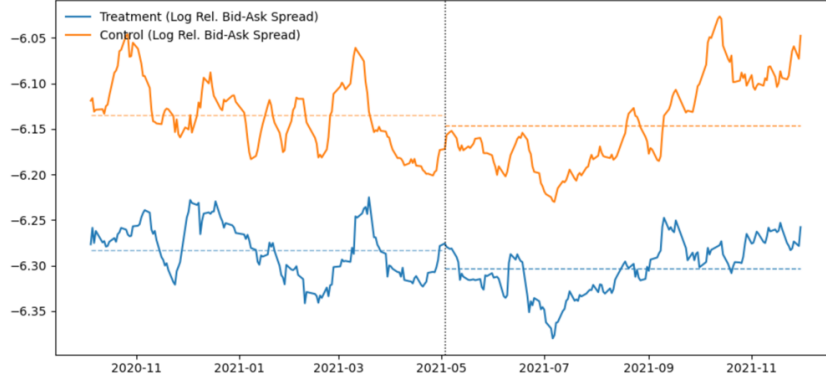


Figure 4: *Log(Spread) Ban 2*

In the third ban, the average spread for the treatment group stayed constant over the whole observation period. For the control group the spread increased in the post-ban period (see figure 5).

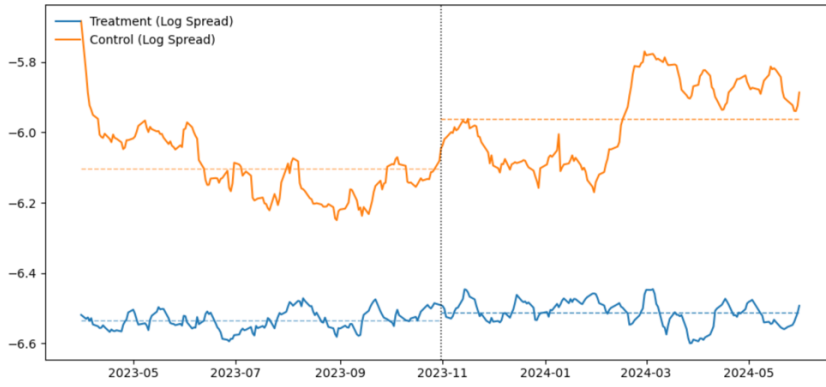


Figure 5: *Log(Spread) Ban 3*

3.2.2 Amihud Illiquidity Index

The second proxy employed in this study is the Amihud Illiquidity Index, which captures the price impact of trading activity. Unlike the bid–ask spread, which reflects transaction costs at the point of trade execution, the Amihud Illiquidity Index measure focuses on the sensitivity of asset prices to trading volume, thereby highlighting a different dimension of liquidity.

The Amihud Illiquidity Index for stock i on day t is defined as:

$$\text{Amihud Illiquidity Index}_{i,t} = \frac{R_{i,t}}{\text{Volume}_{i,t}}$$

Equation 2: *Amihud Illiquidity Index*

where $R_{i,t}$ is the absolute return of stock i on day t and $\text{Volume}_{i,t}$ represents the daily trading volume in monetary terms. A higher value of the index indicates that a relatively small amount

of trading volume is associated with a large price change, reflecting illiquid market conditions. Conversely, lower values suggest that prices are less sensitive to trading, implying greater liquidity.

To enhance comparability for $\text{Volume}_{i,t}$ across firms and markets, all price data for Ban 1 and Ban 3 were converted into US-Dollars using a fixed exchange rate corresponding to the date of each respective ban. A time-varying exchange rate was deliberately not applied in order to avoid introducing exchange-rate fluctuations into the analysis. The exact conversion rates used are documented in the Appendix.

To ensure consistency, all trading volumes were then expressed in monetary terms by multiplying the USD-denominated prices by the number of shares traded. Days with zero or missing trading volume were excluded, as the Amihud measure cannot be computed in such cases. This procedure yields a firm-day-level liquidity metric that complements the bid–ask spread by capturing the price-impact dimension of liquidity.

From an economic perspective, the Amihud index provides insights into how trading restrictions such as short-selling bans influence the efficiency of price formation. If bans reduce market participation and depth, price reactions to trades are expected to become stronger, resulting in higher Amihud values. This reflects the broader cost of illiquidity beyond transaction spreads, as even moderate trading volumes can generate disproportionately large price changes in thinner markets.

Graphical Overview

Following the first ban, the average Amihud Illiquidity measure declined notably for the treatment group, indicating improved liquidity, while it increased for the control group (see figure 6).

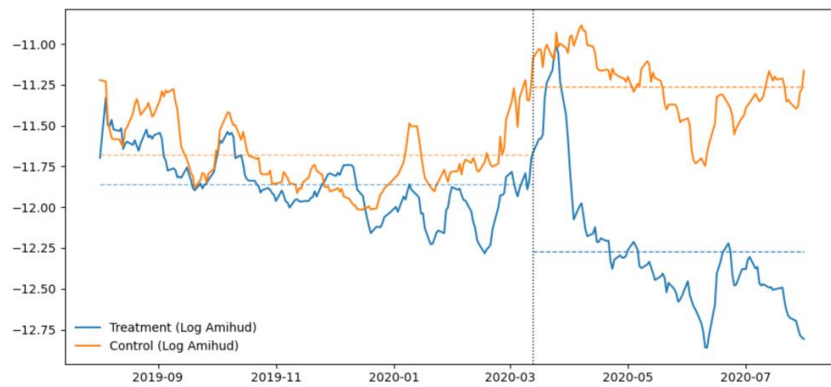


Figure 6: $\text{Log}(\text{Amihud})$ Ban 1

For Ban 2 (see figure 7), the average Amihud Illiquidity index followed a very similar trajectory for both groups, with the post-ban averages increasing for each group compared to the pre-ban period.

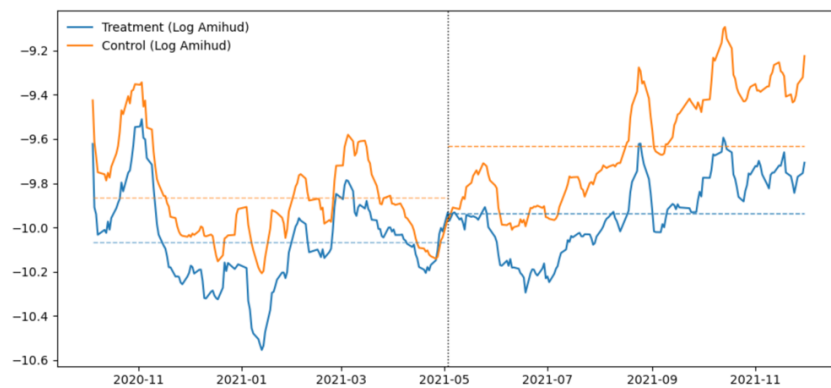


Figure 7: $\text{Log}(\text{Amihud})$ Ban 2

Around the third ban period, the average Amihud Illiquidity index increased for the treatment group, while it decreased for the control group during the post-ban period (see figure 8).

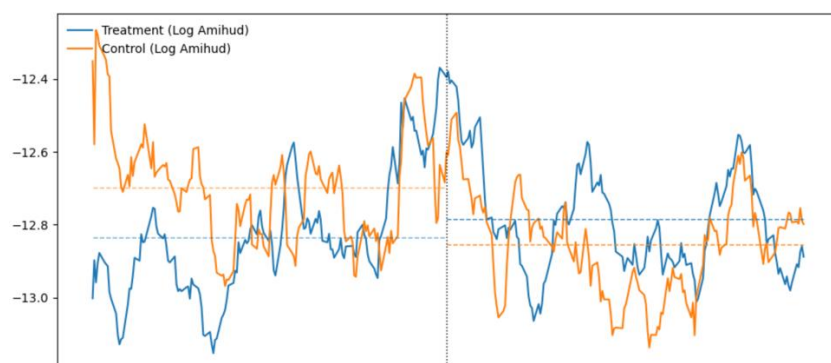


Figure 8: $\text{Log}(\text{Amihud})$ Ban 3

3.2.3 Relative Volatility

Building on the multidimensional nature of volatility, the first proxy employed in this study is Relative Volatility, which relates the intraday price range to the VWAP. This measure captures the extent of price fluctuations during a trading day relative to the market activity that generated them, thereby combining information on both price dynamics and trading intensity.

Relative Volatility for stock i on day t is defined as:

$$Relative\ Volatility = \frac{P_{i,t}^{High} - P_{i,t}^{Low}}{VWAP_{i,t}}$$

Equation 3: Relative Volatility

where $P_{i,t}^{High}$ represents the highest traded price of stock i during day t , $P_{i,t}^{Low}$ the lowest traded price and $VWAP_{i,t}$ the average traded price weighted by the traded volume on the same day. The numerator captures the intraday trading range, while the denominator normalizes this range by the corresponding volume weighted average price. A higher daily range relative to a given VWAP thus reflects higher volatility, while a lower value suggests more stable trading conditions. While the VWAP itself may be affected by the ban through changes in price levels, this normalization is standard practice in the literature to ensure comparability across firms and periods. Moreover, as both treatment and control groups may experience similar price dynamics, the use of a difference-in-differences design mitigates concerns about bias introduced through the denominator.

Observations with missing or zero VWAPs were excluded, as the measure cannot be computed in these cases. The resulting variable is available at the firm-day level and directly reflects the intensity of price movements relative to market activity.

Relative Volatility captures intraday price fluctuations in relation to price levels, as measured by the VWAP and is therefore highly sensitive to changes in market conditions. When short-selling bans reduce liquidity and market depth, prices tend to react more sharply to trading imbalances, resulting in higher Relative Volatility. Conversely, in liquid and well-functioning markets, price movements are absorbed more efficiently, leading to smaller relative intraday price ranges compared to the overall price level.

Graphical Overview

Following the introduction of the first short-selling ban, the average Relative Volatility increased for both the treatment and the control group compared to the pre-ban period. The average rise in the post-ban period was more pronounced for the control group, indicating stronger intraday price fluctuations relative to price levels (see figure 9).

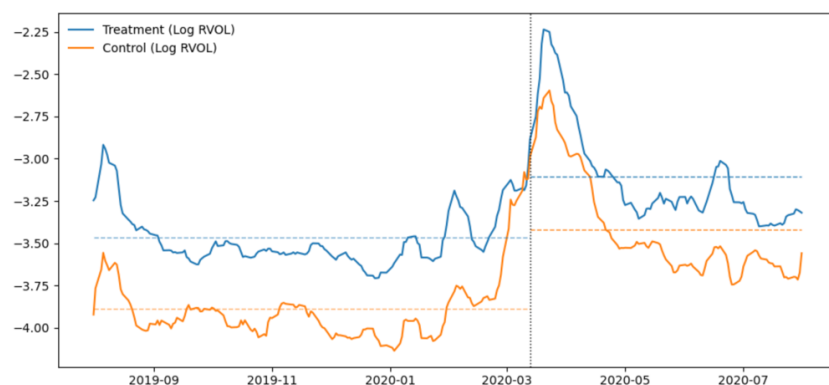


Figure 9: $\text{Log}(\text{RVOL})$ Ban 1

Figure 10 displays that for the second ban, the average Relative Volatility slightly declined in both groups compared to the pre-ban period.

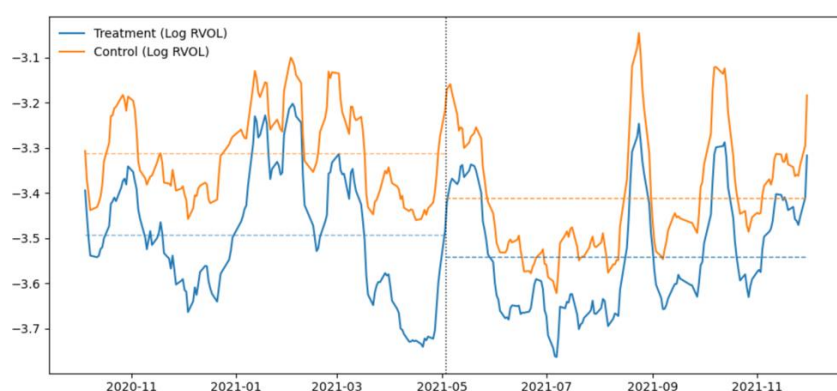


Figure 10: $\text{Log}(\text{RVOL})$ Ban 2

During the third ban period, the average Relative Volatility for the treatment group decreased slightly, while it increased marginally for the control group (see figure 11).

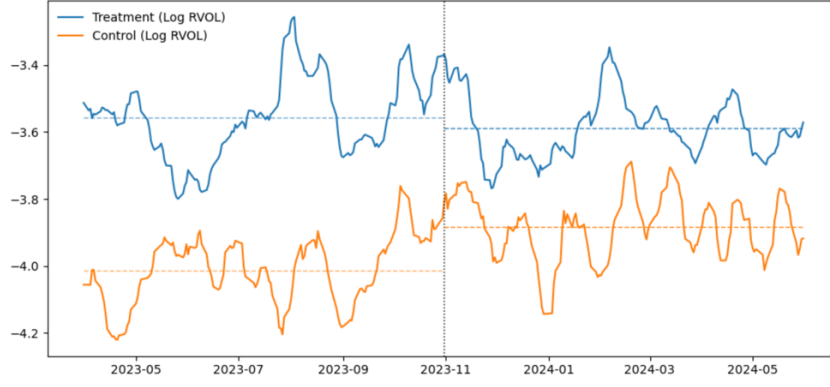


Figure 11: Log(RVOL) Ban 3

3.2.4 Rolling Volatility

Rolling Volatility is employed as the second proxy for volatility and captures the variability of returns over a specified horizon. Unlike Relative Volatility, which focuses on intraday price fluctuations relative to trading activity, Rolling Volatility provides a smoothed measure of uncertainty by aggregating past return dynamics.

Rolling Volatility for stock i on day t is defined as:

$$Rolling\ Volatility = \sqrt{\frac{1}{N} \sum_{k=1}^N (R_{i,t-k} - \bar{R}_t)^2}$$

Equation 4: Rolling Volatility

where $R_{i,t-k}$ represents the return of stock i on day $t-k$, $\bar{R}_t$ is the mean return over the same window and N is the length of the rolling window. In this study, the volatility is calculated on a 20-day rolling basis. For the initial period, where 20 past observations are not yet available, a shorter rolling window is applied, starting with 5 days and gradually increasing until the full 20-day window is reached. From day 20 onwards, volatility is consistently measured using the full 20-day horizon.

Return data were calculated based on daily closing prices, which were obtained from Refinitiv Datastream. Rolling Volatility was then computed at the firm-day level, providing a dynamic measure that smooths short-term fluctuations while capturing persistent changes in uncertainty.

Higher values of Rolling Volatility indicate that returns have been more variable over the past trading days, reflecting heightened market uncertainty and risk. In the context of short-selling bans, volatility may increase if bans reduce liquidity and market efficiency, leading to larger and more persistent return fluctuations. Conversely, in more liquid and stable market environments, Rolling Volatility remains lower as price movements are absorbed more effectively.

Graphical Overview

Following the introduction of the first short-selling ban, the average Rolling Volatility increased for both the treatment and the control group compared to the pre-ban period (see figure 12).

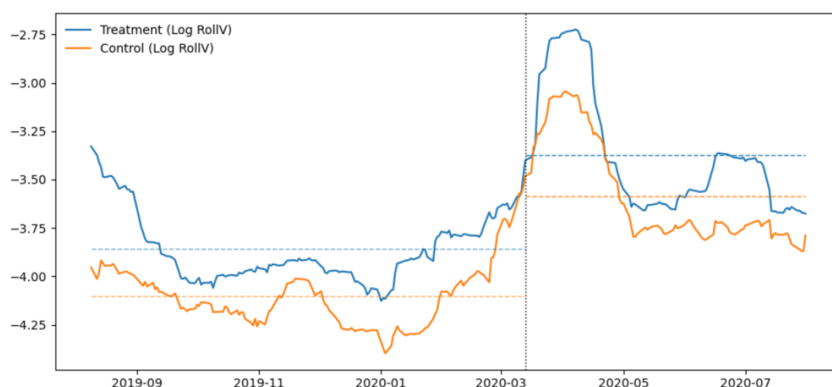


Figure 12: *Log(RollV) Ban 1*

Figure 13 shows that around the second ban, the average Rolling Volatility declined for the treatment and control group in the post-ban period.

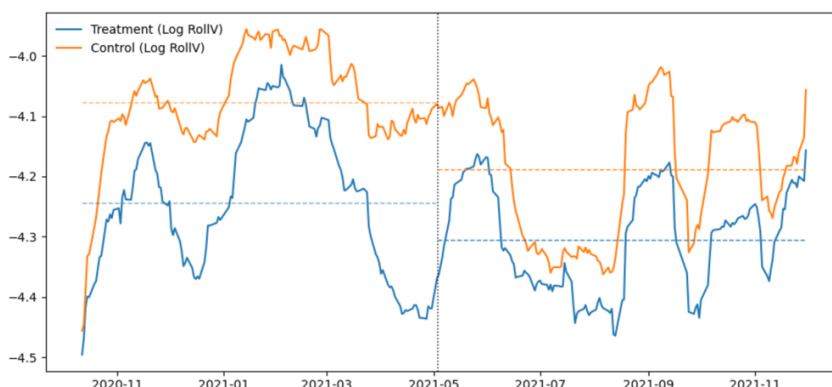


Figure 13: *Log(RollV) Ban 2*

During the third ban period, the average Rolling Volatility for the treatment group remained relatively stable compared to the pre-ban period, while the control group's average level increased more prominently (see figure 14).

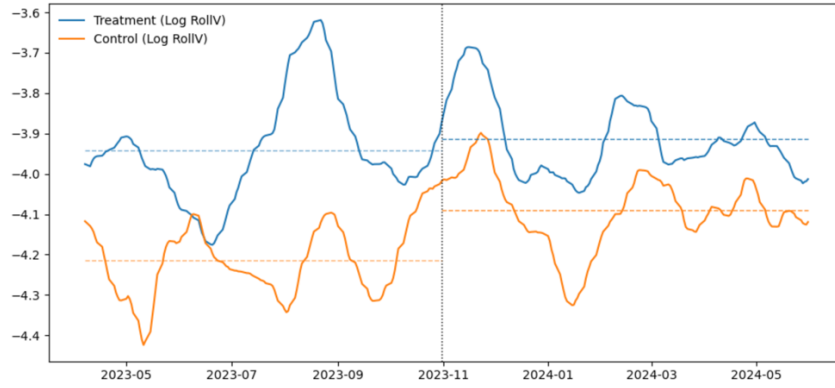


Figure 14: Log(RollV) Ban 3

3.2.5 Parkinson Volatility

Parkinson Volatility is employed as an alternative proxy for volatility that exploits the information contained in daily high and low prices. Unlike Rolling Volatility, which relies on closing returns over a specified horizon, the Parkinson estimator focuses on intraday price ranges to measure volatility more efficiently. By incorporating the extreme price movements within a trading day, this measure captures the realized variability of asset prices without being affected by the closing price alone.

Mathematically, the Parkinson estimator is closely related to the concept of Relative Volatility, as both rely on the daily price range (High – Low) as a measure of intraday variation. However, while Relative Volatility expresses this range relative to a price level (such as VWAP), Parkinson Volatility applies a logarithmic transformation and variance scaling factor to obtain a more statistically efficient estimate of volatility. The similarity in the calculation is visible in the graphical representation of the variables.

Parkinson Volatility for stock i on day t is defined as:

$$Parkinson\ Volatility_{i,t} = \frac{1}{4 \ln(2)} * \left[\ln \left(\frac{High_{i,t}}{Low_{i,t}} \right) \right]^2$$

Equation 5: Parkinson Volatility

where $High_{i,t}$ and $Low_{i,t}$ represent the highest and lowest traded prices of stock i on day t , respectively. The logarithmic transformation ensures scale invariance and makes the measure robust to differences in price levels across stocks.

Daily high and low prices were obtained from Refinitiv Datastream and the Parkinson Volatility was computed at the firm–day level. This estimator provides an efficient measure of daily volatility by utilizing the full intraday range rather than relying solely on closing returns, thereby reducing noise and increasing precision compared to standard return-based volatility measures.

Higher values of Parkinson Volatility indicate that intraday price ranges are wider, reflecting increased uncertainty and market stress. In the context of short-selling bans, Parkinson Volatility may rise if trading restrictions impair liquidity and hinder price discovery, leading to larger intraday price swings. Conversely, in stable and liquid market conditions, narrower trading ranges result in lower Parkinson Volatility, as prices fluctuate within a more confined interval throughout the day.

Graphical Overview

Following the introduction of the first short-selling ban, the average Parkinson Volatility increased for both the treatment and the control group compared to the pre-ban period (see figure 15).

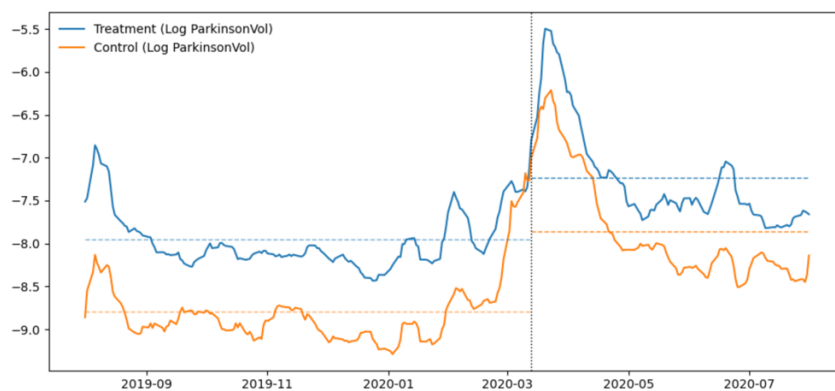


Figure 15: Log(Parkinson Vol) Ban 1

Around the second ban, the average Relative Volatility slightly declined in both groups compared to the pre-ban period (see figure 16).

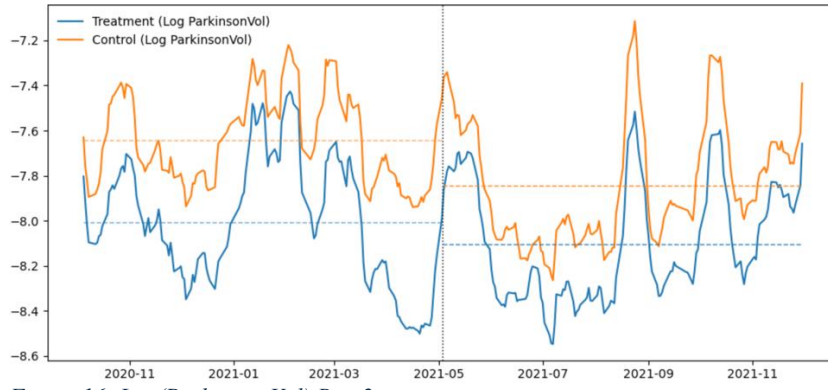


Figure 16: $\text{Log(Parkinson Vol)}$ Ban 2

During the third ban period, the average Relative Volatility for the treatment group decreased slightly, while it increased marginally for the control group (see figure 17).

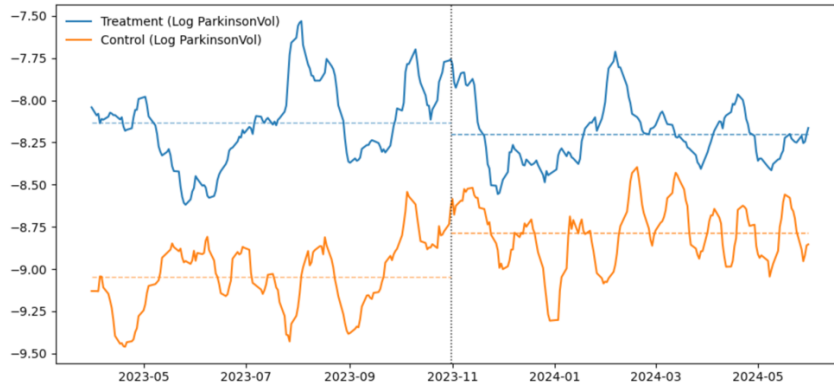


Figure 17: $\text{Log(Parkinson Vol)}$ Ban 3

3.2.6 Excess Returns

Overpricing is examined through the use of excess returns, which measure the deviation of an individual stock's performance from that of a relevant control group. The idea is that, under efficient markets and absent distortions, the returns of treatment and control firms should move in parallel once common risk factors are accounted for. Persistent positive deviations of treatment firms relative to their peers can therefore be interpreted as signs of potential overpricing, while negative deviations indicate underperformance. This approach is consistent with the notion that regulatory interventions such as short-selling bans may artificially inflate stock prices by removing downward pressure from the market.

Excess returns for firm i on day t are calculated as:

$$\text{Excess Return}_{i,t} = \text{Return}_{i,t} - \overline{\text{Return}}_{m,t}$$

Equation 6: Excess Return

where $\text{Return}_{i,t}$ is the daily return of firm i on day t and $\overline{\text{Return}}_{m,t}$ is the average return of the control group firms on the same day. The market return used for calculating excess returns corresponds to Japanese firms for Ban 1 and Ban 3 and South Korean firms that remained subject to the short-selling ban for Ban 2.

Daily returns were computed from closing prices obtained from Refinitiv Datastream. For each day, the average return of the control group was calculated and then subtracted from the individual firm returns in the treatment group to generate the excess return series. The result is a firm-day level measure of relative performance that isolates potential distortions in price levels caused by the ban.

Higher values of excess returns imply that treatment firms outperform their benchmark peers, which may indicate that stock prices are sustained at levels above their fundamental value due to the absence of short sellers. Conversely, insignificant or negative excess returns would suggest that no systematic overpricing occurred. This measure therefore directly links short-selling restrictions to possible distortions in market valuation.

Graphical Overview

Across all three ban periods, the figures 18 - 20 illustrate the evolution of excess returns for the treatment and control groups. The excess returns of the control group remain relatively stable throughout each period, showing minimal fluctuations around zero. This stability is expected, as excess returns are calculated relative to the average market return of the control group itself, meaning deviations within that group tend to cancel out. In the first ban period, excess returns exhibit strong variance, likely driven by the market crash at the onset of the COVID-19 pandemic, which caused extreme volatility and abrupt price corrections. Comparing the pre- and post-ban periods, the average excess returns for the first and third bans remain relatively similar, indicating no substantial shift following the introduction of the bans. In contrast, during the second ban, the average excess returns in the post-ban period show a clear increase, suggesting a positive market reaction among the firms affected by the lifting of the short-selling restrictions.

Importantly, the figures display $\log(1 + \text{Excess Return})$, rather than $\log(\text{Excess Return})$. This transformation is used to maintain consistency with the statistical analysis, as it allows negative excess returns to remain in the dataset. A standard logarithmic transformation would exclude negative values, which would bias the results upward by systematically removing downside

movements. Using $\log(1 + \text{Excess Return})$ therefore preserves the full return distribution and avoids distorting the estimation and visual interpretation.

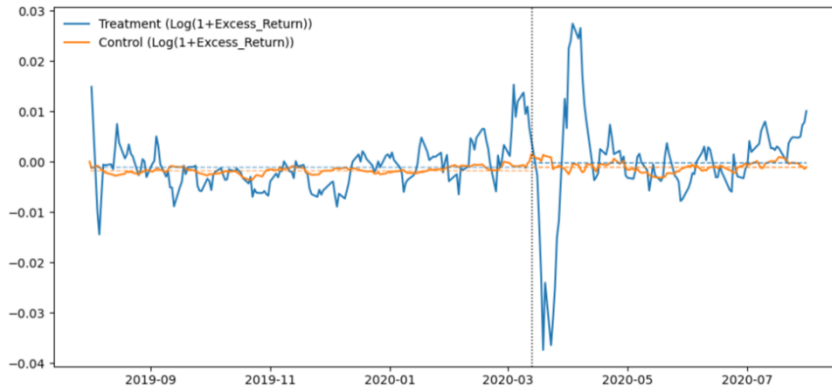


Figure 18: $\log(1+\text{Excess Return})$ Ban 1



Figure 19: $\log(1+\text{Excess Return})$ Ban 2

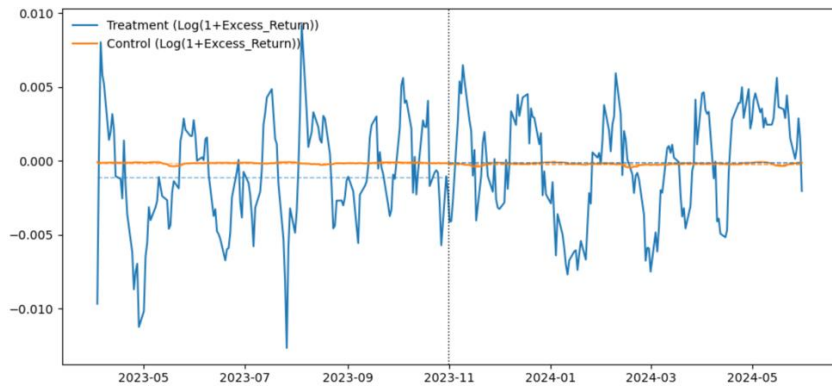


Figure 20: $\log(1+\text{Excess Return})$ Ban 3

3.3 Empirical Framework

Building on the previous discussion of short-selling bans and their potential market implications, this section introduces the empirical framework of the analysis. Prior research suggests that such bans can influence both market liquidity and volatility by restricting trading mechanisms and altering price discovery processes (Beber and Pagano, 2013, p. 379). Given

the recurring implementation of short-selling restrictions in South Korea, this study aims to empirically assess their impact on key market dimensions.

The central research question of this thesis is therefore:

“How did the Short-Selling Bans in South Korea affect South Korean equity markets?”

To address this question, a difference-in-differences framework is applied that compares the performance of the treatment group to an appropriate control group before and after the introduction of short-selling restrictions. This empirical strategy allows for isolating the causal impact of the bans under the key assumption of parallel trends.

For the first and third ban periods, Japanese firms serve as the control group, as Japan did not impose short-selling bans during the research period, shares a comparable industrial composition and thus provides a suitable benchmark for South Korea. To further improve comparability, firms in South Korea and Japan are matched based on industry classifications provided by Refinitiv Datastream as well as market capitalization. For the latter, the values were converted into US-Dollars using exchange rates as of the respective ban introduction dates. The exchange rates per ban are shown in the Annex.

For the second ban period, an internal control group is applied, consisting of South Korean firms that were constituents of the KOSPI200 or KOSDAQ150 indices. This setup ensures comparability within the same market environment, as only some firms were subject to the partial ban while others remained unaffected.

The validity of the DiD framework depends on the parallel trends assumption, which requires that treatment and control firms would have followed similar trajectories in the absence of the intervention. The parallel trend assumption was first examined visually in chapter 3.2 by comparing the average developments of the dependent variables between the treatment and control groups. This graphical inspection was then followed by a formal evaluation using an event study and a complementary placebo analysis. Both approaches are discussed in more detail in Section 4.1, Parallel Trend Analysis.

When interpreting the regression results, particular attention must be paid to cases where the parallel trends assumption does not hold. If treatment and control firms already display diverging patterns in the pre-ban period, the DiD estimator may conflate the effects of the ban

with pre-existing differences, leading to biased estimates. In such cases, the causal interpretation of the results becomes questionable and findings should be treated with caution.

Given that the dependent variables are often skewed in financial data, a log-linear DiD specification is applied. This transformation improves the distribution of residuals and facilitates the interpretation of coefficients in percentage terms. In doing so, all liquidity and volatility proxy variables are transformed using the natural logarithm, which is appropriate since these measures are strictly non-negative. For excess returns, however, a $\log(1 + \text{Excess Return})$ transformation is used instead, because excess returns can take negative values. Applying a standard logarithm would exclude these negative observations and thereby introduce an upward bias in the estimation, whereas $\log(1 + \text{Excess Return})$ preserves the full return distribution.

Figures 21 and 22 display example QQ-plots of the residuals of one dependent variable and illustrate how the assumption of normality improves after applying the log transformation. The left plot, based on the level specification, shows noticeable deviations from the red reference line at both tails, indicating the presence of extreme values and a departure from normality. In contrast, the right plot, which displays the residuals after the log transformation, shows a much closer alignment with the reference line.

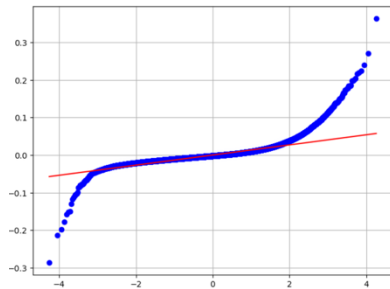


Figure 22: QQ-Plot Residuals (Level)

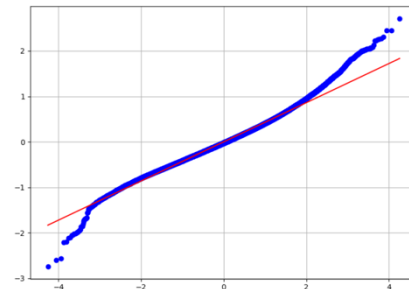


Figure 21: QQ-Plot Residuals (Log)

Consequently, the baseline model is defined as:

$$\text{Log}(Y_{i,t}) = \alpha + \beta_1 \text{DiD}_{i,t} + \mu_i + \tau_t + \varepsilon_{i,t}$$

Equation 7: Main DiD-Model

where $Y_{i,t}$ represents the outcome variable for firm i at time t , $DiD_{i,t}$ is the treatment indicator. Firm fixed effects μ_i capture unobserved heterogeneity at the company level, while time fixed effects τ_t absorb market-wide shocks. To ensure reliable inference, standard errors are clustered at the firm level and Driscoll–Kraay corrections are applied in a robustness check to account for potential autocorrelation and cross-sectional dependence.

Use of AI Tools

In the course of this thesis, the author employed ChatGPT as a supplementary coding assistant. While the author acquired fundamental Python skills during his studies and independently applied them to build the empirical framework used in this research, certain complex analytical steps were supported by ChatGPT. The tool was used exclusively to improve the technical implementation of the empirical analysis. All conceptual decisions, interpretation of results and the academic writing of the thesis were carried out independently by the author.

4. Analysis of Results

This chapter presents the empirical results of the analysis. Section 4.1 examines the parallel trend assumption, providing both graphical evidence and formal statistical tests for each dependent variable and ban period. Section 4.2 reports the main estimation results of the difference-in-differences regressions, highlighting the average treatment effects of the short-selling bans on different liquidity and volatility proxies and return measures.

4.1 Parallel Trend Analysis

Difference-in-differences identifies causal effects under the assumption that, absent the intervention, treated and control units would have evolved in parallel on average. If this parallel-trends assumption fails, meaning systematic pre-ban differences in trends exist, post-ban estimates can be biased because they partly reflect pre-existing divergence rather than the policy itself. For this reason, pre-trends are explicitly tested for every outcome before any post-ban effects are discussed.

The core diagnostic is an event-study specification estimated with two-way fixed effects. For each month k relative to the ban date, lead/lag indicators are included, omitting one pre-ban month ($k=-2$) as the baseline. The model absorbs entity and time fixed effects and uses cluster-robust standard errors at the entity level. Outcomes that are strictly positive (e.g., Bid-Ask Spreads, Amihud, Rolling Volatility etc.) are analyzed on the log scale. Outcomes that may

take negative values but exceed -1 (returns) are transformed using $\log(1+x)$. Non-admissible observations are removed prior to logging. Coefficients β_k measure the treated-minus-control difference in month k relative to the baseline, net of fixed effects. The complete event study outcome tables are displayed in the appendix.

Visual inspection of the event-study plots provides an immediate check of parallelism: if the pre-ban coefficients (leads) fluctuate tightly around zero with confidence intervals that include zero, the parallel-trends assumption appears plausible. Pronounced or systematic deviations indicate potential violations. To complement the visual evidence with a formal joint test, Wald tests are conducted on groups of event coefficients.

As an additional pre-ban robustness exercise, a series of placebo DiD regressions is estimated that shifts the ban date to earlier months $k \in \{-6, \dots, -1\}$. For each placebo cut, a one-regressor DiD with entity and time fixed effects and cluster-robust errors is run. Under valid parallel trends, these placebo coefficients should be small and statistically insignificant. Systematic and significant placebo effects point to pre-existing divergence between treated and control firms.

Interpretation follows directly from these diagnostics. If the Wald test is not significant and placebo estimates are negligible, post-ban event coefficients can be treated as substantially more credible measures of policy effects. If the Wald test rejects and/or placebo estimates are systematic and significant, post-ban dynamics are interpreted with caution, noting that they may reflect the continuation of pre-existing trends rather than the causal impact of the ban.

4.1.1 Relative Bid-Ask Spread

The event-study results for $\text{Log}(\text{Spread})$ around the first short-selling ban are illustrated in figure 23. The estimated coefficients before the ban (for months $k < 0$) fluctuate closely around zero in the earlier pre-period but start to decline noticeably from $k = -3$ onward. This downward movement may visually suggest a mild anticipation effect, indicating that treated firms began to experience narrowing spreads shortly before the ban was implemented. The regression output confirms that most early pre-ban coefficients are small and statistically insignificant, while those closer to the ban date become slightly more negative. However, the formal Wald test for the pre-period yields a p-value of 0.021, leading to a rejection of the null hypothesis of equal pre-trends at the 5% level. This implies that, although the visual deviations are moderate, there is statistical evidence of slight non-parallel pre-ban dynamics between treated and control firms.

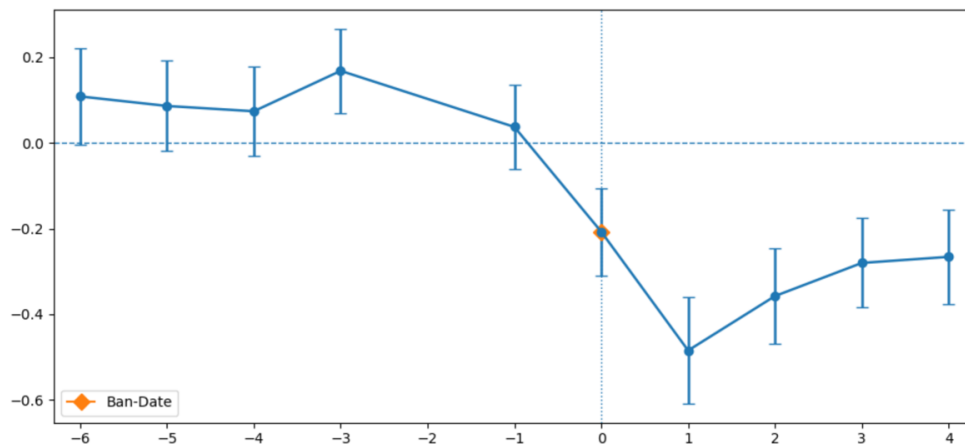


Figure 23: Event Study Log(Spread) Ban 1

The event-study results for the second short-selling ban are shown in figure 24. The estimated coefficients before the ban ($k < 0$) fluctuate closely around zero without any discernible trend and their confidence intervals consistently overlap zero. This visual pattern suggests the absence of systematic pre-ban differences between treated and control firms. While a small negative deviation appears at $k = -3$, it is short-lived and statistically insignificant, providing no clear indication of anticipatory behavior or divergent pre-trends. The regression output supports this interpretation: all pre-ban coefficients are small and statistically insignificant at conventional levels. However, the Wald test for the pre-period yields a p-value of 0.015, which leads to a rejection of the null hypothesis of identical pre-trends at the 5% significance level. This result indicates mild statistical evidence of non-parallelism despite the absence of a visible trend in the plot.

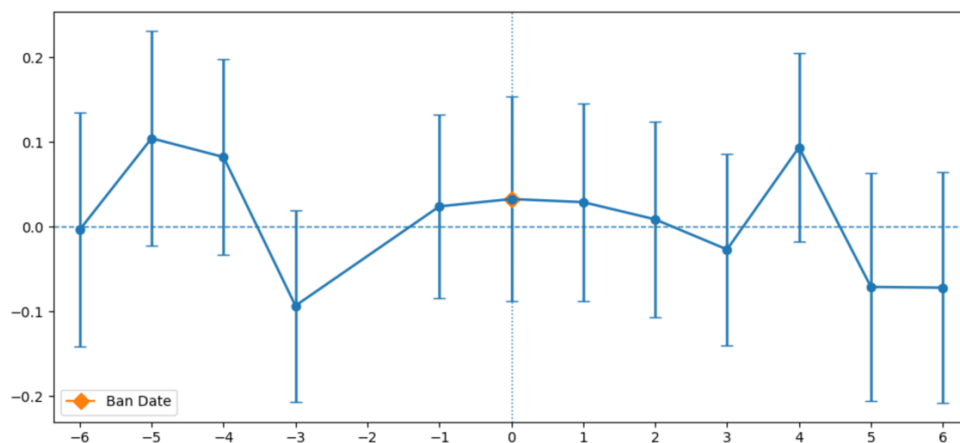


Figure 24: Log(Spread) Ban 2

Figure 25 shows that the estimated pre-ban coefficients are consistently negative, with a noticeable incline beginning around $k = -4$ to $k = -1$. This upward trajectory suggests that treated firms experienced widening Bid-Ask Spreads relative to the control group even before the intervention. Visually, this could be interpreted as an anticipation effect, indicating that market

participants may have adjusted their behavior in advance of the ban. The regression results reinforce this observation: the pre-ban coefficients are negative and, in several cases, statistically significant at conventional levels. The Wald test for the pre-period ($p = 0.005$) confirms this pattern statistically, rejecting the null hypothesis of parallel pre-trends. Thus, there is strong evidence that the parallel-trends assumption is violated prior to the ban.

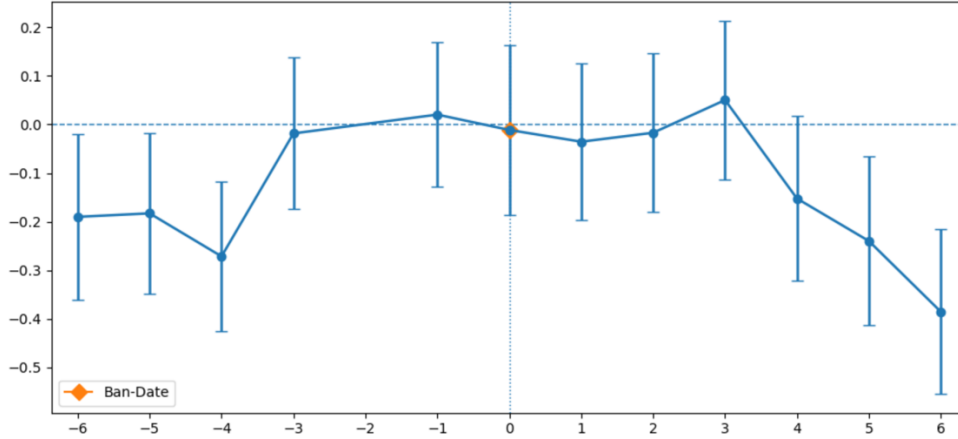


Figure 25: Log(Spread) Ban 3

As a robustness check to the event-study results, the placebo-DiD analysis for Log(Spread) examines whether the parallel-trends assumption holds in the pre-ban period (table 5). For Ban 1, all placebo coefficients from $k = -6$ to $k = -1$ are negative and highly significant (all $p < 0.001$), indicating systematically narrower spreads for treated firms already before the ban. This pattern constitutes clear evidence against parallel pre-trends for Ban 1. For Ban 2, the placebo estimates are small in magnitude and statistically insignificant across all leads (DiD between roughly -0.051 and 0.015 ; p between about 0.23 and 0.89), providing no indication of differential pre-ban dynamics and thus supporting the parallel-trends assumption. For Ban 3, the placebo coefficients are consistently positive and statistically significant throughout $k = -6$ to $k = -1$, implying that treated firms already exhibited wider spreads relative to controls ahead of the intervention.

Taken together, these placebo results reinforce the conclusions from the event studies: the parallel-trends assumption is violated for Ban 1 and Ban 3, while it appears plausible for Ban 2. Consequently, post-ban effects for Bans 1 and 3 should be interpreted with caution, as they may partly reflect pre-existing divergence, whereas Ban 2 provides a cleaner environment for causal interpretation.

	Ban 1		Ban 2		Ban 3	
Placebo k	DiD	p-value	DiD	p-value	DiD	p-value
-6	-0.243350	0.000000	0.014851	0.798818	0.345493	0.000000
-5	-0.247899	0.000000	0.014858	0.771150	0.215088	0.000070
-4	-0.258188	0.000000	-0.030115	0.507785	0.173318	0.000732
-3	-0.276090	0.000000	-0.050857	0.228234	0.186705	0.000094
-2	-0.338331	0.000000	-0.013666	0.739399	0.127921	0.002296
-1	-0.356012	0.000000	0.005703	0.889102	0.079656	0.043759

Table 5: Bid-Ask Spread placebo test summary

4.1.2 Amihud Illiquidity Index

As shown in figure 26, the pre-ban event coefficients are mostly positive and rise toward the ban date (e.g., $\beta_{-4} = 0.30$, $\beta_{-5} = 0.16$), with only the month immediately before the ban close to zero. This pattern indicates that treated firms were already less liquid than the control group in the months leading up to the intervention. The Wald test for the pre-period strongly corroborates this: $p < 0.001$. Hence, the null of parallel pre-trends is rejected and the parallel-trends assumption seems to be violated.

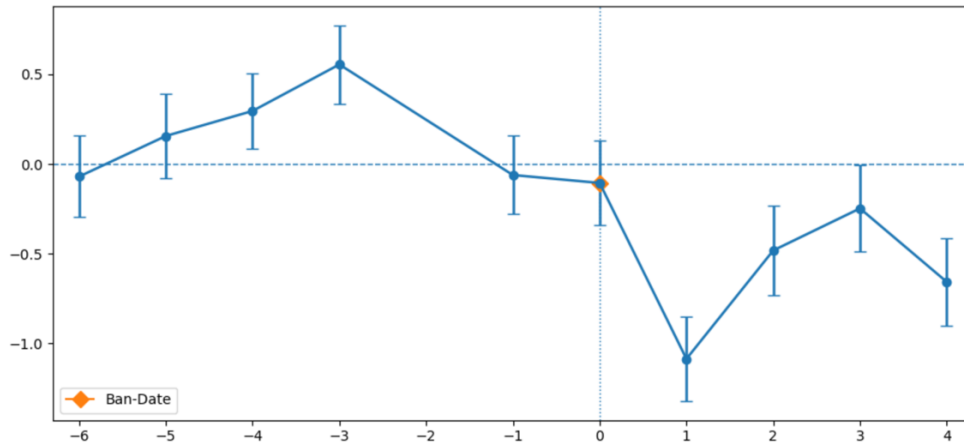


Figure 26: Log(Amihud) Ban 1

In figure 27, the pre-event coefficients fluctuate around zero with alternating signs and wide confidence intervals, rather than showing a clear upward or downward drift. The joint Wald test for the pre-ban months corroborates the visual impression: the null of parallel pre-trends cannot be rejected ($p = 0.342$). Taken together, the figure and the test indicate no systematic pre-ban divergence in liquidity between treated and control firms for Ban 2.

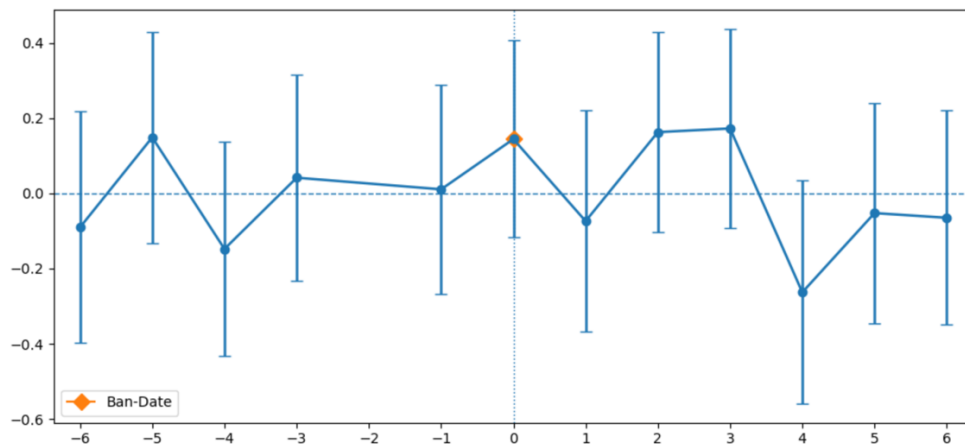


Figure 27: *Log(Amihud) Ban 2*

The event study on *Log(Amihud)* for Ban 3 (see figure 28) reveals that the estimated coefficients in the months preceding the short-selling ban are predominantly negative, suggesting that treated stocks were already somewhat more liquid than the control group prior to the regulation. The Wald test for the pre-ban period ($p < 0.01$) statistically confirms this observation, as the null hypothesis of parallel pre-trends must be rejected. Consequently, the parallel trends assumption is violated, indicating that the liquidity trajectories of treated and control firms diverged even before the policy intervention.

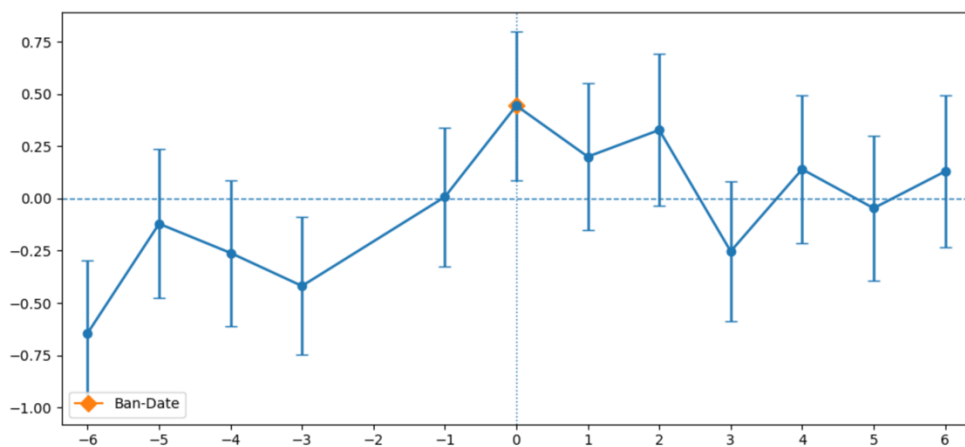


Figure 28: *Log(Amihud) Ban 3*

The placebo-DiD analysis in table 6 serves as a robustness check for the previously presented event-study results, examining whether treated and control firms followed similar pre-trends before the respective short-selling bans. For Ban 1, the placebo effects become increasingly negative and statistically significant from $k = -5$ to $k = -1$ (e.g., $\beta_{-1} = 0.593$, $p < 0.001$), while the earliest month $k = -6$ ($p = 0.158$) shows weaker and insignificant effects. This pattern indicates systematic pre-ban differences between treated and control firms, thereby violating the parallel-trends assumption for Ban 1. In contrast, for Ban 2, all placebo coefficients are small and statistically insignificant ($0.33 < p < 0.92$), providing no evidence of differential pre-

trends and supporting the validity of the parallel-trends assumption. For Ban 3, the placebo coefficients from $k = -5$ to $k = -1$ are consistently positive and highly significant (e.g., $\beta_{-1} = 0.410$, $p < 0.001$), suggesting that treated firms already exhibited higher Amihud values before the actual intervention, implying a violation of the parallel-trends assumption. Overall, these placebo-DiD results reinforce the conclusions drawn from the event studies: pre-trend violations are evident for Ban 1 and Ban 3, whereas Ban 2 displays stable pre-treatment dynamics consistent with parallel trends. Accordingly, post-ban effects for Ban 1 and Ban 3 should be interpreted with caution, as they may partly capture pre-existing divergences, while Ban 2 offers a cleaner setting for causal inference.

	Ban 1		Ban 2		Ban 3	
Placebo k	DiD	p-value	DiD	p-value	DiD	p-value
-6	-0.131238	0.158210	0.104366	0.418714	n.a.	n.a.
-5	-0.116151	0.098847	0.104643	0.329287	0.666557	0.000001
-4	-0.219430	0.001359	0.009663	0.920131	0.416745	0.000132
-3	-0.350383	0.000000	0.056937	0.517461	0.402771	0.000050
-2	-0.560932	0.000000	0.035243	0.668823	0.469883	0.000000
-1	-0.592690	0.000000	0.042680	0.577622	0.409737	0.000001

Table 6: Amihud placebo test summary

4.1.3 Relative Volatility

The event-study coefficients for Ban 1 (figure 29) in the months before the intervention are mostly small and statistically not significant (p-value between 0.12 and 0.68), but there is a clear, statistically significant uptick right before the ban ($k = -1$: $\beta = 0.222$, $p < 0.001$). The joint pre-period Wald test strongly rejects the null of equal pre-trends ($p < 0.001$), indicating that treated firms' Relative Volatility was already moving differently from the control group prior to the policy. Visually, the series rises into $k = -1$ and then collapses at the ban date ($k = 0$: $\beta = -0.713$, $p < 0.001$), a pattern consistent with an anticipation spike followed by a sharp adjustment at implementation.

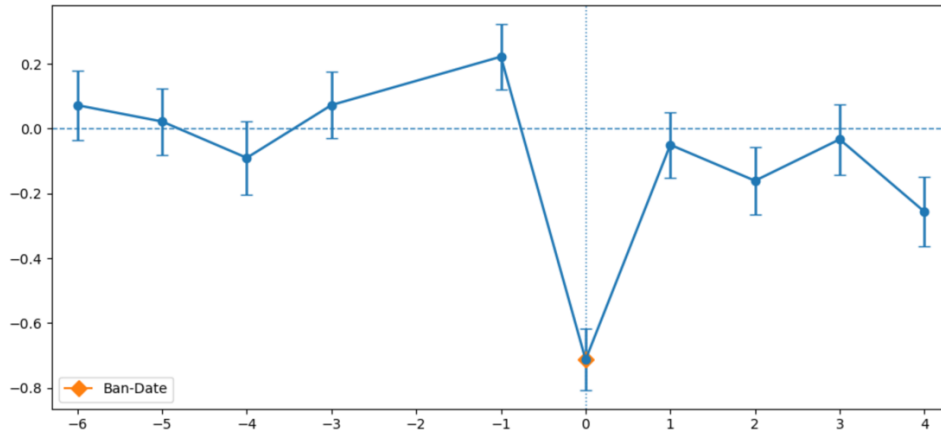


Figure 29: Log(RVOL) Ban 1

For Ban 2, the event-study results (figure 30) indicate relatively stable pre-ban dynamics between treated and control firms. The estimated coefficients before the ban fluctuate around zero without a consistent trend and none are strongly significant apart from minor effects at $k = -6$ ($\beta = -0.150$, $p = 0.032$) and $k = -1$ ($\beta = -0.158$, $p = 0.027$). The Wald test for the pre-period confirms the absence of systematic deviations, failing to reject the null of parallel pre-trends ($p = 0.188$). This suggests that, overall, treated and control stocks followed comparable volatility trajectories prior to the intervention.

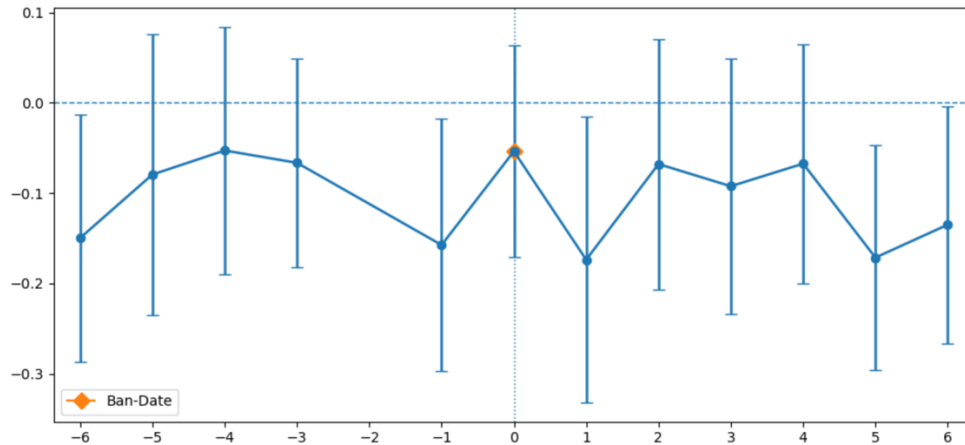


Figure 30: Log(RVOL) Ban 2

The event-study for Ban 3 (figure 31) shows a slight downward pattern in the pre-ban months: several lead coefficients are negative and statistically different from zero, indicating that treated firms already exhibited lower Relative Volatility than the control group before the intervention. The Wald test for the pre-period strongly rejects parallel pre-trends ($p < 0.001$), so the identifying assumption is violated for Ban 3.

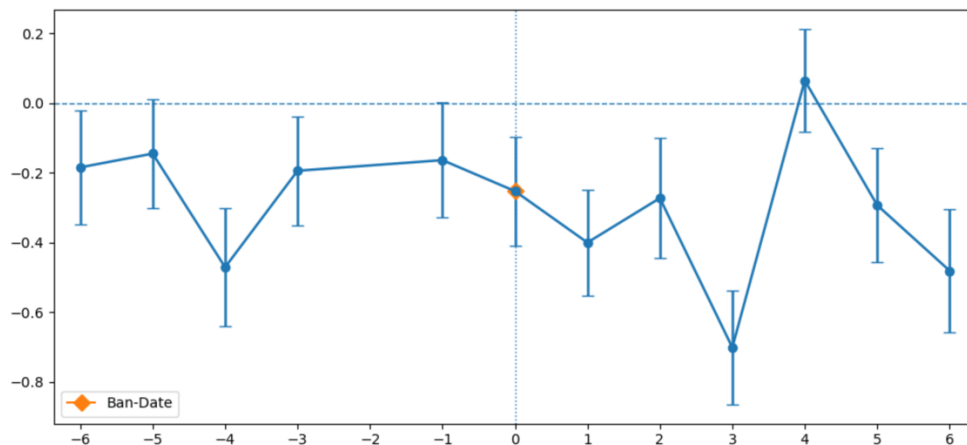


Figure 31: Log(RVOL) Ban 3

The robustness check to the RVOL event-study results (see table 7) exhibits the following results. Ban 1 shows uniformly negative and highly significant placebo effects across all pre-ban leads (all $p < 0.001$), indicating systematic pre-trend differences between treated and control firms and thus a violation of the parallel-trends requirement. Ban 2 exhibits very small and statistically insignificant placebo coefficients throughout (p between 0.20 and 0.93), providing no evidence of differential pre-trends and therefore supporting the parallel-trends assumption for this episode, which is consistent with the benign pre-ban profile in the event study. Ban 3 shows mostly negative and significant placebo effects in the more distant leads ($k = -6, -5, -4$ and -1 ; $p < 0.03$), while the coefficients nearer to the ban ($k = -3, -2$) are small and not significant. This pattern suggests meaningful pre-ban divergence for Ban 3 as well, in line with the event-study evidence that rejects parallel pre-trends. Overall, the placebo checks corroborate the interpretation that causal post-ban effects are most credible for Ban 2, whereas estimates for Ban 1 and Ban 3 should be interpreted with caution due to pre-existing differences.

	Ban 1		Ban 2		Ban 3	
Placebo k	DiD	p-value	DiD	p-value	DiD	p-value
-6	-0.370526	0.000000	-0.076424	0.198888	-0.140761	0.030099
-5	-0.278534	0.000000	-0.004173	0.931719	-0.120019	0.029998
-4	-0.239111	0.000000	-0.005780	0.902584	-0.135130	0.002274
-3	-0.187097	0.000000	-0.016360	0.709788	-0.038982	0.353542
-2	-0.214045	0.000000	-0.020606	0.625429	-0.054452	0.143395
-1	-0.225469	0.000000	-0.038995	0.356596	-0.123017	0.000423

Table 7: RVOL placebo test summary

4.1.4 Rolling Volatility

The event-study profile for Log(RollV) exhibits clear pre-ban movements rather than a flat pre-trend. Several pre-treatment coefficients are sizable and statistically significant. Consistent with the visual pattern, the Wald test for the pre-period strongly rejects the joint null that all pre-

coefficients equal zero ($p < 0.001$), indicating that treated and control firms did not follow parallel trends prior to the ban.

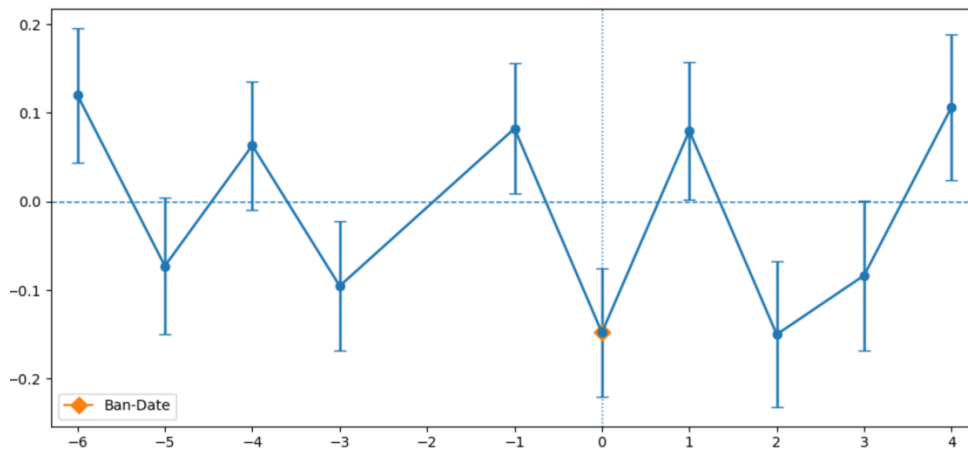


Figure 32: Log(RollV) Ban 1

The pre-ban coefficients in the event study (see figure 33) fluctuate narrowly around zero and are mostly insignificant. Jointly, the Wald test for the pre-period does not reject parallel trends ($p = 0.301$), which is consistent with the visual impression of small, alternating pre-treatment estimates. One exception is the month right before the ban ($k = -1$), where the regression table reports a modest negative coefficient ($p = 0.026$). While this single lead hints at a slight dip just before the intervention, it is not strong enough in combination with the other leads to overturn the pre-trend test.

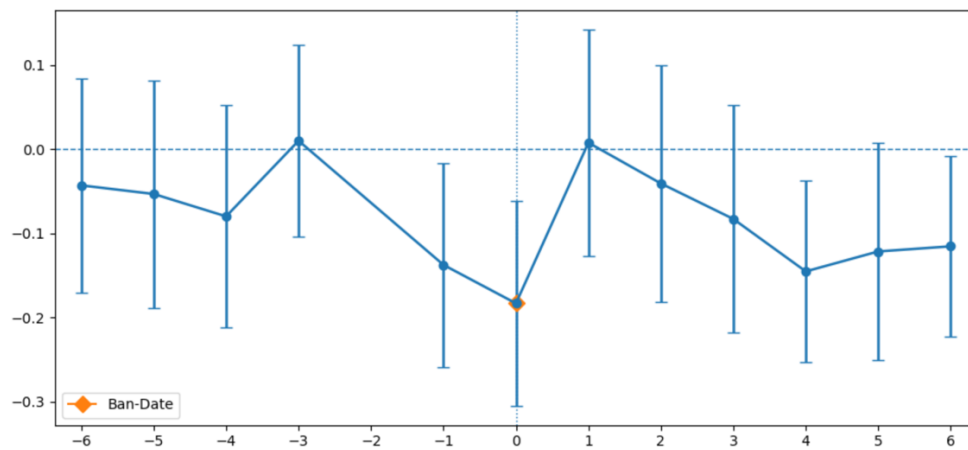


Figure 33: Log(RollV) Ban 2

The event-study coefficients display a clear pattern already before the ban. From $k = -6$ to $k = -1$ the pre-treatment estimates are uniformly negative and precisely estimated in the panel regression (all $p < 0.001$). Visually, the series inclines toward the ban date from $k = -4$ to $k = -1$ (see figure 34) and the joint Wald test for the pre-period rejects equality to zero very strongly ($p < 0.001$). Hence, the parallel-trends assumption is violated for Ban 3 and treated firms' Rolling Volatility was already deviating relative to the control group prior to the intervention.

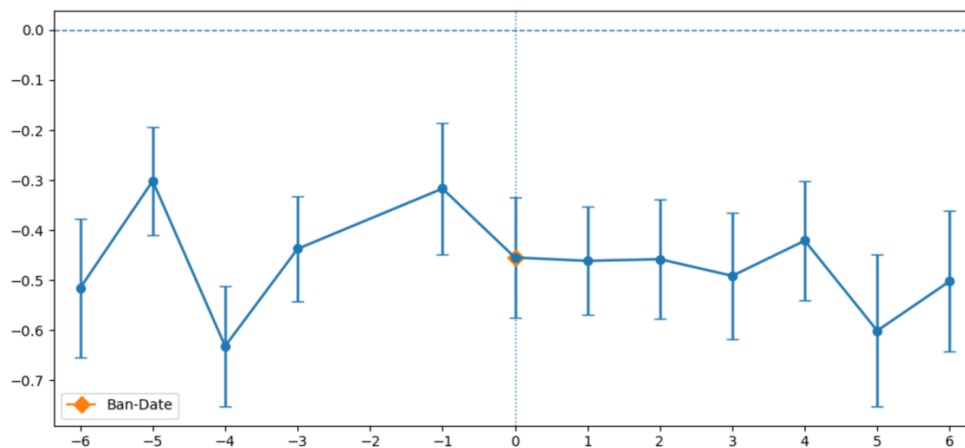


Figure 34: Log(RollV) Ban 3

For Ban 1, the placebo estimates are consistently negative (see table 8) and highly significant across all leads ($p < 0.001$), ranging from -0.52 at $k = -6$ to -0.11 at $k = -1$. This strong and systematic decline indicates that the treated firms already experienced substantially lower Rolling Volatility before the actual ban, implying that parallel trends are not satisfied for Ban 1. Consequently, the estimated post-ban effects in the event study for Ban 1 should be interpreted with caution, as pre-existing differences likely drive much of the observed variation. In contrast, for Ban 2, all placebo coefficients are small and statistically insignificant, with p-values between 0.11 and 0.30. This finding supports the validity of the parallel-trends assumption in the pre-ban period and strengthens confidence in the event study interpretation for Ban 2, where observed post-ban changes are less likely to stem from pre-existing volatility dynamics. Finally, for Ban 3, the placebo coefficients are statistically insignificant for most leads, except for $k = -1$ ($p = 0.014$). Although this single significant pre-period point suggests a minor deviation before the event, the overall pattern does not reveal a systematic pre-trend. Thus, the placebo evidence for Ban 3 is largely consistent with parallel pre-ban trajectories, but the significant estimate at $k = -1$ warrants some caution when attributing post-ban effects solely to the intervention. Overall, the placebo DiD results reinforce the event study findings: while Ban 2 and, to a lesser extent, Ban 3 satisfy the parallel-trends assumption and show credible post-ban effects, Ban 1 clearly violates it, limiting the interpretability of its estimated treatment impact.

	Ban 1		Ban 2		Ban 3	
Placebo k	DiD	p-value	DiD	p-value	DiD	p-value
-6	-0.523710	0.000000	-0.136480	0.107531	n.a.	n.a.
-5	-0.339154	0.000000	-0.084018	0.166821	0.086049	0.175436
-4	-0.203045	0.000000	-0.064101	0.208456	-0.031435	0.545000
-3	-0.181747	0.000000	-0.046031	0.303310	0.060293	0.157252
-2	-0.120939	0.000000	-0.062788	0.137551	0.050334	0.177732
-1	-0.105906	0.000003	-0.066534	0.115918	-0.090763	0.013922

Table 8: RollV placebo test summary

4.1.5 Parkinson Volatility

Figure 35 shows that the series of pre-ban coefficients increases in the month immediately preceding the intervention. Estimates are close to zero for $k = -6$ to $k = -3$, then turn clearly positive at $k = -1$ (around +0.45 on the log scale), before plunging at the ban month to a large negative value. The regression output confirms this pattern, as the pre-period dummy for $k=-1$ is positive and highly significant (0.446, $p < 0.001$), while earlier leads are individually small and statistically insignificant. The joint Wald test confirms that the pre-period coefficients are decisively different from zero ($p < 0.001$), rejecting the parallel-trends assumption and indicating an anticipatory increase in volatility for treated firms immediately before the ban.

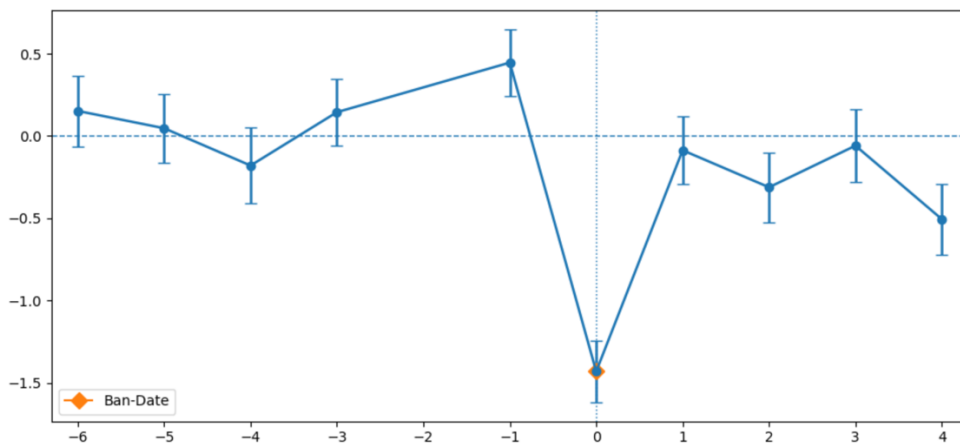


Figure 35: Log(PVOL) Ban 1

Figure 36 shows that the pre-ban coefficients fluctuate slightly below zero between $k = -6$ and $k = -3$, indicating relatively stable volatility levels before the intervention. Around $k = -1$, a minor negative deviation is visible, followed by a slight recovery around the event month ($k = 0$). Overall, the pre-event pattern does not suggest any pronounced anticipatory behavior or systematic divergence between treatment and control firms prior to the ban. This interpretation is supported by the regression results and Wald tests. Individually, none of the pre-period coefficients are statistically significant except for the coefficient of $k = -6$, which is weakly negative (-0.302, $p = 0.031$). The joint pre-ban Wald test also fails to reject the null of equality to zero ($p = 0.179$), implying that the parallel-trends assumption cannot be rejected.

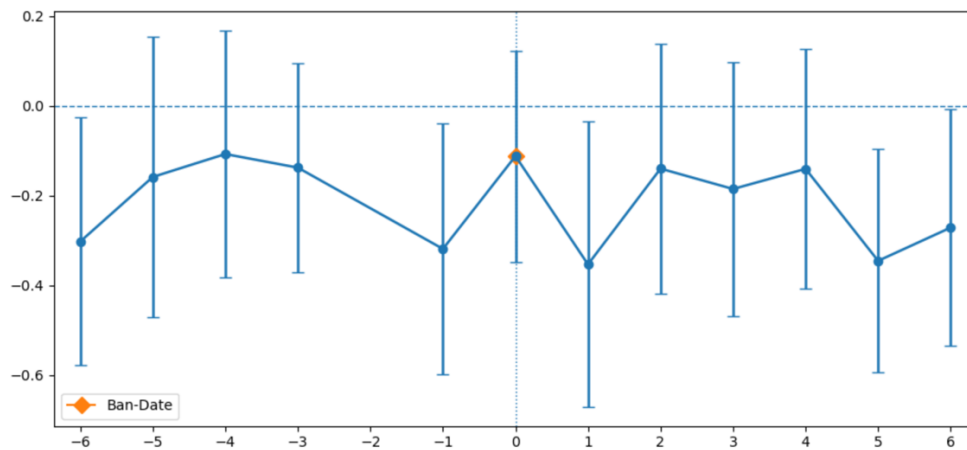


Figure 36: Log(PVOL) Ban 2

Figure 37 illustrates that the pre-ban coefficients for Ban 3 are consistently negative across all months prior to the intervention, suggesting that treated firms already exhibited lower Parkinson Volatility compared to control firms before the ban. The regression output strongly supports this visual impression. Most pre-ban dummies are negative and several are statistically significant indicating that treated firms already experienced lower volatility prior to the ban. The Wald test for the pre-period rejects the null hypothesis of equal pre-ban coefficients ($p < 0.001$), providing clear evidence of pre-trend differences and a violation of the parallel-trends assumption.

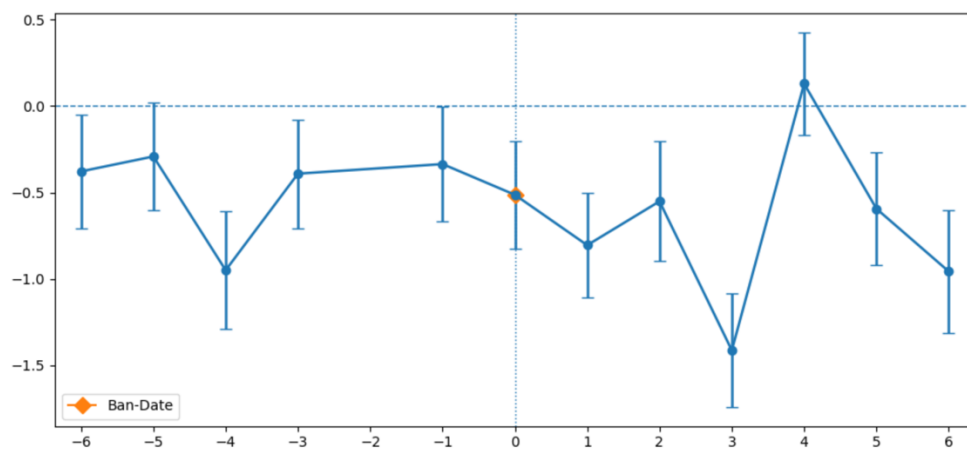


Figure 37: Log(PVOL) Ban 3

The following table shows that for Ban 1, all placebo coefficients are large, negative and highly significant across the entire pre-ban window. This pattern indicates that treated firms already experienced significantly lower Parkinson Volatility compared to control firms before the intervention, thereby violating the parallel-trends assumption and suggesting that part of the observed post-ban effect may reflect pre-existing differences rather than a true policy impact. In contrast, Ban 2 shows small and statistically insignificant placebo coefficients throughout the pre-ban window, with p-values ranging between 0.20 and 0.93. This stability implies the

absence of systematic differences between treatment and control firms prior to the intervention, supporting the validity of the parallel-trends assumption and strengthening the causal interpretation of the post-ban results for this event. For Ban 3, the placebo results reveal several significant pre-ban coefficients, particularly at $k = -6$, $k = -5$, $k = -4$ and $k = -1$, while the middle pre-ban months ($k = -3$ and $k = -2$) remain insignificant. This mixed but overall negative and significant pattern suggests that treated firms had already exhibited lower volatility levels before the ban, indicating a deviation from perfect parallel trends.

Taken together, table 9 displays that the placebo-DiD evidence aligns closely with the findings from the event studies. While Bans 1 and 3 display clear signs of pre-trend divergence and should therefore be interpreted with caution, Ban 2 exhibits stable pre-treatment dynamics, providing the strongest empirical basis for a credible causal interpretation of the policy's effect on Parkinson Volatility.

	Ban 1		Ban 2		Ban 3	
Placebo k	DiD	p-value	DiD	p-value	DiD	p-value
-6	-0.737022	0.000000	-0.153484	0.198974	-0.283538	0.029598
-5	-0.557173	0.000000	-0.008638	0.929705	-0.238414	0.031425
-4	-0.477977	0.000000	-0.013105	0.890130	-0.270181	0.002347
-3	-0.372833	0.000000	-0.034292	0.697745	-0.076842	0.361694
-2	-0.425257	0.000000	-0.041919	0.620883	-0.108409	0.146188
-1	-0.446972	0.000000	-0.079094	0.351571	-0.246757	0.000415

Table 9: *ParkV* placebo test summary

4.1.6 Excess Returns

Figure 38 shows that pre-ban coefficients hover near zero for $k = -5$ to $k = -4$, dip modestly at $k = -3$ and then rise to a clearly positive estimate at $k = -1$. The regression output is consistent with this pattern. Among the pre-period periods, $k = -6$, $k = -4$, $k = -3$ and $k = -1$ are significantly different from zero (all $p < 0.001$), while the coefficient for $k = -5$ is insignificant ($p = 0.711$). Joint tests underline this: the pre-period Wald test rejects equality of the pre-ban coefficients ($p < 0.001$), indicating systematic differences in pre-ban dynamics between treated and control firms.

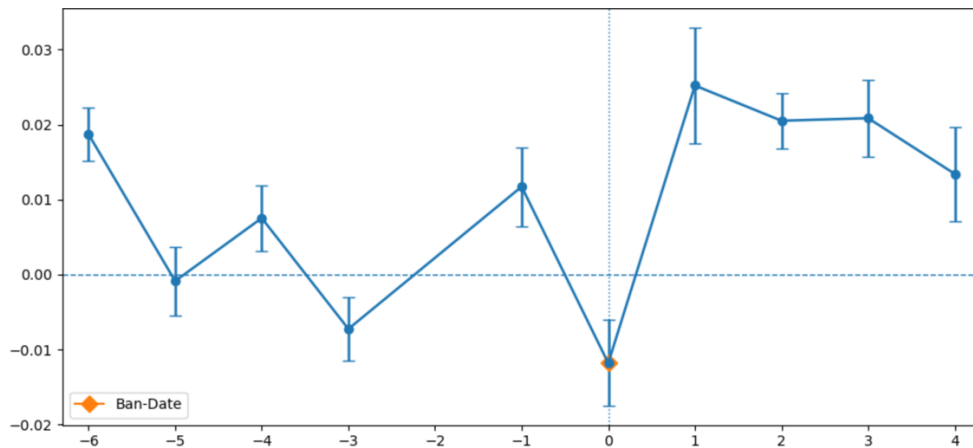


Figure 38: $\text{Log}(1+\text{Excess Return})$ Ban 1

For Ban 2, the event-study plot for Excess Returns (see figure 39) shows a fairly flat and close-to-zero pattern before the ban. From $k = -6$ to $k = -1$ the coefficients hover around a few basis points (slightly negative at most) and there is no systematic drift into the ban month. The individual pre-period estimates for $k = -6$ and $k = -1$ are modestly negative and statistically significant ($p = 0.027$ and $p = 0.032$) but the remaining leads are small and imprecise. Consistent with the visual impression, the joint pre-period Wald test does not reject equality of the pre-ban coefficients at conventional 5% levels ($p = 0.082$). Taken together, the evidence points to no strong pre-trend differences between treated and control firms for Ban 2, at most a weak signal at the 10% level due to the two isolated negative leads. So the parallel-trends assumption appears broadly plausible in this case.

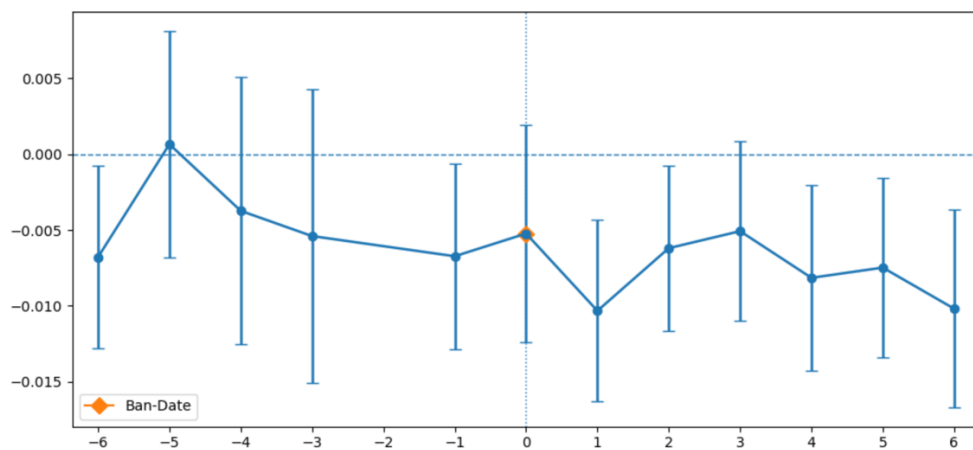


Figure 39: $\text{Log}(1+\text{Excess Return})$ Ban 2

Figure 40 shows that the pre-ban event-time coefficients are consistently negative from $k = -6$ to $k = -1$, with $k = -4$ being the least negative. In particular, the series moves from roughly -0.018 at $k = -6$ to about -0.018 again at $k = -1$, indicating that treated firms earned lower excess returns than controls throughout the months leading up to the intervention. The regression estimates mirror this visual impression. Several pre-ban dummies are negative and statistically

significant pointing to systematic pre-treatment differences in excess returns. Joint tests underline this: the Wald pre-ban test rejects equality of the pre-ban coefficients ($p < 0.001$), implying a violation of the parallel-trends assumption.

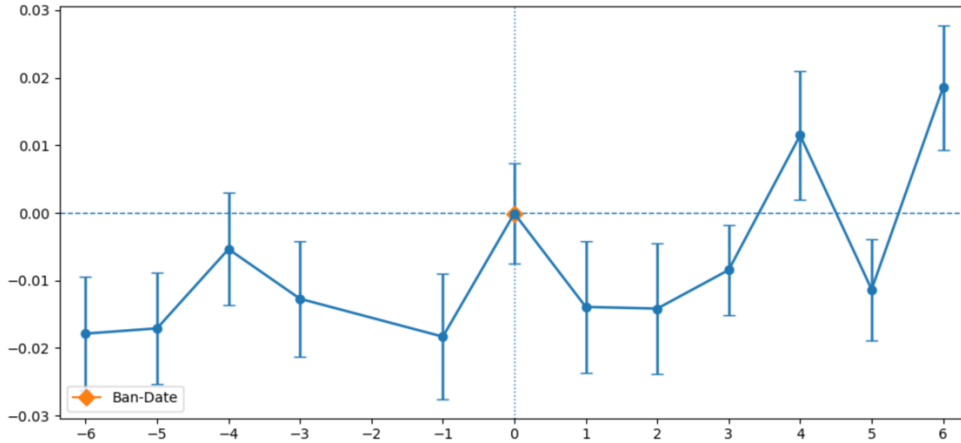


Figure 40: $\text{Log}(1+\text{Excess Return})$ Ban 3

The results of the placebo difference-in-differences regressions in table 10 broadly confirm the patterns observed in the event studies. For Ban 1, the placebo results display systematic and statistically significant deviations well before the true policy date. In particular, the coefficients for $k = -6$, $k = -5$ and $k = -4$ are negative and highly significant, while the coefficients closer to the actual intervention turn positive. This clear pre-event drift confirms that treated and control firms did not follow parallel trends prior to the ban, reinforcing the event study's conclusion that the parallel-trends assumption is violated for Ban 1. In contrast, the placebo estimates for Ban 2 are small in magnitude and statistically insignificant across all placebo months (p-values ranging from 0.16 to 0.97, with only a marginal result at $k = -1$, $p = 0.073$). This pattern suggests an absence of systematic pre-trends and supports the credibility of the parallel-trends assumption in the case of Ban 2. For Ban 3, the placebo estimates are uniformly positive and strongly significant across all pre-ban months, indicating that treated firms already exhibited higher excess returns than controls well before the intervention. This consistent pre-ban divergence mirrors the event-study findings and signals a clear violation of the parallel-trends assumption.

Taken together, the placebo-DiD results provide a robustness check that aligns closely with the event study analysis: Ban 1 and Ban 3 display strong evidence of pre-trend differences, casting doubt on a strict causal interpretation of their post-ban effects, whereas Ban 2 shows no indication of such pre-trends, lending greater confidence to its post-ban estimates.

	Ban 1		Ban 2		Ban 3	
Placebo k	DiD	p-value	DiD	p-value	DiD	p-value

-6	-0.015693	0.006193	-0.001885	0.514300	0.012307	0.000001
-5	-0.012999	0.000000	-0.000090	0.966656	0.012877	0.000000
-4	-0.004329	0.000720	-0.002548	0.187467	0.008908	0.000000
-3	-0.002259	0.103515	-0.002611	0.161696	0.009544	0.000000
-2	0.004365	0.025036	-0.002255	0.219245	0.006479	0.000004
-1	0.008081	0.001537	-0.003077	0.073073	0.012307	0.000001

Table 10: Excess Return placebo test summary

4.2 Main Results

This section reports the main estimation results for the three short-selling bans. For each dependent variable per ban period, a separate difference-in-differences regression with firm and day fixed effects is estimated. The coefficients capture the average treatment effect of the short-selling bans on various measures of liquidity, volatility and return measures.

Each table presents the results for the respective ban. Each column corresponds to a different dependent variable, including the bid–ask spread, Amihud Illiquidity, Relative Volatility, Rolling Volatility, Parkinson Volatility and excess returns. Standard errors are reported in parentheses, using both firm-clustered and Driscoll–Kraay robust (AR1) specifications. All regressions include firm and day fixed effects and the within R^2 is reported for model fit.

4.2.1 Main Results of Ban 1

The estimated DiD coefficients for the Bid–Ask Spread are negative and highly significant across all specifications, indicating that the introduction of the short-selling ban was associated with a notable reduction in Spreads for the treatment group relative to the control group. In the baseline regression with firm-clustered standard errors, the coefficient of -0.424 implies an average decline of around 35% in the logarithmic spread after the ban, a result that remains robust when applying AR(1)-corrected standard errors. As shown in the graphical representation of the average Bid-Ask Spread in Section 3.2.1, the treatment group already exhibited lower spreads than the control group before the ban, suggesting a structural level difference between the two groups. Following the ban’s introduction in March 2020, spreads declined further for the treatment group, while they rose sharply for the control group. This divergence accounts for the strong negative DiD estimate and reflects a relative improvement in liquidity among banned firms. However, the placebo analysis (chapter 4.1.1) reveals that this gap partly existed before the intervention. The placebo coefficients for the pre-ban months ($k = -6$ to -1) are already negative and significant, implying that part or all of the estimated effect may reflect pre-existing differences rather than a causal impact of the ban.

For the Amihud Illiquidity measure, the estimated DiD coefficients are negative and highly significant across both specifications. The coefficient of -0.85 suggests that the short-selling ban was associated with a considerable reduction in illiquidity for the treatment group compared with the control group, indicating an improvement in market liquidity after the policy intervention. The effect remains robust under AR(1)-corrected standard errors, supporting the stability of the result. The graphical evidence from Section 3.2.2 illustrates a similar dynamic pattern. Before the ban, the treatment and control groups followed similar trajectories. After the introduction of the ban in March 2020, the Amihud values for the treatment group dropped sharply, while those for the control group increased in their mean, resulting in a widening gap consistent with the regression findings. However, the placebo analysis presented in chapter 4.1.2 reveals that the difference between both groups started to emerge in the months leading up to the ban. Placebo coefficients for $k = -6$ to $k = -1$ are already negative and mostly statistically significant, indicating that the treatment group's liquidity was improving relative to the control group even before the intervention. Therefore, while the regression results point to a strong decline in Amihud Illiquidity following the short-selling ban, part or all of the estimated effect may capture pre-existing dynamics rather than a causal policy impact.

Across all three volatility measures, meaning Relative Volatility, Rolling Volatility and Parkinson Volatility, the estimated DiD coefficients are uniformly negative and statistically significant. These results suggest that the introduction of the short-selling ban was associated with lower return volatility for the treatment group relative to the control group, indicating a potential stabilizing effect on price fluctuations. The magnitude of the coefficients is economically meaningful, with reductions of approximately 9% for RVOL, 4% for RollV and 12–18% for ParkV, depending on the specification. However, the validity of these effects is limited by the lack of parallel pre-trends. The placebo analysis indicate that the treatment group already exhibited systematically lower volatility levels in the months preceding the ban. In particular, the placebo coefficients for all pre-ban periods ($k = -6$ to -1) are consistently negative and statistically significant, implying that volatility differences between the two groups existed well before the policy intervention. This persistent pre-treatment gap suggests that the estimated post-ban declines in volatility may not solely be driven by the short-selling restrictions themselves but are at least partly and possibly entirely, attributable to the pre-existing differences in volatility trends between the treatment and control groups.

For excess returns, the estimated DiD coefficients are small in magnitude and only marginally significant under the AR(1)-robust specification. The coefficients of approximately 0.005

indicate that, on average, the short-selling ban did not lead to any economically meaningful change in excess returns for the treatment group relative to the control group. Moreover, the pre-treatment trends were not parallel, suggesting that the observed differences in excess returns cannot be clearly attributed to the ban itself.

	Std. errors	DiD	Day FE	Stock-level FE	Nr of obs.	Parallel trend	R ²
Spread	CE	-0.4244*** (0.0205)	Yes	Yes	143,830	No	0.0541
	AR1	-0.4244*** (0.0237)	Yes	Yes	143,830	No	0.0541
Amihud	CE	-0.8455*** (0.0506)	Yes	Yes	137,271	No	0.0462
	AR1	-0.8455*** (0.0627)	Yes	Yes	137,271	No	0.0462
RVOL	CE	-0.0930*** (0.0186)	Yes	Yes	143,721	No	0.0696
	AR1	-0.0930*** (0.0186)	Yes	Yes	143,283	No	0.0696
RollV	CE	-0.0440** (0.0216)	Yes	Yes	151,207	No	0.0316
	AR1	-0.0440** (0.0370)	Yes	Yes	151,207	No	0.0316
ParkV	CE	-0.1852*** (0.0372)	Yes	Yes	143,721	No	0.0695
	AR1	-0.1157** (0.0497)	Yes	Yes	142,691	No	0.0695
ExcessR	CE	0.0047 (0.0036)	Yes	Yes	153,372	No	0.0012
	AR1	0.0047* (0.0025)	Yes	Yes	153,372	No	0.0012

Reported R² is within R² from a two-way fixed effects regression (time and firm fixed effects)

*** p < 0.01, ** p < 0.05, * p < 0.10**

Table 11: Ban 1 main results

4.2.2 Main Results of Ban 2

The DiD coefficient for the log bid–ask spread is very small ($\beta \approx -0.006$) and statistically insignificant under both firm-clustered and AR(1) standard errors. Economically, this corresponds to roughly a 0.6% decline in spreads for treated firms after the partial ban lift. The within-R² is near zero which is common with two-way FEs. Because the parallel-trend test is satisfied, the finding can be interpreted credibly: conditional on parallel pre-trends, the ban did not materially affect quoted spreads in this sample.

For Amihud, the DiD estimate is $\beta \approx -0.045$. With firm-clustered errors it is not significant, but with AR(1)-robust errors it is significant at the 5% level. In magnitude, this implies about a 4.5% reduction in Amihud Illiquidity for treated firms post ban lift. Again, the parallel-trend

condition holds, which supports a causal reading of this modest decrease, while acknowledging the low explanatory power typical for FE models (very low within- R^2).

For all three volatility measures (RVOL, RollV, ParkV) the estimated DiD coefficients are negative, indicating that volatility declined for the treatment group after the lifting of the short-selling ban, relative to the control group, which remained subject to the ban. Although the estimated magnitudes are moderate, the consistent negative sign across all three proxies points toward a stable and meaningful pattern. For Relative Volatility, the coefficient of -0.0376 corresponds to an average decline of roughly 3.7% in volatility after the ban was lifted. The result is statistically significant under the AR(1)-robust specification but not under firm-clustered standard errors, suggesting that significance depends on the treatment of serial correlation. Similarly, for Rolling Volatility, the coefficient of -0.0471 implies a 4.6% reduction in volatility for firms in the treatment group. This effect is highly significant at the 1% level when AR(1)-robust standard errors are used, whereas the clustered specification produces a less precise estimate. Finally, for Parkinson Volatility, the clustered coefficient (-0.0752) is not statistically significant, but the AR(1)-robust estimate (-0.0444) is significant at the 1% level, corresponding to a reduction in volatility of about 4.3%. Overall, these results suggest that the lifting of short-selling restrictions was followed by a decline in volatility among the newly unbanned firms, while volatility remained higher for firms that continued to face short-selling constraints. Importantly, the parallel trend assumption is satisfied for all three volatility measures, implying that both groups displayed similar pre-ban volatility dynamics. Hence, the post-ban decline can be more confidently attributed to the policy change rather than pre-existing differences. Nevertheless, the relatively low within- R^2 values indicate that the model explains only a small fraction of overall volatility variation

With the ban lifted for the treatment firms, the DiD coefficient is -0.0014 and statistically significant at the 5% level under both firm-clustered and AR(1)-robust errors. Interpreting excess returns in daily decimal units, this corresponds to an average -0.14 percentage point underperformance of the newly unbanned stocks relative to the still-banned control group in the post period. The parallel-trend tests are satisfied, so this small negative post-ban gap is identified off comparable pre-ban trends. Economically, however, the effect is modest and the very low within- R^2 indicates limited explanatory power so the lifting of the ban appears to have at most a minor impact on relative performance.

	Std. errors	DiD	Day FE	Stock- level FE	Nr of obs.	Parallel trend	R^2
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Spread	CE	-0.0062 (0.0274)	Yes	Yes	81,455	Yes	3.43e-05
	AR1	-0.0062 (0.0075)	Yes	Yes	81,455	Yes	3.43e-05
Amihud	CE	-0.0452 (0.0467)	Yes	Yes	77,435	Yes	-0.0027
	AR1	-0.0452** (0.0198)	Yes	Yes	77,435	Yes	-0.0027
RVOL	CE	-0.0376 (0.0290)	Yes	Yes	81,525	Yes	0.0025
	AR1	-0.0376** (0.0128)	Yes	Yes	81,525	Yes	0.0025
RollV	CE	-0.0471 (0.0373)	Yes	Yes	80,422	Yes	0.0062
	AR1	-0.0471*** (0.0087)	Yes	Yes	80,422	Yes	0.0062
ParkV	CE	-0.0752 (0.0582)	Yes	Yes	81,525	Yes	0.0025
	AR1	-0.0444*** (0.0144)	Yes	Yes	81,525	Yes	0.1992
ExcessR	CE	-0.0014** (0.0007)	Yes	Yes	82,082	Yes	-0.0001
	AR1	-0.0014** (0.0004)	Yes	Yes	82,082	Yes	-0.0001

Reported R^2 is within R^2 from a two-way fixed effects regression (time and firm fixed effects)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ **

Table 12: Ban 2 main results

4.2.3 Main Results of Ban 3

The DiD estimates in the following table show a sizable and precisely estimated reduction in the spread for treated stocks relative to controls (DiD ≈ -0.125 , $p < 0.001$ under both clustered and AR(1) errors). Interpreted on the log scale, this corresponds to roughly an 11–12% lower spread for the treatment group after the ban window. However, the time-series plot for Ban 3 in chapter 3.2.1 makes clear that treated firms already had materially lower spreads than the control firms before the intervention. Around the ban date, the average spread of the treatment group remains low and fairly stable, while the control group's spread increases, which mechanically produces a negative DiD. Consistent with this, the formal parallel-trends checks for Ban 3 indicate that the assumption is not satisfied. Taken together, while the regression yields a negative and statistically significant DiD coefficient, the pre-existing level difference and failed parallel-trends tests suggest that a meaningful part or potentially the entirety of this estimated effect can be attributed to pre-ban differences in spreads rather than a causal impact of the reintroduced short-selling restrictions.

The DiD estimates indicate an increase for the Amihud Index for the treated firms relative to the control group once the third ban was implemented. The Amihud coefficient is positive and

highly significant across both standard-error specifications ($\text{DiD} \approx 0.196$; $p < 0.01$). In logarithmic terms, this corresponds to roughly a 22% increase in the Amihud Illiquidity measure for the treated firms compared with the control group after the ban, suggesting a notable decline in trading liquidity among the newly banned stocks. The graphical evidence shown in chapter 3.2.2 supports this finding: following the introduction of the ban, the average Amihud value for the treatment group rises, whereas the control group experiences a decline, visually reinforcing the positive post-ban difference captured in the regression. However, the parallel trend assumption is violated, as the treatment group already exhibited higher Amihud values prior to the ban. This is further confirmed by the placebo analysis in chapter 4.1.2, where the pre-ban coefficients ($k = -5$ to -1) are positive and statistically significant, indicating persistent pre-existing differences and an upward trend for the treatment firms even before the policy intervention. Therefore, while the regression and figure both suggest worsening liquidity after the ban, the magnitude of this effect likely reflects not only the policy change itself but also underlying pre-ban disparities between the two groups.

For Ban 3, when short-selling restrictions were reintroduced for all Korean firms, the results for the three volatility measures (RVOL, RollV and ParkV) consistently show negative and highly significant DiD coefficients. The estimates suggest that, following the renewed short-selling ban, volatility among Korean firms declined relative to Japanese firms. Specifically, the coefficients indicate reductions of approximately 16% for Relative Volatility 9% for Roll volatility and around 29% for Parkinson Volatility, implying a broad decrease in price fluctuations after the policy intervention. These results are robust across both firm-clustered and Driscoll–Kraay (AR1) standard errors, confirming their statistical precision. However, when viewed together with the placebo analysis, the interpretation becomes more nuanced. The pre-ban placebo tests show that the parallel-trend assumption is not satisfied for any of the three measures. For Parkinson Volatility, all placebo coefficients for the months preceding the ban are strongly negative and statistically significant, indicating that Korean firms already exhibited substantially lower volatility levels than their Japanese counterparts even before the restrictions were reinstated. A similar pattern appears for Relative Volatility, where consistently negative and significant pre-ban coefficients suggest that the treatment group was already less volatile in the months leading up to the ban. The case of Rolling Volatility is somewhat weaker, as its placebo coefficients are smaller and occasionally positive, but still hint at mild pre-treatment divergence between the groups. Taken together, these findings imply that the observed post-ban reduction in volatility for Korean firms cannot be interpreted as a purely causal effect of the short-selling restrictions. Instead, it likely reflects a combination of the market stabilization

following the policy announcement and pre-existing differences between Korean and Japanese firms, which had already shown lower volatility levels before the intervention. Therefore, while the regression results suggest improved market stability in Korea after the ban, part of this apparent improvement may stem from structural differences rather than the policy itself.

The DiD coefficient for excess returns is very small with an increase of 0.0013 in the post-ban period. On the log scale this corresponds to an increase of roughly 0.13%. Moreover, the parallel-trend assumption is not satisfied, so any apparent post-ban change in excess returns should be interpreted with caution. Taken together, the results provide no convincing evidence that the reintroduced ban had a meaningful effect on relative stock performance.

	Std. errors	DiD	Day FE	Stock-level FE	Nr of obs.	Parallel trend	R ²
Spread	CE	-0.1251*** (0.0270)	Yes	Yes	66,982	No	-0.0124
	AR1	-0.1251*** (0.0222)	Yes	Yes	66,982	No	-0.0124
Amihud	CE	0.1959*** (0.0563)	Yes	Yes	64,939	No	-0.0029
	AR1	0.1959*** (0.0414)	Yes	Yes	64,939	No	-0.0029
RVOL	CE	-0.1726*** (0.0269)	Yes	Yes	66,992	No	-0.0103
	AR1	-0.1726*** (0.0275)	Yes	Yes	66,992	No	-0.0103
RollV	CE	-0.0978*** (0.0320)	Yes	Yes	70,434	No	-0.0145
	AR1	-0.0978*** (0.0210)	Yes	Yes	70,434	No	-0.0145
ParkV	CE	-0.3446*** (0.0539)	Yes	Yes	66,992	No	-0.0102
	AR1	-0.3446*** (0.0549)	Yes	Yes	66,992	No	-0.0102
ExcessR	CE	0.0013*** (0.0003)	Yes	Yes	71,370	No	0.0004
	AR1	0.0013 (0.0011)	Yes	Yes	71,370	No	0.0004

Reported R² is within R² from a two-way fixed effects regression (time and firm fixed effects)

*** p < 0.01, ** p < 0.05, * p < 0.10

Table 13: Ban 3 main results

4.3 Interaction Effects

To analyze whether the impact of the short-selling restrictions varies across firms of different sizes, an interaction analysis based on market capitalization is conducted. The underlying idea is that market reactions to short-selling bans may differ systematically between small, mid-sized and large firms.

Initially, a simple median split was explored. However, this approach proved suboptimal because the two resulting groups were too heterogeneous, causing potential differences in treatment effects to be “washed out” by excessive within-group variation. To obtain more homogeneous size-based subgroups, the sample is instead partitioned using the 30th and 70th percentiles of the market capitalization distribution. Firms below the 30th percentile are classified as small-cap firms, those above the 70th percentile as large-cap firms and the remaining firms constitute a mid-cap reference group.

In the empirical specification, the mid-cap group serves as the baseline category. Interaction terms between the DiD indicator and the size-group dummies allow the treatment effects for small- and large-cap firms to be compared directly against this benchmark. This framework makes it possible to assess whether short-selling restrictions affect firms differently along the size dimension and whether these effects are amplified or mitigated among the smallest or largest firms in the market.

The analysis is based on a TWFE difference-in-differences framework. To allow the treatment effect to vary across firm-size groups, interaction terms between the treatment indicator and the firm-size dummies are included. The specification is given by:

$$Log(Y_{i,t}) = \alpha + \beta_1 DiD_{i,t} + \beta_2 (DiD_{i,t} * Large_i) + \beta_3 (DiD_{i,t} * Small_i) + \mu_i + \tau_t + \varepsilon_{i,t}$$

Equation 8: DiD Model for Interaction Analysis

where $Y_{i,t}$ denotes the respective dependent variable, μ_i are firm fixed effects and τ_t are time fixed effects. The coefficient β_1 captures the effect of the short-selling ban for firms in the middle size group (the reference level). The coefficients β_2 and β_3 show whether the effect differs for large and small firms relative to the middle group. Thus, the total treatment effects are $\beta_1 + \beta_2$ for large firms and $\beta_1 + \beta_3$ for small firms. Standard errors are estimated using two complementary approaches. First, standard errors clustered at the firm level are employed to account for within-firm serial correlation and heteroskedasticity. Second, Driscoll–Kraay standard errors with an AR(1) kernel structure are used as an alternative correction that additionally addresses cross-sectional dependence and serial correlation across firms.

Table 14 reports the number of firms in each size group (small, middle and large) separately for the treatment and control groups per ban.

	Ban 1	Ban 2	Ban 3
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Category	Treatment	Control	Treatment	Control	Treatment	Control
Small	89	89	36	58	11	32
Middle	118	118	45	62	40	50
Large	89	89	54	52	72	29
Sum	296	296	135	172	123	111

Table 14: Interaction analysis - distribution of companies

4.3.1 Interaction Effects of Ban 1

Table 15 reports the interaction results for Ban 1 based on firm size. Overall, the findings indicate relatively limited heterogeneity across market-capitalization groups, although several patterns emerge once interaction effects are examined separately for small and large firms. However, these results must be interpreted with caution, as the parallel-trend assumption is violated for Ban 1.

For $\log(\text{Spread})$, the baseline DiD coefficient is negative and highly significant, indicating that Bid–Ask Spreads declined in response to the short-selling ban. Some heterogeneity appears at the lower end of the size distribution: for small firms, the interaction term becomes statistically significant only under AR(1) standard errors, with a coefficient of -0.0222 ($p < 0.05$). This implies that small firms experienced an additional reduction in spreads compared with mid-caps. In contrast, for large firms no statistically meaningful deviation from the reference group is found, as the interaction coefficients (0.0057 under both CE and AR1) remain insignificant.

For $\log(\text{Amihud})$, the AR(1) specification reveals notable differences across firm size. The baseline effect again shows a substantial decline (-0.9067), but large firms exhibit a significantly weaker reduction, with an interaction coefficient of 0.1803 ($p < 0.01$), meaning that their illiquidity fell roughly 20% less than for mid-cap firms. Small firms also show less decline in illiquidity: the coefficient for the small-firm interaction is 0.0405 ($p < 0.05$).

Turning to $\log(\text{RVOL})$, the AR(1) estimates show consistent heterogeneity. While the baseline DiD effect is again negative (-0.1123), both large and small firms experience significantly smaller reductions in return volatility than the mid-cap reference group. The large-firm interaction coefficient amounts to 0.0372 ($p < 0.01$) and the small-firm interaction to 0.0382 ($p < 0.01$), implying that volatility declined less strongly in both the upper and lower tails of the size distribution.

For $\log(\text{RollIV})$, size-based differences are relatively limited. The baseline effect indicates a small but significant decline in volatility (-0.0520). Large firms, however, display a relative

increase in volatility compared to the reference group, with the interaction coefficient (0.0423) nearly offsetting the negative baseline effect of mid-sized firms. This result is statistically significant only under the AR(1) specification and suggests that large firms experienced virtually the same level of volatility as before the ban. For small firms, no statistically significant interaction effects are detected.

The results for $\log(\text{ParkinsonVol})$ follow a similar pattern than $\log(\text{RVOL})$: although the baseline DiD coefficient is negative (-0.2236), both large and small firms show notably weaker volatility declines compared with mid-cap firms. Under AR(1), the large-firm interaction term is 0.0735 ($p < 0.01$), while the small-firm interaction is 0.0765 ($p < 0.01$), indicating that the tightening in volatility was substantially less pronounced across the size spectrum.

Finally, for $\log(1 + \text{Excess Returns})$, no economically meaningful or statistically robust interaction effects emerge. The point estimates are small in magnitude and statistically weak, suggesting that the ban did not produce differential effects on excess returns across firm sizes.

Dep. Variable	Errors	DiD	DiD_Large	DiD_Small	TW-FE	R ²
Log(Spread)	CE	-0.4139*** (0.0267)	0.0057 (0.0428)	-0.0222 (0.0363)	Yes	-0.0774
	AR1	-0.4139*** (0.0243)	0.0057 (0.0084)	-0.0222** (0.0086)	Yes	-0.0774
Log(Amihud)	CE	-0.9067*** (0.0709)	0.1803* (0.0971)	0.0405 (0.0969)	Yes	0.0042
	AR1	-0.9067*** (0.0641)	0.1803*** (0.0227)	-0.0405** (0.0191)	Yes	0.0042
Log(RVOL)	CE	-0.1123*** (0.0242)	0.0372 (0.0346)	0.0382 (0.0338)	Yes	-0.0267
	AR1	-0.1123*** (0.0312)	0.0372*** (0.0103)	0.0382*** (0.0096)	Yes	-0.0267
Log(RollV)	CE	-0.0520* (0.0294)	0.0423 (0.0409)	0.0030 (0.0401)	Yes	-0.0217
	AR1	-0.0520*** (0.0201)	0.0423*** (0.0097)	0.0030 (0.0094)	Yes	-0.0217
Log(ParkV)	CE	-0.2236*** (0.0484)	0.0735 (0.0693)	0.0765 (0.0678)	Yes	-0.0265
	AR1	-0.2236*** (0.0624)	0.0735*** (0.0206)	0.0765*** (0.0191)	Yes	-0.0265
Log(1+ExcessR)	CE	0.0011 (0.0024)	-0.0009 (0.0006)	-0.0003 (0.0007)	Yes	0.0002
	AR1	0.0011*** (0.0004)	-0.0009* (0.0005)	-0.0003 (0.0005)	Yes	0.0002

Reported R² is within R² from a two-way fixed effects regression (time and firm fixed effects)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 15: Interaction analysis results Ban 1

4.3.2 Interaction Effects of Ban 2

Table 16 presents the interaction results for Ban 2, examining whether the effects of the partial short-selling reopening differ across firms of different sizes. In contrast to Ban 1, the parallel-trend assumption is satisfied for this period, allowing for a causal interpretation of the estimated coefficients.

For Log(Spread), the estimated effects reveal only limited heterogeneity across firm sizes. The interaction term for small firms shows a weakly significant negative coefficient under AR(1) (−0.0220), suggesting that spreads narrowed slightly more for small firms compared to mid-cap firms. No significant differences are observed for large firms.

For Log(Amihud), stronger size-based asymmetries emerge under the AR(1) specification. The baseline effect is negative but not statistically significant, indicating no clear overall response for mid-cap firms. In contrast, the large-firm interaction coefficient is significantly negative (−0.0913), implying a markedly stronger reduction in illiquidity for large caps relative to the reference group. The corresponding coefficient for small firms is also negative but not statistically significant (−0.0186), suggesting no reliable differential effect for smaller firms.

Across all volatility measures, a clear and coherent pattern emerges. Under AR(1), the baseline DiD coefficients for Relative Volatility, Rolling Volatility and Parkinson Volatility are all significantly negative, indicating that mid-cap firms experienced a reduction in volatility following the lifting of the ban. Large firms exhibit even more pronounced declines in volatility, as reflected by sizable and highly significant interaction terms. In contrast, the volatility effects for small firms are positive and significant across all AR(1) specifications. Because these interaction coefficients exceed the negative baseline DiD estimates in absolute magnitude, the total effect for small-cap firms becomes positive. This means that, unlike mid and large firms, small-cap volatility did not decrease after the partial ban lift but instead increased in absolute terms once short selling was reinstated for the treatment group.

Finally, for Log(1 + Excess Return), the coefficients are statistically significant in the AR(1) specification, but the magnitudes are extremely small and lack economic relevance. As such, no meaningful return effects can be inferred from these estimates.

Dep. Variable	Errors	DiD	DiD_Large	DiD_Small	TW-FE	R ²
Log(Spread)	CE	-0.0006 (0.0293)	-0.0010 (0.0435)	-0.0220 (0.0557)	Yes	8.836e-05

	AR1	-0.0006 (0.0087)	-0.0010 (0.0100)	-0.0220* (0.0116)	Yes	8.836e-05
Log(Amihud)	CE	-0.0038 (0.0649)	-0.0913 (0.0717)	-0.0186 (0.0894)	Yes	-0.0025
	AR1	-0.0038 (0.0240)	-0.0913*** (0.0289)	-0.0186 (0.0259)	Yes	-0.0025
Log(RVOL)	CE	-0.0199 (0.0402)	-0.0702 (0.0456)	0.0449 (0.0554)	Yes	0.0035
	AR1	-0.0199* (0.0112)	-0.0702*** (0.0140)	0.0449*** (0.0135)	Yes	0.0035
Log(RollV)	CE	-0.0229 (0.0483)	-0.0897 (0.0555)	0.0510 (0.0731)	Yes	0.0081
	AR1	-0.0229** (0.0106)	-0.0897*** (0.0135)	0.0510*** (0.0123)	Yes	0.0081
Log(ParkV)	CE	-0.0399 (0.0805)	-0.1404 (0.0913)	0.0903 (0.1109)	Yes	0.0035
	AR1	-0.0399* (0.0225)	-0.1404*** (0.0280)	0.0903*** (0.0271)	Yes	0.0035
Log(1+ExcessR)	CE	-0.0012 (0.0008)	-0.0015 (0.0010)	0.0017 (0.0013)	Yes	0.0002
	AR1	-0.0012** (0.0005)	-0.0015*** (0.0005)	0.0017*** (0.0006)	Yes	0.0002

Reported R^2 is within R^2 from a two-way fixed effects regression (time and firm fixed effects)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 16: Interaction analysis results ban 2

4.3.3 Interaction Effects of Ban 3

Table 17 presents the size-based interaction results for Ban 3. As in Ban 1, the parallel-trend assumption is not satisfied, implying that the coefficients cannot be interpreted causally. Nonetheless, the interaction patterns provide informative descriptive evidence on how firms of different size categories responded around the reintroduction of the full short-selling ban.

For bid–ask spreads, the baseline effect is significantly negative across both error specifications (–0.1735), indicating that mid-cap firms experienced a marked decline in spreads following the ban. The interaction terms suggest mild heterogeneity. Under the AR(1) specification, large firms display a small but statistically significant positive deviation (0.0967), while small firms exhibit a significant negative deviation (–0.0927).

The Amihud Illiquidity measure shows a noticeably different pattern. Under AR(1), the baseline coefficient for mid-cap firms is significantly positive (0.1613), indicating a deterioration of price-based liquidity following the ban. Large firms reinforce this illiquidity increase, with a strongly positive interaction effect of 0.0851. In contrast, small firms exhibit a large negative and statistically significant coefficient (–0.1729). Because this negative adjustment outweighs the positive baseline effect in absolute value, the overall effect for small

caps switches sign, thus Amihud decreases for small firms, even as it increases for mid- and large-cap firms.

Across all three volatility proxies a consistent structure emerges. Using clustered errors, the baseline effects for mid-cap firms are significantly negative, indicating a reduction in volatility following the ban. For example, RVOL declines by -0.2357 , RollV by -0.1670 and ParkV by -0.4707 . Large-cap firms again display significantly positive interaction coefficients across all measures (0.1167 , 0.1312 and 0.2332 , respectively), implying that the volatility decline is noticeably weaker for large firms. Small-firm interaction coefficients are negative but not statistically significant under clustered errors, suggesting no clear differential effect relative to mid-caps. Under AR(1), the pattern sharpens. All baseline volatility effects remain strongly negative and precisely estimated. Large-cap interactions continue to be significantly positive across all measures, indicating a systematically attenuated volatility reduction for these firms. Small-cap interactions, however, become significantly negative under AR(1), implying that small firms experienced an even stronger volatility decrease relative to mid-caps.

Finally, the estimates for $\text{Log}(1+\text{ExcessReturn})$ reveal no economically meaningful effects. Although several coefficients reach statistical significance, their magnitudes remain extremely small, indicating that neither the ban nor the heterogeneous size effects materially influenced excess returns

Dep. Variable	Errors	DiD	DiD_Large	DiD_Small	TW-FE	R ²
Log(Spread)	CE	-0.1735*** (0.0464)	0.0967* (0.0509)	-0.0927 (0.0771)	Yes	-0.0101
	AR1	-0.1735*** (0.0228)	0.0967*** (0.0127)	-0.0927*** (0.0220)	Yes	-0.0101
Log(Amihud)	CE	0.1613* (0.0966)	0.0851 (0.1006)	-0.1729 (0.2100)	Yes	-0.0021
	AR1	0.1613*** (0.0463)	0.0851*** (0.0260)	-0.1729*** (0.0486)	Yes	-0.0021
Log(RVOL)	CE	-0.2357*** (0.0430)	0.1167** (0.0483)	-0.0584 (0.0931)	Yes	-0.0075
	AR1	-0.2357*** (0.0289)	0.1167*** (0.0142)	-0.0584** (0.0244)	Yes	-0.0075
Log(RollV)	CE	-0.1670*** (0.0543)	0.1312** (0.0585)	-0.0859 (0.1154)	Yes	-0.0082
	AR1	-0.1670*** (0.0241)	0.1312*** (0.0112)	-0.0859*** (0.0266)	Yes	-0.0082
Log(ParkV)	CE	-0.4707*** (0.0861)	0.2332** (0.0968)	-0.1166 (0.1862)	Yes	-0.0075
	AR1	-0.4707*** (0.0576)	0.2332*** (0.0284)	-0.1166** (0.0489)	Yes	-0.0075

Log(1+ExcessR)	CE	0.0017*** (0.0005)	-0.0006 (0.0005)	-0.0006 (0.0005)	Yes	0.0004
	AR1	0.0017 (0.0013)	-0.0006 (0.0007)	-0.0006 (0.0009)	Yes	0.0004

Reported R^2 is within R^2 from a two-way fixed effects regression (time and firm fixed effects)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 17: Interaction analysis results ban 3

5. Conclusion

This chapter discusses and interprets the empirical findings presented in the previous section. It aims to synthesize the main and interaction results across all three short-selling bans and assess their implications for market liquidity, volatility and stock performance. The discussion proceeds in four parts. First, a summary of the main and interaction results is provided, highlighting consistent patterns and notable differences across the ban periods. Second, the main methodological and contextual limitations of the analysis are addressed, including the validity of the identification strategy and external market factors. Third, the findings are compared to prior empirical studies on short-selling bans to evaluate their alignment with or divergence from the existing literature. Finally, the chapter concludes with an outlook, outlining directions for future research and potential extensions based on the most recent policy developments in South Korea.

The results across the three short-selling bans reveal a differentiated pattern regarding market liquidity, volatility and stock performance. For the first ban, liquidity and volatility measures are consistent and point in the same direction. Both the Bid–Ask Spread and the Amihud Illiquidity measure decrease, indicating an improvement in market liquidity. All three volatility indicators also decline, suggesting that overall market volatility was reduced. No economically meaningful effect on excess returns can be identified. The interaction results sharpen the insights: volatility declines most strongly for mid-sized firms, more moderately for the additional effects for large- and small-sized firms, indicating that the stabilizing effect of the ban was not uniform across the size distribution.

For the second ban, which corresponds to the partial lifting of short-selling restrictions for the treatment group, the findings again show a uniform improvement in liquidity and a simultaneous decline in volatility across all measures. This indicates that the reopening of short selling did not harm, but rather improved market quality. As before, excess returns remain unaffected. Importantly, the interaction analysis reveals a clear and internally coherent size pattern across all volatility measures: the volatility of mid-cap firms decreases substantially,

large-cap firms experience an even stronger volatility reduction, while small-cap firms exhibit a significant increase in volatility relative to mid-caps. In absolute terms, volatility even rises for small firms after the partial ban lift.

In contrast, the results for the third ban provide a more nuanced picture of liquidity effects. While the Bid–Ask Spread decreases, indicating improved transaction-based liquidity, the Amihud Illiquidity measure increases, suggesting a deterioration in price-impact liquidity. For both the Spread and the Amihud measure, large-capitalisation firms exhibit positive coefficients, whereas small-capitalisation firms experience negative coefficients. Notably, for Amihud, the negative coefficient for small-cap stocks is sufficiently large to offset the positive effect observed for mid-caps, implying that overall illiquidity for small firms actually declines slightly.

Volatility, however, again shows a coherent structure. Mid-cap firms experience a clear volatility reduction, large-cap firms exhibit significantly higher volatility relative to mid-caps, while small firms display a stronger volatility decrease. Thus, whereas the partial ban lift (Ban 2) tightened volatility most for large firms and raised it for small firms, the reinstatement of the full ban (Ban 3) produces the reverse pattern: large-cap volatility rises relative to mid-caps, while small-cap volatility falls even more strongly. Excess returns, similar to the previous banning periods did not show any economically meaningful results.

Regarding the limitations of this analysis, it must be emphasized that the parallel-trend assumption is violated for Ban 1 and Ban 3. The Japanese peer firms used as control groups, although matched based on sectors and market capitalization, did not follow parallel pre-ban dynamics and thus do not represent a fully adequate counterfactual. In contrast, the identification strategy for Ban 2 appears more robust, as the non-reopened South Korean firms constitute a suitable internal control group. Another limitation concerns potential confounding effects during Ban 1, as the COVID-19 market crash occurred simultaneously with the implementation of the ban and may have influenced liquidity and volatility independently of the policy intervention. A further limitation arises from the continued occurrence of illegal short-selling activities during the ban periods. As discussed in the theoretical framework, these violations were a major motivation for the introduction of the third ban, which aimed to establish a stricter regulatory and monitoring system for short-selling practices. Several publicly reported cases revealed that certain investment banks continued short-selling despite the restrictions (FSC, 2025). The full extent of these activities remains unknown, creating

potential bias in the estimated treatment effects. Specifically, if firms officially classified as banned were still subject to short-selling, the treatment group would no longer represent a fully homogeneous sample. Consequently, the measured effects of the bans may be partially attenuated or distorted by these unobserved short-selling activities. Another limitation is the comparability of the results for Ban 2 and Ban 3 with prior research. Historically, short-selling bans have predominantly been implemented during periods of severe market distress, such as the Global Financial Crisis of 2008 or the COVID-19 crisis (ESMA, 2004, p. 8). Consequently, much of the existing empirical evidence reflects market conditions characterized by heightened uncertainty and systemic risk. In contrast, both the partial lifting of the ban and the reintroduction of the full ban occurred in relatively stable market environments. This difference in underlying market conditions may explain why the findings of this thesis partly diverge from earlier studies. Lastly, the overall R^2 values of the regressions are relatively low, indicating that the models explain only a small portion of the total variation in the dependent variables. While such low explanatory power is common in market microstructure research, it still highlights that a substantial share of short-term market fluctuations remains unexplained.

In comparison with the existing literature, the findings partly diverge from earlier empirical evidence for the liquidity measures. Several studies, such as Beber and Pagano (2013) and Boehmer et al. (2014) report that short-selling bans deteriorate market quality, leading to wider Bid-Ask Spreads and higher Amihud Illiquidity measures. In contrast, in this thesis the results for Ban 1 and Ban 3 show declining spreads, which runs counter to the widespread notion that bans impair liquidity. The findings for Ban 2, however, align with the interpretation suggested by Beber and Pagano (2013), who document that short-selling bans typically reduce market liquidity. Consequently, the lifting of the ban should improve it as trading restrictions are eased. This pattern is partly reflected in the results for the treatment group, where Amihud decline, hence liquidity increases once short selling is reinstated.

Comparing the results for volatility with existing research, Spolaore & Le Moign (2024, p. 25) working paper finds that volatility decreased for the European stocks subject to the 2020 short-selling bans, a pattern that aligns with the volatility reductions observed here for Ban 1 and Ban 3. However, it is important to emphasize that the parallel-trend assumption is violated for Ban 1 and Ban 3.

In contrast, Helmes et al. (2016) analyse an exceptionally long eight-month window before and after the short-selling ban during the 2008 Global Financial Crisis. Their study documents a

notable increase in volatility following the introduction of the ban. This medium-term rise in volatility aligns with the findings for Ban 2 in the present analysis: after the lifting of the ban, volatility declines consistently across all volatility measures. This pattern implies that volatility must have been higher for the treatment group during the first banning period, suggesting that the short-selling restriction, rather than stabilising markets, may have amplified volatility. However, further analysis would be required to determine whether the observed volatility decrease in the treatment group reflects a persistent effect of the ban lift or whether volatility merely appears lower after Ban 1 because it had surged during the first ban period as a result of pandemic-related market turbulence.

The analysis of the three short-selling ban periods shows no economically meaningful impact on excess returns. This outcome is consistent with the findings Spolaore & Le Moign (2024, p. 25) and Beber and Pagano (2013), who document that short-selling bans implemented during the 2008–2009 financial crisis did not systematically influence cumulative excess returns across the 30 countries examined. In their study, restricted and unrestricted stocks performed similarly, suggesting that short-selling bans generally fail to alter return dynamics in a material way. The only exception identified by Beber & Pagano (2013, p. 378) concerns U.S. financial stocks, which exhibited temporary positive abnormal returns at the onset of the ban. However, these effects were closely tied to simultaneous policy actions, most notably the introduction of the TARP program, rather than to the ban itself. Once these confounding interventions are taken into account, the apparent price support largely disappears, reinforcing the conclusion that the ban alone did not generate economically meaningful excess returns. Taken together, both the prior literature and the empirical evidence from the three ban periods examined here point to the same conclusion: short-selling bans do not materially affect stock returns. In none of the observed episodes do the results indicate economically relevant deviations in price behavior attributable to the restrictions, suggesting that such bans provide little influence over market performance.

Looking ahead, since the short-selling ban was fully lifted on March 31, 2025, this presents new opportunities for future research. Subsequent studies can investigate the long-term market effects following the complete reopening of short selling and assess whether the effects observed in liquidity and volatility persist once unrestricted short selling resumed. Moreover, future research should focus on employing domestic control groups to improve comparability and address the limitations arising from cross-country differences. The unique setting of the partial ban offers an ideal framework for further analysis, allowing researchers to exploit

within-country variation to better isolate causal effects. Based on that, greater attention should be devoted to the second ban period.

Beyond the specific context of South Korea, the question arises to what extent the findings can be generalized to other markets or time periods. South Korea's regulatory framework, investor structure market composition differ substantially from those in other economies (The Korea Times, 2023). The broader research landscape also underscores that results may vary across settings, as evidenced by the contrasting findings documented in recent studies. For instance, Spolaore & Le Moign (2024, p. 25) report volatility reductions following the 2020 European short-selling bans, while Helmes et al. (2016, p. 19) identify a medium-term increase in volatility during the Global Financial Crisis. Similar patterns of divergence emerge in the empirical analysis conducted in this thesis, where volatility effects differ across bans and across measurement approaches. These inconsistencies raise important questions about whether such differences stem from the choice of volatility proxy, from underlying market conditions, or from heterogeneous policy effects across individual bans.

Future research could therefore investigate more systematically whether cross-study discrepancies are driven by methodological variation or by genuine cross-ban heterogeneity. More broadly, comparative analysis across countries and regulatory regimes would help assess whether the Korean case represents a broader policy pattern or a market-specific anomaly. Such work would not only strengthen the external validity of existing findings but also deepen our understanding of how regulatory design and market structure jointly shape the effects of short-selling bans.

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Annex

Model with Volume and VWAP as control variables

A central consideration in the specification of the baseline model was whether trading volume in shares and VWAP should be included as control variables.

To ensure that the results are not driven by the modeling choice of not including them, all regressions were estimated both with and without Volume and VWAP. The corresponding analysis show that the inclusion of these controls does not materially change the estimated DiD effects for any of the three bans. Coefficient magnitudes, signs and statistical significance remain highly comparable across specifications. This further supports the decision to exclude these variables from the baseline model and ensures that the main results are not influenced by post-treatment or mechanical endogeneity concerns.

Ban 1:

Dep. Variable	Std. errors	DiD	VWAP	Volume	Day FE	Stock-level FE	R ²
Log(Spread)	CE	-0.4203*** (0.0202)	5.322e-07 (6.859e-07)	-9.588e-06*** (2.945e-06)	Yes	Yes	0.0538
	AR1	-0.4203*** (0.0234)	5.322e-07 (6.419e-08)	-9.588e-06*** (1.212e-06)	Yes	Yes	0.0538
Log(Amihud)	CE	-0.7930*** (0.0469)	-3.755e-07*** (1.393e-06)	-0.0001*** (2.057e-05)	Yes	Yes	0.0807
	AR1	-0.7930*** (0.0623)	-3.755e-07 (2.295e-07)	-0.0001*** (9.560e-05)	Yes	Yes	0.0807
Log(RVOL)	CE	-0.1200*** (0.0177)	-1.414e-07 3.508e-07	5.58e-05*** (1.036e-05)	Yes	Yes	0.1169
	AR1	-0.1138*** (0.0304)	4.226e-06*** (6.076e-07)	4.888e-05*** (4.245e-06)	Yes	Yes	0.0077
Log(RollV)	CE	-0.0505** (0.02016)	-3.497e-07 (4.437e-07)	3.127e-05*** (6.79e-06)	Yes	Yes	0.0556
	AR1	-0.0505** (0.0225)	-3.497e-07*** (9.746e-08)	3.127e-05*** (2.996e-06)	Yes	Yes	0.0556
Log(ParkV)	CE	-0.2394*** (0.0355)	-2.75e-07 (7.016e-07)	0.0001*** (2.087e-05)	Yes	Yes	0.1174
	AR1	-0.1567*** 0.0500	5.6e-06*** 9.633e-07	8.1e-05*** 7.288e-06	Yes	Yes	0.2444
Log(1+ExcessR)	CE	0.0028 0.0037	1.497e-07*** 2.846e-08	2.469e-06*** 4.756e-07	Yes	Yes	0.0181
	AR1	0.0028 0.0029	1.497e-07** 6.114e-08	2.469e-06*** 2.169e-07	Yes	Yes	0.0181

Reported R² is within R² from a two-way fixed effects regression (time and firm fixed effects)

*** p < 0.01, ** p < 0.05, * p < 0.10

Table 18: Results Ban 1 with control variables

Ban 2:

Dep. Variable	Std. errors	DiD	VWAP	Volume	Day FE	Stock-level FE	R ²
Log(Spread)	CE	0.0050 (0.0268)	-1.51e-06*** (3.154e-07)	-1.252e-06 (9.493e-07)	Yes	Yes	0.0122
	AR1	0.0050 (0.0080)	-1.51e-06*** (5.061e-08)	-1.252e-06** (5.683e-07)	Yes	Yes	0.0122
Log(Amihud)	CE	-0.0365 (0.0441)	-1.414e-06** (5.847e-07)	-3.224e-05*** (6.591e-06)	Yes	Yes	0.0266
	AR1	-0.0365 (0.0275)	-1.414e-06*** (1.627e-07)	-3.224e-05*** (4.723e-06)	Yes	Yes	0.0266
Log(RVOL)	CE	-0.0465* (0.0272)	1.447e-06** (5.764e-07)	2.96e-05*** (5.68e-06)	Yes	Yes	0.0763
	AR1	-0.0465*** (0.0123)	1.447e-06*** (1.067e-07)	2.96e-05*** (3.916e-06)	Yes	Yes	0.0763
Log(RollV)	CE	-0.0576 (0.0358)	1.647e-06** (6.477e-07)	1.854e-05*** (3.473e-06)	Yes	Yes	0.0507
	AR1	-0.0576*** (0.0145)	1.647e-06*** (1.066e-07)	1.854e-05*** (2.496e-06)	Yes	Yes	0.0507
Log(ParkV)	CE	-0.0931* (0.0545)	2.899e-06** (1.155e-06)	5.988e-05*** (1.148e-05)	Yes	Yes	0.0776
	AR1	-0.0931*** (0.0186)	2.899e-06*** (1.667e-07)	5.988e-05*** (7.375e-06)	Yes	Yes	0.0776
Log(1+ExcessR)	CE	-0.0012** (0.0006)	2.505e-08* (1.289e-08)	1.536e-06*** (2.744e-07)	Yes	Yes	0.1055
	AR1	-0.0012*** (0.0004)	2.505e-08*** (3.565e-09)	1.536e-06*** (1.787e-07)	Yes	Yes	0.1055

Reported R² is within R² from a two-way fixed effects regression (time and firm fixed effects)

*** p < 0.01, ** p < 0.05, * p < 0.10

Table 19: Results Ban 2 with control variables

Ban 3:

Dep. Variable	Std. errors	DiD	VWAP	Volume	Day FE	Stock-level FE	R ²
Log(Spread)	CE	-0.1267*** (0.0269)	1.192e-07 (5.886e-07)	-2.477e-06 (1.97e-06)	Yes	Yes	-0.0127
	AR1	-0.1267 (0.0222)	1.192e-07 (1.455e-07)	-2.477e-06 (1.389e-06)	Yes	Yes	-0.0127
Log(Amihud)	CE	0.1793*** (0.0507)	-7.3e-06*** (1.382e-06)	-6.512e-05*** (9.143e-06)	Yes	Yes	0.0281
	AR1	0.1793*** (0.0399)	-7.3e-06*** (3.155e-07)	-6.512e-05*** (5.73e-06)	Yes	Yes	0.0281
Log(RVOL)	CE	-0.1555*** 0.0244	3.572e-06*** 7.276e-07	5.617e-05*** 1.292e-05	Yes	Yes	0.0624
	AR1	-0.1555*** (0.0269)	3.572e-06*** (1.979e-07)	5.617e-05*** (4.896e-06)	Yes	Yes	0.0624
Log(RollV)	CE	-0.3104 (0.0489)	7.159e-06 (1.458e-06)	0.0001 (2.604e-05)	Yes	Yes	0.0629
	AR1	-0.3104*** (0.0537)	7.159e-06*** (3.962e-07)	0.0001*** (9.841e-06)	Yes	Yes	0.0629
Log(ParkV)	CE	-0.0889*** (0.0053)	3.435e-06*** (1.083e-07)	3.056e-05*** (2.269e-06)	Yes	Yes	0.0353

	AR1	-0.0889 (0.0218)	3.435e-06 (2.24e-07)	3.056e-05 (2.524e-06)	Yes	Yes	0.0353
Log(1+ExcessR)	CE	0.0017*** (0.0003)	3.633e-08*** (1.004e-08)	1.434e-06** (5.736e-07)	Yes	Yes	0.0144
	AR1	0.0017 (0.0012)	3.633e-08 (1.195e-08)	1.434e-06*** (2.263e-07)	Yes	Yes	0.0144

Reported R² is within R² from a two-way fixed effects regression (time and firm fixed effects)

*** p < 0.01, ** p < 0.05, * p < 0.10

Table 20: Results Ban 3 with control variables

Event Study Output tables

Ban 1:

	Spread	Amihud	RVOL	RollV	ParkV	ExcessR
Intercept	-1.2065*** (0.0177)	-11.542*** (0.0383)	-3.4089*** (0.0180)	-3.7797*** (0.0134)	-7.8379*** (0.0359)	-0.0023*** (0.0006)
K=-6	0.1083* (0.0571)	-0.0702 (0.1158)	0.0723 (0.0546)	0.1200*** (0.0387)	0.1522 (0.1092)	0.0187*** (0.0018)
K=-5	0.0861 (0.0536)	0.1559 (0.1213)	0.0220 (0.0527)	-0.0728* (0.0394)	0.0469 (0.1058)	-0.0009 (0.0023)
K=-4	0.0736 (0.0501)	0.2954*** (0.1082)	-0.0902 (0.0583)	0.0632* (0.0370)	-0.1798 (0.1168)	0.0075*** (0.0022)
K=-3	0.1680*** (0.0501)	0.5544*** (0.1112)	0.0728 (0.0519)	-0.0954** (0.0371)	0.1446 (0.1039)	-0.0073*** (0.0021)
K=-1	0.0370 (0.0504)	-0.0615 (0.1115)	0.2221*** (0.0517)	0.0824** (0.0375)	0.4458*** (0.1036)	0.0117*** (0.0027)
K=0	-0.2076*** (0.0520)	-0.1058 (0.1206)	-0.7133*** (0.0483)	-0.1479*** (0.0367)	-1.4311*** (0.0966)	-0.0118*** (0.0029)
K=1	-0.4846*** (0.0636)	-1.0857*** (0.1207)	-0.0499 (0.0518)	0.0797** (0.0395)	-0.0868 (0.1037)	0.0251*** (0.0039)
K=2	-0.3576*** (0.0569)	-0.4819*** (0.1272)	-0.1608*** (0.0537)	-0.1500*** (0.0420)	-0.3118*** (0.1076)	0.0205*** (0.0019)
K=3	-0.2803*** (0.0532)	-0.2474** (0.1227)	-0.0330 (0.0556)	-0.0834* (0.0430)	-0.0589 (0.1116)	0.0209*** (0.0026)
K=4	-0.2661*** (0.0561)	-0.6568*** (0.1234)	-0.2564*** (0.0548)	0.1064** (0.0419)	-0.5059*** (0.1095)	0.0134*** (0.0032)

*** p < 0.01, ** p < 0.05, * p < 0.10**

Table 21: Event study output table Ban 1

Ban 2:

	Spread	Amihud	RVOL	RollV	ParkV	ExcessR
Intercept	-6.2283*** 0.0172	-9.7641*** (0.0390)	-3.3537*** (0.0167)	-4.1479*** (0.0159)	-7.7334*** (0.0335)	0.0020** (0.0009)
K=-6	-0.0037 (0.0705)	-0.0904 (-0.1560)	-0.1499** (0.0700)	-0.0429 (0.0650)	-0.3025 (0.1406)	-0.0068** (0.0031)
K=-5	0.1044 (0.0649)	0.1487 (0.1432)	-0.0796 (0.0794)	-0.0531 (0.0690)	-0.1587 (0.1596)	0.0007 (0.0038)
K=-4	0.0823 (0.0588)	-0.1478 (0.1450)	-0.0530 (0.0698)	-0.0797 (0.0672)	-0.1077 (0.1405)	-0.0037 (0.0045)
K=-3	-0.0934 (0.0578)	0.0415 (0.1404)	-0.0666 (0.0590)	0.0101 (0.0579)	-0.1381 (0.1187)	-0.0054 (0.0049)
K=-1	0.0239 (0.0553)	0.0106 (0.1415)	-0.1576** (0.0713)	-0.1374** (0.0618)	-0.3189 (0.1429)	-0.0067** (0.0031)
K=0	0.0326 (0.0616)	0.1440 (0.1335)	-0.0539 (0.0599)	-0.1831*** (0.0628)	-0.1123 (0.1199)	-0.0052 (0.0037)
K=1	0.0290 (0.0594)	-0.0739 (0.1503)	-0.1738** (0.0809)	0.0078 (0.0687)	-0.3534 (0.1622)	-0.0103*** (0.0031)

K=2	0.0086 (0.0588)	0.1626 (0.1357)	-0.0680 (0.0707)	-0.0406 (0.0716)	-0.1402 (0.1419)	-0.0062** (0.0028)
K=3	-0.0270 (0.0577)	0.1723 (0.1351)	-0.0925 (0.0720)	-0.0828 (0.0690)	-0.1855 (0.1442)	-0.0051* (0.0030)
K=4	0.0937* (0.0568)	-0.2630 (0.1513)	-0.0675 (0.0677)	-0.1451*** (0.0551)	-0.1407 (0.1361)	-0.0082*** (0.0031)
K=5	-0.0713 (0.0688)	-0.0525 (0.1497)	-0.1715*** (0.0634)	-0.1214* (0.0659)	-0.3455 (0.1270)	-0.0075** (0.0030)
K=6	-0.0721 (0.0696)	-0.0650 (0.1452)	-0.1351** (0.0669)	-0.1153** (0.0547)	-0.2715 (0.1342)	-0.0102*** (0.0033)

*** p < 0.01, ** p < 0.05, * p < 0.10**

Table 22: Event study output table Ban 2

Ban 3:

	Spread	Amihud	RVOL	RollV	ParkV	ExcessR
Intercept	-6.2649*** (0.0288)	-12.675*** (0.0633)	-3.5534*** (0.0300)	-3.8071*** (0.0212)	-8.1236*** (0.0601)	0.0044*** (0.0017)
K=-6	-0.1900** (0.0867)	-0.6445*** (0.1765)	-0.1844** (0.0837)	-0.5155*** (0.0703)	-0.3778** (0.1680)	-0.0179*** (0.0043)
K=-5	-0.1828** (0.0847)	-0.1210 (0.1817)	-0.1450* (0.0795)	-0.3018*** (0.0548)	-0.2915* (0.1598)	-0.0171*** (0.0042)
K=-4	-0.2713*** (0.0786)	-0.2605 (0.1782)	-0.4709*** (0.0867)	-0.6316*** (0.0616)	-0.9491*** (0.1740)	-0.0054 (0.0042)
K=-3	-0.0184 (0.0796)	-0.4186** (0.1685)	-0.1941** (0.0793)	-0.4372*** (0.0534)	-0.3908** (0.1603)	-0.0127*** (0.0044)
K=-1	0.0203 (0.0757)	-0.0075 (0.1701)	-0.1637* (0.0842)	-0.3169*** (0.0667)	-0.3355** (0.1690)	-0.0183*** (0.0047)
K=0	-0.0113 (0.0895)	0.4441** (0.1816)	-0.2526*** (0.0793)	-0.4545*** (0.0609)	-0.5132*** (0.1587)	-7,857e-05 (0.0038)
K=1	-0.0354 (0.0826)	0.2000 (0.1795)	-0.4001*** (0.0776)	-0.4613*** (0.0552)	-0.8045*** (0.1555)	-0.0139*** (0.0050)
K=2	-0.0170 (0.0836)	0.3277* (0.1848)	-0.2726*** (0.0881)	-0.4579*** (0.0605)	-0.5509*** (0.1767)	-0.0142*** (0.0049)
K=3	0.0503 (0.0833)	-0.2529 (0.1705)	-0.7012*** (0.0839)	-0.4911*** (0.0646)	-1.4126*** (0.1683)	-0.0084** (0.0034)
K=4	-0.1524* (0.0865)	0.1397 (0.1815)	0.0648 (0.0756)	-0.4209*** (0.0604)	0.1292 (0.1517)	0.0115** (0.0049)
K=5	-0.2398*** (0.0887)	-0.0465 (0.1759)	-0.2930*** (0.0834)	-0.6007*** (0.0774)	-0.5936*** (0.1669)	-0.0113*** (0.0038)
K=6	-0.3853*** (0.0864)	0.1294 (0.1855)	-0.4811*** (0.0901)	-0.5020*** (0.0717)	-0.9564*** (0.1805)	0.0186*** (0.0047)

*** p < 0.01, ** p < 0.05, * p < 0.10**

Table 23: Event study output table Ban 3

All Wald Test p-values (Pre Ban/Post Ban)

For each model, two Wald tests are reported to evaluate the joint significance of the event-study coefficients:

	Ban 1		Ban 2		Ban 3	
Variables	Pre Ban	Post Ban	Pre Ban	Post Ban	Pre Ban	Post Ban
Spread	0.0214	0.0000	0.0154	0.1009	0.0052	0.0000
Amihud	0.0000	0.0000	0.3419	0.0329	0.0004	0.0045
RVOL	0.0000	0.0000	0.1879	0.1672	0.0000	0.0000
RollV	0.0000	0.0000	0.3001	0.0072	0.0000	0.0000

ParkV	0.0000	0.0000	0.1793	0.1633	0.0000	0.0000
ExcesR	0.0000	0.0000	0.0821	0.0145	0.0000	0.0000

Table 24: Results Wald tests

Placebo Test Outcome Tables

Relative Bid-Ask Spread:

Ban 1:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.09.2019	-0.243350	0.047128	-5.163600	0.000000
-5	01.10.2019	-0.247899	0.036194	-6.849172	0.000000
-4	01.11.2019	-0.258188	0.031532	-8.188114	0.000000
-3	01.12.2019	-0.276090	0.030146	-9.158555	0.000000
-2	01.01.2020	-0.338331	0.028268	-11.968709	0.000000
-1	01.02.2020	-0.356012	0.028298	-12.581033	0.000000

Table 25: Placebo test Spread Ban 1

Ban 2:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.11.2020	0.014851	0.058261	0.254895	0.798818
-5	01.12.2020	0.014858	0.051076	0.290893	0.771150
-4	01.01.2021	-0.030115	0.045467	-0.662355	0.507785
-3	01.02.2021	-0.050857	0.042200	-1.205121	0.228234
-2	01.03.2021	-0.013666	0.041080	-0.332675	0.739399
-1	01.04.2021	0.005703	0.040894	0.139450	0.889102

Table 26: Placebo test Spread Ban 2

Ban 3:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.04.2023	0.345493	0.059816	5.775896	0.000000
-5	01.05.2023	0.215088	0.054009	3.982472	0.000070
-4	01.06.2023	0.173318	0.051271	3.380418	0.000732
-3	01.07.2023	0.186705	0.047743	3.910594	0.000094
-2	01.08.2023	0.127921	0.041922	3.051411	0.002296
-1	01.09.2023	0.079656	0.039489	2.017174	0.043759

Table 27: Placebo test Spread Ban 3

Amihud Illiquidity Index:

Ban 1:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.09.2019	-0.131238	0.092992	-1.411280	0.158210
-5	01.10.2019	-0.116151	0.070364	-1.650710	0.098847
-4	01.11.2019	-0.219430	0.068474	-3.204549	0.001359
-3	01.12.2019	-0.350383	0.064605	-5.423487	0.000000
-2	01.01.2020	-0.50932	0.064827	-8.652728	0.000000
-1	01.02.2020	-0.592690	0.065069	-9.108643	0.000000

Table 28: Placebo test Amihud Ban 1

Ban 2:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.11.2020	0.104366	0.129047	0.808745	0.418714
-5	01.12.2020	0.104643	0.107251	0.975680	0.329287

-4	01.01.2021	0.009663	0.096368	0.100275	0.920131
-3	01.02.2021	0.056937	0.087958	0.647326	0.517461
-2	01.03.2021	0.035243	0.082382	0.427799	0.668823
-1	01.04.2021	0.042680	0.076638	0.556911	0.577622

Table 29: Placebo test Amihud Ban 2

Ban 3:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-5	01.05.2023	0.666557	0.135034	4.936224	0.000001
-4	01.06.2023	0.416745	0.108865	3.828083	0.000132
-3	01.07.2023	0.402771	0.099128	4.063159	0.000050
-2	01.08.2023	0.469883	0.089446	5.253243	0.000000
-1	01.09.2023	0.409737	0.084588	4.843884	0.000001

Table 30: Placebo test Amihud Ban 3

Relative Volatility:

Ban 1:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.09.2019	-0.370526	0.043984	-8.424015	0.000000
-5	01.10.2019	-0.278534	0.031664	-8.796524	0.000000
-4	01.11.2019	-0.239111	0.026943	-8.874849	0.000000
-3	01.12.2019	-0.187097	0.026748	-6.994691	0.000000
-2	01.01.2020	-0.214045	0.026176	-8.177146	0.000000
-1	01.02.2020	-0.225469	0.024981	-9.025677	0.000000

Table 31: Placebo test RVOL Ban 1

Ban 2:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.11.2020	-0.076424	0.059476	-1.284958	0.198888
-5	01.12.2020	-0.004173	0.048701	-0.085688	0.931719
-4	01.01.2021	-0.005780	0.047220	-0.122407	0.902584
-3	01.02.2021	-0.016360	0.043959	-0.372170	0.709788
-2	01.03.2021	-0.020606	0.042206	-0.488212	0.625429
-1	01.04.2021	-0.038995	0.042295	-0.921986	0.356596

Table 32: Placebo test RVOL Ban 2

Ban 3:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.04.2023	-0.140761	0.064875	-2.169740	0.030099
-5	01.05.2023	-0.120019	0.055281	-2.171064	0.029998
-4	01.06.2023	-0.135130	0.044243	-3.054273	0.002274
-3	01.07.2023	-0.038982	0.042012	-0.927874	0.353542
-2	01.08.2023	-0.054452	0.037203	-1.463615	0.143395
-1	01.09.2023	-0.123017	0.034858	-3.529128	0.000423

Table 33: Placebo test RVOL Ban 3

Rolling Volatility:

Ban 1:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.09.2019	-0.523710	0.040775	-12.843778	0.000000

-5	01.10.2019	-0.339154	0.029507	-11.494092	0.000000
-4	01.11.2019	-0.203045	0.024320	-8.348745	0.000000
-3	01.12.2019	-0.181747	0.022120	-8.216412	0.000000
-2	01.01.2020	-0.120939	0.022116	-5.468463	0.000000
-1	01.02.2020	-0.105906	0.022803	-4.644487	0.000003

Table 34: Placebo test RollV Ban 1

Ban 2:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.11.2020	-0.136480	0.084782	-1.609781	0.107531
-5	01.12.2020	-0.084018	0.060761	-1.382766	0.166821
-4	01.01.2021	-0.064101	0.050953	-1.258044	0.208456
-3	01.02.2021	-0.046031	0.044712	-1.029505	0.303310
-2	01.03.2021	-0.062788	0.042273	-1.485295	0.137551
-1	01.04.2021	-0.066534	0.042310	-1.572511	0.115918

Table 35: Placebo test RollV Ban 2

Ban 3:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-5	01.05.2023	0.086049	0.063493	1.355259	0.175436
-4	01.06.2023	-0.031435	0.051930	-0.605338	0.545000
-3	01.07.2023	0.060293	0.042618	1.414724	0.157252
-2	01.08.2023	0.050334	0.037338	1.348086	0.177732
-1	01.09.2023	-0.090763	0.036885	-2.460692	0.013922

Table 36: Placebo test RollV Ban 3

Parkinson Volatility:

Ban 1:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.09.2019	-0.737022	0.088262	-8.350404	0.000000
-5	01.10.2019	-0.557173	0.063457	-8.780327	0.000000
-4	01.11.2019	-0.477977	0.054056	-8.842315	0.000000
-3	01.12.2019	-0.372833	0.053652	-6.949065	0.000000
-2	01.01.2020	-0.425257	0.052525	-8.096309	0.000000
-1	01.02.2020	-0.446972	0.050091	-8.923225	0.000000

Table 37: Placebo test ParkV Ban 1

Ban 2:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.11.2020	-0.153484	0.119469	-1.284711	0.198974
-5	01.12.2020	-0.008638	0.097914	-0.088222	0.929705
-4	01.01.2021	-0.013105	0.094859	-0.138149	0.890130
-3	01.02.2021	-0.034292	0.088292	-0.388397	0.697745
-2	01.03.2021	-0.041919	0.084747	-0.494641	0.620883
-1	01.04.2021	-0.079094	0.084895	-0.931665	0.351571

Table 38: Placebo test Parkv Ban 2

Ban 3:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.04.2023	-0.283538	0.130279	-2.176388	0.029598
-5	01.05.2023	-0.238414	0.110757	-2.152580	0.031425

-4	01.06.2023	-0.270181	0.088737	-3.044750	0.002347
-3	01.07.2023	-0.076842	0.084231	-0.912271	0.361694
-2	01.08.2023	-0.108409	0.074586	-1.453476	0.146188
-1	01.09.2023	-0.246757	0.069823	-3.534035	0.000415

Table 39: Placebo test ParkV Ban 3

Excess Return:

Ban 1:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.09.2019	-0.015693	0.005731	-2.738288	0.006193
-5	01.10.2019	-0.012999	0.001998	-6.506355	0.000000
-4	01.11.2019	-0.004329	0.001280	-3.383323	0.000720
-3	01.12.2019	-0.002259	0.001387	-1.628278	0.103515
-2	01.01.2020	0.004365	0.001947	2.241370	0.025036
-1	01.02.2020	0.008081	0.002550	3.168900	0.001537

Table 40: Placebo test Excess Return Ban 1

Ban 2:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-6	01.11.2020	-0.001885	0.002890	-0.652219	0.514300
-5	01.12.2020	-0.000090	0.002157	-0.041806	0.966656
-4	01.01.2021	-0.002548	0.001933	-1.318354	0.187467
-3	01.02.2021	-0.002611	0.001866	-1.399668	0.161696
-2	01.03.2021	-0.002255	0.001835	-1.228747	0.219245
-1	01.04.2021	-0.003077	0.001716	-1.792884	0.073073

Table 41: Placebo test Excess Return Ban 2

Ban 3:

Placebo k	Placebo cut	Placebo DiD	Std. errors	t-value	p-value
-5	01.05.2023	0.012307	0.002463	4.997119	0.000001
-4	01.06.2023	0.012877	0.001777	7.245409	0.000000
-3	01.07.2023	0.008908	0.001620	5.498244	0.000000
-2	01.08.2023	0.009544	0.001527	6.251303	0.000000
-1	01.09.2023	0.006479	0.001405	4.611672	0.000004

Table 42: Placebo test Excess Return Ban 3

FX Rates

The following table reports the exchange rates used for the standardisation of monetary variables at three relevant points in time: 13 March 2020, 30 April 2021 and 31 October 2023. These exchange rates were applied in two contexts within the empirical analysis. First, they were used to harmonise market capitalisation values across South Korean and Japanese firms for the matching procedure, ensuring comparability between treated and control units. Second, the same rates were employed to convert local-currency prices into US dollars when computing traded values in the denominator of the Amihud Illiquidity measure for Ban 1 and 3. This conversion was necessary because traded volume is calculated as the product of price and the

number of shares traded and differing local currencies would otherwise distort cross-country comparisons.

The exchange rates shown below reflect how many US Dollar correspond to one South Korean Won and Japanese Yen on each of the three dates.

	Ban 1	Ban 2	Ban 3
South Korean Won	0.000825938	0.000901002	0.000737339
Japanese Yen	0.009259259	0.009174312	0.006591958

Table 43: FX Rates