The Effects of Different Anonymity Regimes on Liquidity at NASDAQ Nordic Exchanges

Does anonymization increase market quality?

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By

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Abstract

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Key words: Liquidity, Market Quality, Post-trade anonymity (PoTA), Voluntary Post-trade Anonymity (vPoTA), Adverse Selection.

Purpose: The purpose of this paper is to investigate the effect of various post-trade anonymity regimes on the liquidity of the Stockholm, Helsinki, and Copenhagen exchanges in 2014, 2019, 2020, and 2022.

Theoretical perspectives: The theoretical perspectives for this paper include information asymmetry, adverse selection, informational value of broker codes, trader dynamics under anonymity, order anticipation, and market maker dynamics.

Methodology: This study utilizes a unique quasi-natural setup to which a Difference-in-Differences technique is applied in order to estimate OLS regressions and evaluate the causal impact of post-trade anonymity on market liquidity. The regressions use *Bid-ask Spread* as the main dependent variable, but *Turnover* is also tested, with treatment and time indicators, as well as their interaction term, as the main explanatory variables.

Empirical foundation: The empirical foundation consists of four unique anonymization regimes, introduced to different indexes in different years on the Nasdaq Nordic. The number of firms examined during our event ranges from 156 in 2014, to 988 in 2022. The number of observations ranges from 21,043 to 137,942.

Conclusions: The key takeaway from this paper is that the introduction of voluntary post-trade anonymity (vPoTA) and post-trade anonymity (PoTA) yielded mixed results. The introduction of vPoTA in 2014 indicated no statistically significant results. Moreover, the incremental move from vPoTa to PoTA did not significantly improve liquidity in 2019 or 2020. Lastly, the move from complete transparency to PoTA in 2022 showed highly statistically significant results: Bid-ask Spreads decreased by an average of 10.2% while Turnover increased by 15.3%, on average, for the Mid-cap, Small-cap, and First North Index.

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Keywords and concepts

Keyword	Description
Liquidity	Refers to the ability to purchase or sell substantial amounts of a security, quickly, anonymously, and without causing significant fluctuations in its price.
Market quality	A measurement of market efficiency defined by a multitude of characteristics such as liquidity, bid-ask spreads, and market transparency.
Bid-ask Spread	The difference between the highest bid and lowest ask that traders are ready to accept. It represents a transaction cost for traders. Lower spread means higher liquidity and market quality.
Turnover	Turnover represents the total amount of transacted shares over a day times the respective share price for every transaction in expressed in euro.
MPID	Market participant identifiers, also called broker codes are used to identify unique market participants.
PoTA	Post-trade anonymity refers to when market participant identifiers (MPIDs) are hidden after executed trades.
Pre-trade anonymity/transparency	Market participant identifiers are hidden/shown from unexecuted orders in the limit order book.
vPoTA	Voluntary post-trade anonymity means that market participants can choose to show their MPIDs or hide them completely from executed trades.
Bilateral transparency	The two parties of a trade see each other’s MPIDs after a transaction, but no third parties can.
Multilateral transparency	Third parties can see MPIDs after a transaction, a version of post- and pre-trade transparency.
Sponsored access	Non-members of the Nasdaq Nordic can trade through an existing member of the exchange.

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1.0 Introduction

The introduction discusses market regulation, focusing on trading anonymity, its purpose, and its development in recent decades. The motivation behind the study is outlined, along with the research question, the main findings, its empirical contribution, and the structure of the paper.

1.1 Background

In financial markets, liquidity, an integral indicator of market quality, is an essential element that investors seek. Liquidity refers to the ability to purchase or sell substantial amounts of a security, quickly, anonymously, and without causing significant fluctuations in its price (Gregoriou et al., 2005). Exchange operators such as Nasdaq are entrusted with providing trading platforms that improve the liquidity of the market (Nasdaq OMX Nordic, n.d.a). In June 2008, Nasdaq introduced post-trade anonymous reporting for its Nordic exchanges. Post-trade anonymity (PoTA) refers to when market participant identifiers (MPIDs) are hidden after executed trades (see Table I for a visualization). For stocks under the new regime, all broker codes were removed from all real-time data feeds. Less than a year later, on April 14, 2009, the initial experiment reverted to transparent post-trade reporting, because of member consultation (Notified, 2009). Anonymity is the norm in all major U.S and European exchanges, and Nasdaq argued that introducing anonymity to the Nordic exchanges would improve efficiency (Nasdaq, 2007).

Theoretical models have shown that higher information asymmetry is correlated with wider bid-ask spreads because market makers use the spread as a buffer against adverse selection costs associated with information asymmetry (Copeland and Galai, 1983; Glosten and Milgrom, 1985; Madhavan, 2000). In return, market makers help minimize volatility by providing a mechanism for better price discovery and trade execution (Chakraborty and Kearns, 2011). In transparent markets, informed traders[1] incur transaction costs because their trade signals provide information to other market participants. Displaying their orders exposes them to adverse selection costs and front-running by other traders (Harris, 1996). This results in reduced liquidity for large traders and increased trading costs (Harris, 1997). Trading anonymity can therefore help increase liquidity in the market but may be at the expense of uninformed traders (Linnainmaa and Saar, 2012). Kovtunenko (2008) argues that the effect of

[1] Informed traders refer to institutional/professional traders, and traders with other informational advantages while uninformed traders refer to retail traders or traders with no informational advantage.

trading anonymity depends on assumptions of trader behaviour, and that further research is needed to better understand its impact on market quality.

Vocal investors and financial media have argued that anonymization undermines liquidity, arguing that front-running and piggyback riding are important tools for uniformed traders,[2] who, in turn, provide liquidity in the market. These trading strategies become harder to utilize after anonymization is introduced (Swee, 2022). Authorities such as the European Union continuously strive for and implement new regulations and directives to enhance market transparency (Dang et al., 2020; European Commission, n.d.), similarly, transparency is a cornerstone for the integrity of the U.S. securities market (SEC, 2009). Thus, popular demand and market regulators seem to advocate for greater market transparency. Why then, would exchange operators push for trading anonymity?

In recent decades, the topic of market regulation has been a common theme by policy makers (Dang et al., 2020).[3] Since 2014, Nasdaq has introduced varying degrees of post-trade anonymity regimes on its Nordic exchanges Stockholm, Helsinki, and Copenhagen (excluding Reykjavik). These policies were implemented in a staggered manner on different indexes at different times, meaning that it is possible to study and compare the effects of anonymization on different sub-groups of firms (see Table II). Nasdaq (2007) argues that anonymizing trading by hiding market participant identifiers (MPIDs), pending and after a trade, reduces market impact costs and allows for more automated trading. But even if trade anonymity is binary, the application of it does not have to be. Nasdaq allowed for voluntary post-trade anonymity (vPoTA) from 2014 to 2020 in selected exchanges, meaning that market participants could choose to trade with full transparency or be fully anonymous (Nasdaq, 2014a). Other relatively recent market developments are the introduction of more advanced technological innovation such as the INET trading system and central counterparty clearing (CCP), which helped enhance market stability, reduce systematic risk, significantly reduced transaction costs, and allowed for high-frequency trading (Baird, 2010).

[2] Front-running refers to when traders execute orders based on advance knowledge of pending transactions, while piggyback riding refers to traders mimicking the trades of institutional traders, hoping to benefit from the price movements. These trading strategies are harder to follow under trading anonymity.
[3] This includes policy reforms like MiFID I & II, MiFIR and the Commodity Futures Trading Commission's (CFTC) reform of Commitment of Traders Reports.

1.2 Motivation

Over the last few decades, empirical studies on post-trade anonymization have produced conflicting evidence on whether anonymization leads to improved liquidity, and as a result, better market quality. A few researchers, mainly looking at the 90s and early 00s, documented declines in market quality following the introduction of anonymization (Waisburd, 2003; Poskitt et al., 2011; Pham et al., 2015). Other research, and especially the most recent literature, has reported improved market quality, making it challenging to reach a consensus (Comerton-Forde and Tang, 2009; Hachmeister and Schiereck, 2010; Friedrich and Payne, 2014; Dang et al. 2020; Dennis and Sandås, 2020; Meling, 2021). Dennis and Sandås (2020) argue that the reason behind their results may be thanks to a more significant regulatory change than previous studies. In other words, the magnitude of change in regulation may be a predictor for its impact on liquidity. Furthermore, Comerton-Forde and Tang (2009) found that the impact of anonymity is greater for larger firms. Our study offers a unique context, presenting the opportunity for a quasi-natural experiment for different anonymity regimes on multiple exchanges and different indexes with varying firm sizes. In our settings, we investigate the effect of various changes to anonymity after the implementation of INET, introduction of central counterparty clearing (CCP) and new regulatory frameworks, while also studying Small- and Mid-cap firms, contrary to earlier research. This unique context adds to the novelty and relevance of our research.

1.3 Purpose and research question

The purpose of this paper is to analyse the liquidity effect of various post-trade transparency policy regimes on different Nasdaq Nordic exchanges and indexes. We study the effect of vPoTA and PoTA on different indexes on the Stockholm, Helsinki, and Copenhagen exchanges, for the years 2014, 2019, 2020, and 2022, respectively. We do this by conducting a Differences-in-Differences (DiD) approach to infer if the introduction of Nasdaq's policy regimes has significantly impacted the liquidity of the market. The study is motivated by a lack of consensus in the empirical literature, new regulatory settings, and new market conditions.

The following research questions are used for the paper:

> RQ^{1}: *Does the introduction of post-trade anonymity impact the liquidity for stocks trading on the Nasdaq Nordic?*

RQ^2: Does *firm-size affect the liquidity impact for stocks receiving anonymity at Nasdaq Nordic?*

RQ^3: *Does the magnitude of regulatory change affect the impact on liquidity?*

1.4 Main findings

The main findings of this paper are that the introduction of vPoTA in 2014 and PoTA in 2019, 2020 and 2022 had mixed effects on liquidity. The introduction of vPoTA in 2014 provided inconclusive evidence regarding its impact on liquidity. Sponsored trading through Merrill Lynch and technological advancements in the 2010s are likely to have mitigated the impact of the regulation. In 2019 and 2020, the incremental shift from voluntary to complete anonymity (PoTA) did not significantly impact liquidity, which was in-line with our expectations. Potential explanations for these outcomes include the regulatory changes for these years being too small, voluntary post-trade anonymity being available since 2014, rendering the change inconsequential, but also exogenous factors such as Covid-19 and other market regulations such as MiFID II and MiFIR.

In 2022, the transition from multilateral transparency to complete anonymity (PoTA) for the Small-cap, Mid-cap, and First North indexes produced highly statistically significant, yet somewhat delayed improvements to liquidity. The treatment firms experienced an ~10.2% additional average decrease in bid-ask spreads and a ~15.3% increase in turnover compared to the control group of Large-cap firms (excluding Main indexes). The substantial regulatory change in 2022 likely had a significant impact on the liquidity of the treatment group, enhancing market quality despite technological advancements, new market regulations, and other factors.

1.5 Contribution

Our primary contribution to the existing research is examining voluntary and post-trade anonymity, at different exchanges owned by the same exchange operator, under new market regulation, with INET and central counterparty (CCP) clearing. Since the 2000s, electronic trading has become widespread, and by the 2010s, CCP clearing become standard. Together with INET, trading costs has been significantly reduced (Banque de France, 2020; Baird, 2010). The dynamics of trading are, therefore, different today. Unlike Dennis and Sandås (2020), who analysed the shift to anonymity prior to CCP implementation, and unlike Friedrich and Payne

(2014) and Hachmeister and Schiereck (2010), who focused on the initial adoption of CCP, our study addresses the impact of changes to anonymity after CCP clearing was introduced. We simultaneously study the impact of anonymity on multiple indexes, including Small- and Mid-caps, which have not been studied before. Our research also investigates the single largest policy reform on the Nasdaq Nordic, a transition from multilateral transparency, where everyone can view MPIDs, to complete anonymity overnight.

1.6 Structure of the paper

The rest of the paper is divided into the following parts. Parts two and three outline the regulatory and institutional setting for our study. Part four reviews the empirical and theoretical background, outlining the most important findings and theories. Part five establishes the study's hypothesis. Part six explains the data collection and defines the variables used. Part seven explains the methods used in the study, and the paper ends with an empirical analysis and a conclusion in parts eight and nine, respectively.

2.0 Regulatory Framework

This section deep dives into the regulatory framework of the Nordic exchange market, including EU and local exchange regulation, to clarify exogenous changes in the market dynamics over the 2000s.

On March 13th 2006, Nasdaq Nordic introduced pre-trade anonymity for every index, which removed all MPIDs from unexecuted orders in the limit order book (Meling, 2021). Many exchanges, such as the London Stock Exchange, Frankfurt Stock Exchange, and Nasdaq, continued their anonymization by introducing post-trade anonymity in the early 2000s, where MPIDs were removed after an executed trade (Hachmeister and Schiereck, 2010; Friederich and Payne, 2014). Although the Nordic exchanges were, for a long time, known for their relatively high level of transparency, equity markets have changed drastically since the 2000s, especially in terms of high-frequency trading and execution speed. In early 2010, Nasdaq Nordic launched the INET trading system (Nasdaq, 2010), which introduced several new initiatives, such as harmonized tick sizes, CCP clearing, and a capped fee structure for exchange members. According to the then Nasdaq Nordic president, Hans-Ole Jochumsen, overall transaction costs were estimated to drop by as much as 84% by the end of 2010 compared to just a year before as a result of the new initiatives. The initiatives were explicitly aimed at boosting the attractiveness of the exchanges for high-frequency traders (HFTs), expecting to include around ten high-frequency traders among their members by the end of 2010, leading to an estimated 25% boost to overall trading volumes (Baird, 2010).

In 2014, on the Nasdaq Nordic, interest for anonymous trading could be observed through what is called "sponsored access", where institutional investors chose to route their trades through existing members of the exchange, effectively anonymizing their trades as the broker code contained no valuable information[4] (Bursell, 2015).

In February 2014, before the introduction of voluntary post-trade anonymity, Merrill Lynch International (broker code MLI) had a market share of total exchange turnover of almost 12%, making it the largest broker on the Nasdaq Nordic. In May 2014, after the introduction of vPoTA, Merrill Lynch International's market share had dropped to only 5% while the previous non-member, Hudson River Trading Europe, had become the 6th largest broker by volume, responsible for 5% of total turnover (Nasdaq, 2014c; Nasdaq, 2014d). The introduction of vPoTA in early 2014 resulted in a reduced need for sponsored access trading on the Nasdaq Nordic as the trades could be anonymized without a sponsoring broker. The large degree of sponsored trading on the Nasdaq Nordic exchanges before 2014 was a core argument for introducing vPoTA:

> *"[Sponsored trading] is problematic as it counteracts transparency. We cannot see, and others cannot see, who is trading behind this firm [the sponsoring broker], while our market surveillance body cannot have a direct relationship with them" (Bursell, 2014).*

As such, Nasdaq implied that reduced visibility of counterparties would enhance transparency from the viewpoint of the exchange, as investors who previously traded via sponsored access would become paying members of Nasdaq Nordic themselves, allowing Nasdaq to communicate directly with them. Nasdaq Nordic further highlighted additional benefits of anonymity: lower transaction costs, decreased impact of trades on market prices, and a boost to the overall competitiveness of their markets. On the other hand, critics of anonymization argue that it disadvantages smaller and less informed traders because they now can't emulate the trading strategies of more knowledgeable traders (Dennis and Sandås 2020).

[4] Sponsored access means that non-members of the Nasdaq Nordic can trade through an existing member, in this case Merrill Lynch. As such, regardless of who trades through the sponsored broker, the broker code will read MLI, removing any informational value of the broker code and effectively anonymizing the sponsored trades.

A common belief among both institutional and retail investors is that an efficient market is marked by robust surveillance and high liquidity, allowing for the swift execution of market orders without significant price impacts. As a result, Nasdaq Nordic has progressively increased the level of post-trade anonymity through the implementation of several different anonymity regimes, starting in 2008 and completing the full anonymization of the Nasdaq Nordic exchanges in December 2022. The different anonymity regimes are explained in detail in sections 3.2-3.4.

2.1 Markets in Financial Instruments Directive

Markets in Financial Instruments Directive (MiFID) is a comprehensive regulatory regime introduced in November 2007 by the EU, which aimed at enhancing investor protection, competition, and transparency. This was to be achieved by regulating the relationship between investment firms and its clients. MiFID imposed new requirements on investment firms and exchange operators to publicly disclose certain pre- and post-trade data, including the top five quotes and the depth of order books. Stock exchange operators had to provide trade quotes and volume in near real-time. Furthermore, MiFID abolished the concentration rule, which previously dictated that all trading must occur on regulated domestic stock exchanges, allowing for a more diversified trading landscape with alternative venues and internal order flows (Meling, 2021).

The effect MiFID had on the order flow in European markets was mainly increased competition which it accomplished by eliminating the 'concentration rule', introducing alternative trading venues, permitting investment firms to match orders internally, and requiring these firms to seek the most favourable options when executing orders on behalf of their clients (Meling, 2021). When the concentration rule was active, all trading had to take place on regulated and domestic stock exchanges. With its removal, and as a result, the introduction of alternative trading venues, Nasdaq Nordic quickly lost market share to its competitors Chi-X and Turquoise (Bursell, 2008).

2.2 MiFID II and Markets in Financial Instruments Regulation

MiFID II and Markets in Financial Instruments Regulation (MiFIR) were implemented across the European Union on January 3, 2018 (ESMA, 2021). Together, the two regulations aim to make financial markets more efficient and transparent, enhancing investor protection. MiFID II is an updated version of MiFID I in response to lessons from the 2008 financial crisis and the evolving landscape of financial products and services. The two directives are similar, with the key differences being in scope and legal forms. MiFIR, intertwined with MiFID II, focused on reporting requirements and transaction execution, such as pre- and post-trade transparency. Together, they aimed to enhance financial market transparency by forcing broader reporting for financial instruments and transactions. The regulation required publishing of bid/offer prices and trading interest depth prior to transactions and focused on disclosing trade prices, volumes, and time of trades post-trade (European Commission, 2014; Finansinspektionen, 2023). Furthermore, it set up rules for when block trades may receive delayed publication, and it introduced restrictions on trading in dark pools (Dang et al. 2020).[5] In essence, the new regulations were designed to enhance the quality of the market with increased transparency, efficiency, and integrity by tightening regulations around transactions, and protecting investors by providing them with more information and reducing the risks potential for market manipulation.

2.3 Central counterparty clearing

Central counterparty clearing plays a critical role in the financial markets by enhancing market stability and reducing systemic risk. A central counterparty (CCP) is a financial institution operating in the securities and derivatives market. These institutions are tasked with processing transactions post-execution, transferring ownership on the due date, and facilitating efficient corporate transaction processing (Banque de France, 2020). CCP houses function as intermediators in transactions, by acting as the counterpart to buyers and sellers (Nasdaq, 2009). This process, known as clearing, is pivotal in managing the risk that may arise if one party to the transaction defaults on their obligations. CCP houses contribute to improving market quality by providing a more stable and reliable trading environment. By managing and mutualizing counterparty credit risk, CCP clearing houses help maintain market integrity, especially during financial stress.

Before the introduction of CCP clearing, trades were bilaterally settled, meaning that the trade parties knew each other’s identities after a closed transaction. This meant that even with post-trade anonymity in effect, trades were only bilaterally transparent and not fully anonymous. With CCP clearing, the counterparty for buyers and sellers is the CCP clearing house itself, meaning that the trade parties have no interaction with each other and cannot see

[5] Dark pools are private ‘shadow’ exchanges for trading securities, set up by a large financial institution, allowing investors to hide their identity for pre-trade transactions.

who the respective party in the transaction is. Introducing CCP clearing allows for fully anonymous post-trade reporting and reduces direct exposure to counterparties. By facilitating increased market participation and reducing the perceived counterparty risk, CCP clearing houses improve liquidity in financial markets, which could lead to narrower bid-ask spreads as the cost of executing trades decreases with more buyers and sellers in the market (ESMA, 2004. While CCP clearing allows for trades to go from bilaterally transparent to fully anonymous, it is not necessary for it to be that way. One example is Small-cap indexes on the Nasdaq Nordic which got CCP clearing in 2019 but did not receive complete post-trade anonymity until late 2022.

3.0 Institutional framework

The institutional framework outlines the Nasdaq Nordic, its exchanges, and how trading policies has developed since 2008.

3.1 The Nasdaq Nordic

The Nasdaq Nordic group is a subsidiary of Nasdaq Inc., an American exchange operator that operates in Northern and Baltic security marketplaces (Nasdaq OMX Nordic, n.d.a). The group owns exchanges in Stockholm, Helsinki, Copenhagen, Reykjavik, and the Baltics. Nasdaq categorizes its listed companies by size into three segments: Large-, Mid-, and Small-cap. Classification is based on market capitalization: companies with a market cap over EUR 1bn are Large-cap, under EUR 150mn are Small-cap, and those in between are Mid-cap (Nasdaq, 2023). Each market also includes a Main index based on the most traded stocks in each respective market: Stockholm OMXS30, Copenhagen OMXC20/25,[6] and Helsinki OMXH25, respectively. The index inclusion for the Main indexes reflects the most traded stocks in each market. Furthermore, all index constituents are re-evaluated each year, except for the Main indexes, which are reviewed bi-annually (Nasdaq, 2023; Nasdaq, n.d.). Lastly, they also have an exchange called First North, which was created for growth companies.

[6] In December 2017, Copenhagen changed its Main index constituency from 20 stocks to 25 stocks (Nasdaq, 2017).

3.2 Post-trade anonymity in 2008 & 2009

Nasdaq Nordic implemented post-trade anonymous reporting on June 2, 2008, and it applied to all equity-related markets in Helsinki, Reykjavik, and the five most traded stocks in Stockholm. Stocks under the new model, has their MPIDs removed from real-time feeds and were only visible to the counterparties of the specific trades. Meanwhile, stocks in Stockholm (except for the five) and Copenhagen remained transparent. The argument for introducing anonymous trading was that all major international exchanges were using PoTA, and through the internationalization of capital markets, more international players had become active in the Nordic markets. Additionally, Nasdaq Nordic argued that PoTA was favourable for electronic trading and increased market efficiency; the introduction was argued to increase liquidity in the market by attracting automated trading volumes (Nasdaq, 2007). In 2009, Nasdaq Nordic reverted the change to transparency for all stocks but the five most traded in Helsinki and stocks listed in Iceland. There was no explanation given for this reversion other than that the decision was based on "member consultation." Nasdaq Nordic also introduced central counterparty clearing for all Large-cap stocks as well as all Mid-cap stocks listed in Helsinki (Nasdaq, 2009).

3.3 Voluntary post-trade anonymity in 2014

In March 2014, Nasdaq introduced voluntary post-trade anonymity (vPoTA) for all CCP-cleared stocks, which, at the time, included all Main indexes and Large-cap stocks trading in Stockholm, Helsinki, and Copenhagen (Nasdaq, 2014b). The new, updated model meant that market participants could choose to hide their MPIDs from the public, with the default setting being transparency. This meant that all current and former Large-cap and OMXS30 (Stockholm), OMXC25 (Copenhagen), and OMXH25 (Helsinki) were available for anonymous trading through vPoTA. All remaining stocks continued trading with multilateral transparency as before.

3.4 Post-trade anonymity in 2019, 2020 & 2022

Complete and mandatory post-trade anonymity (PoTA) was introduced in April 2019 for OMXS30, OMXC25, and OMXH25, which meant that members could no longer choose to trade anonymously, they had to. This time, Nasdaq only introduced mandatory PoTA for the Main indexes, leaving current and former Large-cap (not part of the Main indexes) stocks with vPoTA, and Small-, Mid-cap, and First North indexes with full post-trade transparency (Nasdaq, 2019).

In April 2020, another similar model was introduced. This time, Nasdaq expanded full post-trade anonymity to the remaining Nasdaq Nordic Large-cap stocks that were not part of the Main indexes. Onwards, stocks that leave the Large-cap index revert to transparency, keeping the Mid-cap, Small-cap, and First North indexes transparent as they were in the 2019 model (Nasdaq, 2020).

The latest model was introduced in December 2022, and it expanded PoTA to Mid-cap, Small-cap, and First North. Making trading in all Nasdaq Nordic markets anonymous. This was the final step in a journey towards full anonymization of trading on the Nasdaq Nordic markets. In a statement given to the Swedish newspaper Affärsvärlden, Nasdaq's Head of Media Relations, Erik Gruvfors, said:

> *"Through this, we will be able to create a more efficient market with better liquidity, narrower spreads, and higher trading volumes, which we saw when we introduced this [post-trade anonymity] on the Large-cap segment and also see support for in existing research."* (Swee, 2022).

4.0 Literature review

The literature review is divided into an empirical and theoretical section. It begins by discussing pre- and post-trade anonymity, highlighting areas of divergence in the research. Additionally, the theoretical section explores informational asymmetry, adverse selection costs, market dynamics under anonymity, and other relevant factors to liquidity.

Discourse from research about pre- and post-trade transparency focuses on determining if anonymity leads to improvements in market quality compared to transparency. The empirical research has established no real consensus as studies has found evidence supporting both sides. There is, however, a common theme from more recent literature which suggests that anonymity does lead to some improved market quality, some reporting a skewed benefit for institutional investors at the expense of other.

4.1 Empirical literature

4.1.1 Post-trade anonymity

Comerton-Forde and Tang (2009) looked at the impact of removing MPIDs from central limit order books in the Australian Stock Exchange (ASX), finding narrower spreads and increased order book depth thanks to reduced exposure risk for traders. They also found that large and high liquid stocks receive more benefit from anonymity. Other empirical studies have found results on the same theme, that trading anonymity reduces transaction costs and increases liquidity. This was brought forward by Hachmeister and Schiereck (2010) and Friederich and Payne (2014), who examined the move from bilateral transparency (MPIDs shown only to the trade counterparties) to anonymity with the introduction of CCP clearing on the XETRA Frankfurt and London SETS exchanges. Furthermore, Dang et al. (2020) found that while market regulations aim to increase transparency, delays in trade publication result in a lower price impact, benefit market quality and assists dealers in risk management.

Dennis and Sandås (2020) looked at post-trade anonymity on the Nasdaq Nordic in 2008 and 2009, utilizing a difference-in-differences (DiD) model while matching stocks that received anonymity to firms outside of the change – finding that spreads decreased by 50 basis points on average. Their paper is unique as they looked at a switch from multilateral transparency to bilateral which then reverted to multilateral a year later. The reason for switching to bilateral transparency and not full anonymity was because the Nasdaq Nordic exchanges had not yet introduced CCP clearing, meaning that full anonymity was not possible to achieve. These results are supported by Meling (2021), who studied the market quality impact of post-trade anonymity on the Oslo Stock Exchange between 2008 and 2010, where the 25 stocks with the highest turnover traded anonymously. Meling utilized a regression discontinuity design finding that trading volume increased with more than 50% and bid-ask spreads reduced by 40%. This increase in trading volume was largely explained by institutional investors, with retail investors not adjusting their trading.

All literature does not support these conclusions, however Poskitt et al. (2011) found increased bid-ask spreads and adverse selection costs following regulatory changes to anonymity. They also found that anonymous trading on the New Zealand exchange increased its market share of trading volume for cross-listed stocks on the Australian stock exchange. They also found that institutional investors seem to benefit from PoTA on the behalf of other investors. Similar conclusions were drawn by Pham et al. (2015), who found that trading volume was halved in the South Korean market following anonymity.

This split in the research may be because the effects of anonymity have changed from the 1990s to the mid-2000s due to market-wide trends, meaning that these studies faced different settings than Meling (2021), Dennis and Sandås (2020), Hachmeister and Schiereck

(2010), and Friederich and Payne (2014) who studied the effects in 2010s. Meling (2021) brings forward that most studies on this topic suffer from nonexogenous variations, which does not allow for a separation of the effect of anonymity from confounding factors. Dennis and Sandås (2020) argues that earlier studies looked at smaller regulatory changes, from bilateral transparency to anonymity, while they looked at multilateral to complete anonymity. In other words, the magnitude of a regulatory change may affect its market impact.

4.1.2 Pre-trade anonymity

The literature surrounding pre-trade anonymity delves into its effects on market behaviour, pricing strategies, and overall market quality. Boehmer et al. (2005) and Eom et al. (2007) studied pre-trade transparency and its effects on the NYSE and Korean stock exchanges. Increased transparency led to better price discovery, reduced transaction costs, higher liquidity, and moderate volatility. They also found that trading strategies change following increased transparency, lowering the price impact of trades, and generally resulting in increased market quality. Contrary to this, Madhavan et al. (2005) studied the introduction of a computerized system on the Toronto stock exchange, making order book information accessible in real time to the public. After an increase in transparency, no improvement was found in market quality. Moreover, execution costs increased, and traders started to limit their trades, resulting in reduced liquidity. They also note that market transparency is a common presumption for regulators, even if it does not necessarily lead to improved market quality. In more recent studies, Kovaleva and Iori (2015) and Martínez and Tapia (2020) found evidence that reduced transparency can enhance market quality by improving price discovery, lowering transaction costs, increasing liquidity, and moderating volatility. They showed that market liquidity improves after pre-trade anonymization is introduced. While increased transparency generally promotes market quality and competition, the reduction of transparency can also yield positive outcomes under certain circumstances.

4.2 Theoretical concepts

4.2.1 Market maker dynamics

In many stock exchanges around the world, market makers are used to provide a market for investors wanting to trade. While market makers provide liquidity, they get compensated by getting special rigrehts by the exchange to post different prices for purchases and sales, often quoting both a buy and sell price. The hope is to profit from the difference in the two quoted prices which make up the bid-ask spread. The market maker buys at a price P_b, and sells at a higher price P_a, making their theoretical compensation defined as $P_a - P_b$ (Gregoriou et al., 2005). Being ready to buy and sell whenever, market makers provide a continuous mechanism for price discovery and trade execution, helping to minimize price volatility and reduce the bid-ask spread (Chakraborty and Kearns, 2011).

4.2.2 Information asymmetry, adverse selection, and market maker costs

Information asymmetry occurs when one party in a transaction, typically the trader, possesses more, or superior information relative to the other party - the market maker. This information disparity introduces an adverse selection risk for market makers, who may inadvertently end up getting exploited by informed traders by getting into transactions at a disadvantage, which increases their costs. Informed traders, having access to material, non-public, or just better interpreted information about a company or market trends, have a distinct advantage over those who are less informed (Copeland and Galai, 1983).

As discussed, market makers are partly compensated through the quoted spread. The bid-ask spread is supposed to cover costs such as order processing, inventory holding, and protection from adverse selection. Glosten and Milgrom (1985) discuss the adverse selection problem, highlighting that the bid-ask spread partly serves as a defence mechanism for the market maker. They model this phenomenon in a microstructure framework, showing how the bid-ask spread set by market makers inherently includes a component to compensate for adverse selection costs. The model demonstrates that the wider the spread, the greater the perceived risk of adverse selection, as market makers aim to protect themselves against potential losses from informed trading (Glosten and Milgrom, 1985). Copeland and Galai (1983) report similarly that market makers are assumed to optimize their position by adjusting the bid-ask spread to maximize the difference between expected income from traders looking for liquidity and expected losses to traders who have an informational advantage. As such, how market makers set the spread is always a trade-off between potential income and potential losses as a result of information asymmetry.

Informed traders can exploit their informational advantage by anticipating future market movements and trading accordingly before these changes become public knowledge. For example, an informed trader might sell stocks just before negative news is released to the public, leading a market maker to buy at what initially seems like a reasonable price but soon turns out to be too high (Kyle, 1985). This kind of strategic trading based on private information leads to what is known as the "winner's curse" for market makers, where the probability of making an unprofitable trade is increased due to the asymmetric information held by the trader. Empirical studies further support this model, finding that markets with higher information asymmetry tend to exhibit wider spreads, which act as a buffer for market makers against the risks of adverse selection (Madhavan, 2000).

4.2.3 Broker code concentration and the informational value of MPIDs

Ellis et al. (2002) provided an analysis on the nature of dealer markets for Nasdaq stocks. They discovered that trading in individual stocks tends to be dominated by a single broker. They also found that broker markets are surprisingly concentrated, and that bid-ask spreads increase when the dominant broker increases its market share. In a similar fashion, Schultz (2003) found that dealers can have competitive advantages in given stocks, where they can exploit their informational advantages to make decisions. Individual investors can also use broker code IDs to assess market conditions and use them to make decisions (Frino et al., 2010). Linnainmaa and Saar (2012) and Johnstone and Zheng (2010) further support this by concluding that the signalling value in broker codes has a significant impact on prices, even when traders work their orders (i.e., split them into small orders and execute them dynamically). Linnainmaa and Saar (2012) also note that while anonymity can improve liquidity, it may also negatively impact the informational efficiency of stock prices, to the disadvantage of uninformed traders.

Dennis and Sandås (2020) infer that in trading environments where post-trade counterparties are disclosed through broker codes, it is reasonable to anticipate that traders use several different brokers to work their orders to obscure any predictive information the codes may carry. Informed traders are, therefore, likely to use multiple brokers and work their orders to avoid front-running by other traders. Poskitt et al. (2011) concur by noting how institutional traders prefer hidden identities when they execute multiple (large) trades. Front-running is costly as it increases transaction costs and use up liquidity that would have otherwise gone to larger traders. This, in turn, leads to a reduction in the liquidity available to large traders and increases their trading costs (Harris, 1997).

4.2.4 Trader dynamics in anonymous trading environments

Informed traders in transparent markets face transaction costs due to the information their trades signal to other market participants. By displaying their orders, traders risk adverse selection costs and front-running by other traders (Harris, 1996).

Increased costs can affect informed trader's strategies, who react to market volume and liquidity (Buffa, 2013; Yang & Zhu, 2019). Similarly, Simaan et al. (2003) and Christie and Schultz (1994) argue that pre-trade transparency has deterring effects on market competition and that increased competition should lead to a decrease in spreads. After the introduction of anonymity via the Electronic Communication Networks (ECN), price competition increased, leading to narrower spreads. Kovtunenko (2008) analysed the strategic behaviour of dealers under different post-trade transparency regimes. He argues that increased post-trade transparency can lead to wider spreads and higher profits for dealers. The theory suggests that anonymity's effect on market quality depends on the assumptions made about trader behaviours; this is why empirical research is needed to better understand the impact post-trade anonymity has on market quality.

4.2.5 Order anticipation

Order anticipation is a strategy used by traders that involves predicting future market orders before they are executed and acting on its informational value. To combat this, Harris (1997) notes that traders work their orders to surrender less information. In a setting where few brokers make up a significant proportion of trades, even a small amount of information can be enough for anticipators to act in front-running or piggyback riding. Friederich and Payne (2014) looked at the effect of order anticipation in the London Stock Exchange following the introduction of CCP in 2001 and found that anonymity reduce traders' ability to predict orders, and in turn, increase liquidity. Lastly, Meling (2021) discusses how traders in transparent markets use bluffing techniques to hide intentions, reducing the trading pattern that others follow.

5.0 Hypotheses

This section presents the hypotheses of the paper. Given that the anonymization of the Nasdaq Nordic exchanges was implemented in stages, featuring varying degrees of anonymity, we have developed separate hypotheses for each of the three main regulatory regimes.

5.1 Hypothesis I: The 2014 voluntary post-trade anonymity model

First, we examine the impact of the switch from multilateral transparency to voluntary post-trade anonymity, which was introduced to Main index and Large-cap stocks in 2014.[7] As vPoTA is introduced, we expect adverse selection to decrease because of broker codes being removed from executed orders, following the argument of Dennis and Sandås (2020) and Linnainmaa and Saar (2012). With broker codes hidden, informed traders are likely to experience lower adverse selection costs because it becomes more difficult for uninformed traders to engage in front-running, piggybacking, or using order anticipation strategies to capitalize on the informed market participant's orders (Poskitt et al., 2011; Harris, 1997). This is due to the signalling value of MPIDs (Harris, 1997). As a result, informed traders should be able to transact more freely and at a lower adverse selection cost, leading to improved liquidity. Similarly, market makers who adjust their quoted bid-ask spreads to cover costs related to adverse selection are expected to benefit from the introduction of anonymity. As the risk of a market maker being exploited by other market participants decreases, they can offer tighter bid-ask spreads. As a result, trading costs are reduced, which should increase market liquidity (Glosten and Milgrom, 1985). Consequently, the introduction of vPoTA in 2014 should significantly impact market liquidity through a decrease in bid-ask spreads and an increase in turnover. We therefore expect to reject the null hypothesis and develop the following alternative hypothesis:

- *$H0^1$: Liquidity is not impacted by the introduction of vPoTA in 2014.*
- *$H1^1$: Liquidity is impacted by the introduction of vPoTA in 2014.*

5.2 Hypothesis II: The 2019 & 2020 post-trade anonymity model

Secondly, we examine the impact of the incremental increase in anonymity for stocks within the Main Indexes, which transitioned from 2014's vPoTA regime to the new, stricter regime of PoTA in 2019. In contrast with 2014, when both the Main and Large-cap indexes were under vPoTA, in 2019, only the Main index stocks were moved to PoTA. Therefore, the remaining stocks in the Large-cap index, still trading under vPoTA, served as the control group. In 2020, the remaining stocks in the Large-cap indexes shifted from vPoTA to PoTA, joining the Main index stocks in the new regime. For this event, the Main index stocks, which already switched to PoTA in 2019, serve as the control group.

[7] In the 2014 event, the Main and Large-cap indexes serve as the treatment group while the Mid-cap index serves as the control.

We do not expect to see any significant increase in liquidity after either of these regulatory changes, as institutional traders had the choice to trade anonymously in these stocks since 2014. As a result, most of the decrease in information asymmetry, adverse selection costs, and consequent increase in liquidity should have already happened in 2014. Additionally, in line with the argument presented by Dennis and Sandås (2020), we believe that a mere change in anonymity does not necessarily lead to an impact on liquidity; rather, the magnitude of change is what has an impact. In this case, the change is minimal, as every member who wanted to trade anonymously has had the opportunity to do so for several years. Therefore, we do not expect the introduction of PoTA to significantly impact market quality and we do not expect the null hypotheses and develop the following alternative hypotheses:

- *$H0^2$: Liquidity is not impacted by the introduction of PoTA in 2019 & 2020.*
- *$H1^2$: Liquidity is impacted by the introduction of PoTA in 2019 & 2020.*

5.3 Hypothesis III: The 2022 post-trade anonymity model

Lastly, we examine the final step of anonymization of the Stockholm, Copenhagen, and Helsinki exchanges on the Nasdaq Nordic, which took place in 2022. On December 1st, 2022, trading in the Small-cap, Mid-cap and First North indexes went from multilateral transparency to PoTA. As a result of the change, we expect a lessened degree of adverse selection costs both for informed traders and market makers alike. This, in turn, should lead to an increase in market liquidity as informed and institutional traders no longer risk their orders being exploited by front-running and order anticipation strategies. Consequently, informed traders should be able to transact more freely, and at a lower trading cost, as they no longer have to *work* their orders by executing them dynamically (Harris, 1996; Linnainmaa and Saar 2012; Johnstone and Zheng, 2010).

Similarly, market makers are expected to quote lower spreads after the introduction of PoTA, which should decrease the spread and improve liquidity. As broker codes are hidden, informed traders can no longer exploit market makers with their informational advantage which should lead to lower adverse selection costs and a lower quoted spread (Copeland and Galai, 1983; Glosten and Milgrom, 1985; Kyle, 1985)

Additionally, previous literature such as Comerton-Forde and Tang (2009) have found that the anonymization of broker codes affects larger firms more than it affects smaller firms. We nonetheless expect to see significant improvements in liquidity for the event in 2022. This is due to the argument by Dennis & Sandås (2020) that predicts that a "smaller" change in anonymization should have limited effect on liquidity. In our 2022 event, the magnitude of change is by far the largest of our four events. As informed traders and market makers were previously highly exposed to exploitation in the multilateral transparency regime of the Mid-cap, Small-cap and First North indexes pre-2022, a switch to complete and mandatory PoTA should lead to significant improvements to liquidity. Consequently, we expect to reject the null hypothesis and develop the following alternative hypothesis:

- *$H0^3$: Liquidity is not impacted by the introduction of PoTA in 2022.*
- *$H1^3$: Liquidity is impacted by the introduction of PoTA in 2022.*

6.0 Data and sample description

This section describes the data sources, how it was collected, categorized and used. Furthermore, it discussed the exclusions made and outlines the sample size.

6.1 Sample selection

The empirical analysis utilizes data from the Nasdaq Nordic stock exchanges and the Bloomberg Terminal, covering a specific period defined by our event window. We gather daily price data and financial metrics via Bloomberg. Additionally, the "Equity Trading by Company and Instrument" monthly reports from Nasdaq Nordic supplied data on non-trading aspects such as ICB industry classifications and inclusion in index segments (Nasdaq OMX Nordic, n.d.b). Our initial dataset comprise all companies listed on the Nasdaq Nordic Stock Exchanges, categorized into Large- Mid- and Small-cap indexes. These categorizations are based on market capitalization (market cap) thresholds that are set annually by the exchange, with reclassifications occurring every January. Our analysis focuses on stocks within the Main index and Large- Mid- and Small-cap segments across the Stockholm, Helsinki, and Copenhagen stock exchanges.

Preferential shares are removed due to their characteristically low liquidity and sparse trading volume. These shares often exhibit infrequent trading activities, which can skew the analysis of bid-ask spreads by introducing noise and outliers that do not accurately reflect the market dynamics of more actively traded securities. Furthermore, preferential shares are often considered hybrid instruments and as our study is concerning equity instruments only, they were removed. Dual-class shares are also removed from the sample. For example, if a company has issued two shares, Class A, and Class B shares, the less liquid one was removed. These secondary class shares typically differ in voting rights and market participation, where usually, shares with higher voting rights (e.g., Class A) tend to be less liquid compared to their counterparts. The lower trading volume observed in these shares could lead to less reliable bid-ask spread data, which may not be indicative of the broader market trends. By focusing on the main shares, which are traded more frequently, the study aims to analyse a more robust dataset that better represents general market conditions. We also exclude firms that shifted indexes during our sampling period to avoid contamination between the treatment and control groups. Moreover, the Main index constituents (OMXS30, OMXH25, OMXC20/25) are separated from the Large-cap sample to form a separate Main list index. We specifically selected our sample from the Nasdaq Nordic exchanges, and more precisely, from stocks that are listed on the Stockholm, Copenhagen, and Helsinki exchanges. We do this because anonymity policies were implemented in a staggered manner over the period from 2014 to 2022, which provides us with a quasi-natural setting for testing the impact of these policies. The total sample includes 1,662 companies: 1,107 from the Stockholm Stock Exchange, 299 from the Helsinki Stock Exchange, and 256 from the Copenhagen Stock Exchange. The 2014 event has a total of 240 firms, 2019 has 156, 2020 has 278, and 2022 has 988 total firms, as detailed in Table III.

7.0 Methodology

The methodology section is used to discuss the study's choice of approach, the model specification, and its execution. Pre-regression diagnostics are discussed, as well as robustness checks to provide further accuracy in the models, which is then discussed in the empirical analysis.

7.1 Difference-in-differences and model specification

Nasdaq Nordic introduced four anonymity models starting with vPoTA in 2014 and PoTA in 2019, 2020, and 2022 for several different markets and index segments. The four unique models constitute quasi-natural experimental settings with clear event markers and distinct treatment groups, allowing us to apply a Difference-in-Differences (DiD) model to observe the differential effect of the treatment over time between a treatment group and a control group.

We construct two regression models which serve as our baseline DiD models. Model (1) is the most basic, containing no covariates while Model (2) contains dependent variables, explanatory variables and covariates which have been shown to explain variations in stock liquidity according to Harris (1994), Dennis and Sandås (2020), and Meling (2021). Both base models are run using robust standard errors. We run Model (1) and (2) on both of our dependent variables. The dependent and explanatory variables and covariates are further expanded on in sections 7.2 and 7.2.1.[8]

Main model specification:

$$(2)\ Y_{it} = \beta_0 + \beta_1 \text{Post}_t + \beta_2 \text{Treatment}_i + \beta_3 (\text{Post}_t \cdot \text{Treatment}_i) + \beta_4 \text{Market Cap (log)}_{it} + \beta_5 \text{Stock Price (log)}_{it} + \beta_6 \text{Price Variability}_{it} + \beta_7 \text{Free Float}_{it} + \varepsilon_{it}$$

Utilizing a DiD model allows us to evaluate the causal impact that post-trade anonymity has on market liquidity. The DiD model relies on three critical assumptions. First, the treatment and control group must remain consistent before and after the policy or treatment change, meaning that firms are not allowed to leave or enter the treatment or control group during the study period. Second, it is essential that there is no interference between the treatment and control groups, future treatment should not impact previous outcomes, and treatment should not fluctuate with a stock's liquidity. The third and most crucial assumption is the parallel trends assumption, which assumes that the outcome for the treatment group would have evolved in parallel with the mean outcome of the control group if it were not to receive treatment (Roth et al., 2022). The DiD approach differs from cross-sectional methods in that it does not necessarily require treatment and control groups with similar characteristics or balanced covariates before the treatment occurs. In a DiD analysis, a covariate that varies between the treatment and control groups and relates to the outcome is not inherently problematic. On the other hand, if a covariate influences the direction of the outcome, it is considered a confounder as it can lead to incorrect conclusions. Thus, it must be assumed that the control group's post-event outcome serves as a reliable stand-in for what would have happened to the treatment group had they not received the treatment (Zeldow and Hatfield, 2021).

7.2 Dependent and explanatory variables

[8] Variables will henceforth be in cursive.

To evaluate the impact of post-trade anonymity on liquidity, we use two established metrics as our dependent variables: the daily average *Bid-ask Spread* (%) and the total daily *Turnover*, in euros. The *Bid-ask Spread* serves as a primary indicator of liquidity at the stock level and, when combined with Turnover, provides a robust measure of overall market quality. This is in line with Dennis and Sandås (2020) and Meling (2021). Our empirical model also includes three explanatory variables:

1. **Treatment**: This dummy variable identifies whether a stock is part of the treatment group (Treatment = 1) or the control group (Treatment = 0).
2. **Post**: This variable denotes the timing relative to the introduction of treatment, with Post = 1 representing the dates after the event and 0 indicating the dates before the event.
3. **Post * Treatment**: This interaction term quantifies the differential impact of the treatment on the treatment group compared to the control group. The coefficient of this interaction term estimates the causal effect of the treatment, isolating the effect attributable to the treatment from other potential confounding factors.

7.2.1 Covariates

Harris (1994) provides a theoretical structure to understand variations in *Bid-ask Spreads*. He suggests incorporating the following variables into the analysis: a metric of trading activity, a volatility measure, an indicator of market maker competitiveness, and a variable that quantifies the level of information asymmetry. Harris also recommends analysing the impact of stock price minimum variation thresholds (tick-sizes) on the spread because stock prices vary within the set increments and can impact the *Bid-ask Spread*. Building on Harris's concepts, Meling (2021) and Dennis and Sandås (2020) apply similar principles. Meling (2021) utilizes *Share Price (log)*, *Intraday Returns*, *Market Cap (log)*, *Price/Book*, and several company operating metrics. Dennis and Sandås leverage propensity score matching using Market Cap (log), *Share Price (log)*, *Return Volatility*, *Broker Concentration*, and the average *Bid-ask Spread*. Taking these studies into account, we incorporate four covariates into our regression model. These covariates are *Market Cap* (log), *Stock Price* (log), *Price Variability* - the daily price variation measured as the daily high divided by the daily low, minus one, in percent, and the *Free Float* (log), which refers to the shares of a company that are available for public trading and are not restricted.

Table VI represents the correlation coefficients between the pairs of our variables for the events of 2014, 2019, 2020, and 2022. As observed in the table, the correlations between variables are consistent through every event and with what is economically expected. *Bid-ask Spread* shows a significant negative correlation with *Turnover* and *Market Cap,* which indicates that higher trading volume and higher firm sizes are associated with lower spreads. The negative correlation between *Bid-ask Spread* and *Stock Price* suggests that as stock prices increase, the *Bid-ask Spread* decreases, which is indicative of the diminishing effect of tick-sizes. Lastly, *Price Variability* shows, as expected, a positive correlation with *Bid-ask Spread* while *Free Float* has a negative correlation, meaning that as free float increases, the *Bid-ask Spread* decreases.

7.3 Event windows

The Difference-in-Differences method follows an event-study framework. To accurately assess the impact of the introduction of PoTA, we establish event windows surrounding each introduction date, ensuring enough trading days before and after the event. In the 2014, 2019, and 2020 events, the pre-event window begins on the first trading day of the year, as it aligns with the Nasdaq Nordic's annual market cap segment review. In the market cap segment review, select stocks are moved in and out of indexes (based on *Market Cap* or *Turnover* criteria), which may create issues with sample selection biases. By making sure that the event window starts right after the index adjustments have been made and ends before the following segment review, we can safely assume that there is no risk of companies moving in and out of their assigned group and contaminating our results. As the 2022 event takes place in December, its pre-event window starts in October and as such, firms in our treatment and control group changes indexes and anonymity status during the event window. Due to this, we drop any firms that change index during the 2022 event. The post-event windows are then modelled to mirror the length of the respective pre-event windows except for national holidays and other non-trading days. The 2014, 2019, 2020, and 2022 event windows are 112, 132, 126, and 104 days long respectively. A visualization of the event-window configuration can be found in Figure I.

7.3.1 Formation of treatment and control groups

Before the DiD method can be implemented, the treatment and control groups must be formed. The gradual implementation of anonymity provided a unique quasi-natural setting where different market segments were assigned differing types of anonymity at different times. This created ‘naturally’ occurring treatment groups that do not change over our event windows, which allows us to assign the treatment effect to these firms specifically.

In 2014, anonymity was introduced to the Main and Large-cap index segments, moving the included firms from multilateral transparency to vPoTA. Therefore, they serve as the treatment group. In the 2014 event, the Mid-cap index served as the control group as it remained fully transparent during the period and was deemed the most comparable segment. Next, in 2019, the Main indexes were used as the treatment group as they transitioned from vPoTA to mandatory and complete anonymity (PoTA). In this event, the Large-cap index segments served as the control group as they were still trading under vPoTA, allowing us to examine the incremental effect of mandatory anonymity. Continuing with 2020, the Large-cap indexes (excl. Main indexes) moved from vPoTA to PoTA and are used as the treatment group; in this case, the still multilaterally transparent Mid-cap index is used as the control group. Lastly, for the 2022 event, the Mid- and Small-cap indexes and First North were moved from multilateral transparency to mandatory PoTA; here, the large-cap index (excl. Main index stocks) is used as the control group as it traded under PoTA.

By basing the treatment groups on distinct and well-defined market segments assigned by Nasdaq Nordic that remained unchanged during our event windows, we minimize any selection bias while ensuring there is no contamination between the treatment and control groups. Each of these steps is designed to ensure the consistency of groups throughout the study period, minimize interference between them, and provide preliminary bases for assessing the effect of anonymization. Table IV shows the structure of our treatment and control groups for all four events.

7.4 Univariate tests

Before conducting the DiD regressions, we need to understand the characteristics of our data. Using t-tests, we can examine the difference in means between our treatment and control groups. We begin our empirical analysis by conducting simple univariate tests to compare the differences in means between firms in the control groups and firms in the treatment groups over the entire study period. The t-tests are conducted separately for each event, the results of our t-tests can be found in Table VII.

7.5 Difference-in-Differences regressions

In the second step of our empirical analysis, we explore the relationship between measures of liquidity (*Bid-ask Spread* and *Turnover*) and the anonymization of MPIDs with the previously specified model. We report a basic pooled ordinary least squares (POLS) regression without any covariates (Model 1) and one that includes all covariates that are specified in section 7.1 (Model 2). The two base regression models are then extended in Model (3), where we use the same specification as in Model (2) but also apply clustered robust standard errors on a firm basis to control for the potential issue of heteroskedasticity, which is to be expected in panel data. The decision to use clustered robust standard errors is supported by the White test, which indicates strong support for the existence of heteroskedasticity in all four events. After specifying Model (3), we run the Hausman test and find strong support for Fixed effects (FE) in all four events, rejecting the null hypothesis at the 1% level. This result indicates a correlation between unobserved entity-specific characteristics and our independent variables

Fixed effects help us control for, and minimize omitted variable bias, and is often a more efficient but also more restrictive estimator when compared to random effect. As such, FE should yield more reliable results in the presence of entity-specific characteristics.

Consequently, we extend our model and implement a FE DiD regression with two-way clustered standard errors (Model 4), where the standard errors are clustered both by firm and by date. By clustering at the firm level, we account for autocorrelation within firms over time, ensuring that firm-specific effects or trends do not influence our data points across time. Furthermore, by clustering on the time variable, we address cross-sectional correlation, as there might be events or shocks that affect multiple firms at specific points in time simultaneously. Going forward, Model (4) is used as our main model. This ensures that our regression model accounts for any unobserved, time-invariant characteristics specific to each firm, thereby improving the robustness and accuracy of our regression.

7.6 Propensity-score matching

As an additional step of our empirical method, we will introduce propensity-score matching (PSM) that, when applicable, can adjust confounding effects by balancing the treatment and control groups on observable covariates. We apply PSM to match firms in the treatment group, to which anonymity measures are introduced, with their closest peers in the control group that remain transparent.

Following the methodologies employed by Friederich and Payne (2014) and Dennis and Sandås (2020), we utilize PSM to establish a matched sample where the control and treatment groups exhibit more similar characteristics, thereby enhancing the comparability of our results. In their studies, Friederich and Payne (2014) match their treatment and control group on average company size and total turnover, while Dennis and Sandås (2020) match on company size, return volatility, stock price, and broker concentration. We, therefore, match our treatment and control groups on *Market Cap*, *Share Price*, and *Price Variability*. Due to the highly unbalanced treatment and control group in 2022, regarding the number of firms, we matched every control firm with its two nearest neighbours in the treatment group. The nearest neighbour's method may introduce bias to the score-matching method, but the trade-off is losing a lot of observations which would decrease the power of our models. After that, the matched sample is used to run a new regression model. The method of PSM is generally favoured in DiD analyses as it should help to reduce bias between groups; by employing PSM combined with DiD, we address the challenge of selection bias.

7.7 Parallel trends – Pre-DiD diagnostics

In our setting, the first two assumptions of the DiD method can be assumed to hold. Firstly, the treatment and control groups are stable over the entirety of our event windows and do not change at any point. Secondly, there is no interference between the groups; future treatment does not impact previous outcomes. However, the third and most critical assumption of the DiD, parallel trends, inherently involves a counterfactual scenario – it considers what the outcome would have been had the treatment not been implemented. Since this alternate reality is impossible to observe, we infer it by assuming that, in the absence of treatment, the change in the outcome for both the treatment and the control groups would have been consistent over time.

To control for parallel trends, we illustrate line graphs of the average *Bid-ask Spread* for all pre-event windows, which can be found in Figures II-VIII. We observe that the average Spreads and Turnover are, as expected, widely different between the treatment and control groups but that the change in outcome remains relatively consistent over time in all four tests. This indicates that the trends and changes to *Bid-ask Spreads* are largely parallel between our treatment and control groups.

7.8 Post DiD falsification and robustness tests

To ensure the validity and robustness of our DiD regressions on the effects of PoTA in 2014, 2019, 2020, and 2022, we implemented a series of falsification and robustness tests. These tests are designed to confirm that our findings are not artefacts of anomalous data or events immediately surrounding the implementation periods of anonymization.

As the first step in our robustness check, we remove the five days prior to anonymization and the five days after anonymization for all four of our events. The regressions are then run again on the new sample. This approach aims to mitigate the impact of potential short-term fluctuations which could distort the analysis, such as front-running the announcement or delayed reactions by market participants to the new anonymity rules. By removing five days, we ensure that our results are representative of the more stable, long-term effects of anonymization and are not influenced by immediate market speculations or adjustments.

Following the methodology outlined by Roberts and Whited (2013), we conducted a falsification test that involves performing the same DiD analysis as previously stated, but on pre-event data, assuming that no treatment effect should be observable if our model is correctly specified. For this test, we replicated our regression models using data from only the pre-event periods before anonymization was implemented. The logic behind this falsification test is straightforward: since the anonymization had not yet occurred during these earlier periods, any significant treatment effects detected would suggest the presence of underlying trends or biases in the data that could invalidate our main findings. Therefore, the expected outcome is that estimated treatment effects are statistically indistinguishable from zero, confirming the absence of pre-existing trends that could confound our analysis of the actual treatment periods.

8.0 Empirical analysis

This chapter introduces the results of the study, the interpretation, and the analysis of the data, followed by a test of each respective hypothesis. A discussion follows the respective anonymity regime, and a robustness analysis is conducted. The empirical analysis ends with discussing the study's method limitations.

8.1 Sample description

Table V reports the summary statistics for all four events in Panel A to D. As a result of the consistent right-skew in our datasets, we decided to winsorize all variables at 1% and 99% to minimize the impact of outliers and to help create more normally distributed samples. Panel A for 2014 shows that the *Bid-ask Spread* differ between 0.046% to ~2.84% suggesting varying market conditions and trading activity across firms. The mean *Bid-ask Spread* is 0.445%, and the median 0.304%, indicating a right-skewed distribution pulling the mean above the median. Similarly, *Turnover*, reflecting the volume of trade, averages 8.848 million EUR, with a median of 1.497 million EUR, further showing the skewed nature of trading activity. Moreover, *Market Cap* varies greatly, averaging 4,252 million EUR with a standard deviation of 8,668 million EUR, indicating, large differences in firm sizes. *Stock Price* displays similar characteristics where the mean is skewed due to some large outliers while *Price Variability* shows a quite balanced distribution.

In 2019, we find a smaller difference between the treatment and control firms because the control group consisted of the Main index while the treatment group was the Large-cap index. Yet, the standard deviation of *Market Cap*, being 12,190 million EUR, is much larger the both the mean (7,841) and median (3,384). We see a significant right-skew due to a few very large firms. This leads to our variable *Turnover* receiving a similar pattern, as firm size is heavily related to *Turnover*. The *Bid-ask Spreads* for Large-cap firms are much narrower than smaller firms, which is why we see a mean of only 0.146%. The standard deviation of the variable is 0.141, however. For 2020, we see a similar pattern as in 2019.

We find in Panel C that 2022 has a significantly larger sample size than the three previous events with over 130,000 observations. The reason for this is that Nasdaq introduced PoTA for all Mid- Small-cap and First North firms, which created a much larger treatment group to examine. This leads to the *Market Cap* ranging from 1.269 to 7,158 million EUR. The standard deviation for *Bid-ask Spreads* also significant, with 2.836 compared to the mean 0.595 and median 0.397.

Furthermore, we applied a logarithmic transformation to every variable except for *Price Variability*. This exception was made because the distribution of *Price Variability* shows much less skew compared to other variables. This approach has ensured that the statistical analyses are robust and less sensitive to extreme values, facilitating more reliable comparisons across the different groups within our study.

8.2 Univariate test results

Table VII shows the results from a two-sample t-test examining the differences in means of our key variables between the treatment and control groups. Our results align with theoretical predictions; we note significant differences between the control and treatment groups across all four policy regimes. Such differences are not only expected but also indicative of the distinct nature of the firms within each index category. Larger firms typically represent more established and stable entities with lower *Bid-ask Spreads* and higher *Turnover,* whereas smaller firms have greater volatility, larger spreads, with less shares being bought and sold.

The large difference between the treatment and control groups comes from the substantial difference in the size of firms included in each respective index. However, a Difference-in-Differences analysis should not be significantly affected by these discrepancies. This is because a DiD approach primarily seeks to identify relative changes within the groups over time rather than absolute differences.

8.3 The 2014 regime

Table VIII shows the regression results for the 2014 regime using the *Bid-ask Spread* (log)[9]. The results for our main explanatory variable *Treatment * Post*, shows no significance in our main Model (4). We initially found significant results, suggesting an additional reduction in *Bid-ask Spreads* for the treatment group following the introduction of vPoTA. But the significance disappears in Models 2-4 after introducing covariates and clustering standard errors on firm and time identifiers. In Model 2, the model has likely missed accounting for autocorrelation and clustered data, giving false significance. By introducing clustered standard errors in Model 3, we find that our results disappear, addressing the potential grouping of data, meaning that the results from the simpler models are likely to be overstated. Out of the three explanatory variables, we only find significant results for the variable *Treatment* at a 1% level, with a coefficient of -0.653. In this log-linear relationship, this means that the B*id-ask spread* for the treatment group is ~48% lower than the control group. This is to be expected as we find a strong negative correlation between firm size and *Bid-ask Spreads* in Table VI.

The control variable, *Market Cap,* is significantly negatively correlated with *Bid-ask Spreads,* meaning that larger firms have lower average *Bid-ask Spreads*. In model 4, we find a coefficient of -0.371 significant at a 1% level, meaning that a percent increase in *Market Cap* leads to a 0.371% decrease in the *Bid-ask Spread*. Moreover, *Price Variability,* which is highly statistically significant, indicates that larger swings in stock prices, lead to higher *Bid-ask Spreads,* on average. The variable *Stock Price* indicates that the *Bid-ask Spread* decreases when *Stock Price* increases, on average, which is consistent with the idea of the diminishing effect of tick sizes.

[9] Henceforth, variables are referred to without “log” for increased readability.

Table X represents the *Turnover* regressions. In our Main model (4), we find highly significant results (1% level) for the explanatory variables *Treatment* and *Post*, but no significance for the interaction term *Treatment * Post*. The *Treatment* variable has a coefficient of 0.776, which indicates that the treatment group has, on average, ~117.2% higher *Turnover* than the control group. This can be explained by the firms' size being highly correlated with *Turnover* (see Table VI). As the treatment is made up of Large-cap firms while the control group are Mid-caps, this is to be expected. Furthermore, the *Post* variable indicates that the treatment and control groups together have 1.8% lower *Turnover* after the introduction of vPoTA, on average. Two of the remaining covariates are statistically significant at a 1% level; these are *Market Cap* and *Stock Price*. *Price Variability* has no significance in our primary model.

The results for both the *Bid-ask Spread* and *Turnover* regressions are inconclusive for 2014. We use our main Model (4) to test the hypotheses that we expected to reject, but we cannot show that *Liquidity* is impacted by the introduction of vPoTA in 2014. Therefore, we cannot reject the null hypotheses: *$H0^1$: Liquidity is not impacted by the introduction of vPoTA in 2014.*

Comerton-Forde and Tang (2009) found that larger and highly liquid stocks received greater benefit of anonymity compared to smaller firms. They argue that the main reason for the reduction in spreads stems from a reduction in adverse selection. In other words, traders face less risk trading against someone with an information advantage, as no one knows who they are trading against. Their argument should be applicable to the introduction of vPoTA for Large-cap and Main index stocks, which are the most liquid stocks on the exchange. Why, then, do we not find the expected results for the 2014 regime? Before the introduction of vPoTA in 2014, some investors opted for sponsored access through Merrill Lynch to hide their MPIDs, effectively making their trades anonymous. However, this service became obsolete after Nasdaq introduced vPoTA, causing Merrill Lynch to drastically lose market share. This prior method of anonymous trading might explain why we did not find significant results, as investors who wanted to hide their trades already had a system in place to do so before Nasdaq's regulatory change. This would, in turn, have already limited uninformed traders from engaging in front-running and order anticipation strategies (Poskitt et al., 2011; Harris, 1997), reducing the impact of PoTA on trader behaviour. In effect, Dennis and Sandås's (2020) argument that the size of the regulatory change impacts the outcome can be applicable to 2014. Even if the regulatory change was, in practice, reasonably large, by moving from full transparency to voluntary anonymity, traders could already hide their trades via Merrill Lynch, weakening the impact of this regulatory effect.

Another important factor that may explain the non-significant results is the technological advancements introduced in 2010. The introduction of INET and CCP clearing has dramatically reduced trading costs and, by extension, decreased *Bid-ask Spreads* (a form of transaction cost) and led to more high-frequency traders, which improved liquidity. This reduction in spreads, trading costs, and overall improvement of market quality might explain why empirical studies that test on pre-2010 data find such large reductions in spreads in their samples, while we, in a modern market context do not.

8.4 The 2019 & 2020 regimes

In 2019 and 2020 we study the change to PoTA from vPoTA on the Main and Large-cap indexes, respectively. These events represent smaller regulatory changes compared to those in 2014 and 2022, as they involved a shift from a voluntary, anonymous regime to forced anonymity. Consequently, this change is less profound than the transition from complete transparency to voluntary anonymity. In Tables VIII and IX, we present the *Bid-ask Spread* regression results. We find no statistical significance for the interaction term *Treatment * Post* for either event. Model 2 for 2020 initially found significant results, which were lost when introducing clustered standard errors. The explanatory variable *Treatment* is highly statistically significant (1% level) throughout all Models in 2019, with a coefficient of –0.268 in Model 4, indicating that *Bid-ask Spreads* were ~23.5% lower for the treatment group. The 2020 event shows similar results but with weaker statistical significance. The economic implication, however, remains similar to 2019, although it has a smaller coefficient of –0.146. Both events, therefore, indicate that the *Treatment* group has smaller *Bid-ask Spreads* than their respective control group. In 2020, we find highly statistically significant results that the *Bid-ask Spread* for the entire sample increased by ~16% after the introduction of PoTA.

The *Turnover* results are found in Table X and XI. We find conflicting results for 2019 and 2020 for the interaction term *Treatment * Post*, where 2019 shows an additional reduction in *Turnover* following the introduction of PoTA, while 2020 reports an increase. Both results are statistically significant at the 1% level. In 2019, the coefficient is -0.081, and the results indicates that treatment firms saw an additional decrease in *Turnover* by an average of ~7.8%, while in the following year, the results show an average increase of ~25.5% with a coefficient of 0.219. The 2019 regression tells us that *Turnover* for the treatment group (Main indexes) on average is ~94.9% higher in the control group (Large-cap excl. Main indexes), before the introduction of PoTA. Similarly in 2020, the treatment group (Large-cap excl. Main index) had on average ~13.4% higher *Turnover* than the control group (Mid-cap). Moreover, the covariates for both events are similar both statistically and economically, except for *Stock Price* which shows no significance for the year 2019.

We found no evidence to support that the 2019 and 2020 events had a significant impact on the liquidity in the treatment group. We, therefore, cannot reject the null hypothesis: *$H0^2$: Liquidity is not impacted by the introduction of PoTA in 2019 & 2020.* Again, as the magnitude of the regulatory changes might affect its impact, it could serve as an explanation for the results in 2019 and 2020. The magnitude of change was not particularly large for neither of these regimes. The regulation brought post-trade anonymity from a voluntary, anonymous, transparent setting where traders could already hide their trades if they so wished. Thus, the reform may not be large enough to have a significant impact on *Bid-ask Spreads*. Furthermore, the decrease in adverse selection costs should have already happened in 2014, after the introduction of vPoTA, meaning that an improvement in liquidity should have already happened. Therefore, vPoTA may already have limited uninformed traders from engaging in front-running, which would explain why the move to PoTA resulted in no improvements to liquidity.

The Turnover regression results are surprising for 2019. We find a highly significant (1% level) additional reduction in Turnover for the treatment group compared to the control group, on average. This implies that turnover volume, on average, decreased, and market quality decreased after trading at the main indexes became fully anonymous. On one hand, these results are in line with the findings of Pham et al. (2015), on the other, it stands out from predictions made by Linnainmaa and Saar (2012). It also contradicts the findings of Comerton-Forde and Tang (2009), who found that larger firms receive greater benefits from anonymity. The 2019 regime affected Main index firms, the largest firms in the market, meaning that this regulatory change should have its greatest effect on these types of firms. Moreover, the economic implication of the coefficient is not very impactful, as it is quite small. Furthermore, when observing our data, we find that *Turnover* tends to be a much noisier and more volatile measurement when compared to the *Bid-ask Spread* which tends to be more stable over time. This might influence the outcomes of our regressions, hence why we consider the *Bid-ask Spread* to be our main dependent variable of interest.

The significant increase in *Turnover* in 2020 might also suffer from confounders because of the Covid-19 crisis, which likely also explains the increase in *Bid-ask Spreads* for the whole sample after the event. In times of distress, stock markets tend to exhibit significantly increased volatility and, as a result, increased turnover, and higher spreads. At its worst, the OMXS30 index had decreased as much as 33% in just under three and a half weeks (Börsdata), leading us to be sceptical regarding the *Turnover* results in 2020. Additionally, regulatory changes from the European Union, such as MiFID I and II and MiFIR, aimed at enhancing market efficiency by restricting dark pools, increasing some aspects of transparency, and reducing market manipulation, may have contributed to more efficient markets (Dang et a. 2020). Research looking at the market quality impact of MiFID II has found conflicting evidence that the regulation had adverse effects on market quality following a reduction in sell-side coverage (Anselmi and Petrella, 2021; Jenkinson et al., 2023).

8.5 The 2022 regime

In Table IX, we find the *Bid-ask Spread* regression results from our final event, the introduction of PoTA to all remaining indexes, which are Mid-cap, Small-cap, and First North. This was a switch from complete transparency to full anonymity – the largest regulatory change of Nasdaq policy regimes. We find highly statistically significant results for the interaction term *Treatment * Post* in all four Models, showing a coefficient of -0.108 for the main Model (4). This indicates that the firm's part of the treatment groups saw an average additional decrease of ~10.2% in *Bid-ask Spreads* compared to the control group. The other two explanatory variables, *Treatment* and *Post,* are also significant at the 1% level. For our main Model (4), *Treatment* has a coefficient of 0.463, while *Post* has a coefficient of -0.047. This means that the Treatment group has, on average, ~58.9% higher *Bid-ask Spreads* than the control group (as expected due to the substantial firm size difference between the groups), and that the entire sample saw an average decrease in *Bid-ask Spreads* of ~4.6% following anonymity. Our data sample indicates, therefore, that all firms saw lower *Bid-ask Spreads* after anonymization, while the treatment group received a larger additional decrease compared to the control group.

All four covariates are statistically significant throughout all models. *Market Cap* has a strong negative relation to *Bid-ask Spreads,* valid throughout all our events. The covariate *Stock Price* has a positive relation to the *Bid-ask Spread,* with similar statistical and economic impact to 2020, but contrary to 2014 and 2019. *Free Float* also has the same sign as 2019 and 2020 and has similar coefficients, although 2022 indicates a lower economic impact, with a coefficient of –0.332. Lastly, *Price Variability* is similar to all three other events, with a coefficient of 3.594.

In Table XI, we find the regression results with *Turnover* as the dependent variable. We find highly statistically significant results for both *Treatment * Post* and the *Treatment* variable, significance is lost in the *Post* variable for Models 1,3 and 4. The results from the main Model (4), indicate that treatment firms on average saw an additional increase in *Turnover* by ~15.3%, after anonymity, compared to control firms. Moreover, we find that the *Treatment* firms (Mid-Small-cap and First North), on average, have ~52.5% lower daily average *Turnover* than the control firms before PoTA. Lastly, the results suggest that all firms received an increase in *Turnover* following the introduction of PoTA, although the *Post* coefficient has no statistical significance.

Both regressions for 2022 yielded highly statistically significant results, as anticipated in our hypothesis development. Using our main Model (4), we reject the null hypothesis: *$H0^3$: Liquidity is not impacted by the introduction of PoTA in 2022*. Interestingly, despite anonymization in 2022 being introduced to medium-sized to very small firms, it is the only event where we found highly statistically significant results. Although, Comerton-Forde and Tang (2009) argue that highly liquid and large firms receive greater benefit from anonymity, we believe their argument may still hold. This might be because the regulatory change in 2022 was arguably the most significant one in Nasdaq Nordic's history, leading us to anticipate meaningful results. This event stands out from the previous events because the affected indexes transitioned from a completely transparent setting to complete anonymity (PoTA). Another notable aspect of this new regime is that much smaller firms received PoTA, whereas previously, only large firms had been "experimented" on over the years. This shift created a unique research context, enabling us to determine whether empirical predictions hold for smaller firms.

A common strategy for traders is to anticipate orders, predicting future orders by acting on broker code information. By introducing PoTA, this strategy was rendered obsolete. Our results align with the findings of Friederich and Payne (2014), who found increased liquidity following trader's inability to predict orders, thereby reducing adverse selection costs (Madhavan, 2000). In an anonymous market, (informed) traders no longer must work orders or use bluffing techniques to reduce transaction costs (Meling, 2021; Harris, 1996). Our results are in line with Dennis and Sandås (2020), Meling (2021), and Hachmeister and Schiereck (2010), who also found increased liquidity following anonymity.

To complement the regression results, we illustrate the immediate and longer-term effect of treatment on the *Bid-ask Spread* after the introduction of PoTA in 2022 in Graph VIII. While there is an immediate effect on the treatment group right after the treatment takes place, the *Bid-ask Spread* quickly returns to the parallel trend. It is not until January that we suddenly see a large, pronounced differentiation between the treatment and control groups, showcasing a somewhat delayed reaction to the anonymization. The reason behind this effect is unknown.

8.6 Tertile test

To deepen our analysis, we conducted tertile regressions on the *Bid-ask Spread* for the events of 2014 and 2022 to see if the results were different for different sizes of firms. We split the samples into three sizes, Large, Medium, and Small, based on their *Market Cap*. Panel A, showing the 2014 results in table XII, shows significance at the 5% level for the Small tertile, with a coefficient of –0.059 for the interaction term *Size * Post*. Interestingly, these results contrast with the findings of Comerton-Forde and Tang (2009), who noted that larger firms benefit more from anonymity. The Medium and Large tertiles have no statistical significance however, making these results inconclusive. One possible explanation for our findings is that the larger Large-cap firms had more of its trading executed through Merrill Lynch (Sponsored trading), and therefore saw a non-significance decrease in Bid-ask Spreads following PoTA. We note that this is speculative.

The results in Panel B for the 2022 event, on the other hand, align with the findings of Comerton-Forde and Tang (2009). The three tertiles indicate that the larger the firm, the greater the decrease in Bid-ask Spreads. The interaction term *Size * Post* is highly statistically significant. It goes from -8.0% for the Small tertile to -10.4% in the Medium and -11.8% in the Large tertile, indicating that larger firms within our "smaller" sample benefit more from anonymity. Given the conflicting evidence and differences between the treatment groups in the 2022 and 2014 events, we are led to believe that there potentially is a *sweet spot* where anonymization has its most pronounced effect on liquidity.

8.7 Robustness checks

To further verify the legitimacy of our results, we employed additional robustness checks. The first utilized Propensity Score Matching (PSM), the results are presented in Table XIII. Even with PSM, which enhances the comparability between the treatment and control groups, we still observed significant results at the 1% level in both *Bid-ask Spreads* and *Turnover*. The only exception is a slight reduction in significance, from a 1% level to a 5% level, for the interaction term in the *Turnover* regression for 2022. The second test excludes five days before and after the event day to control for potential noise effects. The regression using the dependent variable *Bid-ask Spread* (Table XIV) shows no improvements in statistical significance for 2014, 2019, and 2020, which is expected. The event of 2022 keeps its significance at the 1% level on all variables. The regression using the dependent variable *Turnover* keeps the statistical significance, with only a slight change in its economic impact. In the third robustness test, we conducted a falsification test by running our main regression model for 2022 on pre-event data, assuming that no treatment effect should appear, as the anonymization had not yet happened. As shown in Table XV, the results disappear for both the *Bid-ask Spread* and *Turnover* regression with only a slight change in economic impact.

In essence, our robustness tests confirm the validity of our results. We maintain both statistical significance and economic implications after applying PSM and the "five-day noise test" across all events. The falsification test yielded no significant results for the interaction term, indicating that the results are not random. Our regression results using our main Model (4) throughout all events, should therefore, be robust.

8.8 Method limitations

The main issue we encounter regarding the limitations of our model is that of confounding factors. Earlier papers such as Friederich and Payne (2014) and Hachmeister and Schiereck (2010), faced significant challenges with confounders because their treatment and control groups are made up of firms listed in different countries with different exchange operators. This may introduce a variety of extraneous variables that differ by region, such as economic conditions, regulatory environments, and market dynamics, which can obscure the true effect of the treatment.

To address these issues, our study draws inspiration from the approach employed by Dennis and Sandås (2020), which compares firms across Sweden, Denmark, and Finland - all operated under the Nasdaq Nordic umbrella. This uniformity in stock exchange operations across the sample helps mitigate some confounders related to cross-market comparisons, particularly those regarding regulatory dynamics and market operations. However, despite this more controlled setting, our approach is not without vulnerabilities. For instance, the extended pre- and post-event periods in our study could still allow uncontrolled cross-country differences to affect trading behaviours. These differences might stem from cultural or economic disparities that are not directly linked to the exchange operator itself.

Moreover, specific events, such as the COVID-19 pandemic in 2020 introduced unprecedented market volatility and could have significantly influenced trading behaviours, thereby complicating the isolation of the treatment effects from these external shocks. Acknowledging these complexities, our study employs a DiD approach rather than a Regression Discontinuity Design. While RDD, as used by Meling (2021), offers a robust framework by focusing on a sharp cutoff for treatment assignment and thereby minimizing confounders, it was not feasible for our cross-market setup.

One solution to mitigate cross-market confounders, would be to analyse the Stockholm, Copenhagen, and Helsinki exchanges separately. This would allow us to explore localized impacts of the treatment group and offer a setting without cross-exchange comparisons. However, such an approach would need careful consideration of each market conditions and might limit the generalizability of findings across the broader Nasdaq Nordic region.

9.0 Conclusion

The final chapter of the paper is based on the empirical analysis, which is used as a background for potential conclusions to be made.

This paper investigates the impact of various post-trade anonymity regimes on liquidity within the Stockholm, Copenhagen, and Helsinki exchanges. The paper aims to answer the following research questions:

> RQ^1: *Does the introduction of post-trade anonymity impact the liquidity for stocks trading on the Nasdaq Nordic?*
>
> RQ^2: Does *firm-size affect the liquidity impact for stocks receiving anonymity as Nasdaq Nordic?*
>
> RQ^3: *Does the magnitude of regulatory change affect the impact on liquidity?*

By analyzing the transition from transparency, voluntary, and forced post-trade anonymity across various index segments in 2014, 2019, 2020, and 2022, our research aims to understand how regulatory changes in post-trade anonymity affect market quality.

The introduction of vPoTA in 2014 gave inconclusive evidence of having an impact on liquidity. We argue that sponsored trading through Merrill Lynch may have mitigated the impact of the regulation, along with the technological advancements made in the 2010s. In 2019 and 2020, the incremental move from voluntary to complete anonymity (PoTA) gave no significant improvement in liquidity. The results found no support for decreased *Bid-ask Spreads,* and inconsistent results for *Turnover*. Explanations for the results may be noise effects, market regulation such as MiFID II/CCP clearing, or other exogenous factors such as Covid-19, affecting and distorting the data in our sample. In 2022, the switch from full multilateral transparency to complete anonymity (PoTA) for the Mid-cap, Small-cap, and First North indexes gave highly statistically significant results for a somewhat delayed improvement in Liquidity. Where treatment firms showed an ~10.2% additional average decrease in bid-ask spreads, and ~15.3% increase in turnover, compared to its control group (Large-cap excl. Main index). The substantial regulatory change in 2022 likely caused a significant impact on the liquidity of the treatment group, increasing market quality, even after technological advancements, new market regulation, and other factors.

The paper contributes to the literature on liquidity and post-trade anonymity in several ways. Firstly, our research investigates trading anonymity under new market conditions, with INET and CCP clearing, and under the regulation of MiFID II and MiFIR, which has significantly decreased trading costs and changed market dynamics. Secondly, our results indicate that there might be a sweet spot where anonymization has its most pronounced effect on liquidity. Lastly, our results support the argument by Dennis and Sandås (2020) that the magnitude of regulatory change may affect its impact on liquidity.

Future research should investigate whether Nasdaq's decision to anonymize the entire Nordic market has different effects on different market participants, aiding some investors more than others, as proposed by Linnainmaa and Saar (2012). For example, research should explore whether there is an optimal trade-off between better market liquidity at the expense of uninformed traders.

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11.0 Tables

Table I

Below is an illustrative table that showcases the effect of MPID anonymization (PoTA) from transparency. MPIDs are visible to third parties under transparency and hidden following anonymity.

Before post-trade anonymity				
Buyer	Seller	Volume	Price	Time
MLI	AVA	7 859	33,72	12:59:54
AVA	NRD	200	33,84	12:59:25
NRD	MLI	1 123	33,70	12:59:20

After post-trade anonymity				
Buyer	Seller	Volume	Price	Time
-	-	7 859	33,72	12:59:54
-	-	200	33,84	12:59:25
-	-	1 123	33,70	12:59:20

Table II

The table below represents Nasdaq OMX Nordic's anonymity models, showing the date for policy change, the respective index, and their corresponding post-trade reporting policy. vPoTA means voluntary post-trade anonymity, and PoTA refers to Post-trade anonymity.

Date	**Mid-cap, Small-cap, First North**	**Large-cap (excl. Main indexes)**	**Main indexes**
Apr. 2009	Transparent	Transparent	Transparent
Mar. 2014	Transparent	vPoTA	vPoTA
Apr. 2019	Transparent	vPoTA	PoTA
Apr. 2020	Transparent	PoTA	PoTA
Dec. 2022	PoTA	PoTA	PoTA

Table III - Total sample distributions for 2014 to 2022

The table represents the distribution of firms amongst the three exchanges, Stockholm (SSE), Helsinki (HSE), and Copenhagen (CSE).

Event	SSE	HSE	CSE	Total
2014	136	60	44	**240**
2019	89	33	34	**156**
2020	191	49	38	**278**
2022	691	157	140	**988**

Table IV

The table below represents the treatment and control groups for the events 2014, 2019, 2020, and 2022, showing what market segment that serves as the treatment and control group for the corresponding years.

Event	Treatment	Control
2014	Large-cap (inc. Main indexes)	Mid-cap
2019	Main indexes	Large-cap (excl. Main indexes)
2020	Large-cap (excl. Main indexes)	Mid-cap
2022	Mid-cap, Small-cap, First North	Large-cap (excl. Main indexes)

Table V: Summary statistics

Table IV showcases descriptive statistics for all four event periods, using the variables *Bid-ask Spread (%), Turnover (MEUR), Market Cap (MEUR), Stock Price, Free Float* (missing for 2014), and *Price Variability*. *Bid-ask Spread* is the daily average difference between the bid and ask price over, at least, 10 observations expressed as a percentage. *Turnover* represents the total amount of transacted shares over a day times the respective share price for every transaction in EUR. *Market Cap* represents that size of the firm, calculated as the total outstanding shares times the share price in EUR. *Stock Price* is the share price converted into EUR. *Free Float* refers to the total number of shares that can be traded publicly as a percentage. Lastly, *Price Variability* is a measurement of the percentual difference between the intraday high and low stock price, calculated as $\frac{Price\ high}{Price\ low} - 1$. All variables except *Price Variability* are winsorized.

Panel A: 2014	**Mean**	**Median**	**SD**	**Min**	**Max**	**N**
Bid-ask Spread (%)	.445	.304	.480	.046	2.84	31,290
Turnover (MEUR)	8.848	1.497	17.438	.006	96.552	31,290
Market Cap (MEUR)	4,252.323	1,058.260	8,668.242	129.081	53,218.793	31,290
Stock Price (EUR)	18.537	9.364	44.404	.329	388.369	31,290
Price Variability (%)	.022	.019	.014	.002	.08	31,290
Panel B: 2019	**Mean**	**Median**	**SD**	**Min**	**Max**	**N**
Bid-ask Spread (%)	.146	.109	.141	.029	.951	21,043
Turnover (MEUR)	13.881	6.78	17.751	.078	95.804	21,043
Market Cap (MEUR)	7,840.889	3,383.943	12,189.960	666.56	90,290.82	21,043
Stock Price (EUR)	31.46	14.02	88.422	1.129	774.832	21,043
Free Float (%)	73.562	77.71	21.138	15.653	100	21,043
Price Variability (%)	.022	.019	.012	.006	.074	21,043
Panel C: 2020	**Mean**	**Median**	**SD**	**Min**	**Max**	**N**
Bid-ask Spread (%)	.595	.397	.568	.063	3.174	34,665
Turnover (MEUR)	3.481	.957	6.674	.008	40.784	34,665
Market Cap (MEUR)	1,448.525	566.639	2,688.129	84.182	19,226.012	34,665
Stock Price (EUR)	12.434	8.307	14.726	.34	95.824	34,665
Free Float (%)	65.893	68.487	22.062	5.351	100	34,665
Price Variability (%)	.046	.037	.031	.006	.171	34,665
Panel D: 2022	**Mean**	**Median**	**SD**	**Min**	**Max**	**N**
Bid-ask Spread (%)	2.563	1.693	2.836	.097	15.392	137,942
Turnover (MEUR)	.597	.029	1.757	0	11.673	137,942
Market Cap (MEUR)	449.423	62.201	1,133.558	1.269	7,158.694	137,942
Stock Price (EUR)	7.27	2.44	13.802	.013	94.94	137,942
Free Float (%)	66.633	68.055	23.324	11.708	100	137,942
Price Variability (%)	.056	.041	.051	0	.295	137,942

Table VI: Pairwise correlation tables

The table showcases the statistical relationship across our four event 2014, 2019, 2020, and 2022 between the dependent variables *Bid-ask Spread (log), Turnover (log), and the covariates Market Cap (log), Stock Price (log), Free Float (log,* missing for 2014), and *Price Variability*. Each panel set represents the correlation coefficients of each variable in the given year (Panel A is missing the variable *Free float* (log)$_{i,\ t.}$

Panel A: 2014	(1)	(2)	(3)	(4)	(5)	
(1) Bid-ask Spread (log) $_{i,t}$	1.000					
(2) Turnover (log)$_{i,t}$	-0.867***	1.000				
(3) Market Cap (log)$_{i,t}$	-0.836***	0.791***	1.000			
(4) Stock Price (log)$_{i,t}$	-0.319***	0.269***	0.335***	1.000		
(5) Price Variability$_{i,t}$	0.231***	0.029***	-0.209***	-0.128***	1.000	
Panel B: 2019	(1)	(2)	(3)	(4)	(5)	(6)
(1) Bid-ask Spread (log) $_{i,t}$	1.000					
(2) Turnover (log)$_{i,t}$	-0.852***	1.000				
(3) Market Cap (log)$_{i,t}$	-0.710***	0.667***	1.000			
(4) Stock Price (log)$_{i,t}$	-0.125***	0.145***	0.320***	1.000		
(5) Free Float (log)$_{i,t}$	-0.441***	0.467***	0.143***	-0.156***	1.000	
(6) Price Variability$_{i,t}$	0.195***	0.111***	-0.178***	-0.081***	0.046***	1.000
Panel C: 2020	(1)	(2)	(3)	(4)	(5)	(6)
(1) Bid-ask Spread (log) $_{i,t}$	1.000					
(2) Turnover (log)$_{i,t}$	-0.774***	1.000				
(3) Market Cap (log)$_{i,t}$	-0.695***	0.667***	1.000			
(4) Stock Price (log)$_{i,t}$	-0.126***	0.115***	0.356***	1.000		
(5) Free Float (log)$_{i,t}$	-0.310***	0.351***	0.012**	-0.095***	1.000	
(6) Price Variability$_{i,t}$	0.274***	0.155***	-0.160***	-0.157***	0.025***	1.000
Panel D: 2022	(1)	(2)	(3)	(4)	(5)	(6)
(1) Bid-ask Spread (log) $_{i,t}$	1.000					
(2) Turnover (log)$_{i,t}$	-0.806***	1.000				
(3) Market Cap (log)$_{i,t}$	-0.816***	0.761***	1.000			
(4) Stock Price (log)$_{i,t}$	-0.529***	0.453***	0.718***	1.000		
(5) Free Float (log)$_{i,t}$	-0.078***	0.135***	-0.071***	-0.144***	1.000	
(6) Price Variability$_{i,t}$	0.316***	-0.023***	-0.297***	-0.316***	0.077***	1.000

*** p<0.01, ** p<0.05, * p<0.1*

Table VII: T-test

The table represents the univariate tests comparing the means of the control and treatment groups for 2014, 2019, 2020, and 2022. The table includes the number of observations, the main and median values, the differences in means and the standard error. The variable *Free Float (log)* $_{i,t}$ is missing for 2014. Asterisks refers to the statistical significance levels *** p<0.01, ** p<0.05, * p<0.1.

2014	Observations Control	Observations Treatment	Mean Control	Mean Treatment	dif	St Err
Bid-ask Spread (log)	16,990	14,300	-0.564	-2.131	1.568***	.007
Turnover (log)	16,990	14,300	12.690	15.873	-3.183***	.018
Market cap (log)	16,990	14,300	19.968	22.283	-2.315***	.009
Stock price (log)	16,990	14,300	1.851	2.578	-.727***	.012
Price variability	16,990	14,300	.025	.019	.005***	.000
2019	Observations Control	Observations Treatment	Mean Control	Mean Treatment	dif	St Err
Bid-ask Spread (log)	11,338	9,705	-1.803	-2.668	.865***	.008
Turnover (log)	11,338	9,705	14.725	16.609	-1.885***	.016
Market cap (log)	11,338	9,705	21.456	22.869	-1.413***	.011
Stock price (log)	11,338	9,705	2.376	3.01	-.635***	.014
Free float (log)	11,338	9,705	4.128	4.378	-.251***	.005
Price Variability	11,338	9,705	.022	.022	.001***	0
2020	Observations Control	Observations Treatment	Mean Control	Mean Treatment	dif	St Err
Bid-ask Spread (log)	24,118	10,547	-0.556	-1.611	1.054***	.008
Turnover (log)	24,118	10,547	12.975	15.277	-2.302***	.018
Market cap (log)	24,118	10,547	19.730	21.634	-1.903***	.009
Stock price (log)	24,118	10,547	1.881	2.38	-.498***	.012
Free float (log)	24,118	10,547	4.093	4.126	-.033***	.005
Price Variability	24,118	10,547	.048	.042	.006***	.001
2022	Observations Control	Observations Treatment	Mean Control	Mean Treatment	dif	St Err
Bid-ask Spread (log)	15,316	122,626	-1.556	.597	-2.153***	.008
Turnover (log)	15,316	122,626	14.662	9.951	4.71***	.018
Market cap (log)	15,316	122,626	21.569	17.625	3.943***	.013
Stock price (log)	15,316	122,626	2.582	.347	2.236***	.015
Free float (log)	15,316	122,626	4.154	4.112	.042***	.004
Price Variability	15,316	122,626	.035	.06	-.026***	.001

Table VIII: Bid-ask Spread regression tables for 2014 & 2019

The table below shows eight regression models analysing the *Bid-ask Spread* (in log) for 2014 and 2019. The models differ in the method used, and the variables used. The table shows the dependent variables, the coefficients, standard errors, and the respective significance levels. *Free Float (log)* $_{i,t}$ is missing for 2014.

Panel A: 2014	**Model 1**	**Model 2**	**Model 3**	**Model 4**
Variable (log)	Bid-ask Spread	Bid-ask Spread	Bid-ask Spread	Bid-ask Spread
Method	POLS	POLS	POLS	FE
$Treatment_t$	-1.550***	-0.653***	-0.653***	-0.653***
	(0.010)	(0.011)	(0.092)	(0.091)
$Post_i$	-0.026***	-0.016**	-0.016	-0.016
	(0.009)	(0.008)	(0.014)	(0.015)
$Treatment_i$ * $Post_t$	-0.032**	-0.021*	-0.021	-0.021
	(0.013)	(0.011)	(0.021)	(0.019)
Market Cap $(log)_{i,t}$		-0.371***	-0.371***	-0.371***
		(0.003)	(0.035)	(0.034)
Stock Price $(log)_{i,t}$		-0.029***	-0.029	-0.029
		(0.003)	(0.035)	(0.035)
Price $Variability_{i,t}$		3.968***	3.968***	3.968***
		(0.230)	(0.980)	(0.984)
Constant	-0.548***	6.809***	6.809***	6.809***
	(0.007)	(0.068)	(0.672)	(0.671)
Observations	31,290	31,290	31,290	31,290
R-squared	0.639	0.750	0.750	0.750

Panel B: 2019	**Model 1**	**Model 2**	**Model 3**	**Model 4**
$Treatment_t$	-0.866***	-0.268***	-0.268***	-0.268***
	(0.011)	(0.009)	(0.071)	(0.071)
$Post_i$	-0.025**	-0.008	-0.008	-0.008
	(0.011)	(0.009)	(0.027)	(0.027)
$Treatment_i$ * $Post_t$	0.002	-0.005	-0.005	-0.005
	(0.015)	(0.012)	(0.032)	(0.030)
Market Cap $(log)_{i,t}$		-0.360***	-0.360***	-0.360***
		(0.003)	(0.033)	(0.033)
Stock Price $(log)_{i,t}$		0.062***	0.062	0.062
		(0.003)	(0.038)	(0.038)
Free Float $(log)_{i,t}$		-0.472***	-0.472***	-0.472***
		(0.012)	(0.123)	(0.123)
Price $Variability_{i,t}$		6.696***	6.696***	6.696***
		(0.256)	(1.099)	(1.106)
Constant	-1.790***	7.572***	7.572***	7.572***
	(0.008)	(0.084)	(0.855)	(0.852)
Observations	21,043	21,043	21,043	21,043
R-squared	0.372	0.628	0.628	0.628
Standard errors	Robust	Robust	Firm clustered	Firm-date clustered

Standard errors are in parentheses
*** $p<.01$, ** $p<.05$, * $p<.1$

Table IX: Bid-ask Spread regression tables for 2020 & 2022

The table below shows eight regression models analysing the *Bid-ask Spread* (in log) for 2020 and 2022. The models differ in the method used, and the variables used. The table shows the dependent variables, the coefficients, standard errors, and the respective significance levels.

Panel A: 2020	**Model 1**	**Model 2**	**Model 3**	**Model 4**
Variable (log)	Bid-ask Spread	Bid-ask Spread	Bid-ask Spread	Bid-ask Spread
Method	POLS	POLS	POLS	FE
$Treatment_t$	-1.043***	-0.146***	-0.146*	-0.146*
	(0.011)	(0.010)	(0.080)	(0.080)
$Post_i$	0.147***	0.148***	0.148***	0.148***
	(0.009)	(0.007)	(0.017)	(0.024)
$Treatment_i$ * $Post_t$	-0.023	-0.032***	-0.032	-0.032
	(0.015)	(0.010)	(0.028)	(0.027)
Market Cap $(\log)_{i,t}$		-0.471***	-0.471***	-0.471***
		(0.004)	(0.033)	(0.033)
Stock Price $(\log)_{i,t}$		0.107***	0.107***	0.107***
		(0.003)	(0.028)	(0.028)
Free Float $(\log)_{i,t}$		-0.543***	-0.543***	-0.543***
		(0.007)	(0.062)	(0.062)
Price $Variability_{i,t}$		5.743***	5.743***	5.743***
		(0.102)	(0.399)	(0.453)
Constant	-0.629***	10.411***	10.411***	10.411***
	(0.007)	(0.079)	(0.692)	(0.694)
Observations	34,665	34,665	34,665	34,665
R-squared	0.339	0.639	0.639	0.639

Panel B: 2022	**Model 1**	**Model 2**	**Model 3**	**Model 4**
$Treatment_t$	2.197***	0.463***	0.463***	0.463***
	(0.009)	(0.009)	(0.068)	(0.068)
$Post_i$	-0.113***	-0.047***	-0.047***	-0.047***
	(0.010)	(0.007)	(0.013)	(0.014)
$Treatment_i$ * $Post_t$	-0.067***	-0.108***	-0.108***	-0.108***
	(0.012)	(0.008)	(0.015)	(0.017)
Market Cap $(\log)_{i,t}$		-0.460***	-0.460***	-0.460***
		(0.002)	(0.016)	(0.016)
Stock Price $(\log)_{i,t}$		0.069***	0.069***	0.069***
		(0.001)	(0.012)	(0.012)
Free Float $(\log)_{i,t}$		-0.332***	-0.332***	-0.332***
		(0.004)	(0.038)	(0.038)
Price $Variability_{i,t}$		3.594***	3.594***	3.594***
		(0.044)	(0.180)	(0.183)
Constant	-1.484***	9.476***	9.476***	9.476***
	(0.008)	(0.040)	(0.379)	(0.379)
Observations	137,942	137,942	137,942	137,942
R-squared	0.337	0.718	0.718	0.718
Standard errors	Robust	Robust	Firm clustered	Firm-date clustered

Standard errors are in parentheses

**** p<.01, ** p<.05, * p<.1*

Table X: Turnover regression tables for 2014 & 2019

The table below shows eight regression models analysing *Turnover* (in log) for 2014 and 2019. The models differ in the method used, and the variables used. The table shows the dependent variables, the coefficients, standard errors, and the respective significance levels. *Free Float (log)* $_{i,t}$ is missing for 2014

Panel A: 2014	**Model 1**	**Model 2**	**Model 3**	**Model 4**
Variable (log)	Turnover	Turnover	Turnover	Turnover
Method	POLS	POLS	POLS	FE
$Treatment_t$	3.179***	0.776***	0.776***	0.776***
	(0.028)	(0.028)	(0.212)	(0.212)
$Post_i$	-0.177***	-0.134***	-0.134***	-0.134***
	(0.026)	(0.021)	(0.035)	(0.042)
$Treatment_i$ * $Post_t$	0.005	-0.008	-0.008	-0.008
	(0.037)	(0.029)	(0.047)	(0.044)
Market Cap $(log)_{i,t}$		1.122***	1.122***	1.122***
		(0.009)	(0.092)	(0.091)
Stock Price $(log)_{i,t}$		0.018**	0.018	0.018
		(0.008)	(0.082)	(0.082)
Price $Variability_{i,t}$		35.317***	35.317***	35.317***
		(0.590)	(2.349)	(2.365)
Constant	12.793***	-10.560***	-10.560***	-10.560***
	(0.020)	(0.178)	(1.776)	(1.773)
Observations	31,290	31,290	31,290	31,290
R-squared	0.492	0.687	0.687	0.687
Panel B: 2019	**Model 1**	**Model 2**	**Model 3**	**Model 4**
$Treatment_t$	1.939***	0.667***	0.667***	0.667***
	(0.022)	(0.018)	(0.131)	(0.130)
$Post_i$	0.065***	0.018	0.018	0.018
	(0.023)	(0.016)	(0.027)	(0.037)
$Treatment_i$ * $Post_t$	-0.108***	-0.081***	-0.081**	-0.081***
	(0.032)	(0.025)	(0.034)	(0.029)
Market Cap $(log)_{i,t}$		0.743***	0.743***	0.743***
		(0.008)	(0.081)	(0.081)
Stock Price $(log)_{i,t}$		-0.056***	-0.056	-0.056
		(0.007)	(0.079)	(0.079)
Free Float $(log)_{i,t}$		1.069***	1.069***	1.069***
		(0.022)	(0.226)	(0.225)
Price $Variability_{i,t}$		24.924***	24.924***	24.924***
		(0.528)	(2.078)	(2.227)
Constant	14.692***	-6.074***	-6.074***	-6.074***
	(0.016)	(0.197)	(2.019)	(2.013)
Observations	21,043	21,043	21,043	21,043
R-squared	0.396	0.632	0.632	0.632
Standard errors	Robust	Robust	Firm clustered	Firm-date clustered

Standard errors are in parentheses
**** p<.01, ** p<.05, * p<.1*

Table XI: Turnover regression tables for 2020 & 2022

The table below shows eight regression models analysing *Turnover* (in log) for 2020 and 2022. The models differ in the method used, and the variables used. The table shows the dependent variables, the coefficients, standard errors, and the respective significance levels.

Panel A: 2020	**Model 1**	**Model 2**	**Model 3**	**Model 4**
Variable (log)	Turnover	Turnover	Turnover	Turnover
Method	POLS	POLS	POLS	FE
$Treatment_t$	2.206***	0.126***	0.126	0.126
	(0.022)	(0.021)	(0.156)	(0.156)
$Post_i$	-0.263***	-0.161***	-0.161***	-0.161***
	(0.021)	(0.015)	(0.031)	(0.040)
$Treatment_i$ * $Post_t$	0.193***	0.219***	0.219***	0.219***
	(0.033)	(0.021)	(0.046)	(0.047)
Market Cap $(log)_{i,t}$		1.146***	1.146***	1.146***
		(0.009)	(0.076)	(0.076)
Stock Price $(log)_{i,t}$		-0.145***	-0.145**	-0.145**
		(0.007)	(0.063)	(0.063)
Free Float $(log)_{i,t}$		1.324***	1.324***	1.324***
		(0.013)	(0.099)	(0.099)
Price $Variability_{i,t}$		15.666***	15.666***	15.666***
		(0.214)	(0.766)	(1.038)
Constant	13.105***	-15.452***	-15.452***	-15.452***
	(0.014)	(0.176)	(1.524)	(1.532)
Observations	34,665	34,665	34,665	34,665
R-squared	0.330	0.666	0.666	0.666
Panel B: 2022	**Model 1**	**Model 2**	**Model 3**	**Model 4**
$Treatment_t$	-4.767***	-0.747***	-0.747***	-0.747***
	(0.020)	(0.018)	(0.130)	(0.131)
$Post_i$	0.005	0.035**	0.035	0.035
	(0.021)	(0.015)	(0.024)	(0.034)
$Treatment_i$ * $Post_t$	0.087***	0.142***	0.142***	0.142***
	(0.025)	(0.017)	(0.030)	(0.038)
Market Cap $(log)_{i,t}$		1.157***	1.157***	1.157***
		(0.004)	(0.032)	(0.032)
Stock Price $(log)_{i,t}$		-0.112***	-0.112***	-0.112***
		(0.003)	(0.026)	(0.027)
Free Float $(log)_{i,t}$		0.935***	0.935***	0.935***
		(0.010)	(0.081)	(0.081)
Price $Variability_{i,t}$		12.419***	12.419***	12.419***
		(0.108)	(0.367)	(0.397)
Constant	14.659***	-14.335***	-14.335***	-14.335***
	(0.017)	(0.089)	(0.755)	(0.758)
Observations	137,942	137,942	137,942	137,942
R-squared	0.316	0.689	0.689	0.689
Standard errors	Robust	Robust	Firm clustered	Firm-date clustered

Standard errors are in parentheses

**** p<.01, ** p<.05, * p<.1*

Table XII - Tertile regressions 2022 & 2014

The table below illustrates the *Bid-ask Spread* tertile regression results with fixed effects for 2014 and 2022, respectively. The treatment group is evenly segmented into three tertiles, Small, Mid, and Large firms based on *Market cap (log)*$_{i,t}$. The table shows the dependent variables, the coefficients, standard errors, and the significance levels. *Free Float (log)* $_{i,t}$ is missing for 2014

Panel A: 2014	**(1)**	**(2)**	**(3)**
Bid-ask Spread	Small	Mid	Large
Method	FE	FE	FE
$Size_t$ (treatment)	-0.441***	-0.399***	-0.285*
	(0.095)	(0.149)	(0.171)
$Post_i$	-0.014	-0.014	-0.015
	(0.015)	(0.015)	(0.015)
$Size_i$ * $Post_t$	-0.059**	0.003	-0.010
	(0.026)	(0.036)	(0.023)
Market Cap $(log)_{i,t}$	-0.528***	-0.536***	-0.441***
	(0.052)	(0.056)	(0.046)
Stock Price $(log)_{i,t}$	-0.077***	-0.015	-0.062**
	(0.026)	(0.042)	(0.028)
Price $Variability_{i,t}$	4.086***	4.228***	3.947***
	(1.079)	(1.203)	(1.160)
Constant	10.025***	10.064***	8.266***
	(1.034)	(1.103)	(0.914)
Observations	21709	21709	21852
R-squared	0.606	0.663	0.758
Standard errors	Firm-date clustered	Firm-date clustered	Firm-date clustered

Panel B: 2022	**(1)**	**(2)**	**(3)**
Bid-ask Spread	Small	Mid	Large
$Size_t$ (treatment)	1.226***	0.379**	0.224*
	(0.160)	(0.149)	(0.118)
$Post_i$	-0.080***	-0.040***	-0.025
	(0.014)	(0.014)	(0.016)
$Size_i$ * $Post_t$	-0.083***	-0.109***	-0.125***
	(0.020)	(0.024)	(0.023)
Market Cap $(log)_{i,t}$	-0.320***	-0.501***	-0.560***
	(0.029)	(0.037)	(0.053)
Stock Price $(log)_{i,t}$	0.035**	0.077***	0.104***
	(0.016)	(0.017)	(0.025)
Free Float $(log)_{i,t}$	-0.182***	-0.315***	-0.532***
	(0.064)	(0.062)	(0.066)
Price $Variability_{i,t}$	1.058***	4.039***	5.230***
	(0.191)	(0.302)	(0.678)
Constant	6.032***	10.238***	12.292***
	(0.691)	(0.837)	(1.108)
Observations	55783	55783	57008
R-squared	0.844	0.778	0.595
Standard errors	Firm-date clustered	Firm-date clustered	Firm-date clustered

Standard errors are in parentheses
**** p<.01, ** p<.05, * p<.1*

Table XIII: Propensity Score Matching 2022 results

The table represents fixed effects propensity score marching regression results for the *Bid-ask Spread (log)* $_{i,t}$ and *Turnover (log)*$_{i,t}$ for 2014, 2019, 2020, and 2022. The table shows the dependent variables, the coefficients, standard errors, and the significance levels. *Free Float (log)*$_{i,t}$ is missing for 2014.

Score matching	**2014**	**2019**	**2020**	**2022**
Variable	Bid-ask Spread	Bid-ask Spread	Bid-ask Spread	Bid-ask Spread
Method	FE	FE	FE	FE
Treatment	-0.565***	-0.241***	-0.176**	0.423***
	(0.101)	(0.072)	(0.077)	(0.076)
Post	-0.040	-0.006	0.147***	-0.083***
	(0.028)	(0.024)	(0.036)	(0.029)
Treatment * Post	-0.002	-0.008	-0.029	-0.084***
	(0.031)	(0.028)	(0.040)	(0.030)
Market Cap (log)$_{i,t}$	-0.314***	-0.348***	-0.451***	-0.465***
	(0.042)	(0.032)	(0.042)	(0.016)
Stock Price (log)$_{i,t}$	0.027	0.063	0.084**	0.052***
	(0.060)	(0.044)	(0.032)	(0.013)
Free Float (log)$_{i,t}$	-	-0.417***	-0.625***	-0.306***
		(0.147)	(0.080)	(0.036)
Price Variability$_{i,t}$	3.470***	6.633***	5.930***	1.636***
	(1.115)	(1.095)	(0.405)	(0.237)
Constant	5.328***	7.036***	10.396***	9.627***
	(0.817)	(0.890)	(0.951)	(0.374)
Observations	16045	15284	14285	126089
R-squared	0.508	0.590	0.625	0.605
Variable	Turnover	Turnover	Turnover	Turnover
Treatment	0.533**	0.672***	0.218	-0.905***
	(0.208)	(0.132)	(0.156)	(0.145)
Post	-0.117	0.011	-0.016	-0.007
	(0.077)	(0.038)	(0.069)	(0.067)
Treatment * Post	-0.026	-0.072**	0.064	0.153**
	(0.076)	(0.031)	(0.072)	(0.068)
Market Cap (log)$_{i,t}$	0.997***	0.698***	1.085***	1.131***
	(0.112)	(0.083)	(0.091)	(0.035)
Stock Price (log)$_{i,t}$	-0.072	-0.067	-0.132**	-0.143***
	(0.133)	(0.092)	(0.063)	(0.028)
Free Float (log)$_{i,t}$	-	0.850***	14.457***	0.918***
		(0.295)	(0.889)	(0.080)
Price Variability$_{i,t}$	31.303***	23.930***	1.368***	6.865***
	(2.836)	(2.206)	(0.126)	(0.682)
Constant	-7.211***	-4.023*	-14.385***	-13.305***
	(2.217)	(2.242)	(2.045)	(0.774)
Observations	16045	15284	14285	126089
R-squared	0.493	0.554	0.638	0.568
Standard errors	Firm-date clustered	Firm-date clustered	Firm-date clustered	Firm-date clustered

**** p<.01, ** p<.05, * p<.1*

Table XIV: Robustness test – Five-days removed

The table below represents a robustness test done on all four policy regimes by removing five days prior and after the event day – removing potential noise effects in the data. The table shows the dependent variables, the coefficients, standard errors, and the significance levels. *Free Float (log)$_{i,t}$* is missing for 2014.

Event	**2014**	**2019**	**2020**	**2022**
Variable (log)	Bid-ask Spread	Bid-ask Spread	Bid-ask Spread	Bid-ask Spread
Method	FE	FE	FE	FE
Treatment$_t$	-0.648***	-0.266***	-0.147*	0.444***
Post$_i$	-0.016	-0.009	0.159***	-0.071***
Treatment * Post$_t$	-0.024	-0.008	-0.035	-0.109***
Market Cap (log)$_{i,t}$	-0.371***	-0.361***	-0.468***	-0.469***
Stock Price (log)$_{i,t}$	-0.028	0.061	0.108***	0.055***
Free Float (log)$_{i,t}$	-	-0.475***	-0.544***	-0.322***
Price Variability$_{i,t}$	4.006***	6.587***	5.671***	1.617***
Constant	6.807***	7.618***	10.341***	9.747***
Observations	28,942	19,484	31,897	130,013
R-squared	0.7495	0.6305	0.638	0.707
Variable (log)	Turnover	Turnover	Turnover	Turnover
Treatment$_t$	0.770***	0.677***	0.111	-0.819***
Post$_i$	-0.142***	0.027	-0.170***	-0.014
Treatment * Post$_t$	-0.004	-0.092***	0.227***	0.166***
Market Cap (log)$_{i,t}$	1.122***	0.745***	1.149***	1.131***
Stock Price (log)$_{i,t}$	0.015	-0.057	-0.145**	-0.146***
Free Float (log)$_{i,t}$	-	1.058***	15.957***	0.958***
Price Variability$_{i,t}$	35.411***	25.115***	1.321***	6.814***
Constant	-10.533***	-6.074***	-15.493***	-13.571***
Observations	28942	19484	31897	130013
R-squared	0.687	0.632	0.665	0.674
Standard errors	Firm-date clustered	Firm-date clustered	Firm-date clustered	Firm-date clustered

***** $p<.01$, ** $p<.05$, * $p<.1$*

Table XV: Falsification test – Pre-event data 2022

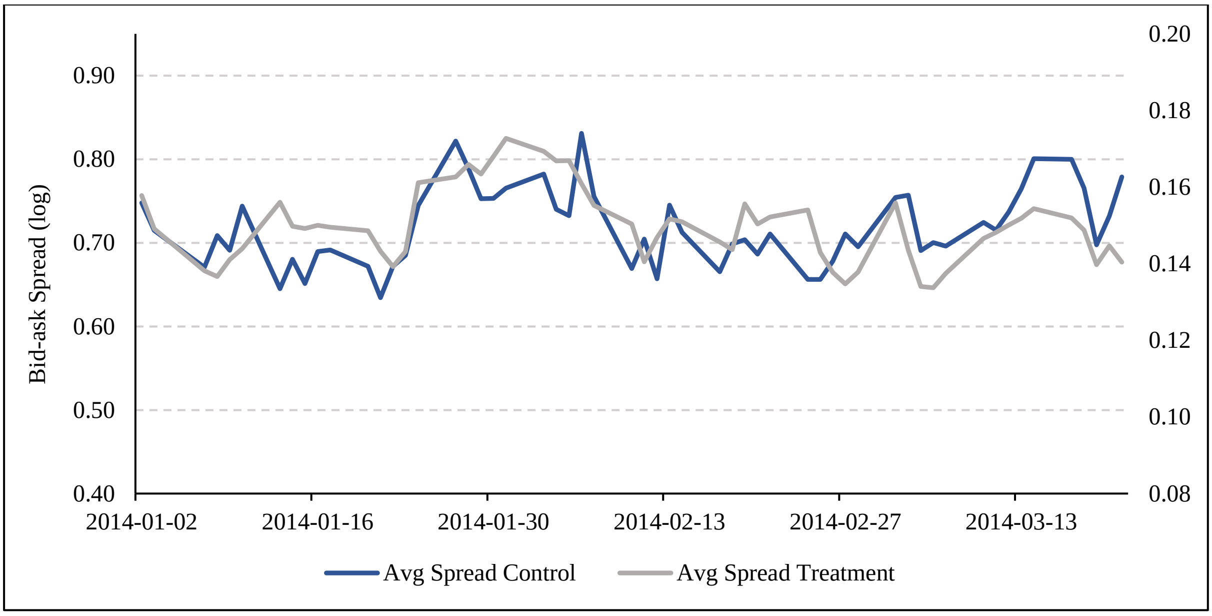
The table below shows the results of our falsification test, where the main regression Model (4) was run on pre-event data for the anonymization in 2022. This test was done to ensure that our model specification was robust and are not mis-specified.

2022	**(1)**	**(2)**
Variable (log)	Bid-ask Spread	Turnover
Method	FE	FE
Treatment	0.529***	-0.731***
	(0.075)	(0.143)
Post	0.044***	-0.074
	(0.014)	(0.043)
Treatment * Post	-0.025	-0.015
	(0.020)	(0.055)
Market Cap	-0.474***	1.153***
	(0.018)	(0.037)
Stock Price	0.059***	-0.167***
	(0.014)	(0.029)
Free Float (log)	-0.301***	0.976***
	(0.047)	(0.080)
Price Variability	0.826**	3.751***
	(0.346)	(0.870)
Constant	9.772***	-13.871***
	(0.445)	(0.864)
Observations	18,522	18,552
R-squared	0.683	0.659
Standard errors	Firm-date clustered	Firm-date clustered

12.0 Graphs & Figures

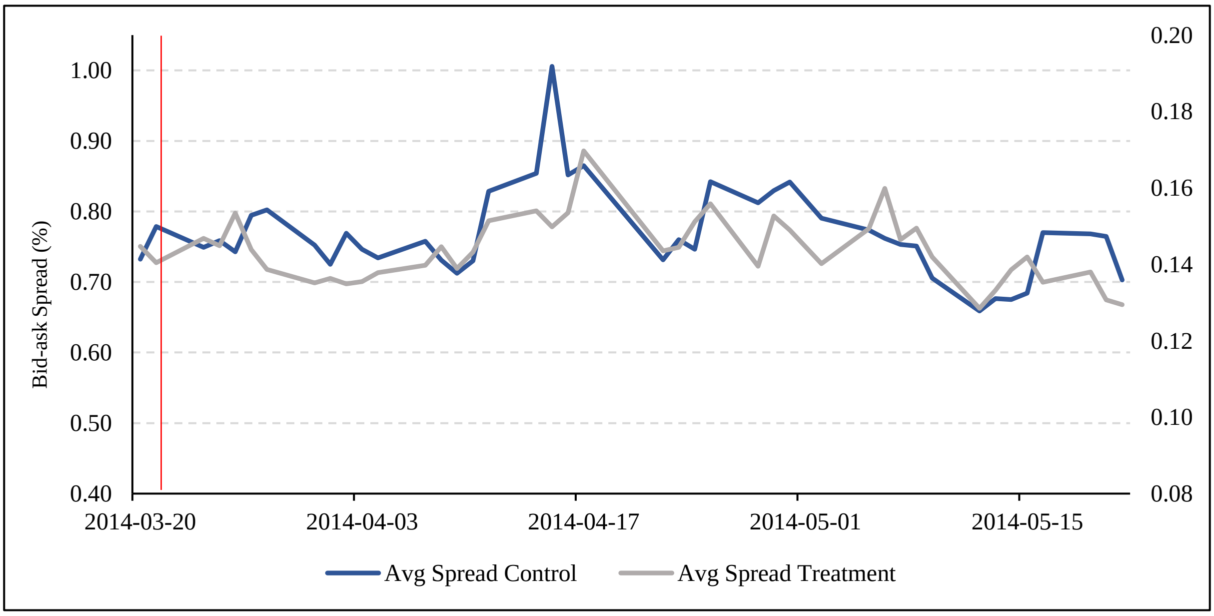
Graph I: Parallel trends - 2014

The graph below illustrates the parallel trends of the *Bid-ask Spread* between the treatment (grey) and control (blue) group in the pre-event period. The groups are largely correlated and move together over time.



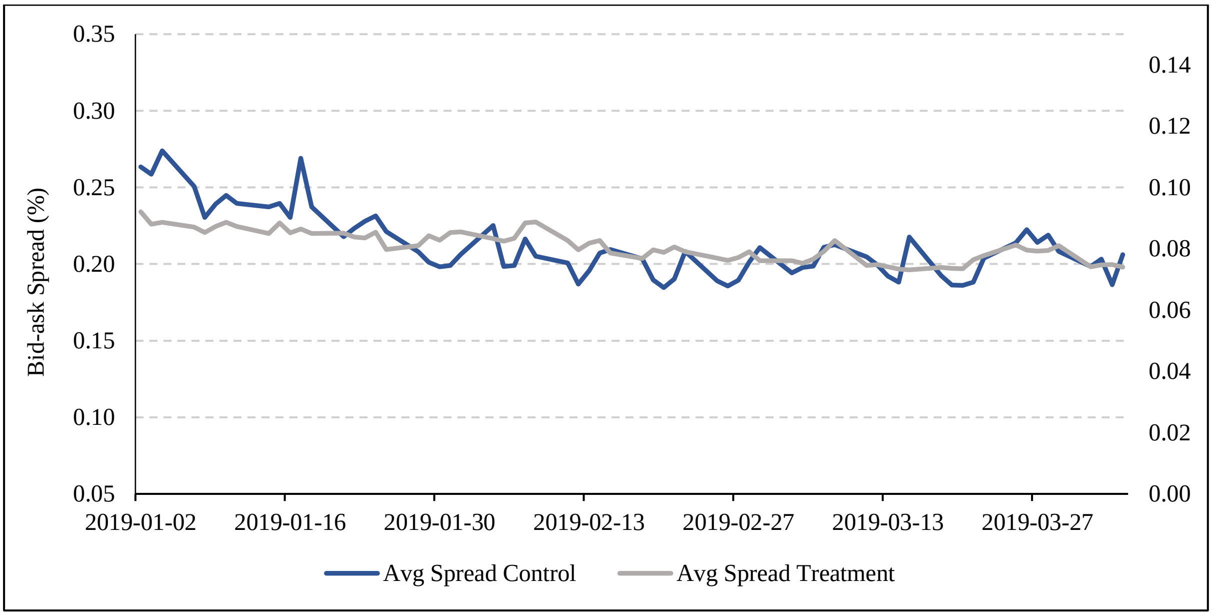
Graph II: Treatment effect - 2014

The graph illustrates the effect of the treatment on the daily average *Bid-ask Spread* for the treatment and control group in the post-event period. The red line marks the introduction of anonymity to the treatment group. While we observe that the control group has a lower average *Bid-ask Spread* compared to the treatment, there is no significant treatment effect observed.



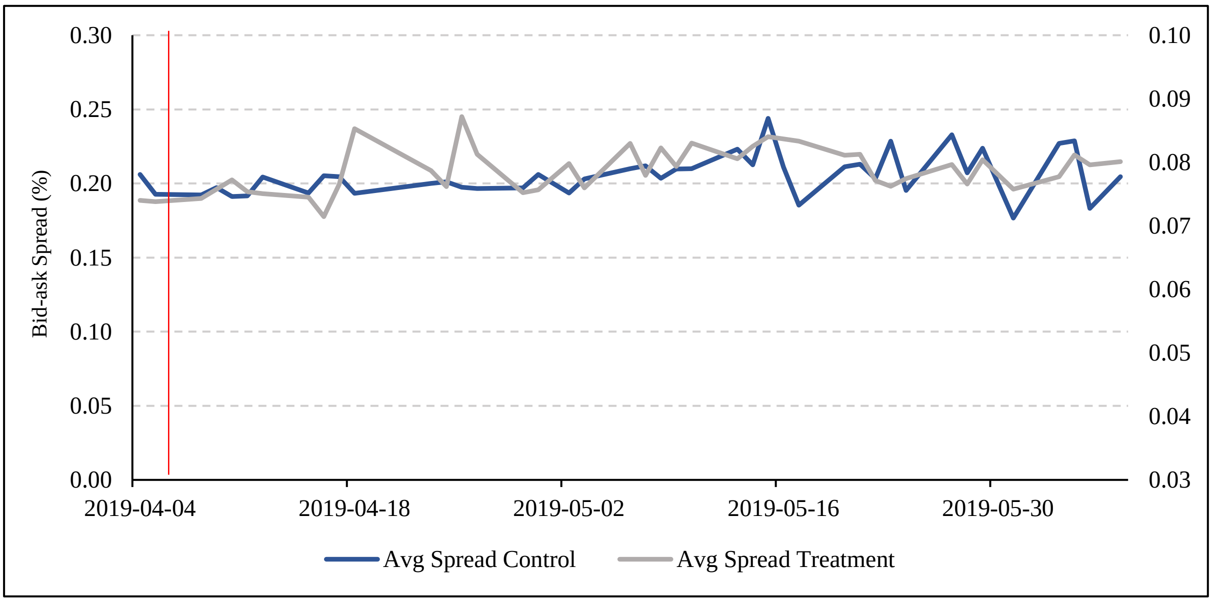
Graph III: Parallel trends - 2019

The graph below illustrates the parallel trends of the *Bid-ask Spread* between the treatment (grey) and control (blue) group in the pre-event period. The groups are largely correlated and move together over time.



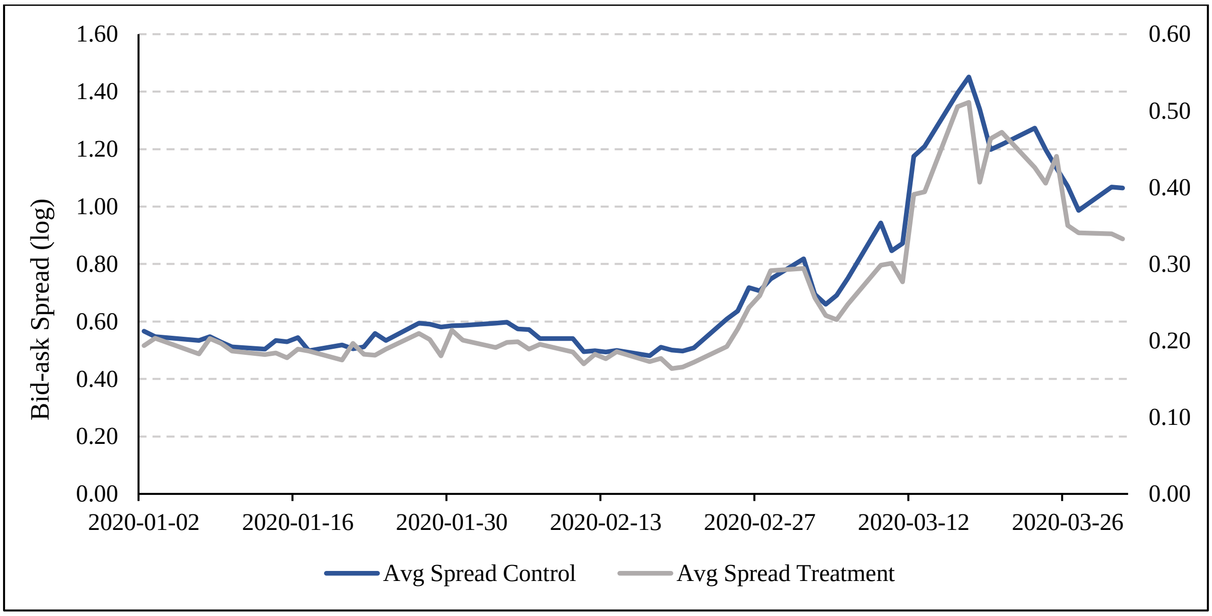
Graph IV: Treatment effect - 2019

The graph illustrates the effect of the treatment on the daily average *Bid-ask Spread* for the treatment and control group in the post-event period. The red line marks the introduction of anonymity to the treatment group. We observe that the control and treatment group have similar *Bid-ask Spreads*, but there is no significant treatment effect observed.



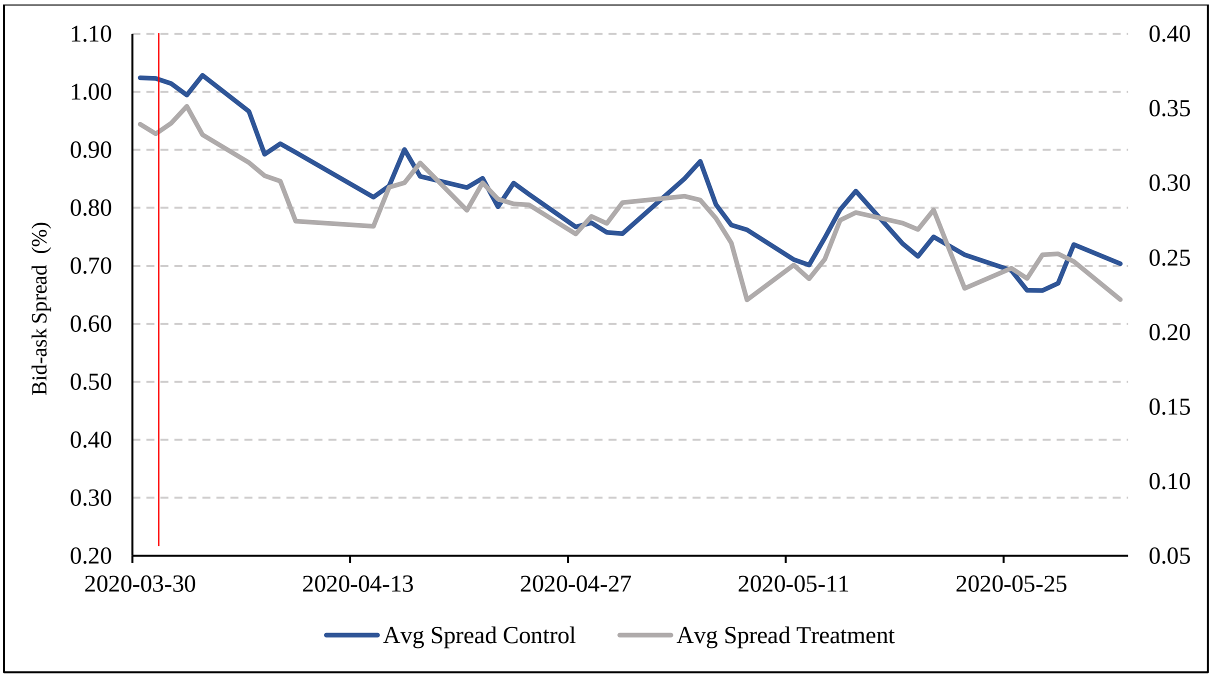
Graph V: Parallel trends - 2020

The graph below illustrates the parallel trends of the *Bid-ask Spread* between the treatment (grey) and control (blue) group in the pre-event period. The groups are largely correlated even under large increases of the *Spread* due to C-19, and move together over time.



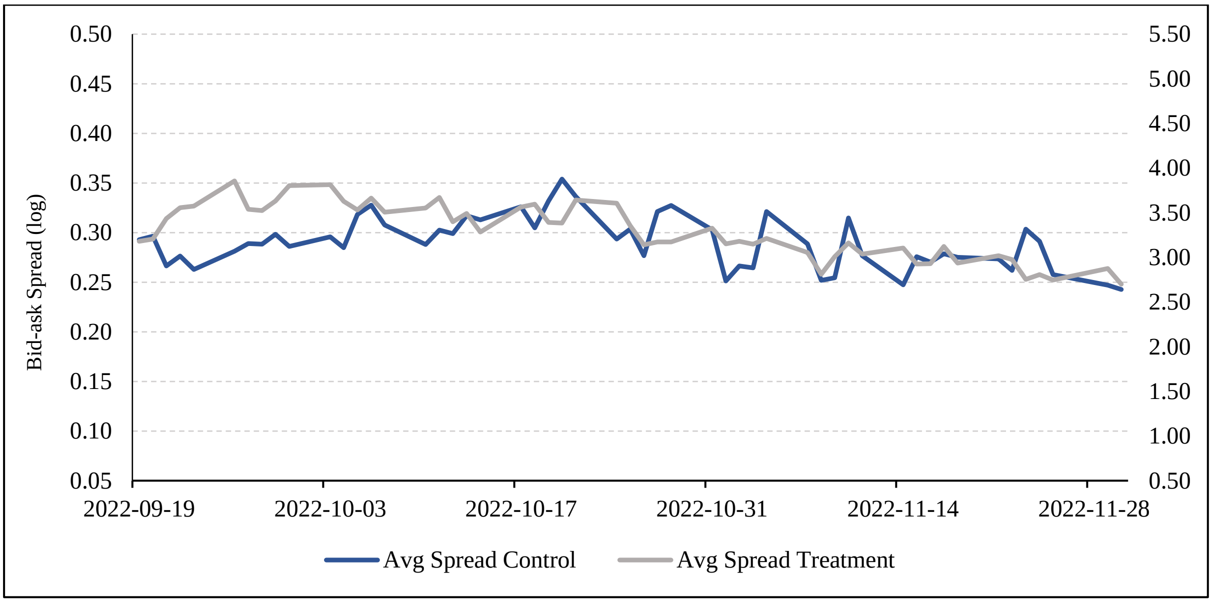
Graph VI: Treatment effect - 2020

The graph illustrates the effect of the treatment on the daily average *Bid-ask Spread* for the treatment and control group in the post-event period. The red line marks the introduction of anonymity to the treatment group. We observe that the control and treatment group have similar *Bid-ask Spreads*, there is no significant treatment effect observed.



Graph VII: Parallel trends - 2022

The graph below illustrates the parallel trends of the *Bid-ask Spread* between the treatment (grey) and control (blue) group in the pre-event period. The groups are largely correlated and move together over time.



Graph VIII: Treatment effect - 2022

The graph illustrates the effect of the treatment on the daily average *Bid-ask Spread* for the treatment and control group in the post-event period. The red line marks the introduction of anonymity to the treatment group. We observe that the treatment group initially sees a large decline in Spreads as the anonymization takes place. It then reverts back and approximately a month alter there is a clear separation from the control group, indicating the treatment effect.

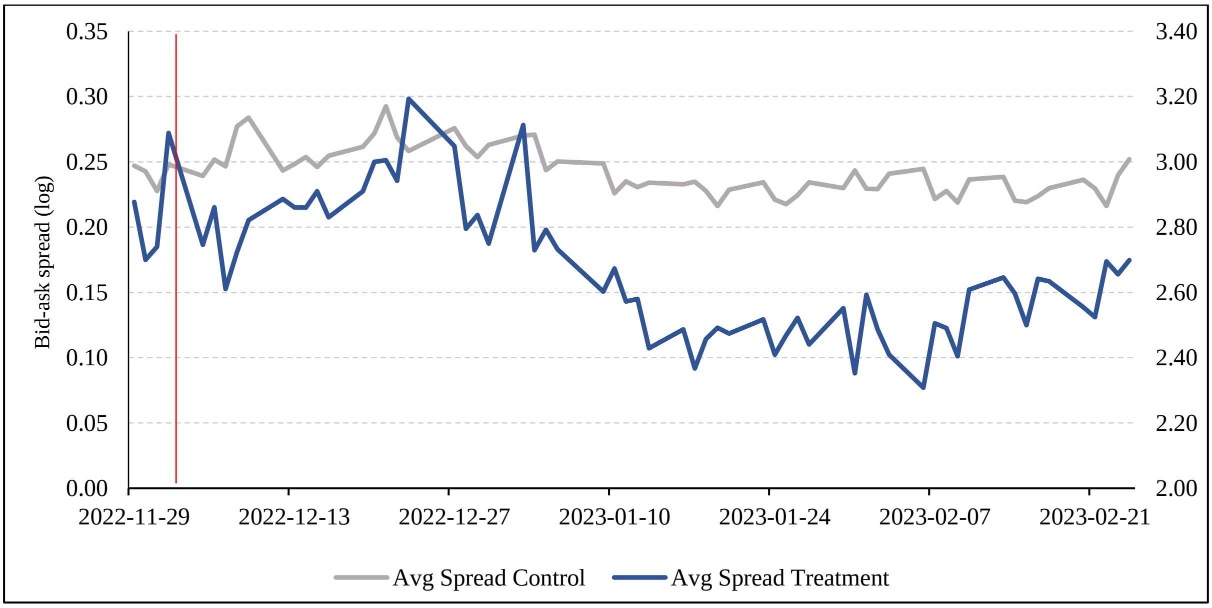


Figure I: Event windows

This figure represents a generic visualization of our four event widows, showing the pre-event window, day of anonymization, and post-event window.