UNIVERSITY OF GOTHENBURG
SCHOOL OF BUSINESS, ECONOMICS AND LAW

Post-MiFID II: Dark Pool Bans and Regulatory Effects on Lit Market Quality

Hugo Hammar* Isak Djudja†

June 15, 2021

Thesis for the degree of Master of Science in Finance

Supervisor: Marcin Zamojski

*gushammahu@student.gu.se
†gusdjudis@student.gu.se

Abstract

Over recent years, increasing popularity of non-transparent trading venues known as dark pools have spurred widespread controversy and debate; in particular regarding their impact on lit market conditions. Following voiced concerns over dark pools' potentially adverse effects on lit market quality, legislative actions have been taken to restrict the amount of trading allowed to be conducted in the dark. Yet, previous empirical and theoretical literature on the subject is divided at large; with no clear consensus on the ultimate effects of dark pool trading on lit markets—rendering regulatory scrutiny a priority. We leverage the cut-off in dark pool trading activity following suspension of 35 securities in Nordic equity markets invoked by the Double Volume Cap rule trough the European MiFID II regulatory framework. We present evidence suggesting detrimental implementation effects of the ban on lit market liquidity—putting to question the justifiability of recent regulatory intervention. Conversely, other measures of liquidity and volatility appear unaffected by the ban. The lack of clarity surrounding the fundamental implementation effects calls for further research in the field of dark pool trading and its ultimate effect on lit market quality.

Acknowledgements

First and foremost, we would like to express our deep gratitude to our supervisor Marcin Zamojski, for his invaluable advice and comments during the entirety of our research process. It is without hesitation we acknowledge that our quality of work would not have been the same without his kind contributions. We also appreciate the institutions and people who have made this thesis possible and provided us with the foundations necessary to perform our research.

Table of Contents

1 Introduction

Over the past decades, financial markets around the world have witnessed a rapid rise in the popularity of dark pools. The trading venues, characterized mainly by their lack of pre-trade transparency, have grown to absorb a substantial share of market liquidity in Europe and across the world.[1] Notwithstanding the increasing popularity of these types of venues—regularly used by institutional investors to exploit benefits arising from this lack of transparency—their very existence is controversial. Most notably, concerns have been raised about the inaccessibility to non-institutional investors, inflicting an unlevel playing field between dark venues and public markets, and the potentially negative impact on market quality dark pools may entail. In these debates, the European Commission has expressed concerns that increased levels of dark pool trading may impede price discovery (European Commission, 2010). In particular, concerns were exacerbated following MiFID I's elimination of the concentration rule in 2007,[2] which was applied in many European countries. As a consequence, competition among trading venues increased—as did the proportion of trading conducted in dark pools (Petrescu and Wedow, 2017). For the purpose of improving pre-trade transparency on equity markets and to ensure high market quality, the revised MiFID II/MiFIR framework was implemented by the European Commission on January 3rd, 2018—under which single securities can be banned from being traded in dark pools for a predetermined period of time.

Both theoretical and empirical research present conflicting results regarding dark pools' ultimate impact on 'lit' market characteristics—and by extension whether

[1] In Europe, by 2016, dark pools constituted over 8% of total volume traded, compared to less than 1% in 2009 (Petrescu and Wedow, 2017).

[2] Under the concentration rule, all equity trading had to take place on domestic stock exchanges.

a dark pool ban would entail redeeming effects or impediments in the pursuit of enhanced market quality. Common across this strand of literature, however, are discussions on a segmenting effect of dark pools; specifically a distinction between informed traders and liquidity traders, and on which venues these categories tend to concentrate. Early studies by Ye (2011), and Zhu (2014), which present conflicting theoretical implications based on this notion, have been highly influential in the discussion. Much of the empirical literature springs from this theoretical foundation, but is divided at large—with conflicting results indicating enhancing, adverse, dynamic, or no effects from dark pool trading on different market quality measures (e.g., Foley and Putniņš, 2016; Comerton-Forde and Putniņš, 2015; Johann et al., 2019). The ambiguity surrounding dark pools' ultimate effects on market quality increases the need for regulatory scrutiny—especially in light of recent regulations in Europe, where literature on dark pools is still particularly scarce.

This paper sheds additional light on dark pools' fundamental impact on lit market characteristics by investigating the first market-wide implementation of a dark pool ban on European securities, taking place in March, 2018. The event considered qualifies as a quasi-natural experiment, where we leverage the cut-off in dark pool trading on a large number of suspended securities introduced by the implementation of the ban. The paper focuses in particular on lit volatility and liquidity through a selection of measures largely inspired by the work of Johann et al. (2019). We examine the implementation effects of the ban by computing realized volatilities and several measures of quoted and trading liquidity based on high-frequency intraday data. The study considers a time period of a total of 23 trading days, spanning 13 days before, and ten days after, the implementation of the ban.

We conduct the analysis above by employing a Difference-in-Differences (DID) model approach, assigning our sample of suspended and non-suspended securities to treatment and control groups, respectively. To ensure adherence to underlying model assumptions—in particular to the strong assumption of parallel trends between the control and treatment group in the DID framework—we follow a sample selection procedure in which a total of 70 securities are matched based on company-specific characteristics along dimensions of geography, industry, and size. The scope of our analysis is limited to Nordic securities, for which high-frequency data is collected from the Swedish House of Finance. The choice of region contributes further to the strand of literature by entailing an interesting, and to our knowledge unresearched, geographical setting in the context of dark pools.

Our results reveal that the impact of the dark pool ban on measures of lit market quality is, to a large extent, ambiguous. Subsequent analysis further underpins the complexity involved in the market dynamics at play, and how dark pool activity and lit market characteristics may be indivisibly intertwined. While we find some evidence of spreads having increased as a result of the ban—robust to several changes in both sample composition and time frame—other measures of liquidity and volatility exhibit no statistically significant changes after the ban. Our results of increasing spreads are supported in research by, e.g., Foley and Putniņš (2016) and Buti, Rindi, and Werner (2011), but are contradictory to the work of Comerton-Forde and Putniņš (2015). Our statistically insignificant results in many of the variables of interest are largely aligned with results of Johann et al. (2019) who are unable to present any significant relationships between the European dark pool ban and measures of lit market quality.

This paper contributes to the literature on dark pools at large, and particularly in the context of regulatory intervention. The results presented provide insight into how lit market characteristics are affected by suspension of securities from dark pool trading, and have implications for both practitioners and regulators. Most notably, the paper underpins the need for scrutinizing dark pool regulations in Europe, as our results imply that the dark pool ban may have proven unsuccessful in its quest to improve lit market conditions for investors. We conclude that additional research is required to disentangle the complete dynamics of dark pools and the true impact of dark pool bans on lit market quality.

The remainder of the paper is structured as follows. Section 2 provides background and an overview of dark pool trading and presents the regulatory framework relevant for the implementation of the ban in Europe. Section 3 provides an in-depth review of previous theoretical and empirical literature on the relationship between dark pools and lit markets. Section 4 establishes the methodological framework and the sampling procedure, and presents descriptive statistics of the data. Section 5 provides the results and a discussion on their implications in relation to previous literature. Section 6 concludes.

2 Background

2.1 Dark Pools

Operated by private institutions—most commonly banks, brokers, or exchange operators—dark pools share several similarities with public markets; most notably the fundamental service of offering market participants a venue to exchange securities. The distinction between dark pools and conventional, lit, markets, lies in transparency. In contrast to public exchanges, dark pools are characterized by a lack of pre-trade transparency, providing investors with the possibility to place market orders without order details being disclosed in a public order book. Accordingly, dark pools provide a solution for limiting transaction costs, particularly in relation to large institutional trades, by making self-inflicted price pressure[3] avoidable for traders.

In recent years, improvements in computing power has increased the need for protection from predatory behaviour exerted by new types of market participants, referred to as high-frequency traders (HFT). The ability among these actors to recognize both block trades and iceberg orders[4] in fractions of a second constitutes a significant risk for institutional investors. Practicing 'frontrunning', HFT traders acquire or sell shares across exchanges, depending on the direction of the institutional trade—entailing significantly higher transaction costs for those institutional investors. The phenomena of HFT and frontrunning has in later years driven an increasing share of institutional investors to dark pools, largely explaining the increase in liquidity on such venues (Petrescu and Wedow, 2017).

While all dark pools share the characteristic of no pre-trade transparency, there are several different subsets of these venues available to traders—each with certain characteristics targeted towards specific investor groups. One of the most distinctive features in the classification of such trading venues concerns how transaction prices are determined. As a public order book does not exist, dark pools rely on other means of determining the reference price of a transaction; most commonly derived from lit exchanges. Among the more common practices is 'midpoint pricing', where—as the term implies—the price is determined based on the midpoint between bid and ask prices, specifically on the market the security in question is primarily

[3]Price pressure refers to when buy or sell orders impact the price of the security in an unfavourable direction for the investor.

[4]Block trades refer to single, large orders, while iceberg orders are split into lots, where orders in the bottom of the order book are hidden until visible trades are executed.

listed on. Among these midpoint dark pools, there are multiple venue types with different mechanisms for the matching of orders—commonly based on either order volume or timeliness. For dark pools in which matching of order timing is prioritized, the transactions occur instantaneously, which may be associated with only partially filled orders (Petrescu and Wedow, 2017). In contrast, for dark pools prioritizing orders based on volume, execution requires that an order of equal size exists on the opposite side of the order book. On such venues, investors are subjected to the risk of passively having to wait for a match to occur, and for the trade to be executed. A plethora of other types of dark pools exist, albeit all share the common denominator of exogenously determined prices, commonly derived from lit markets (Petrescu and Wedow, 2017).

2.2 Regulatory framework

Since MiFID I came into force in 2007 and eliminated the widely applied concentration rule in Europe, competition among trading venues has increased (Petrescu and Wedow, 2017). Following a rise in the proportion of trading conducted in dark pools—partly due to the protection it provides against predatory HFT practices (Petrescu and Wedow, 2017)—concerns about dark pools' impact on market efficiency and price discovery on lit markets has increased among regulators. In 2010, the European Commission expressed specific concerns that increases in dark pool activity "[...] may ultimately affect the quality of the price discovery mechanism on the lit markets." (European Commission, 2010).

For the purpose of improving pre-trade transparency and ensure high quality on lit equity markets, the revised MiFID II/MiFIR framework was implemented on January 3rd, 2018. The regulations, *inter alia*, impose a requirement for all market operators to make public current bid and ask prices as well as the depth of trading interests. Competent authorities can grant Alternative Trading Systems (ATSs) pre-trade transparency waivers under certain prerequisites. Trading systems that derive their reference prices from an external liquid market, or the venue on which the security was first listed for trading, fall under the *reference price waiver*; and include midpoint dark markets. Under these waivers, however, MiFID II/MiFIR stipulate further the extent to which securities can be traded in the dark. In particular, the Double Volume Cap (DVC) mechanism limits the volume that can be traded both in individual dark pools and across all dark trading venues. More specifically, the DVC rule sets out two volume percentage caps, which entail the following (Regulation EU No 600/2014):

1. Given that a trading venue is granted a waiver, trading on that venue shall not exceed 4% of the total volume of trading in that financial instrument on all venues across the Union over the previous 12 months.

2. Total trading under waivers is restricted to 8% of the total volume of trading in that financial instrument on all venues across the Union over the previous 12 months.

That is, when the 4% limit is breached for a given security, the very same security becomes suspended; such that trading in that security cannot be carried out on the relevant trading venue for six months. Furthermore, a security also becomes suspended if the total trading volume under waivers breaches the 8% cap; which results in the security being suspended from all trading under waivers for a six-month period. The DVC mechanism's main *raison d'être* is to ensure that trading under waivers does not harm price formation on lit markets, and to broaden the access to liquidity. Trade data that has been gathered across venues since the implementation of MiFID II does, however, indicate that volumes previously traded in dark pools have partly moved to ATSs, rather than returning to lit markets. Systematic Internalizers (SIs) function as one such substitute to dark pools, where investment firms deal on their own account when executing client orders. The market share of SIs for Nordic listed shares rose by approximately 20 percentage points, to above 25%, the six months following the implementation of MiFID II (Nasdaq, 2018). These facts alone imply potential inadequacies in the scope of recent regulations in Europe, and accordingly that MiFID II may fail to realize any significant improvements in overall quality of lit markets.

3 Literature Review

3.1 Price Discovery

Regulatory debates on dark pools tend to focus on their impact on price discovery.[5] The European Commission has raised specific concerns that the quality of price discovery may suffer from increased levels of dark pool trading (European Commission, 2010). However, neither theoretical nor empirical literature support such an effect unanimously. The question of dark pools' ultimate effects on price discovery, thus, remains unsolved. This increases the need for regulatory scrutiny—especially in light of recent regulations in Europe, where literature on dark pools is particularly scarce. A review of the theoretical and empirical findings regarding dark pools and price

[5] Price discovery refers to the extent implicit information in investor trading is incorporated into market prices in an efficient and timely manner (Lehmann, 2002).

discovery follows.

Two widely cited papers on this issue are those of Ye (2011) and Zhu (2014), who approach dark pools through separate theoretical models and ultimately come to contradicting conclusions. Ye (2011) constructs a theoretical model in a two-period universe which distinguishes between three types of agents—informed traders, liquidity traders, and market makers—and ultimately predicts that dark pool activity should be detrimental to price discovery. On the contrary, Zhu (2014) predicts the opposite; dark pools should effectively improve price discovery on lit markets. These two distinctive and conflicting conclusions stem from different theoretical foundations. Ye (2011) specifically finds that trading in the dark harms price discovery under the assumption that only informed traders are allowed to submit orders in dark pools. As informed investors, by assumption, know the true value of an asset, they will trade more on dark venues than on lit markets to avoid self-inflicted price pressure. While the study acknowledges the existence of lower execution probabilities for informed traders on dark venues, it is deemed secondary to the benefits of preventing information leakage. Therefore, a larger volume of informed trades will go through dark venues without pre-trade transparency, and price discovery on lit markets will be harmed (Ye, 2011).

On the other hand, Zhu (2014) finds that execution risk is the decisive factor driving informed investors to lit markets and liquidity traders to dark pools, which by extension explains the positive impact of dark pools on price discovery. Zhu (2014) distinguishes between informed traders, who act on the basis of detailed knowledge of a stock, and liquidity investors, whose trade decisions are exogenous to the market. Zhu (2014) notes that, as informed orders are positively correlated with asset value, they have a tendency to cluster on either the sell- or buy-side of the market, resulting in lower execution probabilities on dark venues. Moreover, since liquidity orders conversely tend to be uncorrelated with the asset value—these trades are triggered by exogenous reasons rather than information—such clustering is less likely, leading to lower execution risk. This enforces the segmenting effect of dark pools and, thus, increases the concentration of informed traders on lit markets vis-à-vis its dark counterpart—so that information is retained on lit markets and price discovery is improved.

A crucial difference in model assumptions between Ye (2011) and Zhu (2014) calls for attention. Ye (2011) assumes that only informed investors can migrate to dark pools—leading to the conclusion that price discovery may be harmed due to a higher proportion of informed traders on dark venues. On the other hand, Zhu's (2014) reverse assumption of self-selection is part and parcel to the study's subsequent findings. When both informed traders and liquidity traders can choose venue type freely, informed traders will prefer lit venues due to execution risk, and liquidity traders will concentrate on dark venues. Most empirical findings tend to support the predictions made by Zhu (2014), which may suggest that the assumption of self-selection is an important component of reality.

Comerton-Forde and Putniņš (2015) substantiate Zhu's (2014) findings through an empirical study using Australian data. The paper shows that the introduction of a dark pool does lead to a partial separation of informed and uninformed investors, where orders executed in the dark are indeed conducted by less informed investors. The rationale is also consistent with that of Zhu (2014); namely that the risk of non-execution drives informed investors to lit markets. As the proportion of uninformed investors in lit markets decreases, spreads tend to widen, and, accordingly, the aggregated information about fundamental values is reduced (Comerton-Forde and Putniņš, 2015). Comerton-Forde and Putniņš (2015) also find that dark pool trading has effects on informational efficiency. Up to a certain threshold (around 10%

of total volume), dark pool trading is either benign or beneficial, while larger levels of dark pool trading in a stock can harm informational efficiency (Comerton-Forde and Putniņš, 2015). In addition, they find no evidence that block trades without pre-trade transparency harm price discovery. These findings relate to the MiFID II regulation; the implementation of the DVC rule entails an 8% volume cap on total dark pool trading for a given security; and a 'Large-in-Scale'–waiver which permits larger block trades. If there is a discrepancy between MiFID II's threshold at 8% and the true threshold at which dark pool trading stops being benign or beneficial, the DVC rule may contribute to considerable welfare inefficiencies.

Ye (2016) discusses similarly to Comerton-Forde and Putniņš (2015) a sorting effect where, in equilibrium, traders with strong information trade on exchanges; traders with moderate signals about fundamental values trade in dark pools; and traders without information opt out of trading. Ye (2016) defines the result of this sorting as an amplification effect on price discovery; when information precision is high, dark pools enhance price discovery; and when information precision is moderate, dark pools impair price discovery. Adding a dark pool when information precision is high, Ye (2016) explains, shifts only a relatively small proportion of informed investors to dark pools, and combined with a relatively high number of liquidity traders on the dark markets, the informed-uninformed ratio on the lit market is improved. The opposite holds for when information precision is low or moderate, as a large proportion of informed investors shift over to dark pools.

3.2 Volatility

The scant literature surrounding dark pools presents contrasting views on a number of connected issues. One such issue concerns the relationship between asset volatility and dark pool trading. Volatility *per se* tends to play a small part of a broader context in much of the previous literature—for example as a determinant of dark pool trading (e.g., Ready, 2010, and Buti, Rindi, and Werner, 2010), a control variable (e.g., Comerton-Forde and Putniņš, 2015), or one of several metrics for market quality (e.g., Foley and Putniņš, 2016 and Johann et al., 2019). Empirical in-depth studies of the relationship between dark pool trading and volatility are, however, limited. This section gathers and reviews previous research on the specific relationship between lit market volatility and dark pool activity.

Many of the empirical studies addressing volatility and dark pool activity support a negative relationship between the two. Foley, Malinova, and Park (2013) find evidence that increased dark trading leads to reduced volatility and price impact based on data from the Toronto Stock Exchange. Foley and Putniņš (2016), who investigate dark trading's impact on informational efficiency, conclude similarly that dark trading has positive effects on a range of informational efficiency metrics, including high-frequency volatility. Furthermore, Petrescu, Wedow, and Lari (2017) focus on market instability and empirically deduce the impact of dark trading on volatility in times of stress, using data on FTSE100 stocks. The paper finds that dark pool trading can help predict current volatility, and concludes that dark pool trading activity effectively lowers volatility in times of stress. In a more recent study, Anagnostidis, Papachristou, and Varsakelis (2019) investigate changes in market quality by observing data on suspended European securities following the implementation of MiFID II's DVC rule. The study finds that the regulation has led to lit price inefficiencies for non-suspended securities, and increased daily volatility on lit markets for suspended securities. This conclusion largely aligns with the view of previously cited research, and points towards a non-detrimental impact of dark pool activity on volatility on lit markets.

The theoretical support for dark pool activity being associated with lower volatility tends to revolve around a segmentation of traders. The notion is similar to what is proposed by Zhu (2014) and Ye (2011)—with informed traders and liquidity traders concentrating on different types of venues. Research suggests that the availability of dark pools can result in liquidity traders migrating from lit to dark venues (predicted by Zhu, 2014, and substantiated by, e.g., Comerton-Forde and Putniņš, 2015). Theoretical models hold, as Petrescu and Wedow (2017) point out, that such migration removes noise from lit markets; and the subsequent concentration of informed traders on those lit markets, where price formation occurs, may make prices less volatile.

Conversely, if dark orders would otherwise have been publicly displayed, it has been argued that the development of dark pools and use of dark orders could inhibit price discovery (International Organization of Securities Commission, 2010). In that case, migration of order flow to dark venues could reduce overall information in lit markets, and subsequently result in more volatile prices (Petrescu and Wedow, 2017). Comerton-Forde and Putniņš (2015) elaborate on this line of thought, and highlights that clustering may lead to lower execution probabilities in the dark, which keeps informed traders on lit markets. They also note that this segmentation entails higher adverse selection risk and subsequently yields wider bid-ask spreads on the lit market. The authors further claim that segmentation and wider spreads "[...] reduce incentives for costly information acquisition", with lower aggregate information production as a consequence. While volatility per se is no focal point of their study, Comerton-Forde and Putniņš (2015) note that wider spreads tend to be associated with higher intraday volatility. The relationship between dark pool activity and price discovery is found to be concave, with a 'tipping point' for dark trade volume at which the impact on market quality goes from being benign or beneficial, to harming. Petrescu and Wedow (2017) find supporting evidence of a non-linear relationship between dark pool activity and volatility, concluding it to be quadratic.

Another strand of literature investigates volatility and its role as an explanatory determinant of dark pool trading levels. Buti, Rindi, and Werner (2011) argue that low (high) intraday volatility leads to increased dark pool (lit) activity. The rationale relates to execution probabilities: when volatility is uncommonly high on the market, "[...] traders are, all else equal, more likely to forego the uncertain executions associated with dark pools, and instead rely on marketable orders to gain immediacy" (Buti, Rindi, and Werner, 2011). The authors recognize, however, the issue of joint determination between market quality and dark trading, which has been highlighted as one reason for conflicting empirical results in the context of dark pools (Johann et al., 2019). Buti, Rindi, and Werner (2011) use a simultaneous equation system to account for bi-causality[6] and ultimately find additional evidence supporting the fact that more dark pool activity leads to lower short-term volatility.

3.3 Liquidity

The segmentation of uninformed and informed traders between venues has an empirically documented effect on liquidity, which, together with related effects on spreads, may have a non-negligible impact on trading costs. While there are strong theoretical foundations for the impact of dark trading on liquidity hinged on this segmentation, conclusions differ depending on the specific type of opaque market referred to, and

[6]The issue of bi-causality relates here to the joint determination between market quality and dark pool trading activity; and how increased dark pool activity may concurrently be a result and a driver of poor lit market quality.

when accounting for quasi-dark alternatives.

Zhu (2014) theorizes on midpoint dark pools and concludes that, while higher dark trading activity is associated with price discovery improvements, it concurrently has a detrimental impact on lit liquidity. Due to the migration of uninformed investors to dark markets, informed traders are concentrated on lit markets—effectively reducing exchange liquidity due to higher adverse selection on lit markets. Glosten and Milgrom (1985) formulate the same argument; specifically that liquidity providers on the market tend to offset losses from trading against informed investors, with gains from trades against uninformed investors. This induces an increased adverse selection risk for liquidity providers in lit markets, and discourages liquidity provision—ultimately harming liquidity and increasing transaction costs (Zhu, 2014). Contrary to Zhu (2014), Foley and Putniņš (2016) evaluate effects of two-sided, or limit order, dark markets as opposed to midpoint dark pools. The paper conducts a natural experiment following restrictions on dark trading in Canada and Australia, and concludes that dark trading is beneficial to liquidity. Two-sided dark trading, as Foley and Putniņš (2016) explain, "[. . .] lower[s] quoted, effective, and realized spreads, [and] reduces price impact measures of illiquidity". The study does not find any significant and consistent evidence of such an effect from midpoint dark trading. These conclusions are theoretically substantiated by Boulatov and George (2013), who find that in dark limit order markets—where informational rents cannot be expropriated by other investors through displayed orders—informed investors trade more aggressively. The authors subsequently argue that the rise in competition can enhance market quality. Foley and Putniņš (2016) find that this competition yields positive spill-over effects on lit markets; to compete with dark liquidity, liquidity providers narrow down spreads. The same notion is further supported by Buti, Rindi, and Werner (2011), who find that higher dark pool trading is associated with positive effects on market quality through, for example, lower quoted and effective spreads. From a European perspective, MiFID II's reference price waiver of pre-trade transparency stipulates that when possible, the reference price shall be established by obtaining the midpoint price within the current bid and offer prices (Regulation EU No 600/2014). For the purpose of investigating dark pool bans in Europe following the new regulations, emphasis should most heavily be put on literature concerning midpoint dark pool in the context of this paper.

The segmentation arising between different types of investors is supported further by Hatheway, Kwan, and Zheng (2017), who investigate dark pool trading in the U.S. In line with the reasoning of Zhu (2014), the authors find that dark trading enforces an informed-uninformed investor segmentation, which leads to a flight of liquidity from lit markets when uninformed investors migrate. Furthermore, the study finds that the majority of higher spreads accompanying dark trading act as compensation to liquidity providers for trading against informed investors. Hatheway, Kwan, and Zheng (2017) elaborate on this notion and argue that informed investors are discouraged to place limit orders when, in the absence of a national time priority, dark traders can trade ahead of liquidity providers in lit markets. Recent European regulations, however, state that outside the continuous trading phase of a trading session, the reference price shall be obtained from the opening or closing price of the relevant trading section (Regulation EU No 600/2014)—which limits this ability. Hatheway, Kwan, and Zheng (2017) ultimately conclude that dark trading activity is associated with both increased transaction costs, and reduced price discovery. This conclusion contradicts the results presented by Zhu (2014), which predicts opposite effects on price discovery and liquidity from dark pool activity.

Degryse, De Jong, and Van Kervel (2014) conduct an empirical study on fragmentation in lit order books and dark trading. The authors argue, in line with much previous literature, that the migration of uninformed investors to dark venues—referred to as a "cream-skimming effect"—brings along higher adverse selection on lit markets and a detrimental effect on lit liquidity. However, the study's dark trade data convey no information on the identity of the executing venue, nor does it contain information on the order book—leading up to the more general conclusion that costs and benefits of trading venue competition are determined by the type of trading venue (Degryse, De Jong, and Van Kervel, 2014). Moreover, the study uses data on trades executed on other venues than dark pools, including both internalized trades and OTC trades. This fact limits the extent to which the empirical results can be discussed in light of MiFID II's DVC rule, which concerns dark pool trading specifically. In a more recent study, Johann et al. (2019) evaluate the MiFID II regulations, and in particular investigate the impact of dark pool bans when quasi-dark alternatives—such as internalization systems and periodic auctions—exist. The study brings recent regulations to question, as the results indicate that implemented bans on stocks lead to unexpected consequences. The authors find that only a small part of liquidity returns to lit markets when dark pool bans on stocks are imposed, while volume spill-overs into quasi-dark alternatives are significant. The substitutability of these alternative trading systems may, thus, prevent MiFID II from fulfilling its purpose of maintaining a higher proportion of trading on lit markets. The study finds that the dark pool bans' subsequent impact on market liquidity and short-term price efficiency are overall insignificant and negligible.

4 Methodology

4.1 Model

The main objective set forth in this paper is to investigate effects on lit market quality following the implementation of MiFID II in Europe, and in particular the suspensions from dark pool trading invoked by the DVC rule. To this end, we employ a Difference-in-Differences (DID) model approach, exploiting the first market-wide regulatory intervention in dark pool trading in European equity markets. We focus our analysis on measures of volatility and liquidity to deduce the implications of the DVC rule in regards to market quality. The paper leverages the fact that the strict cut-off in dark pool trading imposed by the European Securities and Markets Authority (ESMA) in March, 2018, created a quasi-natural experiment with a clearly defined event and treatment groups. The contextual circumstances therefore allow us to infer any effects of the ban on our volatility and liquidity measures through the DID model approach elaborated on below.

Perhaps the most crucial issue in the context of the DID estimator lies in the assumption of parallel trends in the treatment and control group. Previous research suggests that dark pool trading is related to, for example, both firm-specific characteristics and liquidity conditions (Kwan, Masulis, and McInish, 2015 and Gomber et al., 2016), rendering a need for caution in context of the DID framework and its assumptions. This fact is directly noted and addressed by Johann et al. (2019), who attempt to circumvent the issue of parallel trends by conducting a semi-parametric DID model along with a robust regression discontinuity design, focusing on companies being close to the volume threshold, rather than on stocks on which suspensions are *de facto* imposed. Unlike Johann et al. (2019), we address this issue by employing a sample selection procedure in which securities in the treatment and control groups are matched based on similarities in firm characteristics. As such, the peer selection methodology is designed to ensure similar trends pre- and post-treatment for the two groups. We elaborate on this sample selection procedure further in Subsection 4.2.

The point of departure for subsequent analyses is the following DID regression model:

$$Y_{i,\tau} = \beta_0 + \beta_1 Treated_i + \beta_2 Window_{i,\tau} + \beta_3 Interaction_{i,\tau} + \epsilon_{i,\tau},$$

where $Y_{i,\tau}$ is a measure of liquidity or volatility, depending on the analysis. $Window$ is an indicator variable taking the value (0) before the implementation of dark pool trading suspensions, and (1) during the suspension window. Similarly, the indicator variable $Treated$ specifies if a security was suspended from trading in dark pools by ESMA starting on March 12, 2018 by taking the value (1), and (0) if no such suspension was imposed on the given stock. Furthermore, $Interaction$, is the DID estimate of the effect of the ban. The variable is defined as the interaction term between $Treated$ and $Window$, such that it captures effects from being suspended from dark pool trading during the event window. Thus, in subsequent analyses, $Interaction$ will be the main focus of investigation.

4.2 Data

To conduct our analysis, we gather intraday data from the Swedish House of Finance, including bid and ask quotes, market depth, and transaction prices, with corresponding time stamps. The collected data is sampled at a one second time frequency and is subsequently resampled to appropriate frequencies depending on the application, to avoid, for example, issues of 'microstructure noise'. We address missing values in the data by consistently replacing them with the last available data point.

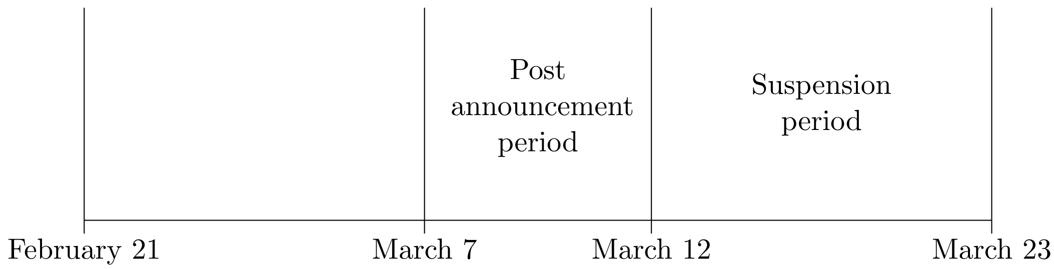
The list of securities which were to be subjected to the first dark pool ban was released by ESMA on March 7, 2018—with the implementation of the ban following on March 12, 2018. As this was the first implementation of the dark pool ban, a large number of stocks were suspended on the same date. As such, the period is particularly beneficial for statistical analysis, as it provides a solid basis for sample selection.[7] We use data covering the time period ranging from February 21, 2018, to March 23, 2018, translating to a total sample of 23 trading days. The time period therefore involves a pre-treatment window size of 13 trading days—including the period between announcement and implementation—and a treatment window of ten trading days. Figure 1 illustrates the timeline. Owing to to time limitations and data constraints, the event window is comparably short. However, as we suspect the majority of the implementation effects of the ban to be instantaneous, our sample should be sufficient in capturing the dark pool ban's fundamental impact on lit market quality. Meanwhile, our sample is concurrently limited by the use of one single event. While this fact may hamper our ability to present results that are representative for all dark pool bans implemented by ESMA, our results should give a strong indication of the underlying effects on lit market quality.

In line with the Difference-in-Differences model approach, our sample selection procedure involves the construction of two separate groups of suspended and non-suspended securities; denoted treatment and control group, respectively. For the selection of securities in the treatment group, we use ESMA's list of suspended securities released on March 7, 2018. First, we limit our sample to focus on the effect

[7]Even though a substantial number of securities were suspended on this date, downtime in the Swedish House of Finance database limited our ability to gather a more comprehensive set of data comprising a larger number of securities.

Figure I: Timeline illustration

The figure illustrates the full sample period with dates relevant to the analysis. On March 7, 2018, ESMA announced the securities to be suspended from dark pool trading as of March 12, 2018. The sample period ranges ten trading days prior to the annoncement, to ten trading days post-implementation.



of the dark pool ban on Nordic equity markets by filtering the list of suspended companies on geography. To limit issues in regard to liquidity and available data points, we subsequently rank these securities on size based on market capitalizations as per March 7, 2018. From this list, we select the 35 largest securities, for which data is available, to constitute the treatment group in our analysis.

The control group is constructed to mimic the treatment group along dimensions of firm size, industry, and geographical location; aiming at aligning pre- and post-treatment trends between the two groups. Matching on these factors concurrently serves the purpose of eliminating, for example, industry-specific effects in the underlying sample. In line with this notion and our geographical scope, we select securities from the Nasdaq OMX Nordic 120 index. We derive our control group by initially matching each security in the treatment group to the largest company in the index which belongs to the same industry. The matching on industry is conducted by ensuring that each pair of securities share the same Bloomberg Industry Classification (BIC) code. To ensure that all securities in our sample can be matched, we use macro sector BIC codes for the industry classification.

Accordingly and in summation, the full sample consists of 70 securities divided into two equally-sized subgroups, with data gathered over a total of 23 trading days. The full list of securities in our sample is presented in the Appendix, Table VIII.

4.2.1 Variables

The main focus of this paper is to assess the effect of the dark pool ban on measures of liquidity and volatility on lit markets. The liquidity analysis largely follows the work of Johann et al. (2019), who apply six measures relating to both quoted and trading liquidity. For quoted liquidity, we compute conventional bid-ask spreads as well as top-of-book depths—denoting, at time τ, the midpoint price as M_τ, and the best bid and ask quotes as Bid_τ^P and Ask_τ^P. Bid_τ^Q and Ask_τ^Q denote the corresponding first-level book depth quantities. Accordingly, we define:

$$\text{QuotedSpread}_\tau = \frac{Ask_\tau^P - Bid_\tau^P}{M_\tau},$$
$$\text{QuotedDepth}_\tau^{Bid} = Bid_\tau^P \times Bid_\tau^Q,$$
$$\text{QuotedDepth}_\tau^{Ask} = Ask_\tau^P \times Ask_\tau^Q$$

where best bid and ask prices are converted to euros, to ensure consistency across the sample securities. For trading liquidity, we follow Johann et al. (2019) and compute effective spreads, realized spreads, and a measure of price impact. The inclusion of the effective spreads allows us to address and capture transaction costs on the market by comparing the midpoint price to the actual transaction price. Relatedly, realized spreads, as concisely described by Johann et al. (2019), capture limit order traders' earned compensation, adjusted for any losses associated with adverse selection. Price impact is a measure of the information content in trades, and is computed by comparing the current midpoint price with the corresponding price at a future point in time. The trading liquidity measures are defined as follows:

$$\begin{aligned}
\text{EffectiveSpread}_\tau &= \frac{2 \times D_\tau \times (P_\tau - M_\tau)}{M_\tau}, \\
\text{RealizedSpread}_\tau^\Delta &= \frac{2 \times D_\tau \times (P_\tau - M_{\tau+\Delta})}{M_\tau}, \\
\text{PriceImpact}_\tau^\Delta &= \frac{2 \times D_\tau \times (M_{\tau+\Delta} - M_\tau)}{M_\tau},
\end{aligned}$$

where D_τ denotes the direction of the trade, assigned value (1) for trades initiated through a buy order, and (-1) for a sell. The trade direction is asserted through the employment of a conventional 'tick test', which in most applications has been shown to perform well (Lee and Ready, 1991). The algorithm behind the signing of each trade involves asserting the direction of the price movement following the previously conducted trade. If the transaction price is higher (lower) than than the previous trade, the trade is considered an 'uptick' ('downtick'), and classified as a buy (sell). If no movement in prices occurred between trades, we look at whether the previous trade was an uptick or downtick, and classify the trade accordingly. For realized spreads and price impact, we use midpoint prices $\Delta = \{5, 10, 15\}$ minutes ahead in time, to limit issues which may arise due to potential illiquidity of stocks in our sample. Given that a forward-fill approach is employed for missing data points, a too short time span in the construction of realized spreads for securities with few trades, would cause the variable to converge to the effective spread. Similarly, our measure of price impact would tend to zero. For the same reasons, we perform additional robustness checks in subsequent analysis; elaborated on in Section 5.

To investigate the impact of dark pool bans on volatility, we compute daily realized volatilities. The measure qualifies as an apt proxy for true volatility and is particularly accepted as such in academia for high-frequency, intraday data (Andersen et al., 2001). The measure is conventionally computed and defined as:

$$\mathrm{RVol}_{i,t} = \sqrt{\sum_{i=1}^{n} r_{i,\tau}^{2}},$$

where $r_{i,\tau}$ corresponds to intraday returns, excluding overnight returns, for company i between time $\tau - 1$ and τ. The measure is based on midpoint prices resampled at five minute frequencies in order to mitigate, to the extent possible, effects of microstructure noise.

To add to the robustness of our models, a number of control variables are included to account for their potentially non-negligible impact on our measures during the time frame. For company-specific press releases—exhibiting the potential to significantly impact both liquidity and volatility of a given stock—we include an indicator variable, $\mathrm{PR}_{i,t}$, assigned value (1) for company i on day t if a press release occurs, and (0) otherwise. Notwithstanding the potential issue of post-announcement drift, the majority of the impact of such press releases on market prices is argued to occur instantaneously. Furthermore, we address potential macroeconomic factors by including a variable, $\mathrm{10yGovB}_{i,t}$, relating to the price on day t of the ten-year government bond issued by the country in which company i is primarily listed. Finally, we include market volatility in our models in an attempt to capture market-wide factors impacting individual securities' volatility and liquidity characteristics. We model market volatilities by running a GARCH(1,1) model based on returns of the Swedish index Nasdaq OMXS30, with data corresponding to the one-year period between March 23, 2017 and March 23, 2018.

4.2.2 Descriptive statistics

An initial review of the descriptive statistics for our two groups—presented below in Table I—reveals a number of noteworthy facts for the purpose of subsequent discussions. The securities included in the treatment group—and thus suspended from dark pool trading within the event window—exhibit higher liquidity as measured solely by the quantity of trades conducted each day. While this number averages around 1,500 in this group, the corresponding figure for the control group is roughly half. The companies included in the treatment group also tend to be larger than their control group equivalents, as measured by market capitalization. Adjacently, we observe lower average quoted spreads in all percentiles for our suspended securities. The same applies to realized volatilities, which similarly tend to be lower among the suspended securities in our sample. The facts presented above are largely consistent with what has been shown in previous literature, supported by, for example, Buti, Rindi, and Werner (2010), who observe that more dark pools are active for larger stocks with lower spreads and lower volatilities. While a further analysis of these facts in isolation lies outside the scope of this paper, a few potential implications for subsequent analyses need to be addressed.

While the level differences in our measures presented do not imply violation of model assumptions *per se*, the low number of trades in the control group, particularly those falling in the lower percentiles, may have implications in the robustness of subsequent results. Even though the sample selection procedure involves sorting securities based on market capitalization, as described in Subsection 4.2, the discrepancy of the number of trades between the two groups is non-negligible. Given the employment of a forward-fill approach for missing values, securities with few trades per day may, therefore, also suffer from high degrees of invariability in the intraday data. As a result, realized spreads could correlate strongly with effective spreads, while values for price impact may take values very close to zero. We attempt to control for this fact in our robustness analysis by adjusting the sample composition using two different approaches involving replacement and exclusion of securities with particularly low liquidity. As established, the descriptive statistics presented here do not imply that the parallel trend assumption under DID estimations is violated. A visual overview of the variables during the time period does not either contest this assumption definitively (see Appendix, Figure II to XV).

Table I: Descriptive statistics

This table provides some descriptive statistics of the treatment and control group, each including a total of 35 securities. Market capitalization (MCap) values are in million euros and are collected only for March 7, 2018. All remaining variables are based on the full sample period of 23 trading days and all 35 securities in the respective group. Realized volatilities and quoted spreads are quoted in percentages, and Trades relates to the number of trades per day.

Treatment group						
Measure	5^{th} prct.	25^{th} prct.	Median	75^{th} prct.	95^{th} prct.	Average
Trades	518.0	973.8	1357.0	1880.5	3189.3	1536.5
MCap	6,405.1	9,140.9	14,970.4	22,011.7	32,552.3	18,531.3
RVol (%)	0.6803	0.8674	1.0100	1.2121	1.6292	1.0749
QuotedSpread (%)	0.0266	0.0379	0.0453	0.0531	0.0832	0.0486
Control group						
Measure	5^{th} prct.	25^{th} prct.	Median	75^{th} prct.	95^{th} prct.	Average
Trades	30.0	104.8	456.0	976.3	2335.0	724.2
MCap	1,025.1	1,800.7	3,849.6	7,929.2	26,872.8	7,605.2
RVol (%)	0.6795	0.9706	1.2137	1.5630	2.2305	1.3086
QuotedSpread (%)	0.0364	0.0629	0.1251	0.3197	0.7750	0.2424

5 Results

The question surrounding dark pool bans' effect on lit market characteristics ultimately concerns which types of traders concentrate in dark pools, and where order flows on those venues move following a dark pool ban. As previously established, research concerning this issue is largely divided in both theoretical and empirical literature. In particular, research diverges concerning where traders with different amounts of information concentrate, and the rationale for such concentration. The novelty of dark pool trading bans, especially in Europe, concurrently limits the extent to which our results can be put in relation to previous research. Still, Johann et al. (2019), who investigate the same DVC suspension, offer such a contextualization—while other studies focusing on dark pool trading activity in general form a foundation for the analysis of our results (e.g., Comerton-Forde and Putniņš, 2015; Foley and Putniņš, 2016; Buti, Rindi, and Werner, 2011). Table II summarizes the results of our main models based on the full sample of a total of 70 securities.[8]

[8]For brevity, Table II only presents realized spreads and the measure of price impact for $\Delta = 5$ minutes. Results for other choices of Δ are qualitatively similar and are available in the Appendix, Table IX.

Table II: Results: Difference-in-Differences estimations – Full sample

This table presents the ban implementation effects on our variables of interest based on random-effects Difference-in-Differences estimations. All marginal effects have been scaled by 10^4 (i.e., presented in basis points), with exceptions for quoted depths. The table presents variable significance by (*) 10%, (**) 5%, and (***) 1%.

Model	Treated	Window	Interaction	10yGovB	PM	log(MCap)	MVol
RVol	-5.146	-5.215	-0.541	-42.259**	2.098	-16.086***	1730.689***
QuotedSpread	-14.010***	-2.938*	2.682*	-0.644	-0.032	-5.310***	446.304
log(QuotedDepth$^{\text{Bid}}$)	0.573***	0.036	-0.016	0.011	0.014	0.311***	-16.419***
log(QuotedDepth$^{\text{Ask}}$)	0.622***	0.073	-0.069	0.459*	0.006	0.252***	-16.203***
EffectiveSpread	-8.454**	-2.015***	1.538**	-4.350	0.908	-3.633**	137.951
RealizedSpread^{5min}	-7.384**	-1.878**	1.872**	-1.398	0.685**	-3.304*	146.644
PriceImpact^{5min}	-0.504*	-0.319	-0.064	-2.753***	-0.252**	-0.745***	19.030

An initial overview of the results suggests that the impact of the ban on volatility and liquidity is not completely clear, with limited supporting evidence for either detrimental or beneficial effects on lit market quality. We note that the DID estimates of the effect of the ban are insignificant across the board, except for measures of spreads. The significance of these measures of transaction costs, however, indicate a potential link between lit market quality, as measured by trading and quoted spreads, and the dark pool ban. Interestingly, the marginal effects of all three measures are positive, implying increasing spreads following suspension from dark pools. These results, therefore, partially contrast those of Johann et al. (2019) who, albeit with a somewhat different methodological approach, find no evidence of any significance in either liquidity or volatility measures. The results also differ from the conclusions on dark pool activity made by Comerton-Forde and Putniņš (2015), who find that higher dark pool trading activity is associated with higher spreads on lit markets.

On the other hand, our results support the findings in empirical research by Buti, Rindi, and Werner (2011), and Foley and Putniņš (2016). The former documents an inverse relationship between dark pool activity and spreads, analogously implying that a dark pool ban should lead to higher spreads. Foley and Putniņš (2016) find similar evidence that higher dark pool activity is associated with lower quoted, effective, and realized spreads, albeit the significance in their results is limited to contexts of two-sided dark trading.

In relation to theoretical research on dark pools, increased spreads are implied neither by Ye (2011), nor Zhu (2014). Ye's (2011) reasoning concerning higher proportions of informed investors in dark pools implies that dark pool bans should lead to increased information content and lower spreads on lit markets—as this would cause those informed investors to migrate to lit venues. Zhu (2014) concludes conversely that informed traders concentrate on lit markets, mainly due to execution risk, and that increased dark pool trading is associated with price discovery improvements—in isolation supporting higher spreads following a dark pool ban. However, Zhu (2014) concurrently argues that increased dark pool trading has a detrimental effect on lit liquidity. The argument relates to adverse selection, and is based on the notion that liquidity providers are unable to offset losses from trading against informed investors when the proportion of liquidity traders (or uninformed traders, as argued by Glosten and Milgrom, 1985) on lit markets is low. As such, neither of these theoretical frameworks appear sufficient to explain our results of increasing spreads on lit markets. Instead, our results may be more aptly explained by arguments presented by Foley and Putniņš (2016). As competition for liquidity arises, e.g., when dark pools are available to traders, liquidity providers are forced to narrow down spreads on lit markets to compete with dark liquidity. In line with this argument, the implementation of a dark pool ban should, by extension of the same reasoning, eliminate such competition. Without the same need among liquidity providers on lit markets to actively take action to attract order flow from dark pools, it is plausible that spreads would increase as a result.

A more nuanced perspective of the forces at work can be introduced by contemplating the dynamics between the theoretical notions presented above. While our results suggest increased spreads as a result of the ban, it is still likely to hold true that the segmentation of traders, with different information and trading rationales, plays a substantial role in the effect of the ban. Following the reasoning of Zhu (2014) concerning this segmenting effect, the dark pool ban introduced to our sample may effectively have led to a migration of liquidity traders from dark to lit venues. As liquidity traders act on reasons exogenous to the efficient price—i.e., irrespective of the proximity between market price and the efficient price of a given security—increased spreads may be a result of increased noise in the market. In this dynamic setting, it is also possible, and perhaps even plausible, that Zhu (2014) is correct in his notion that liquidity providers' inability to offset losses when dark pool activity is high should be associated with increasing spreads. What our results do indicate, however, is that this force may be weaker compared to the competition for liquidity that arises among lit and dark venues, in combination with the additional noise contributed to lit markets by liquidity traders migrating from the dark.

Put in relation to the stated purpose of the DVC rule, the results of increasing effective and realized spreads indicate that recent regulations may be unsuccessful in improving market quality for traders on lit markets. On the other hand, we are unable to present evidence substantiating such a statement in any other measures of market quality. Indeed, realized volatility, quoted depths, and our measure of price impact do not change significantly following the ban, as suggested by our results. The lack of change in, for example, realized volatility, may be perceived as contradictory to the notion of liquidity traders migrating to lit markets, as this would theoretically also imply increased volatility as a result of the dark pool ban—a relationship substantiated by, e.g., Buti, Rindi, and Werner (2011). Although we are unable to provide evidence for significant relationships between the dark pool ban, volatility, and other measures presented herein, there are a number of potential underlying reasons for why such relationships would not be accurately captured.

The lack of significance in realized volatility, as in other measures in our analysis, may be a matter of insufficient data. As we only investigate the implementation of the dark pool ban during one event window, sample period bias cannot be ruled out. A more complete set of data spanning several events could therefore have the potential to confirm significance in several of our measures, which we are unable to substantiate in this analysis. As previously established, it is also possible that the occurrence of highly illiquid stocks, primarily in the control group, introduces issues of data insufficiency. This is of particular interest to address in relation to realized spreads and price impact. If trades are few, our forward-fill approach in addressing missing data points results in realized spreads converging to our effective spreads. Similarly, our measure of price impact would tend to zero. Indeed, by investigating the extent of this issue, we document a correlation between effective spreads and realized spreads of roughly 0.98. To add robustness to these results, we run corresponding DID estimations on our variables of interest based on a limited sample of securities. In this application, the most illiquid securities—and the stocks matched with those securities—are excluded. We define illiquid stocks as securities with less than 300 trades executed on any given day during our time period. Table III presents the model results based on the sample with these illiquid stocks excluded.

Table III:
Results: Difference-in-Differences estimations – Sample with illiquid stocks excluded

This table presents the ban implementation effects on our variables of interest based on random-effects Difference-in-Differences estimations, excluding the most illiquid stocks in the sample. Illiquid stocks are in this setting defined as securities with less than 300 trades on any given day during the time period. The limited sample includes a total of 32 securities. All marginal effects have been scaled by 10^4 (i.e., presented in basis points), with exceptions for quoted depths. The table presents variable significance by (*) 10%, (**) 5%, and (***) 1%.

Model	Treated	Window	Interaction	10yGovB	PM	log(MCap)	MVol
RVol	-6.014	-6.333*	4.142	-76.691***	-0.254	-16.526***	1854.064***
QuotedSpread	-0.564	-0.078	-0.098	-1.284***	0.004	-1.449***	19.642
log(QuotedDepth$^{\text{Bid}}$)	0.247*	0.030	0.028	0.287***	0.015	0.598***	-7.580*
log(QuotedDepth$^{\text{Ask}}$)	0.221	0.039	-0.027	0.367**	-0.008	0.634***	-13.446***
EffectiveSpread	-0.483	-0.300***	0.287***	0.089	-0.062	-1.195***	-17.355
RealizedSpread^{5min}	-0.352**	-0.151	0.393**	1.186	0.275**	-0.420	-29.050
PriceImpact^{5min}	-0.119	-0.181	-0.112	-1.707*	-0.339**	-0.816***	7.927

The results presented in Table III are similar to our previous results with significance in effective and realized spreads, while quoted spreads no longer appear to be significantly impacted by the ban. Supporting the robustness of these results, the correlation between effective and realized spreads drops to 0.44 in this limited sample with liquid stocks. The remaining variables of interest, however, still lack significance at all confidence levels considered.

We add further robustness to these results by running regressions on an additional sample, in which data on the most illiquid stocks are replaced. In this reconstructed sample, data on a given illiquid security in the control group is replaced with data on a different, non-suspended security, that is considered liquid. This procedure involves replacing the illiquid security with the closest match among the non-suspended securities in regards to market capitalization, and is hinged on the condition that the securities share the same industry. This effectively entails that data on 14 securities are duplicated in the underlying sample. Maintaining the definition of illiquid stocks exhibiting less than 300 executed trades on any given day, one industry, containing five securities in each group, is eliminated completely. These securities are excluded from the sample, resulting in a sample size of 60 securities. Table IV presents the results based on this sample.

Table IV:
Results: Difference-in-Differences estimations – Sample with illiquid securities replaced

This table presents the ban implementation effects on our variables of interest based on random-effects Difference-in-Differences estimations, replacing the most illiquid stocks in the control group. Illiquid stocks are in this setting defined as securities with less than 300 trades on any of the days during the time period. Illiquid stocks in the control group are replaced with a corresponding, non-suspended security matched on market capitalization and industry. The sample contains a total of 60 securities. All marginal effects have been scaled by 10^4 (i.e., presented in basis points), with exceptions for quoted depths. The table presents variable significance by (*) 10%, (**) 5%, and (***) 1%.

Model	Treated	Window	Interaction	10yGovB	PM	log(MCap)	MVol
RVol	-12.604**	-3.877	1.655	-58.605***	-1.288	-13.444***	2134.793***
QuotedSpread	-3.304**	0.154	-0.316	-1.419***	0.001	-0.175	58.288**
log(QuotedDepth$^{\mathrm{Bid}}$)	0.500***	0.031	0.001	0.270***	0.019	0.352**	-7.010*
log(QuotedDepth$^{\mathrm{Ask}}$)	0.497**	0.054**	-0.058	0.267**	-0.017	0.368**	-16.812***
EffectiveSpread	-1.913**	-0.250***	0.239***	0.069	0.067	-0.595**	24.937
RealizedSpread^{5min}	-1.436*	-0.159	0.338***	0.842	0.379***	0.155	3.823
PriceImpact^{5min}	-0.317	-0.139	-0.105	-1.747***	-0.318***	-0.942***	14.600

As evident from Table IV, the model results are qualitatively similar to what has been presented above, implying that the results are robust to different manipulations of the data set.

A plausible explanation to the overall absence of significance across our measures concerns the existence of quasi-dark trading venues—as emphasized, and elaborated on, by Johann et al. (2019). These venues offer, albeit not complete, but degrees of transparency, and fall outside the scope of ESMA's DVC legislation. As such, they continue to offer the benefits associated with such transparency, including offering traders protection from potential predatory behaviour from HFT traders on lit markets. As Johann et al. (2019) find quasi-dark venues, such as periodic auctions, to absorb a vast majority of the liquidity from dark venues following the ban, effects on volatility and liquidity on lit markets may in fact be limited—in which case our mere focus on lit markets is insufficient to understand the true underlying dynamics of the ban and its effects. In line with the sole focus on the duality of lit market characteristics and dark pool bans presented here, such an analysis lies outside of the scope of this study. Still, the inconclusiveness surrounding several of the volatility and liquidity measures does put to question whether the implementation of dark pool bans has indeed fulfilled its purpose of improving lit market conditions for traders. In this regard, our results share several similarities with those of Johann et al. (2019), where the lack of change in market quality may be aptly explained by quasi-dark alternatives, and an inadequate scope of the MiFID II framework.

An alternative reason as to why several market quality measures are seemingly unaffected by the ban concerns the potential for dissipating effects. There is a possibility that the effects on lit market quality in the short term are partially driven by a behavioral overreaction on the market following suspension of a security. In this setting, this fact could also entail that these temporary effects on trading behaviour return to previous patterns during the time period considered. To investigate whether any of our measures are subjected to effects which dissipate during the event window, we run additional DID estimations while varying the number of days in the event window.

Table V presents the model results based on the full sample of securities. Realized and effective spreads show significance consistently even under window sizes of four and five days, respectively, or more. Quoted spreads, however, only appear affected by the ban when considering window sizes of seven days and more. As such, the results indicate that the effect on trading spreads is near instantaneous, while quoted spreads appear affected by the ban only when considering larger window sizes. The theoretical reasoning concerning the impact on spreads still applies. Looking at Table V, we also notice that the marginal effects of realized and effective spreads decrease as the number of days in the event window increases—suggesting that the magnitude of the impact of the ban decreases with time.

Table V:
Results: Difference-in-Differences estimations – Full sample – Varying window size

This table presents results of the main model with varying window sizes, where the size is changed by altering the final date of the event window. The results are based on the full sample. The marginal effect of the main variable of interest, Interaction, is presented. All values have been scaled by 10^4 (i.e., presented in basis points). Variable significance is indicated by (*) 10%, (**) 5%, and (***) 1%.

	Interaction						
Model	3 days	4 days	5 days	6 days	7 days	8 days	9 days
RVol	-0.351	1.786	1.477	0.035	1.525	1.433	0.059
QuotedSpread	3.233	3.786	3.577	3.035	2.981*	2.798*	2.752*
log(QuotedDepth$^{\text{Bid}}$)	-0.074	-0.070	-0.034	-0.028	-0.029	-0.005	-0.015
log(QuotedDepth$^{\text{Ask}}$)	-0.107*	-0.113**	-0.083	-0.085	-0.078	-0.068	-0.062
EffectiveSpread	1.477	2.389	2.474*	2.640*	2.516*	2.307**	1.841**
RealizedSpread	1.263	2.256*	2.283*	2.540*	2.360*	2.137**	2.118**
PriceImpact	0.148	0.040	0.114	0.038	0.091	0.109	0.040

While our results show no clear signs of dissipating effects over our limited time period, it could conversely be the case that substantially larger window sizes are required to capture the true effect on certain of our variables. A potential reason for this is that the market may require additional time to adapt to the ban—in particular in the context of our analysis, given the uncertainty associated with the implementation of the very first market-wide ban in Europe. The lack of significance in several of the variables can also reflect potential issues of sample size, which may limit the extent to which the true impact is captured in the applications involving shorter event windows.

In contrast to previous results, quoted depths on the sell-side of the order book are reportedly significantly related to the implementation of the dark pool ban. The theoretical implications of these results would hypothetically entail either less share volume in the top-level order book depth, or lower ask quotes, as a result of the ban. However, it may, perhaps, more plausibly be a sole consequence of the market conditions during this relatively short time span, with a particularly low buying pressure among investors.[9]

[9] During the time period considered, the Nasdaq OMX Nordic 120 dropped from 997.49 to 957.52, corresponding to a negative return of 4.0%

Table VI:
Results: Difference-in-Differences estimations – Sample with illiquid stocks excluded – Varying window size

This table presents results of the main model with varying window sizes, where the size is changed by altering the final date of the event window. The results are based on the sample with illiquid stocks excluded. The marginal effect of the main variable of interest, Interaction, is presented. All values have been scaled by 10^4 (i.e., presented in basis points) except for quoted depths. Variable significance is indicated by (*) 10%, (**) 5%, and (***) 1%.

	Interaction						
Model	3 days	4 days	5 days	6 days	7 days	8 days	9 days
RVol	2.086	5.361	4.537	3.466	5.498	5.864	4.206
QuotedSpread	0.078	0.074	0.086	0.044	0.003	-0.035	-0.080
log(QuotedDepth^{Bid})	0.043	0.032	0.039	0.035	0.039	0.048	0.034
log(QuotedDepth^{Ask})	-0.047	-0.064	-0.040	-0.041	-0.033	-0.023	-0.031
EffectiveSpread	0.306***	0.304***	0.335***	0.321***	0.314***	0.304***	0.286***
RealizedSpread	0.703***	0.564**	0.651***	0.581***	0.423**	0.399**	0.364**
PriceImpact	-0.395	-0.261	-0.317	-0.262	-0.110	-0.098	-0.082

When investigating the corresponding results based on the limited sample of liquid stocks, presented in Table VI, we note that effective and realized spreads still appear to increase as a result of the dark pool ban. In contrast to the full sample, these measures are significantly impacted by the ban even under a window size of three days. In contrast, quoted spreads and quoted depths consistently appear unaffected, indicating that previous results may be influenced by characteristics specific to the securities excluded in the limited sample.

We perform complementary robustness analysis to determine whether there is an impact on our measures of market quality at announcement of the ban, rather than at implementation. This could be the case in a setting in which investors adopt new trading behaviors in advance, and in anticipation, of the actual implementation of the ban and its effects. We perform this analysis this by altering the event window to begin at announcement on March 7, 2018, and cover the days up until the implementation of the ban; making out a window size of a total of three trading days. Table VII presents these results based on the sample with illiquid stocks excluded. Overall, the results show no material impact of ESMA's suspension announcement—which suggests that investors do not act in anticipation of the dark pool ban. From a theoretical perspective this is sound, as there is no clear rationale for investors to divert order flow from dark pools prior to the implementation of the dark pool ban.

Table VII:
Results: Difference-in-Differences estimations – Sample with illiquid stocks excluded – Window beginning at announcement

This table presents the effects on our variables of interest based on random-effects Difference-in-Differences estimations, where the event window is defined as the three trading days between announcement and implementation. The results are based on the limited data sample excluding the most illiquid stocks. Illiquid stocks are in this setting defined as securities with less than 300 trades on any given day during the time period. All marginal effects have been scaled by 10^4 (i.e., presented in basis points), with exceptions for quoted depths. The table presents variable significance by (*) 10%, (**) 5%, and (***) 1%.

Model	Treated	Window	Interaction	10yGovB	PM	MCap	MVol
RVol	-7.338	-4.507	4.149	-66.414*	-0.174	-15.768***	769.765
QuotedSpread	-0.407	-0.424***	0.267***	-1.475	-0.033	-1.699***	10.799
$\log(\text{QuotedDepth}^{\text{Bid}})$	0.315**	-0.009	0.046	0.568***	0.018	0.520***	-5.644
$\log(\text{QuotedDepth}^{\text{Ask}})$	0.305**	0.021	-0.000	0.513**	-0.018	0.546***	-8.002*
EffectiveSpread	-0.380	-0.221**	0.141	0.302	-0.026	-1.340***	-25.727*
RealizedSpread	-0.279	-0.026	-0.282	1.090	0.118	-0.431	-3.754
PriceImpact	-0.155	-0.196	0.407	-2.191	-0.174	-0.900***	-24.452

6 Conclusion

This paper scrutinizes recent regulatory intervention in dark pool trading by investigating the implementation effects of MiFID II's DVC rule on measures of lit market quality. By leveraging the cut-off in dark pool trading activity following suspension of 35 securities in Nordic equity markets, we reveal that the dark pool ban has unclear effects on lit markets. We find evidence for increasing measures of spreads as a result of the ban, indicating a detrimental effect on lit liquidity conditions—which is supported by, e.g., Buti, Rindi, and Werner (2011) and Foley and Putniņš (2016). Conversely, other measures of market quality appear unaffected by the ban, where similar results have been presented in previous research by Johann et al. (2019). The lack of evidence justifying regulatory intervention in dark pool trading to improve market quality for investors render additional research on the subject crucial. The strand of literature would benefit from thorough analysis of the implementation effects of the ban particularly by investigating multiple events using a more comprehensive set of data.

7 References

Anagnostidis, P., Papachristou, G. & Varsakelis, C., 2019. Market quality and dark trading in the post MiFID II era: What have we learned so far? *Economics Letters*, 184.

Andersen, T., Bollerslev, T., Diebold, FE. & Labys, P., Modelling and Forecasting Realized Volatility (March, 2001). *NBER Working Paper No. 8161.*

Boulatov, A. & George, T.J., 2013. Hidden and Displayed Liquidity in Securities Markets with Informed Liquidity Providers. *The Review of Financial Studies*, 26(8), pp. 2096–2137.

Buti, S., Rindi, B. & Werner, I.M. 2010. Diving into Dark Pools. Working Paper Series 2010-10. *Ohio State University, Charles A. Dice Center for Research in Financial Economics.*

Comerton-Forde, C. & Putniņš, T., 2015. Dark trading and price discovery. *Journal of Financial Economics*, 118(1), pp. 70–92.

Degryse, H., de Jong, F. & Kervel, V.van, 2015. The Impact of Dark Trading and Visible Fragmentation on Market Quality*. *Review of Finance*, 19(4), pp. 1587–1622.

European Commission, 2010. Review Of The Markets In Financial Instruments Directive (MIFID). Directorate General Internal Market and Services.

Foley, S. & Putniņš, T.J., 2016. Should we be afraid of the dark? Dark trading and market quality. *Journal of Financial Economics*, 122(3), pp. 456–481.

Foley, S., Malinova, K. and Park, A., 2013. Dark Trading on Public Exchanges. *SSRN Electronic Journal*.

Glosten, L.R. & Milgrom, P.R., 1985. Bid, ask and transaction prices in a specialist market with heterogeneously informed traders. *Journal of Financial Economics*, 14(1), pp. 71–100.

Gomber, P., Sagade, S., Theissen, E., Weber, M.C. & Westheide, C., Spoilt for Choice: Order Routing Decisions in Fragmented Equity Markets (August 31, 2016). *SAFE Working Paper No. 143*,

Hatheway, F., Kwan, A. & Zheng, H., 2017. An Empirical Analysis of Market Segmentation on U.S. Equity Markets. *Journal of Financial and Quantitative Analysis*, 52(6), pp. 2399–2427.

International Organization of Securities Commission, 2010. Issues Raised By Dark Liquidity. Consultation Report.

Johann, T., Putnins, T., Sagade, S. and Westheide, C., 2019. Quasi-Dark Trading: The Effects of Banning Dark Pools in a World of Many Alternatives. *SSRN Electronic Journal*.

Kwan, A., Masulis, R. and McInish, T., 2015. Trading rules, competition for order flow and market fragmentation. *Journal of Financial Economics*, 115(2), pp.330-348.

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

Lehmann, B., 2002. Some desiderata for the measurement of price discovery across markets. *Journal of Financial Markets, 5(3), pp.259-276.*

Nasdaq.com. 2018. Are Double Volume Caps Impacting The Trading Landscape?. [online] Available at: <https://www.nasdaq.com/articles/are-double-volume-caps-impacting-trading-landscape-2018-04-27> [Accessed 8 October 2020].

Petrescu, M. & Wedow, M., 2017. Dark Pools In European Equity Markets: Emergence, Competition And Implications. Occasional Paper Series. European Central Bank.

Petrescu, M., Wedow, M. & Lari, N., 2017. Do dark pools amplify volatility in times of stress? *Applied Economics Letters*, 24(1), pp.25–29.

Ready, M. J.2010. Determinants of Volume in Dark Pool Crossing Networks. *Paper presented at AFA 2010 Atlanta Meetings*, 2010.

Regulation (EU) No 600/2014 of the European Parliament and of the Council of 15 May 2014 on markets in financial instruments and amending Regulation (EU) No 648/2012. (2014). *Official Journal of the European Union* L 173.

Ye, L., 2016. Understanding the Impacts of Dark Pools on Price Discovery. *SSRN Electronic Journal*.

Ye, M., 2011. A Glimpse into the Dark: Price Formation, Transaction Cost and Market Share of the Crossing Network. *SSRN Electronic Journal*.

Zhu, H., 2014. Do Dark Pools Harm Price Discovery? *The Review of Financial Studies*, 27(3), pp.747–789.

8 Appendix

Table VIII: Sample securities

Full sample		*Sample with illiquid stocks replaced*		
Suspended	Non-suspended	Suspended	Non-suspended	Industry
ALFA SS	HUSQ A	ALFA SS	SAAB B	Industrials
ASSA B	SAAB B	ASSA B	SAAB B	Industrials
ATCO A	NIBE B	ATCO A	NIBE B	Industrials
DSV	KBHL B	DSV	NIBE B	Industrials
HEXA B	SWEC B	HEXA B	NCC B	Industrials
KNEBV	FSKRS	KNEBV	NCC B	Industrials
MAERSK A	NCC B	MAERSK A	NCC B	Industrials
MAERSK B	AF B	MAERSK B	NCC B	Industrials
SAND	BEIJ B	SAND	NCC B	Industrials
SKA B	ADDT B	SKA B	NCC B	Industrials
SKF B	NELES	SKF B	NELES	Industrials
VOLV B	SKF A	VOLV B	NELES	Industrials
WRT1V	VOLV A	WRT1V	NELES	Industrials
DANSKE	INVE B	DANSKE	INVE B	Financials
SAMPO	SHB B	SAMPO	INVE B	Financials
SEB A	SEB C	SEB A	INVE B	Financials
SHB A	INDU C	SHB A	INDU C	Financials
SWED A	KINV	SWED A	KINV BB	Financials
TRYG	INTRUM	TRYG	INDU C	Financials
CARL B	ESSITY	CARL B	ESSITY	Consumer Staples
SWMA	ICA	SWMA	ICA	Consumer Staples
ELUX B	THULE	ELUX B	KIND SDB	Consumer Discretionary
HM B	KIND SDB	HM B	KIND SBD	Consumer Discretionary
PNDORA	EVO	PNDORA	EVO	Consumer Discretionary
CHR	BOL	CHR	BOL	Materials
NZYM B	SCA A	NZYM B	SSAB B	Materials
UPM	SSAB B	UPM	SSAB B	Materials
VWS	LUNE	VWS	LUNE	Energy
FORTUM	ORSTED	FORTUM	ORSTED	Utilities
ERIC B	NOKIA	ERIC B	NOKIA	Technology
COLO B	MCOV B			Health Care
DEMANT	VITR			Health Care
GMAB	TTALO			Health Care
NOVO B	ARJO B			Health Care
LUN	ORNAV			Health Care

Table IX:
Results: Difference-in-Differences estimations for different Δ – Full sample

This table presents the effects on realized spreads and price impact for $\Delta = \{10, 15\}$, based on random-effects Difference-in-Differences estimations. The results are based on the full sample with all stocks. All marginal effects have been scaled by 10^4 (i.e., presented in basis points). The table presents variable significance by (*) 10%, (**) 5%, and (***) 1%.

Model	Treated	Window	Interaction	10yGovB	PM	MCap	MVol
RealizedSpread^{10min}	-6.830**	-1.664*	1.892**	-1.047	1.001***	-3.117*	80.755
RealizedSpread^{15min}	-7.274*	-2.084	2.395*	0.352	1.100***	-2.865*	65.234
PriceImpact^{10min}	-1.002**	-0.594*	-0.082	-4.134***	-0.586***	-1.023***	79.684
PriceImpact^{15min}	-1.036	-0.535	-0.209	-5.255**	-0.750**	-1.197***	121.112*

Table X:
Results: Difference-in-Differences estimations – Full sample – Window beginning at announcement

This table presents the effects on our variables of interest based on random-effects Difference-in-Differences estimations. The event window is here defined as the three trading days between the announcement of suspended securities, and the implementation of the ban. The results are based on the full sample with all stocks. All marginal effects have been scaled by 10^4 (i.e., presented in basis points), with exceptions for market capitalization (MCap) and market volatility (MVol). The table presents variable significance by (*) 10%, (**) 5%, and (***) 1%.

Model	Treated	Window	Interaction	10yGovB	PM	MCap	MVol
RVol	-6.676	-4.436	-1.406	-70.492***	3.512	-15.472***	569.568
QuotedSpread	-12.921**	-0.783	-0.261	-11.417	-0.524	-6.431**	587.240
$\log(\text{QuotedDepth}^{\text{Bid}})$	0.629***	-0.064	0.079*	-0.249	0.012	0.243**	-13.599**
$\log(\text{QuotedDepth}^{\text{Ask}})$	0.682***	0.024	-0.025	0.395	0.031	0.205**	-14.875***
EffectiveSpread	-7.519**	0.511	-0.979	-4.584	0.726	-4.210**	88.883
RealizedSpread	-6.808*	0.524	-0.840	-7.048	0.734*	-3.786*	123.237
PriceImpact	-0.485	-0.198	0.059	-2.917**	-0.150	-0.782***	-26.682

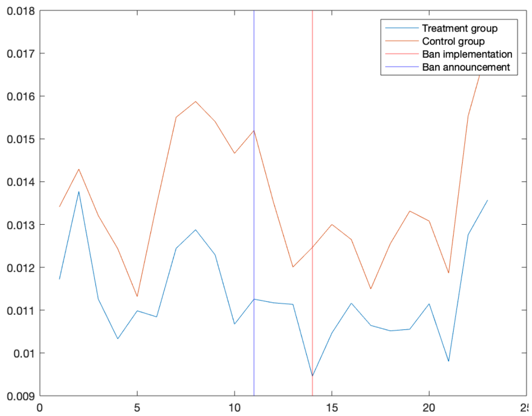


Figure II: RVol –

Full sample

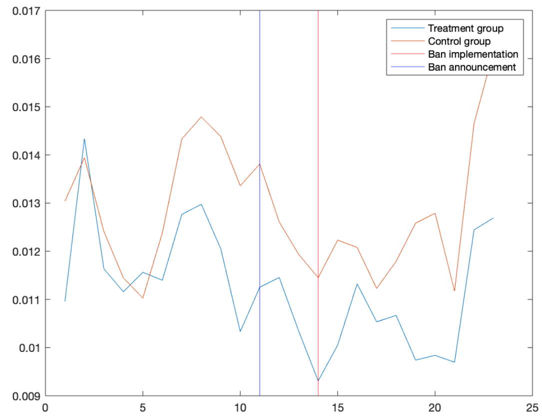


Figure III: RVol –

Sample with illiquid stocks excluded

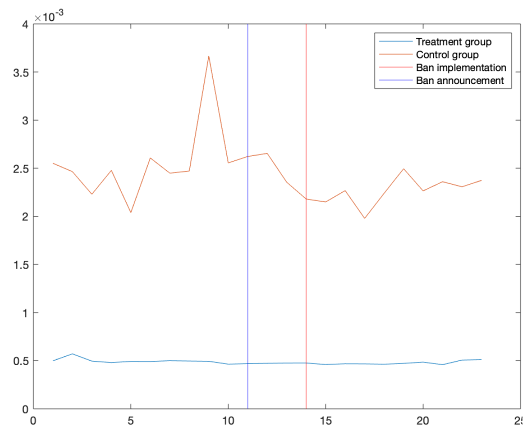


Figure IV: QuotedSpread –

Full sample

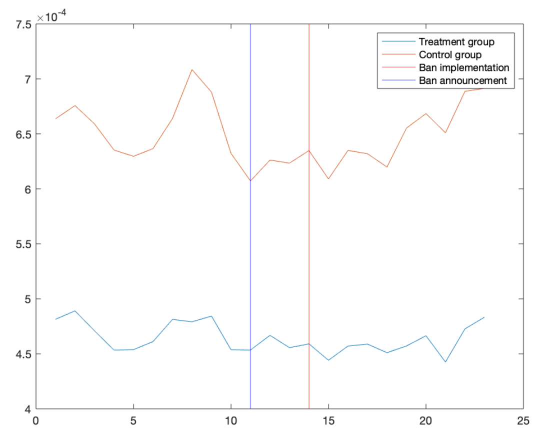


Figure V: QuotedSpread –

Sample with illiquid stocks excluded

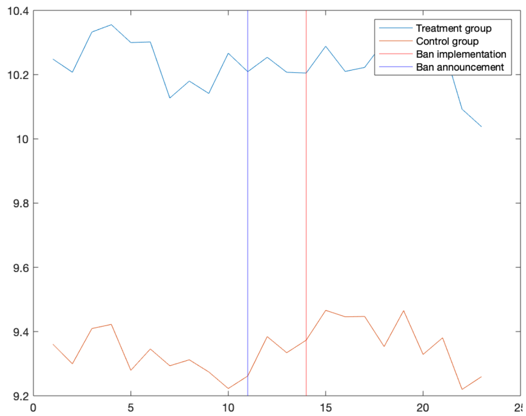


Figure VI: QuotedDepth^{Ask} –

Full sample

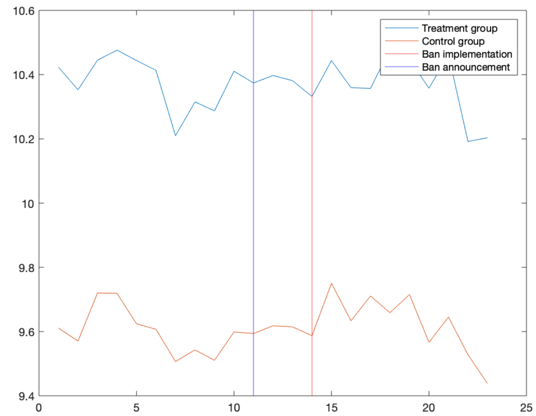


Figure VII: QuotedDepth^{Ask} –

Sample with illiquid stocks excluded

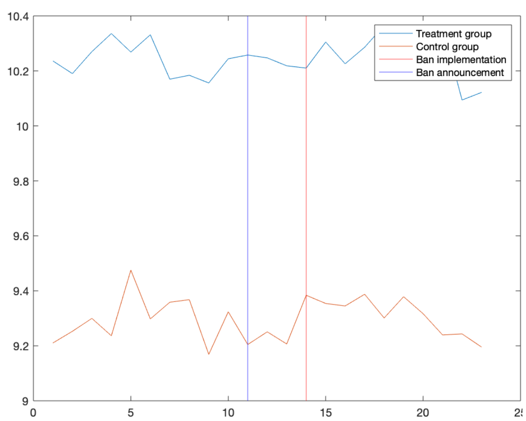


Figure VIII: QuotedDepth^{Bid} –

Full sample

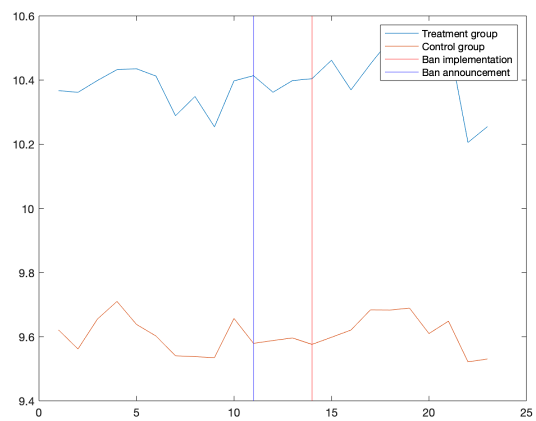


Figure IX: QuotedDepth^{Bid} –

Sample with illiquid stocks excluded

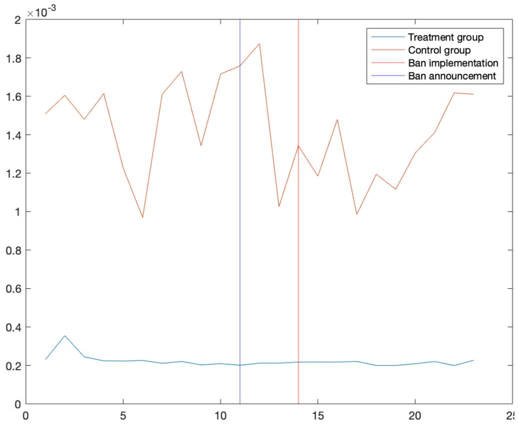


Figure X: EffectiveSpread –
Full sample

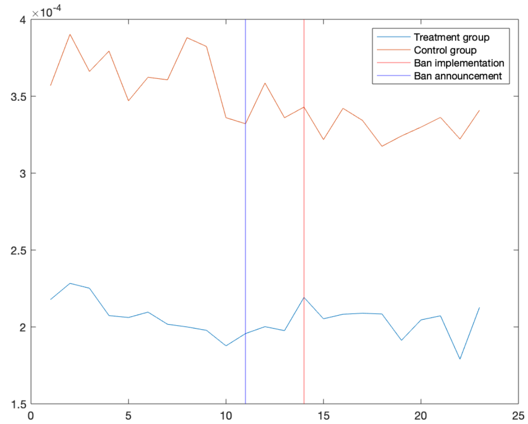


Figure XI: EffectiveSpread –
Sample with illiquid stocks excluded

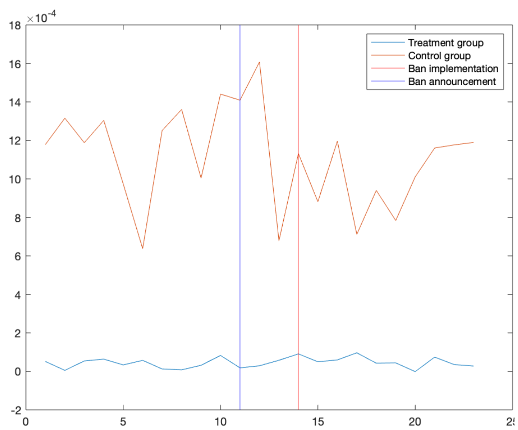


Figure XII: RealizedSpread^{5min} –
Full sample

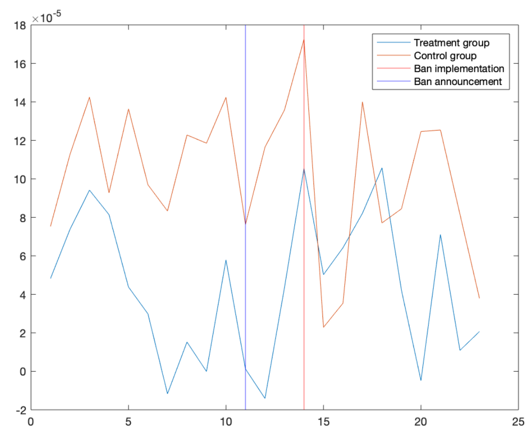


Figure XIII: RealizedSpread^{5min} –
Sample with illiquid stocks excluded

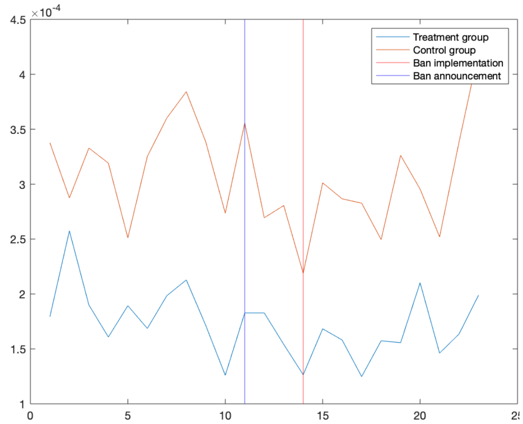


Figure XIV: PriceImpact^{5min} – Full sample

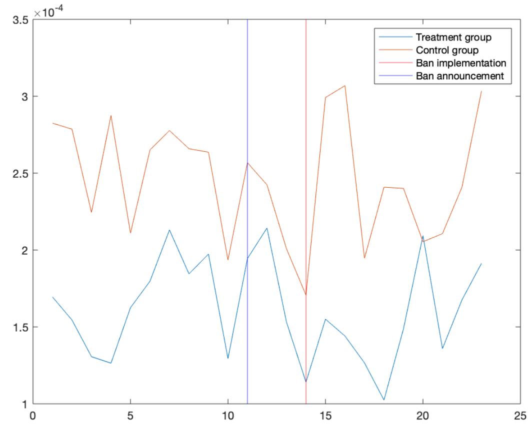


Figure XV: PriceImpact^{5min} – Sample with illiquid stocks excluded