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Working Paper Series

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SAFE Working Paper No. 253

SAFE | Sustainable Architecture for Finance in Europe
A cooperation of the Center for Financial Studies and Goethe University Frankfurt

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Quasi-dark trading: The effects of banning dark pools in a world of many alternatives

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This version: April 30, 2019

Preliminary and incomplete.

Abstract

We show that "quasi-dark" trading venues, i.e., markets with somewhat non-transparent trading mechanisms, are important parts of modern equity market structure alongside lit markets and dark pools. Using the European MiFID II regulation as a quasi-natural experiment, we find that dark pool bans lead to (i) volume spill-overs into quasi-dark trading mechanisms including periodic auctions and order internalization systems; (ii) little volume returning to transparent public markets; and consequently, (iii) a negligible impact on market liquidity and short-term price efficiency. These results show that quasi-dark markets serve as close substitutes for dark pools and consequently mitigate the effectiveness of dark pool regulation. Our findings highlight the need for a broader approach to transparency regulation in modern markets that takes into consideration the many alternative forms of quasi-dark trading.

JEL Classification: G10, G19

Keywords: Dark Pools; Dark Trading; Liquidity; Price Efficiency; MiFID II; Double Volume Caps

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*We thank participants at the 25th Annual Meeting of the German Finance Association, the SAFE Annual Conference, the Annual Meeting of the Swiss Society for Financial Market Research, the Consob, ESMA, and Bocconi Conference on Securities Markets, the Finance Brownbag Seminar at the University of Mannheim, and ESMA for comments and suggestions. Sagade and Westheide gratefully acknowledge research support from the Research Center SAFE, funded by the State of Hessen initiative for research LOEWE.

1 Introduction

Dark pools – venues that provide no pre-trade transparency – are routinely used by investors, especially large buy-side institutions, to manage order exposure costs. These venues are highly controversial due to three main concerns: (i) a lack of level playing field vis-à-vis public markets, (ii) inadequate disclosures concerning order routing decisions,[1] and (iii) a potential to impair public markets' price discovery mechanism. In combination with recent increases in dark pools' market shares[2,3] these arguments have put dark pools at the forefront of regulatory scrutiny. Regulators in the EU, Canada, and Australia have all implemented restrictions or caps on dark trading[4], the most recent being bans introduced in 2018 on dark pools in Europe once their volume reaches a particular threshold. The intention of these regulations is to ensure efficient price discovery and to maintain high levels of liquidity in public markets by keeping the majority of trading on transparent public markets.

Most theoretical and empirical literature explores a dichotomy of lit markets versus dark pools or transparent versus non-transparent trading. This dichotomy however oftentimes is an oversimplification of the reality of today's complex market landscape, which instead involves a wide spectrum of pre-trade transparency or "shades of grey". Besides dark pools and transparent public markets, there exist several other mechanisms that all offer varying degrees of opacity and provide an alternative to trading in dark pools. We refer to these mechanisms as "quasi-dark". Quasi-dark venues include internalization platforms, the over-the-counter market and periodic auction markets, i.e., venues conducting repeated call auctions throughout the day.[5]

[1] Anand, Samadi, Sokobin, and Venkataraman (2019) provide empirical evidence that in such an opaque setting brokers might route orders to markets associated with higher trading costs for their client.

[2] Healthy Markets (2010), citing data from Rosenblatt Securities, report that the market share of US dark pools increased from 4% in 2008 to 18% in 2015. Petrescu and Wedow (2017) report that, in 2016, European dark pools had a market share of approximately 12%, 8%, and 7% for stocks listed in London, Paris, and Frankfurt, respectively.

[3] Another recent development is a decrease in dark pool trade sizes. Dark pools were originally conceived as mechanisms best suited for *large* natural liquidity traders to trade directly with each other while minimizing information leakage. But CFA Institute (2012) and Foley and Putniņš (2016) observe that dark pool and lit market trade sizes in the US and Canadian markets are comparable. Petrescu and Wedow (2017) report that, in the EU, more than 50% of dark pool trades are smaller than €50,000 and only two dark pools actively restrict trading to large block orders.

[4] We refer to "dark trading" for any mechanism that provides no or only little pre trade information.

[5] The US equity market, as of September 2018, is characterized by competition between twelve lit exchanges on the one hand, and 42 Alternative Trading Systems (ATS) encompassing a diverse set of mechanisms on the other hand.

This complex trading landscape raises several important questions: What happens when restrictions or limits are placed on only one form of dark trading? Does dark volume migrate to other (quasi-)dark mechanisms? If so, what does the volume migration reveal about which trading mechanisms are viewed as the closest substitutes to dark pool trading? Do volume spill-overs to quasi-dark venues mitigate the effects of dark trading regulation? What are the effects of such regulations on market quality? And should transparency policies be broader and take into consideration the spectrum of quasi-dark alternatives?

We shed light on these questions using a quasi-natural experiment provided by the European Commission's (EC) Markets in Financial Instruments Directive (MiFID II). The new regulation imposes a complete ban on trades below a size threshold in dark pools for stocks that historically traded more than 8% of their volume in such venues (henceforth referred to as "the ban"). Using a large cross-section of stocks that differ in terms of their size, index membership, liquidity, tick size, and primary listing market, we show that indeed quasi-dark trading plays a significant role in today's markets and impacts the effectiveness of dark trading regulations.

Our analyses reveal complex shifts in trading activity in multiple directions upon the mechanical elimination of small trades in dark pools. Continuous lit markets gain about 1% market share suggesting that the regulation has little success in inducing a shift in trading activity towards lit markets. However, investors – attempting to avoid the (potentially) higher costs associated with information leakage in continuous lit markets – shift almost three times as much order flow towards block trading venues, periodic auctions, and internalizing dealers. These three (quasi-)dark markets gain approximately 0.6%, 1.3%, and 0.8% market share respectively. The ban's impact on market quality is largely negligible. Liquidity, proxied by quoted and effective bid-ask spread and top-of-book depth, remains unchanged and price efficiency deteriorates slightly. These results are not surprising considering most trading in response to the ban does not shift towards the lit market but instead to alternative quasi-dark venues. Using event study methodology, we show that investors had a positive expectation on the ban's implications, evidenced by a positive announcement return experienced by banned stocks. Consistent with the ban's largely insignificant effect on market quality, this announcement return is reversed with the actual implementation of the ban.

We compute the above-mentioned effects by employing two quasi-experimental techniques:

Semiparametric Difference-in-Differences (SP-DID) developed by Abadie (2005) and Regression Discontinuity Design (RDD). A standard DID estimator relies on the strong assumption that outcomes for firms in the treatment and control group follow parallel trends. However, this assumption is not guaranteed to hold in our setting as dark pool activity is correlated with several firm characteristics, liquidity conditions, and the tick size (Kwan, Masulis, and McInish, 2015; Gomber, Sagade, Theissen, Weber, and Westheide, 2016). SP-DID modifies the standard estimator by adjusting the weights of individual firms for their propensity to be banned, which is estimated as a function of observable covariates. For robustness purposes, we also employ an RDD estimator that focuses on stocks close to the 8% cap. To further alleviate concerns about a causal interpretation of our results, we conduct placebo tests using a subset of 24 stocks that were not treated due to inadequate and/or erroneous reporting by trading venues to ESMA.

This paper contributes to the literature on dark market fragmentation by exploiting a regulatory event that had the effect of banning small trades in mid point dark pools. Classical theories of market fragmentation such as Pagano (1989) and Mendelson (1987) argue that consolidation of trading onto a single (or fewer) venue(s) leads to improvements in market quality due to positive network externalities. Harris (1993) however argues that a fragmented market can emerge in equilibrium as heterogeneous agents optimize their venues choices based on their need for anonymity, immediacy, and transparency. Theories of dark pool trading such as Hendershott and Mendelson (2000), Zhu (2014), Buti, Rindi, and Werner (2017), Ye (2011), Brolley (2016), and Menkveld, Yueshen, and Zhu (2017) derive equilibrium strategies for heterogeneously informed investors as a function of the execution probability in dark pools (typically vis-à-vis lit markets), the price improvement offered by dark pools, the type of information traders possess (short-term versus long-term), and other agents' (typically uninformed and/or noise traders) strategies.

The empirical literature on the impact of dark trading on market quality provides conflicting results. Comerton-Forde and Putniņš (2015) evaluate the impact of dark trading on price discovery by examining the effect of rule changes affecting dark trading activity in the Australian market. They find that, at moderate levels, dark trading is beneficial, whereas beyond 10% of total volume it harms price efficiency. Conrad, Johnson, and Wahal (2003) find that institutional order execution costs are substantially lower in mid-quote dark pools. Buti, Rindi, and Werner

(2016) examine the determinants of activity in eleven US dark pools and find that increased dark pool activity is associated with higher liquidity. Foley and Putniņš (2016) rely on regulatory decisions requiring minimum price improvement in Australian and Canadian dark pools as an instrument to causally examine the impact of dark trading on market quality. They find that dark limit order books positively impact market quality. In contrast, they do not find consistent evidence for an impact of mid-point dark pools on market quality. Farley, Kelley, and Puckett (2018) evaluate the exogenous reduction in dark trading observed for one treatment group of the US tick size pilot and find no effect on market quality.

The conflicting results in empirical research likely arise for three reasons. First, the relationship between market quality and trading in dark pools is endogenous with bi-directional causality. Most studies rely on regulatory/rule changes as an identification strategy for causal inference. These rules are typically applied uniformly across all stocks. In contrast, our setting involves cross-sectional heterogeneity in the imposition of the ban. Second, dark pools encompass a variety of market models in terms of their order matching rules, type of ownership, permissible trade sizes, clientèle they cater to, etc.[6] For example, some dark pools operate as independent limit order books, others rely on derivative pricing and match orders at lit market best or mid quotes. Existing studies either differ in the specific market model they analyze or subsume different market models under the dark market label. For example, Farley, Kelley, and Puckett (2018) focus on dark limit order books and internalizing platforms. Our setting is specific in that we analyze a ban on mid-point dark pools. Furthermore, Hatheway, Kwan, and Zheng (2017) find that dark pool trade sizes affect the impact of dark trading on market quality. Our setting is unique in that it includes market places that specifically cater to large or small trade sizes and in that the ban only applies to small trades in dark pools. Finally, the level of fragmentation and the regulatory setting (tick sizes, minimum price improvements, etc.) around dark trading differ substantially across markets. Our setting involves multiple competing trading mechanisms with different levels of pre-trade transparency.

Our analysis has clear policy implications. It sheds light on the extent to which the restrictions were successful in achieving the regulator's dual objective of shifting trading towards lit venues and improving the efficiency of stock prices. Our results highlight that in a market

[6] Mittal (2008) provides a taxonomy of dark pools.

with several shades of dark venues (pre-trade transparency), restricting trading in one dark market mostly shifts investors to other close, albeit imperfect, substitutes. These shifts can have unintended consequences contrary to regulatory expectations.

2 Institutional Background

The implementation of the Markets in Financial Instruments Directive (MiFID) in November 2007 kick-started competition in the trading services industry by allowing exchanges and other venues to compete with each other for order flow. At the same time, MiFID also imposed pre-trade (and post-trade) transparency requirements on trading venues to mitigate potential adverse consequences associated with order flow fragmentation. These requirements include an obligation for venues to make quotes (bid and ask prices) and related depths publicly available.

MiFID also provides specific exemptions from these requirements to certain orders and/or trading venues by instituting four pre-trade transparency waivers: (i) the reference price waiver; (ii) the negotiated trade waiver; (iii) the large-in-scale waiver; and (iv) the order management facility waiver. The *reference price waiver* is used by trading systems that rely on the derivative pricing rule to execute trades at a widely published price obtained from another system. The *negotiated trade waiver* applies to bilaterally negotiated trades that are priced either within the (volume-weighted) bid-ask spread or are subject to conditions other than the current market price. This waiver is most relevant for trading systems handling retail orders, trades attached with special conditions such as Volume-Weighted Average Price (VWAP), portfolio, give-up, special ex/cum dividend trades, and trades involving a non-standard settlement. The *large-in-scale waiver* is applicable to block trades defined as those that are greater than the large in-scale size threshold, which is computed annually based on the average turnover of a stock and ranges from €15,000 to €650,000. Finally, the *order management facility waiver* applies to orders, such as hidden and iceberg orders, held by exchanges in their systems before being disclosed to the market.[7]

[7]Further information about these waivers is available in ESMA's report on Waivers from Pre-Trade Transparency.

2.1 Dark, Quasi-Dark and Lit Trading

In this paper we differentiate between seven different market mechanisms that can be categorized into three shades of pre-trade transparency: At one end of the spectrum, dark pools relying on the reference price waiver and dark pools relying on the large-in-scale (LIS) waiver are markets that offer no pre-trade transparency at all. At the other end of the spectrum, lit markets offer information on the number of orders and volume for several price levels. Traditional auctions, systematic internalizers (SIs), over-the-counter (OTC) markets and periodic auctions only offer a limited degree of transparency and are categorized as quasi-dark mechanisms in this paper.

Dark pools are fully non-transparent multilateral markets that rely on the reference price waiver and/or the large-in-scale waiver to execute orders at midpoint prices derived from the lit markets. On the other end of the transparency distribution, public continuous limit order book markets that offer real-time information on current orders and quotes are represented by lit incumbent exchanges and other lit markets (such as Cboe BXE (formerly Bats), Cboe CXE (formerly Chi-X), and Turquoise).

In between those two extremes of complete/no opacity, quasi-dark mechanisms offer a limited degree of pre-trade transparency: Most incumbent exchanges in Europe start and end trading in the continuous limit order book by a call auction phase. On London Stock Exchange and Deutsche Börse Xetra those are complemented by an intraday auction in the middle of the trading day. Finally, continuous trading can be interrupted by circuit breakers whenever prices leave pre-specified price bands. In such a case prices are determined in an auction before trading in the continuous limit order book resumes. Throughout this paper, we refer to the set of open, close, intraday and volatility auctions as traditional call auctions. These auctions concentrate liquidity at specific times during the day when buyers and sellers are brought together to trade at a single price. During the call phase usually lasting between two and five minutes, orders are collected in a central limit order book and traders can enter, change and delete orders/quotes. Only aggregated information on the current order situation is published. Indicative prices/volumes reveal the conditions at which auction trades would execute if the auction ended at that time. The auction clears at the uncrossing price that maximizes the trading volume that can be executed. Only providing indicative prices, auctions offer less pre-trade information compared to lit markets.

SIs are operated by broker-dealers and high-frequency trading firms to execute client orders against their own inventory on a frequent and substantial basis. An SI thus operates a bilateral system and is not allowed to match third party buying and selling interests. While SIs have to publish bid and offer prices for small volumes and thus provide more pre-trade transparency than auction markets, large orders can be executed at prices other than the current quotes.

The OTC market – primarily involving dealers that execute client trades on an ad-hoc basis – is a residual category encompassing all trading not classified under any of the above categories. No public quotes are available but traders receive pre-trade information during the bilateral negotiation process.

Finally, Periodic auctions operate very similar to the traditional call auctions described above.[8] The key differences are: (i) Auctions do not take place at pre-specified times but, depending on the exact specification of the venue operator, either whenever there is an order in the order book or every time the order book is crossed. Thus, periodic auctions can trade several times during the day. (ii) The auction phase is much shorter, typically lasting for less than one second. During this phase these venues provide pre-trade transparency in the form of indicative prices and volumes. As a consequence of these characteristics, periodic auctions are considered as lit markets under MiFID even though they are opaque outside the auction phase.

2.2 The Ban

In June 2014, the European Commission (EC) adopted the revision to MiFID (called MiFID II) and the Markets in Financial Instruments Regulation (MiFIR) with a focus on, among other areas, non-equity markets such as derivatives and fixed income markets. The equity market specific provisions primarily aim to correct the unintended consequences of MiFID perceived as such by the EC. Most of these rules came into force on 3 January 2018. One of the most significant rules is the introduction of so-called Double Volume Caps (DVCs) on trading in venues that rely on the reference price waiver and/or the negotiated trade waiver.[9] Specifically,

[8] Budish, Cramton, and Shim (2015) model a similar mechanism of call auctions happening frequently at fixed intervals during the day.

[9] Other important equity market specific provisions of MiFID II include introduction of a harmonised tick size regime, specific obligations on firms and exchanges relying on algorithmic and high-frequency trading, a revamp of the regulatory architecture applicable to systematic internalizers, and changes to trade reporting obligations.

any individual venue cannot use either of these two waivers to execute trades in a specific stock for six months if it was responsible for more than 4% of the stock's trading volume in the previous 12 months. Similarly, no venue can use the two waivers to trade a stock for six months if the total trading volume across all venues using the waivers comprised more than 8% of the stock's trading volume in the previous 12 months. The restrictions only apply to small trades defined as trades below the Large In-Scale threshold. Block trades, i.e. trades equal to or greater than the LIS threshold, are exempt from these restrictions in the sense that they are not considered in the DVC computations and are themselves not subject to the ban. We exclusively focus on the 8% cap because, in response to a ban on only one dark pool, investors could easily route their orders to other dark pools, potentially negating any economic effect of the ban. Focusing on a *complete* ban allows for a cleaner estimation of the effects of a prohibition on dark pools. Furthermore, 97.5% of the bans in our sample are due to a breach of the 8% threshold.

The implementation of the dark trading suspensions is based on historical market share data computed by ESMA which relies on volume reports by all registered trading venues. ESMA decides whether to include a stock in the report based on data completeness criteria defined relative to all venues, the most relevant venue, and all dark pools.[10] While the ban was supposed to be effective from January 2018 (along with other provisions of MiFID II), the implementation was delayed until March 2018 due to data quality and completeness issues. This deferral allows us to separate the implementation of the ban from other provisions of MiFID II / MiFIR.

Table 1 provides a timeline of the ban's implementation. Our analysis is based on reports published by ESMA on 7 March 2018 and 10 April 2018. Based on the reports published in March-2018 (based on trading for twelve months ending January-2018 and February-2018), dark trading in 736 firms was suspended due to the 8% cap starting 12 March 2018. On 10 April 2018 a new report (based on trading for twelve months ending March-2018) suspended dark trading in 797 stocks starting 13 April 2018. In addition, the April-2018 report updated the January-2018 and February-2018 reports due to (i) now sufficiently complete data being reported for stocks that were earlier excluded; and (ii) trading venues submitting corrected data for some stocks. Due to these updates, additional instruments that breached the DVCs were banned and

[10]Further details on the implementation of the ban are available at https://www.esma.europa.eu/double-volume-cap-mechanism.

bans for certain stocks implemented in March-2018 were lifted.

[Insert Table 1 about here]

2.3 MiFID II and Changes in Market Structure

Venue operators, anticipating the implementation of the ban, initiated several changes to existing markets and also introduced new quasi-dark trading mechanisms. Carlens and Higgins (2017) summarize several such initiatives undertaken by industry participants. First, since MiFID II, we have seen a rise in periodic auctions as alternative quasi-dark market mechanism. Second, several dark pool operators applied for an exemption from pre-trade transparency obligations under the LIS waiver in addition to the reference price waiver. Furthermore, new block trading venues such as Turquoise Plato Block Discovery emerged. These two industry initiatives allowed market participants willing to execute large blocks to trade in the dark even after the ban kicked in.

In addition to the (quasi-)dark mechanisms already in place, these changes in market structure offer various close alternatives to trading in reference price waiver dark pools.

3 Hypotheses

The ban's impact on market outcomes likely depends on the changes in investors' equilibrium strategies around the imposition of the ban. Existing theories of investors' order routing decisions in fragmented markets provide insights on this question. These studies typically model a market consisting of a dark pool on the one hand and a dealer, specialist or limit order market on the other. Our analysis is complicated by the fact that quasi-dark trading mechanisms coexist alongside lit and dark venues. We thus extend the models' implications by comparing the different market structures along three dimensions – pre-trade transparency, immediacy, and access restrictions – and their specific regulatory setting to generate hypotheses about changes in trading activity and market quality. Pre-trade transparency captures the level of information available to market participants before they submit their orders; immediacy refers to investors' ability to trade a given quantity in short time with a high degree of certainty; and access restrictions refer to venues' ability to prohibit certain investors from accessing their liquidity. Access restrictions depend on whether a particular trading mechanism is organized as a bilateral or multilateral system; the former (latter) are (are not) allowed to impose restrictions.[11]

Models of dark pools incorporating asymmetric information such as Hendershott and Mendelson (2000), Zhu (2014), Ye (2011), and Buti, Rindi, and Werner (2017) derive equilibrium order submission strategies for informed and uninformed investors and conclude that dark pools are likely used by patient traders who are willing to forgo immediacy and/or wish to minimize information leakage while potentially obtaining cost savings associated with a mid-quote execution. Investors who are impatient and/or possess short-term information likely avoid dark pools due to their high execution risk. Ye (2011) shows that informed traders prefer to hide in dark pools when the share of liquidity traders trading in dark pools is given exogenously. Zhu (2014) however argues that, when both informed and liquidity traders trade strategically, the former have a lower execution probability in dark pools as they cluster on the same side of the order book. In Hendershott and Mendelson (2000), informed traders possessing long-lived information trade opportunistically in dark pools whereas those possessing short-lived information trade directly in the dealer market. Empirical evidence also suggests that execution probability (or fill rates) in dark pools are low such that the opportunity costs associated with non-execution can sometimes offset the price improvements offered by them (Næs and Ødegaard, 2006). Buti, Rindi, and Werner (2017) examine the trading strategies of heterogeneous agents possessing differential private values in a market consisting of a dark pool and a limit order book. In their model, the liquidity in the limit order book, tick size constraints, and agents' private valuations determine both limit and market orders migration to the dark pool. Menkveld, Yueshen, and Zhu (2017) rank traders' preference as a pecking order from low-cost-low-immediacy venues to high-cost-high-immediacy venues and show that mid-point dark pools (lit markets) rank at the top (bottom) of this pecking order and dark limit order books rank in the middle.

We hypothesize that traders who cannot access dark pools using the reference price waiver due to the ban will switch to close substitutes. Dark venues that rely on the LIS waiver are probably the closest substitutes. Neither provide any pre-trade transparency or impose any access restrictions but the latter potentially provide lower immediacy due to their large trade

[11] We distinguish access restrictions from market segmentation as in Harris (1993), where investors self-select into their preferred venues based on their trading motives and the markets' design features.

size requirement. In the presence of restrictions on small dark trades, LIS dark venues allow (institutional) investors to continue trading in the dark in case they are willing to trade large blocks.

Hypothesis 1: *Trading volume and market share of dark venues operating under the LIS waiver increase for suspended stocks.*

Conversely, investors preferring smaller trade executions or those unwilling to trade in LIS dark pools have to rely on liquidity in lit and quasi-dark trading mechanisms. OTC and SI dealers are two sources of quasi-dark liquidity for investors wanting to stay away from public markets with full pre-trade transparency. These bilateral mechanisms can restrict access to uninformed and/or retail orders (Seppi, 1990; Madhavan and Cheng, 1997; Hatheway, Kwan, and Zheng, 2017) by relying on their counterparties' identity. Other trader types, like, e.g., HFTs, might not be able to access these markets. Periodic auction markets are presumably very attractive to patient traders – especially those who find it difficult to access liquidity in the OTC or SI markets – as they potentially offer similar cost savings as dark pools with limited, albeit higher, pre-trade transparency. In other words, periodic auctions likely act as a close substitute to dark pools (Carlens and Higgins, 2017). Finally, traditional, scheduled auctions also provide a valuable source of liquidity if investors are prepared to trade at fixed points during the day. These arguments lead to the following hypothesis:

Hypothesis 2: *Trading volume and market share of quasi-dark venues (OTC, SI, periodic auctions, and traditional auctions) increase for banned stocks.*

Considering the types of investors attracted to dark pools in the first place, we expect little shift in natural liquidity towards lit venues. Such investors likely rely on lit markets only if they cannot fulfill their liquidity needs from other less transparent venues. However, the ban effectively imposes a higher level of transparency on the market as a whole, potentially allowing HFTs and other short term traders to more frequently and more precisely identify the presence of natural liquidity. These short-term traders likely exploit this information (back-running) by increasing their participation in lit markets.[12] To the extent this does not reduce participation by natural liquidity traders, we obtain the following hypothesis:

[12] van Kervel and Menkveld (2018) find that HFTs initially provide liquidity to large institutional traders by "leaning against the wind" but later employ back running strategies and trade "with the wind".

Hypothesis 3: *Trading volume and market share of continuous lit markets increase for banned stocks.*

Article 5(1) of MiFIR argues that the ban is implemented *"to ensure that the use of the [reference price and negotiated trade waivers] does not unduly harm price formation."* However, the ban's impact on market quality is likely driven by the above-mentioned shifts across different market mechanisms. Furthermore, knowledge about the presence, direction, and magnitude of natural liquidity can lead to increased adverse selection costs and order flow toxicity for liquidity suppliers due to fast traders employing back-running strategies in the lit markets. At the same time, increased transparency due to reduced uncertainty about the fundamental value likely leads to improvements in liquidity and price efficiency as it allows liquidity providers to quote more aggressively in the lit markets.[13] Hence, we formulate the following competing hypotheses for liquidity:

Hypothesis 4A: *Liquidity improves (bid-ask spreads decrease and/or order book depth increases) for banned stocks.*

Hypothesis 4B: *Liquidity deteriorates (bid-ask spreads increase and/or order book depth decreases) for banned stocks.*

The impact on price efficiency depends on how informed traders and back-running HFTs react to the increased transparency resulting from the ban. Chowdhry and Nanda (1991) and Madhavan (1995) argue that profit-maximizing informed traders trade slowly in a (more) transparent market to disguise their trades. Yang and Zhu (2017) argue that, for this reason, back-running initially harms price discovery (due to reduced trading intensity of the informed trader) but subsequently enhances it (as the back-runner trades alongside the informed trader).[14] This leads to the following competing hypotheses for price discovery:

Hypothesis 5A: *Price discovery improves for banned stocks.*

Hypothesis 5B: *Price discovery deteriorates for banned stocks.*

[13] In the HFT literature, the impact of HFT in the presence of asymmetric information depends on the HFTs' trading strategies (Budish, Cramton, and Shim, 2015), speed differentials across different HFTs (Ait-Sahalia and Saglam, 2013; Biais, Foucault, and Moinas, 2015; Rosu, 2018), and the proportion of liquidity-driven and information-driven trading (Menkveld and Zoican, 2017).

[14] The key variable in their model is precision of the back-runner's signal. If it is sufficiently high, the informed traders, in addition to trading slowly, also randomize their trades.

Finally, as asset prices are positively correlated with liquidity (Acharya and Pedersen, 2005; Amihud and Mendelson, 1986), we expect stock returns to react around the publication of DVCs by ESMA if the market correctly predicts the ban's effect on liquidity. Hence, we formulate the following competing hypotheses:

Hypothesis 6A: *Banned stocks experience positive cumulative abnormal returns on the DVC announcement date in anticipation of a positive effect on liquidity.*

Hypothesis 6B: *Banned stocks experience negative cumulative abnormal returns on the DVC announcement date in anticipation of a negative effect on liquidity.*

4 Data, Variables, and Descriptives

4.1 Stock Selection

We obtain our sample of stocks from the January-2018 and February-2018 ESMA reports published on 7 March 2018 as well as from the updates to these reports published on 10 April 2018. For each stock, these reports contain information about the Relevant Competent Authority (RCA), total EU-wide volume, market shares of trading under the reference price and negotiated trade waivers, the sum of which we refer to as waiver percentage, and the start and end dates of suspensions. These reports altogether include 20,920 stocks. Table 2 describes the different filters applied to obtain the final list of stocks.

[Insert Table 2 about here]

First, we eliminate securities partially banned based on the 4% cap. As mentioned earlier, the vast majority of suspensions implemented are due to total dark trading exceeding the 8% threshold. Next, we remove exchange-traded funds, share warrants, and other share-like securities from our sample. We further exclude stocks that are not classified as "Liquid" for the purpose of MiFID II. This status governs the application of pre-trade and post-trade transparency rules.[15] Next, we eliminate firms that have a 2017 EU-wide trading volume of less than €5 million. Subsequently, we restrict our sample to stocks from countries that make up at least 2% of the filtered stock list (based on the stocks' RCA). The countries fulfilling this criterion

[15]A stock is classified as having a liquid market if it has (i) a free float of at least €100 million; (ii) at least 250 average daily number of transactions; (iii) and an average daily turnover of €1 million.

are: Belgium, Germany, Denmark, Finland, France, Italy, Netherlands, Norway, Poland, Spain, Sweden, and the United Kingdom. Furthermore, we eliminate stocks that experience corporate actions, stock splits, or delistings during our sample period. Next, we drop stocks with a primary listing outside the European Economic Area (EEA) and less than 50% market share in the EEA. We exclude Swiss stocks because the SIX Swiss Exchange does not fall under the regulatory scope of MiFID II.[16] Finally, if multiple share classes of a company are part of our sample, we keep only the most liquid one. This leaves us with a final sample of 1,149 firms. While these steps reduce our sample from 20,920 to 1,149 firms, it still includes 614 of the 736 suspended firms.[17]

We differentiate between three different suspension states: "suspended", "not suspended" and "not in report". 67 firms change their status based on the April-2018 publication. We exclude these firms from our main analysis and focus on the March-2018 publication using the suspension date (12 March 2018) in this report as the event date. Our filtered sample (henceforth referred to as the "main sample") thus includes 1,082 stocks of which 614 were suspended.

Of the 67 stocks that changed their status, one stock switched its status from suspended to not suspended, 21 from not suspended to suspended, 18 from not in report to suspended, and the balance 17 from not in report to not suspended. The change in status to suspended can be due to: (i) the March-2018 report triggering a stock's ban; (ii) or an update to the January-2018 and February-2018 reports triggering the suspension. The latter group includes stocks that would have been suspended already in March if ESMA had obtained the correct/complete volumes in time. We use this group of 24 stocks (henceforth referred to as the "placebo sample") in a separate analysis.

4.2 Variable Description

Our sample period extends from 3 January 2018 – the implementation date of MiFID II – to 11 May 2018. For every stock in the main and placebo samples, we obtain intraday trades and best quotes data from Thomson Reuters Tick History (TRTH) for the stock's most relevant market. We focus on the continuous trading session to measure effects of dark trading restrictions on

[16]Previous steps do not eliminate all Swiss firms as sometimes an EEA national regulator is their RCA.
[17]These 1,149 firms make up 85% of the EU trading volume of all firms in the ESMA reports.

market quality. Hence, we exclude the period before (after) the end (start) of the opening (closing) auction. We also exclude all trades reported to these markets but not executed on their limit order books. Using this data, we generate estimates of trading activity, liquidity and price efficiency for every stock-day.

For stock i and day t, we capture trading activity by computing the trading volume in euros and the number of trades. We capture quoted liquidity by computing the bid-ask spread and top-of-book depth. Specifically, denoting the time of a quote update on day t as t', the best bid and ask quotes as $Bid_{t'}$ and $Ask_{t'}$, the associated quantities as $BidQ_{t'}$ and $AskQ_{t'}$, and the mid-quote at time t' as $M_{t'}$, we compute:

$$QuotedSpread_{t'} = \frac{Ask_{t'} - Bid_{t'}}{M_{t'}}$$

$$QuotedDepth^{Bid}_{t'} = Bid_{t'} \cdot BidQ_{t'}$$

$$QuotedDepth^{Ask}_{t'} = Ask_{t'} \cdot AskQ_{t'}$$

We compute time-weighted averages of these measures for each trading day t.

Next, we compute three measures of trading liquidity: effective spread, realized spread, and price impact. The effective spread captures the actual transaction costs paid by the trader submitting a market order, the realized spread the compensation earned by the limit order trader after adjusting for any losses associated with adverse selection, and the price impact the information content of a transaction. For a trade at time t' we compute:

$$EffectiveSpread_{t'} = \frac{2 \cdot D_{t'} \cdot (P_{t'} - M_{t'})}{M_{t'}}$$

$$RealizedSpread_{t'} = \frac{2 \cdot D_{t'} \cdot (P_{t'} - M_{t'+\Delta})}{M_{t'}}$$

$$PriceImpact_{t'} = \frac{2 \cdot D_{t'} \cdot (M_{t'+\Delta} - M_{t'})}{M_{t'}}$$

where $P_{t'}$ is the transaction price, $D_{t'}$ is the direction of a trade ($+1$ for a buy and -1 for a sell), and Δ is the time taken for the information associated with a trade to be fully impounded into the mid-quote. We obtain three versions of realized spread and price impact associated with $\Delta \in \{10, 30, 60\}$ seconds. All three measures are weighted by the trade size in euros.

To understand the impact of the ban on price efficiency, we construct measures based on autocorrelation and variance ratios at different time intervals. These measures capture deviations of returns from a random walk and thus act as a proxy for short term price efficiency (Boehmer and Kelley, 2009). First, denote $r_{t'}^{\Delta} = ln(M_{t'}) - ln(M_{t'-\Delta})$ as the Δ-second log returns based on the mid-quote. For each stock i and day t, we compute the first order autocorrelation of return measured at interval Δ, denoted by $AutoCorr(r_{t'}^{\Delta})$ and the ratio of return variance calculated over an interval of length m, $r_{t'}^{m}$, to return variance over an interval of length n, $r_{t'}^{n}$, both scaled by the respective time periods, denoted by $VR(n,m)$. We measure daily price efficiency as $|AutoCorr(r_{t'}^{\Delta})|$ and $|1 - VR(n,m)|$. We use absolute values as we are interested in deviations from a random walk in either direction. We compute both measures for different time windows to ensure that our results are robust. Specifically, we use $\Delta \in \{10, 30, 60, 300\}$ and $(n,m) \in \{(30,10), (60,10), (300,10), (60,30), (300,30), (300,60)\}$.

Finally, we compute intraday volatility as the standard deviation of one-minute returns per day, $r_{t'}^{60}$ and the relative tick size for each quote update at time t' as

$$RelativeTick_{t'} = \frac{Tick_{t'}}{M_{t'}}$$

where $Tick_{t'}$ is the tick size (defined by the MIFID II regulation and based on historical price and the average number of trades per day). We also define a variable that takes a value 1 if the bid-ask spread is one tick, and 0 otherwise. The last two measures are averaged through each trading day by time weighting every quote update.

In order to capture shifts in trading activity to different lit, quasi-dark and dark mechanisms, we supplement the above information with weekly snapshots of trading volume from Fidessa in all lit, OTC, SI, traditional and periodic auction markets as well as dark pools. Additionally, we also collect information on block trades in dark pools from Fidessa and from TRTH.[18,19] We combine the block trade data and the dark market data to disentangle dark pool trades under the reference price waiver from those under the LIS waiver.[20]

We use daily exchange rates from Thomson Reuters Eikon to convert all relevant variables

[18]The block trade definition is applied based on realized trading activity. In other words, multiple child orders each smaller than the LIS threshold belonging to a single parent order are not reported as block trades even if the parent order is larger than the LIS threshold.

[19]These data are available at http://fragmentation.fidessa.com/fragulator/ and http://fragmentation.fidessa.com/blocks/.

[20]See Appendix A for the steps taken to obtain the two Fidessa datasets and to match them with each other.

into Euros. We also collect information from Thomson Reuters Eikon on daily prices and returns (used in Section 7 for the event study), share classes and industries (used in the filtering process), and constituents of the main European indices as a potential predictor of investors' trading behavior. Table 3 provides an overview of the different variables used in this paper.

[Insert Table 3 about here]

4.3 Descriptive Statistics

Table 4 provides descriptive statistics for the 1,082 (24) firms in the main (placebo) sample. These are calculated for our entire sample period and thus include both the pre and post event period. We report the (equal-weighted) mean, median and the 5th/95th percentiles for the main sample and the mean and median for the placebo sample. Our sample is broadly representative of the European equity landscape even though our filtering procedure eliminates the most illiquid securities.

[Insert Table 4 about here]

The average (median) stock in the main sample has a firm size of €8.0 (€2.6) billion. There is substantial cross-sectional dispersion, as evidenced by the 5th and 95th percentiles. The stocks in the placebo sample seem to be similar to those in the main sample in terms of their size. The wide size distribution leads to a skewed distribution of trading activity as evidenced by the large difference between mean and median daily trading volume (€61.39 million versus €14.07 million). The median firm trades about 28% of its entire volume on the primary listing market. 33% and 58% of the firms in the main and placebo sample, respectively, are constituents of one of the main European indices. Waiver percentages range between 1.2% and 14.9% for the 5th and 95th percentile, indicating a huge cross-sectional dispersion in pre-event dark pool trading across stocks. The 8% cut-off seems to naturally divide the sample into two groups of firms with nearly equal size (see Suspension Dummy).[21] About 23% of the median firm's waiver percentage originates from trades utilizing the negotiated trade waiver.[22]

[21]The placebo sample has a 100% suspension rate by construction as we only select firms suspended on April 13, 2018.

[22]Negotiated trade waiver and reference price waiver percentages do not fully add up to the total waiver percentage due to rounding in the ESMA reports.

The main sample also exhibits substantial dispersion in its market quality characteristics. Mean (median) quoted spreads and top-of-book depth are 16.73bps (12.82bps) and €21,630 (€14,680). Again, firms in the placebo sample do not seem very different. Unsurprisingly, effective spreads are smaller than quoted spreads as traders' time their market orders when liquidity is high. Median realized spreads are close to zero suggesting a high level of competition among liquidity providers. Median price impact is approximately 10bps. Mean intraday volatility for an average stock-day is 7bps. The mean (median) relative tick size is 7.05bps (5.57bps). The minimum tick size is binding in about 27% of the quote updates, but percentiles indicate that this measure also exhibits wide dispersion. We also observe autocorrelation in prices and variance ratios substantially different from zero, indicating some degree of short-term inefficiency in the market. These measures, again, vary widely across stocks.

[Insert Table 5 about here]

In Table 5, we report the market shares of the different trading mechanisms for the main and placebo sample separately. For the main sample we report the mean market share for suspended/non-suspended stocks and the pre-ban/post-ban period separately. To begin with, we observe that a substantial amount of trading is conducted away from the lit markets in venues that offer lower transparency.[23] The pre-ban market share traded under the reference price waiver of 5.25% (2.80%) for suspended (non-suspended) stocks is lower than previous 12-month-average of 6.02% (see Table 4) based on which the suspension status was determined. This indicates that order routing already shifted away from dark pools. This is most likely because traders change the equilibrium routing behavior in anticipation of the ban. We also observe the mechanical elimination of trading (from 5.25% to zero) under the reference price waiver for the suspended stocks. Interestingly, continuous lit market share decreases after the ban for suspended stocks though non-suspended firms experience a much larger decrease. Suspended stocks also experience a larger increase in the share of traditional auctions and quasi-dark markets like periodic auction, SI and OTC venues as compared to non-suspended firms.[24] Most notably, periodic auction market share increases by 3.7 (1.6) times for suspended

[23] In untabulated results, we also observe that: (i) the average size of a transaction in these (quasi-)dark markets is generally higher; (ii) and there is substantial dispersion in the market shares of all trading mechanisms across stocks.

[24] OTC (SI) market share has decreased (increased) since the implementation of MiFID II. This is because the

(non-suspended) firms.

These unconditional shifts provide a first indication of the limited success of the ban in moving trading towards continuous lit markets. They also highlight the need to control for market-wide changes in trading activity that are unrelated to the ban. Finally, suspended firms, compared to non-suspended ones, do not just have a larger share of trading under the reference price waiver but also have a higher overall (quasi-)dark market share (54% versus 42%) suggesting that their composition is likely different to begin with. The last two points motivate the choice of empirical techniques we employ (see Section 5) to estimate the ban's effect on market outcomes.

Panel B of Table 5 shows the mean market shares of the placebo stocks for three time periods: (i) the phase before the ban kicks in based on the January-/February-2018 reports; (ii) the period in between the January/February-2018 and March-2018 reports; and (iii) the period after the March-2018 report becomes operational. Stocks in the placebo sample, by construction, are not suspended in the first two phases and are suspended in the third phase. Moreover, in the second phase, the not suspended status is due to incorrect/incomplete data in the January-/February-2018 reports. Pre ban market shares of the placebo sample are quite similar to those of the main sample. While we observe a strong reaction of market shares for the suspended stocks after the ban kicks in (see Panel A), placebo stocks react much less intensely albeit in the same direction. However, when the ban (correctly) becomes effective in April, trading under the reference price waiver is halted and, similar to the main sample, volume shifts towards quasi-dark venues such as periodic auctions, SI and OTC markets.

5 Empirical Approach

Our objective is to understand the causal effect of the ban on volumes and market shares of the different trading mechanisms as well as the liquidity and price efficiency on the primary market. For this purpose, we employ two quasi-experimental techniques: The Abadie (2005) semi-parametric difference-in-differences estimator (SP-DID), and a robust Regression Discontinuity Design (RDD) estimator as specified by Calonico, Cattaneo, and Titiunik (2014).

trading venues' discretion to classify themselves as SIs under MiFID no longer exists. As a result, venues that were earlier reporting their trades in the OTC category now report them as SI.

One concern that affects these techniques is the potential for market participants and/or trading venues to strategically increase (decrease) dark pool activity over the measurement period for selected stocks in order to game the ban's outcome. This concern is more likely to affect stocks near the 8% threshold. We plot the number of stocks in different buckets of historical dark pool market share near the 8% threshold in Figure 1 and do not observe any obvious discontinuity. This is unsurprising considering such strategic behavior is likely very expensive as traders need to ensure that a stock remains above or below the 8% threshold on a rolling twelve monthly basis. While individual dark pools may be able to constrain activity on their platform to ensure uninterrupted trading, it is unlikely that all venues collude to achieve such an outcome.[25]

[Insert Figure 1 about here]

5.1 Semi-parametric Difference-in-Differences (SP-DID)

While there is no evidence that the assignment of stocks to the treatment or control is manipulated, observable variables likely predict the probability of a stock being banned. This is already evident in high (low) overall dark pool market share for suspended (not suspended) firms. We provide evidence of this in Subsection 6.1. We therefore use an enhanced version of the standard difference-in-differences (DID) estimator suggested by Abadie (2005). It adjusts for potentially non-parallel trends by re-weighting the differences of post- and pre-event averages of stocks in the control group based on their propensity score, i.e., their probability of being treated, to generate a more credible estimate of the Average Treatment Effect on the Treated (ATT).Specifically, in a first stage regression, we predict the probability of treatment using the following logit regression:

$$P_{ban}(X) = \Lambda\Big(\gamma_0 + \sum_{i=1}^{k} \gamma_i x_i\Big) \tag{1}$$

where Λ is the logit operator and X is a vector of observable covariates used to predict the treatment. X contains averages of the following variables measured in the pre-event period for each stock i: relative tick size, tick constraint, the natural logarithm of the market value, the percentage of trading under the reference price waiver, dummy variables indicating membership

[25]The stocks affected by the 4% cap applicable to individual dark pools – which we do not investigate – are more likely to be affected by such gaming concerns.

of the countries' main stock indices, and dummy variables for the RCAs. We define the pre-event period as beginning on 15 January and ending on 9 March. We exclude the first two weeks of 2018 to allow market participants to adjust their behavior to other MiFID II rules that came into effect on January 3. The post-event period begins on 12 March 2018 and ends on 11 May 2018.[26] For each stock and period, we compute the simple average of all variables of interest.

In the second stage, we employ a modified DID estimator, where we estimate the difference in outcomes between treated and untreated groups controlling for treatment probability. This works by weighting-down (weighting-up) control firms with over-represented (under-represented) values of the covariates. In other words, we assign a high (low) weight to control firms with high (low) propensity scores. This approach can lead to noisy, imprecise estimates if the estimated propensity scores are very close to zero or one. Hence, we restrict the estimation sample to stocks with estimated propensity scores between 5 and 95 percent.

The estimation approach we employ additionally allows for the estimation of heterogeneous treatment effects, i.e., whether the ban's impact differs conditional on the observed covariates. To do so, we center our explanatory variables around zero such that our estimation output still allows us to see the unconditional effect. We include all previously used covariates other than the dummy variables in these regressions.

5.2 Regression Discontinuity Design (RDD)

As another empirical approach, we employ the RDD technique and exploit the discontinuity in the application of the treatment vis-à-vis the running variable – which in our case is the historical waiver percentage – to estimate a Local Average Treatment Effect (LATE). Stocks with a historical waiver percentage just below 8% are eligible for trading under the reference price and negotiated trade waivers, whereas those just above 8% are not.[27] We thus focus our analysis on the sample of stocks whose historical waiver percentage lies close to the 8% threshold. Following Calonico, Cattaneo, Farrell, and Titiunik (2018), we include covariates to improve the precision of our estimates and exploit the panel structure of our data. Specifically,

[26] In untabulated results, we leave a two week gap around the ban's implementation date, to allow for an adaptation period. Thus, we end the pre-event window on 2 March and begin the post-event period on 19 March. Our results are unaffected by this choice.

[27] See Lee and Lemieux (2010) and Imbens and Lemieux (2008), for a general discussion of RDD in economics.

we estimate the ban's effect using the following linear regression specification:

$$Y_{it} = \alpha + \beta Suspension_{it} + \gamma \bar{y}_i^{pre} + \delta WaiverPercentage_i + \varepsilon_{it} \tag{2}$$

where Y_{it} are the different variables of interest, $Suspension_{it}$ is a dummy variable equal to one if stock i has historical waiver percentage greater than 8% and zero otherwise, and β is the main coefficient of interest measuring the causal impact of the ban on Y_{it}. $\bar{y}_i^{pre}$ is the mean value of the dependent variable in the pre-event window. ε_{it} is the residual term and we cluster standard errors by stock. We also estimate an RDD without $\bar{y}_i^{pre}$ and with the dependent variable being the stock-level difference in pre- and post-event mean.

A key identifying assumption underlying a RDD is that stocks near the 8% threshold are almost randomly assigned their suspension status due to exogenous variation in the waiver percentage. In other words, stocks around the cut-off should be similar with respect to other observable characteristics, market participants and venues should not be able to strategically game the running variable, and, except for discontinuity in the treatment, any variation in other relevant variables should be continuous and smooth. To test this assumption, we first estimate the above equation with historical market capitalization, relative tick size and tick constraints as the dependent variables and find that the β coefficient is insignificant in these cases, suggesting that stocks near the 8% threshold do not to differ in terms of these variables. Additionally, as discussed above, Figure 1 alleviates any concerns related to gaming on the part of investors and/or market operators.

One issue in any RDD analysis is the choice of an optimal bandwidth around the threshold used to obtain the estimation sample. A narrower bandwidth allows for more accurately measuring the treatment effect whereas a wider bandwidth improves the statistical power of our estimations due to the inclusion of a larger number of stocks. We employ the bias-corrected Calonico, Cattaneo, and Titiunik (2014) bandwidth with a triangular kernel.[28]

[28] Fan and Gijbels (1996) show the triangular kernel to be optimal for our purpose of estimating local linear regressions at the boundary. As a robustness check, we also employ a rectangular kernel and find that our results remain qualitatively unchanged.

6 Results: Implementation Effects

In this section, we discuss the ban's impact on market outcomes based on the SP-DID and RDD estimations. It is worthwhile to highlight that the SP-DID and RDD estimators are not directly comparable as the former (latter) provides an estimate of the ATT (LATE). Put differently, the RDD describes the ban's impact for the average firm in the local sample near the threshold, whereas the SP-DID estimates the ban's impact for the average firm in the entire treatment group.

6.1 Which Stocks Are Banned?

Before examining the effects of the ban, we consider what predicts whether a stock will be banned. Table 6 provides an overview of the characteristics of suspended and non-suspended stocks. Panel A shows, besides the obvious fact that suspended stocks trade relatively more under the negotiated trade and reference price waivers, they are of a smaller size and trade lower volumes than non-suspended stocks. Panel B shows that suspended stocks have smaller quoted and effective spreads as well as smaller price impacts and realized spreads, though their depth is also smaller. Suspended stocks trade at a smaller relative tick size, too, such that the average tick constraint is similar across the two samples. Differences in the price efficiency measures, displayed in Panel C, are negligible.

[Insert Table 6 about here]

Besides these differences in stock characteristics, we report the distribution of banned firms by country and membership of the main national stock market indices in Figure 2 and Figure 3. Stocks contained in different indices and from different countries strongly differ in their probability of being suspended. For example, constituents of the German, Spanish, and Polish main stock market indices are less likely to be banned, whereas constituents of the main index of the UK and Scandinavian countries have a higher ban probability. Similarly, a small (large) fraction of firms from Germany (UK) are banned.

[Insert Figure 2 about here]

[Insert Figure 3 about here]

The above differences motivate our choice of covariates when estimating the propensity scores using Equation 1. The estimated propensity scores are used to determine the weights of individual observations in estimating the second stage difference-in-difference Abadie (2005) estimator and remain constant for the different dependent variables. Table 7 contains the results of this first stage regression.

[Insert Table 7 about here]

The fraction of trading conducted under the reference price waiver is the strongest predictor of a stock's suspension status. A one standard deviation increase (about 3%) in the fraction of trading under the reference price waiver predicts a 19.5 percentage points increase in the ban probability. This is to be expected because it has a strong positive correlation with the historical total waiver percentage contained in the ESMA reports. However, this link is not completely mechanical because the explanatory variable refers to trading activity spanning only the pre-event period between 15 Jan 2018 and 11 Mar 2018, and thus covering only a small part of the period used to determine the waiver percentage. The insignificant coefficients for relative tick size and tick constraint may seem surprising considering the existing theoretical and empirical evidence to the contrary Buti, Rindi, and Werner (2017); Gomber, Sagade, Theissen, Weber, and Westheide (2016). We obtain this result because the measurement period for the ban mostly falls in 2017 and, starting 2018, EU-wide tick size regimes were changed due to the implementation of MiFID II. A one standard deviation change in log firm size is associated with a 5.3 percentage point increase in treatment probability. Finally, we find that index membership and country dummies have strong effects on the propensity to cross the 8% threshold. In particular, indices such as the CAC40, DAX30, and IBEX35 have a negative effect on the propensity score. The country dummies are expressed relative to the base category, which is Italy. In comparison, German stocks are less likely and British stocks are more likely to experience the ban.[29]

6.2 Shifts in Trading Activity: Winners and Losers

The application of the ban mechanically eliminates small dark pool trades for the affected stocks. We start by examining whether the ban was successful in its primary objective of shifting order

[29] Our results remain qualitatively unchanged when we additionally include liquidity and trading volume in the first (and second) stage regression. This is in line with our expectations as these variables are correlated with firm size and relative tick size.

flow from the dark to lit markets. In Section 3, we develop hypotheses for three distinct sets of trading venues: Dark LIS, quasi-dark, and continuous lit markets. We test these hypotheses by estimating the SP-DID and RDD with $Y_{i,t}$ being the market share of these trading mechanisms. Furthermore, our SP-DID regressions enable us to test the cross-sectional effects of the ban. Table 8 and Table 9 contain the results from these estimations. In untabulated results, we show that the ban had no significant influence on the total turnover, implying that an increase in the market share of a trading mechanism is equivalent to an increase in its volume.

[Insert Table 8 about here]

[Insert Table 9 about here]

The results are consistent with Hypothesis 1. We observe an increase in the market share of dark LIS trades which is statistically significant independent of the estimation approach. The results range between 0.65 and 1.37 percentage points. This also shows that the increase in block trades is smaller than the magnitude of the eliminated trades based on the reference price and negotiated trade waivers, which suggests small and large dark pool trades are no perfect substitutes.

We also obtain evidence in support of Hypothesis 2 for two quasi-dark mechanisms. The coefficients for periodic auctions and SI based on the SP-DID estimations are positive and significant. The coefficients for these two mechanisms based on the RDD are also positive but only significant for periodic auctions. There are no significant changes in the market share of traditional call auctions[30] and the OTC market. The magnitude of the impact on periodic auction market share ranges between 0.9 and 1.3 percentage points. The results for both dark LIS and periodic auctions are economically large compared to their pre-event averages.

Finally, we find mixed evidence for Hypothesis 3. Both approaches show an increase in lit continuous market trading of similar magnitude, though the SP-DID (RDD) point estimate is significant (insignificant). The estimates range from 0.9 to 1.2 percentage points, which is of a similar magnitude compared to the shifts towards dark LIS and periodic auctions, but substantially smaller relative to the pre-event average.

[30]This is true for the aggregate of traditional call auctions, but also confirmed for opening, intraday, closing and volatility auctions separately in untabulated results.

In summary, while we do observe some migration of trading towards lit markets, almost thrice as much volume migrated to dark LIS, periodic auctions and SI. Thus, we conclude that the regulation was only partially successful in shifting order flow towards lit markets. At the same time, quasi-dark markets secure the highest relative gains in market shares. These findings are not affected by cross-sectional differences in firm characteristics.

6.3 Market Quality Implications

In this subsection, we investigate effects of the ban on measures of lit market quality. This includes liquidity and short-term price efficiency of the primary listing venue for each stock. As noted in the previous section, the ban induces an increase in the market share of lit venues. The effects of this increase on market quality will depend on the kind of order flow that moved to lit markets. Furthermore, the ban also increases the level of market-wide transparency allowing limit order traders to incorporate information from quasi-dark markets such as periodic auctions in their quoting behavior on lit markets. This will further have implications for the lit market quality. In Tables 10 and 11, we report the liquidity results and in Tables 12 and 13, we report the price efficiency results.

[Insert Table 10 about here]

[Insert Table 11 about here]

[Insert Table 12 about here]

[Insert Table 13 about here]

Overall, there is no unambiguous evidence for any effect of the ban on liquidity as all unconditional results are statistically insignificant, allowing no firm conclusion with respect to Hypothesis 4A or Hypothesis 4B. Our findings, at least with respect to the SP-DID estimates, do not result from a high degree of noise or a lack of power in our data: our 95% confidence intervals for quoted spread, effective spread, realized spread, and price impact range from less than 0.7 basis points on either side of zero, and the confidence interval for depth excludes changes of more than approximately 0.5%. These insignificant results can be attributed to the relatively low increase in lit market shares and the unclear implications of the ban for the equilibrium order flow composition in lit markets.

Almost all price efficiency point estimates are positive suggesting an increase in deviations from a random walk or reduction in price efficiency. However, only few of these estimates are statistically significant. Altogether, the results thus provide some support for Hypothesis 5B. We do not find evidence of systematic cross-sectional differences based on the stock characteristics under consideration as all coefficients aiming to capture such effects are statistically insignificant for liquidity and price efficiency.

6.4 Placebo Tests

We repeat the SP-DID analyses for the placebo sample of 24 stocks that should have been banned in March-2018 but were not because of insufficient or incorrect data provided to ESMA. These stocks were eventually banned in April-2018. If the results observed in the previous subsections are attributable to the ban, we should observe a delayed reaction consistent with the previous analyses for these stocks. In other words, we should observe no significant change in market outcomes in March-2018, and a reaction consistent with our main analyses in April-2018. We estimate two sets of regressions. In the first one, we set the event date to 12 March 2018, the pre-event period to that used in the main analysis, the post-event period to one month between the reports published in March-2018 and April-2018, and the control group to firms not suspended on the event date. In the second regression, we set the event date to 13 April 2018, the pre-event window to the one month between the publication of the two reports, the post-event window to the period between the event date and the end of our sample period, and the control group to firms suspended on 12 March 2018. We focus our analysis on the unconditional effect and do not attempt to explain cross-sectional differences because of our limited sample size. Tables 14 and 15 show the SP-DiD second stage results for market shares, liquidity, and price efficiency for the March-2018 and April-2018 report respectively. For brevity, we exclude the first stage results.

[Insert Table 14 about here]

[Insert Table 15 about here]

Panel A of Table 14 shows that changes in market shares are different from those observed for the main sample. The magnitude of the change in periodic auction volume and LIS dark pool volume is much smaller. Furthermore, and in contrast to the main sample, there is a reduction in the share of lit continuous order books and periodic auctions. However, all these changes are statistically insignificant. The effect on liquidity (Panel B) is also insignificant with the exception of a weakly significant reduction in price impacts. The price efficiency results (Panel C), again contrary to the main sample results, show no clear pattern, with both positive and negative coefficients, but are insignificant. The results in Table 15 are similar to those in the main analyses: the market share of periodic auctions increases significantly, as does the SI market share. We find no significant effect on liquidity and there is some evidence of a short-term price efficiency worsening, as evidenced by the autocorrelation of midpoint returns which is significant at 10 percent. Altogether, the placebo analysis confirms that the effects observed for the main sample indeed result from the ban itself and not from other confounding changes to the stocks.

7 DVC Ban Announcement Returns

In the previous section we document a positive effect of the ban on the market share of lit venues. Simultaneously, market liquidity remains unchanged and price efficiency slightly deteriorates. Against this background and to test Hypothesis 6A/6B, we explore how market participants evaluate the introduction of the ban by conducting an event study analyzing the returns of affected stocks after the first DVC report's publication on March 7, 2018. We use daily stock returns for all stocks in our main sample from January 2 to March 15. Expected returns are computed using a Carhart (1997) 4-factor-model with country-specific factors from Andrea Frazzini's data library[31,32] We use the period between January 3 and March 2 as our estimation window. As the report was published after trading hours on March 7, we select March 8 as the event date ($t = 0$). The actual ban of suspended stocks started on March 12 ($t = 2$). Abnormal returns on March 8 and March 9 should capture investors' expectations about market liquidity,

[31] Available at http://people.stern.nyu.edu/afrazzin/data_library.htm.

[32] In untabulated results, we also employ the constant mean return model, index adjusted return model, Fama-French 3-factor model using factors from Ken French's homepage and the 4-factor model with European factor loadings. Results remain largely unaffected by these choices.

while abnormal returns starting March 12 will capture investors' learning after having observed the ban's actual impact on market quality.

As we intend to analyze both the announcement effect (before the ban kicks in) and the actual effect (after the ban is implemented), we use $(-1;+6)$ as the event window around March 8, such that it contains one week of trading with the ban in force. Figure 4 shows the Cumulative Abnormal Returns of suspended stocks and non-suspended stocks separately. Returns for suspended stocks are significantly more positive by about 25 basis points on the announcement day.[33] This finding is in line with the conjecture that traders on average anticipated a positive effect of the ban on market quality. The positive announcement effect is fully reversed with the actual implementation of the ban on day 2. The difference between CAR(-1,2) for suspended and non-suspended stocks is insignificantly different from zero. This finding is in line with investors learning that market quality largely remains unchanged after the ban kicks in.

[Insert Figure 4 about here]

To further understand the drivers of announcement and implementation effect, we perform a cross-sectional regression with the following specification:

$$CAR(t_1,t_2)_i = \alpha + \beta Suspension_i + \gamma Dist_i + \delta Suspension_i \cdot Dist_i \ + \varepsilon_i \tag{3}$$

where $CAR(t_1,t_2)_i$ is the cumulative abnormal return for stock i from day t_1 to day t_2, $Suspension_i$ is a dummy variable equal to one for the treatment stocks and zero for the control stocks, $Dist_i$ measures the absolute distance between the actual waiver percentage and 8%, and ε_i is the residual term. As we expect that the news of the ban is more likely to come as a surprise for firms closer to the 8% threshold,[34] we include the interaction term $Suspension_i \cdot Dist_i$.[35] Table 16 shows the results.

[Insert Table 16 about here]

[33] This finding's statistical significance is confirmed by parametric as well as non-parametric test statistics.

[34] For example, the news that most of the FTSE100 stocks would be affected by the ban was probably not a surprise. See *The TRADE* article from 2017 titled "Nine-in-ten FTSE 100 stocks to hit dark cap in January".

[35] In untabulated results, we include further control variables (and their interaction with the suspension dummy) in our regression specification. Specifically, we use the mean of log market capitalization, trading volume, several liquidity and price efficiency measures, the market shares of the different venue types, and tick constraints over the estimation window. Our results remain unchanged.

We confirm the above results by showing that β is positive and economically significant with about 54 and 21 basis points higher daily returns for suspended stocks in the $(-1;+1)$ and $(0;0)$ window, respectively. With the actual implementation of the ban, however, CARs for suspended stocks decrease and are insignificantly different from those of non suspended stocks for the $(-1;6)$ window. Controlling for the absolute distance between the waiver percentage and 8%, these findings are confirmed. The interaction term, while insignificant, supports the argument that stocks closer to the 8% thresholds exhibit a stronger announcement effect.

In summary, we find evidence, consistent with Hypothesis 6A, of a positive announcement effect for suspended stocks.[36] With the actual implementation of the ban this effect is reversed, consistent with investors learning that the positive effect on liquidity did not materialize.

8 Conclusion

We use the MiFID II regulation in the EU to evaluate the causal impact of banning non-block trading in midpoint dark pools on market outcomes. The setting of our quasi-natural experiment is unique in that it is characterized by competition between fully transparent, lit venues and several shades of dark venues that offer either partial or no transparency. Most modern equity markets, especially in the developed world, have evolved into such as structure.

We observe that the ban leads to an increase in trading activity across not just continuous lit markets but also across internalization platforms, periodic auctions and block trading venues. In fact, the shift in trading towards (quasi-)dark markets is almost three times as large as the shift towards continuous markets. Contrary to regulators' and markets' expectations, but in line with the minor increase in the market share of lit venues, we observe no effect on liquidity but a deterioration in short-term price efficiency for firms affected by the restrictions. This suggests that a regulatory intervention in one trading mechanism leads to complex changes in the composition of order flow across multiple alternative trading venues which can potentially destroy welfare.

Our results also highlight the necessity for a better understanding of competition between dark pool, public markets, and quasi-dark markets. From a policy-making perspective, our

[36] In untabulated results, we observe that the Placebo stocks behave similar to non-suspended stocks around March 8, but like suspended stocks around the second ESMA DVC report's publication on 11 April 2018.

results point to a need for caution in designing and implementing such restrictions. Specifically, regulators should carefully consider the impact of market regulation on equilibrium strategies of investors and profit-maximizing venue operators.

A Data Documentation

A.1 Matching Fidessa Fragulator and Thomson Reuters Eikon

Fidessa Fragulator provides weekly Turnover, Volume and Trade Count for five trading mechanisms: Auctions (AUC), Lit Markets (LIT), systematic internalizers (SI), over-the-counter (OTC), and dark venues (DARK). This data must be manually accessed for each firm. Fidessa Fragulator relies on a firm's Reuters Instrument Code (RIC) to provide the trading activity data, whereas the ESMA reports identify firms by their ISIN. While ISINs uniquely identify an instrument, RICs identify instrument-venue combinations.[37] We obtain the above data for each firm in our sample period using the following steps:

- From the ESMA reports, we obtain the firm's ISINs.
- For each ISIN, we obtain the RIC from Thomson Reuters Eikon.
- We use the RIC to download the trading activity data from Fidessa Fragulator. This is done by entering the RIC into the "Stock Selector" window and then selecting "All exchanges/currencies" in the "Listing filter" field, which ensures that all RICs of the same instrument lead to the same results.
- For some firms, RICs are either missing in Eikon or lead to no/incorrect results in Fragulator. In such cases, we identify the correct RIC by entering the firm's ISIN or firm name obtained from Eikon in Fidessa's "Stock Selector" window. Upon entering this information, Fidessa provides a list of relevant RICs for the firm. Before using the RIC we ensure that the different RICs suggested by Fidessa lead to the same output.

Using this stepwise procedure, we are able to establish a one-to-one match between ISINs and RICs for all our sample firms. We confirm the correctness of our matches by comparing prices from Eikon with implied prices from Fragulator.[38] One issue with the Fragulator database is that firms are dropped once they are delisted. To avoid a survivorship bias, we download data from Fragulator right after the end of our sample period on May 11, 2018.

[37] Each RIC contains a prefix and suffix separated by a period. For example, the Commerzbank RIC (CBKG.DE) contains the firm name (CBKG) in the prefix and Deutsche Börse (DE) in the suffix.

[38] Prices in Eikon and Fragulator for the same instrument are not necessarily denominated in the same currency. We convert all numbers into Euros using exchange rates from Eikon.

A.2 Matching Fidessa Fragulator and Fidessa Block Trading Data

Dark market volumes reported in the Fidessa Fragulator include trades under the LIS and reference price waivers. For our analyses, it is crucial to disentangle trading under the two waivers. For this purpose, we use Fidessa's "Top of the Blocks" database, which provides the number and Turnover (in Euros and exchange currency) of all trades above the LIS threshold from the following venues: Cboe BXE and CXE Dark Order Books, Cboe Large in Scale Service, Euronext Block, Turquoise Plato, Instinet BlockMatch, Liquidnet, Posit, and UBS MTF. This list excludes the Nordic dark pools (Copenhagen, Helsinki and Stockholm) and Goldman Sachs' Sigma-X. We obtain block trades executed in the Nordic dark pools from TRTH and aggregate them to a weekly frequency to coincide with the Fidessa's Top of the Blocks database. Sigma-X does not use the LIS waiver.[39] Hence, we allocate the entire Sigma-X volume reported in Fidessa Fragulator to trading under the reference price waiver.

Matching block trading to Fragulator data, we cannot rely on the ISIN to RIC mapping obtained in Subsection A.1 as the RICs in the block database, specifically the suffix, correspond to the block trading venue. In some cases, even the RIC prefix for the same instrument is different. Finally, some RICs are entirely missing in the block database. Thus, we proceed as follows: We first generate a list of all firms in the block trading dataset and find the ISIN for every firm:

1. For firms that have a corresponding RIC in the block database:
   - We enter the RIC into Fragulator's "Stock Selector"-field and note the corresponding ISIN and instrument name. This process provides results for more than 90% of the stocks in our sample.
   - If we do not obtain any output in Fragulator for the full RIC, we use the RIC prefix to identify the ISIN.
2. For firms that do not have a corresponding RIC in the block database, we use the firm name in Fragulator to identify the ISIN.

Testing the matching quality is non-trivial as we only observe trading turnover and not

[39] See Sigma-X Manual.

trading volume and thus cannot calculate implied prices. Instead, we employ the following steps:

– We manually compare firm names from the block database to those from Fragulator and Eikon.

– We test whether the exchange rate implied by the block database is identical to the exchange rate from Fragulator and Eikon.

– Two of the above listed venues, Cboe Large-in-Scale Service and Euronext Block, exclusively use the LIS waiver. For those venues, we ensure that the turnovers from Fragulator and the block database perfectly match.

A.3 Disentangling LIS and REF Trading Activity

We merge the Fragulator database and the block database (generated in Subsections A.1 and A.2) based on ISIN, week and venue. If both datasets were perfect, one would simply need to substract the block volume from the total dark volume to obtain the volume traded under the reference price waiver. However, we face several obstacles:

– Exchange rates used in the block database are sometimes scaled by powers of 10.[40]

– Trading volumes from both sources do not always perfectly match due to implied prices being measured at different points in time during the week.

– Potential rounding errors.

We resolve these issues as follows:

– We calculate the implied exchange rates from the block database by dividing the Euro turnover by the exchange currency-turnover. We then compare these implied exchange rates with those used to convert data from Eikon and Fragulator.[41] We expect some rounding errors as prices are measured at different points in time and we use the exchange rate from Eikon on Monday as the point of comparison. Thus, we accept the implied

[40]These errors in exchange rates do not exhibit any obvious pattern and seem arbitrary.
[41]Remember that Eikon and Fragulator do not necessarily report the instrument in the same currency.

exchange rates as correct if:

$$\left|\frac{\text{implied exchange rate}_{Block,i,j,t}}{\text{exchange rate}_{x,i,j,t}} - 1\right| < 0.05$$

for instrument i on venue j in week t and $x \in$ (Eikon,Fragulator).

– If the above test fails, we rescale the exchange rate in the block database by choosing a value for $y \in [-10; 10]$ that ensures:

$$\left|\frac{\text{implied exchange rate}_{Block,i,j,t} \cdot 10^y}{\text{exchange rate}_{x,i,j,t}} - 1\right| < 0.05$$

– Finally, we calculate $Turnover_{REF,i,j,t} = Turnover_{Fragulator,i,j,t} - Turnover_{Block,i,j,t}$ and set it equal to 0 if $\frac{Turnover_{REF,i,j,t}}{Turnover_{Fragulator,i,j,t}} < 0.05$. Furthermore, in less than 0.1% of the observations we observe that $Turnover_{REF,i,j,t} < 0$. We manually inspect these instances and set $Turnover_{REF,i,j,t} = 0$ and $Turnover_{LIS,i,j,t} = Turnover_{Fragulator,i,j,t}$.

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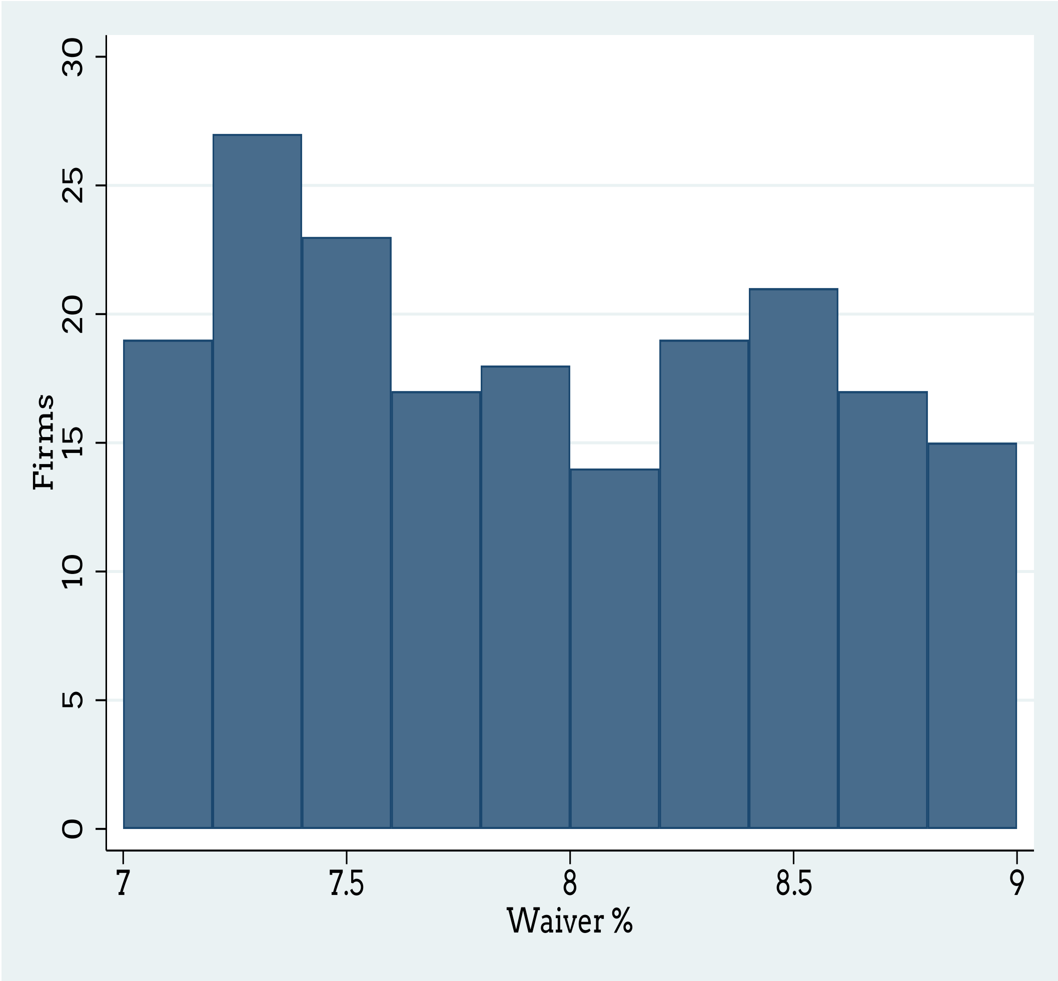

Figure 1. Firms Around the 8% Threshold

This figures plots the number of firms in each 0.2% waiver percentage bucket around the cutoff of 8%. *Waiver percentage* is defined as the maximum of the waiver percentages as reported in the January-2018 and February-2018 ESMA reports published on 7 March 2018. These reports determine the waiver percentage based on EU-wide trading in the reference price and negotiated trade waiver in the twelve month prior to the respective report, i.e., January 2017 to December 2017 and February 2017 to January 2018.

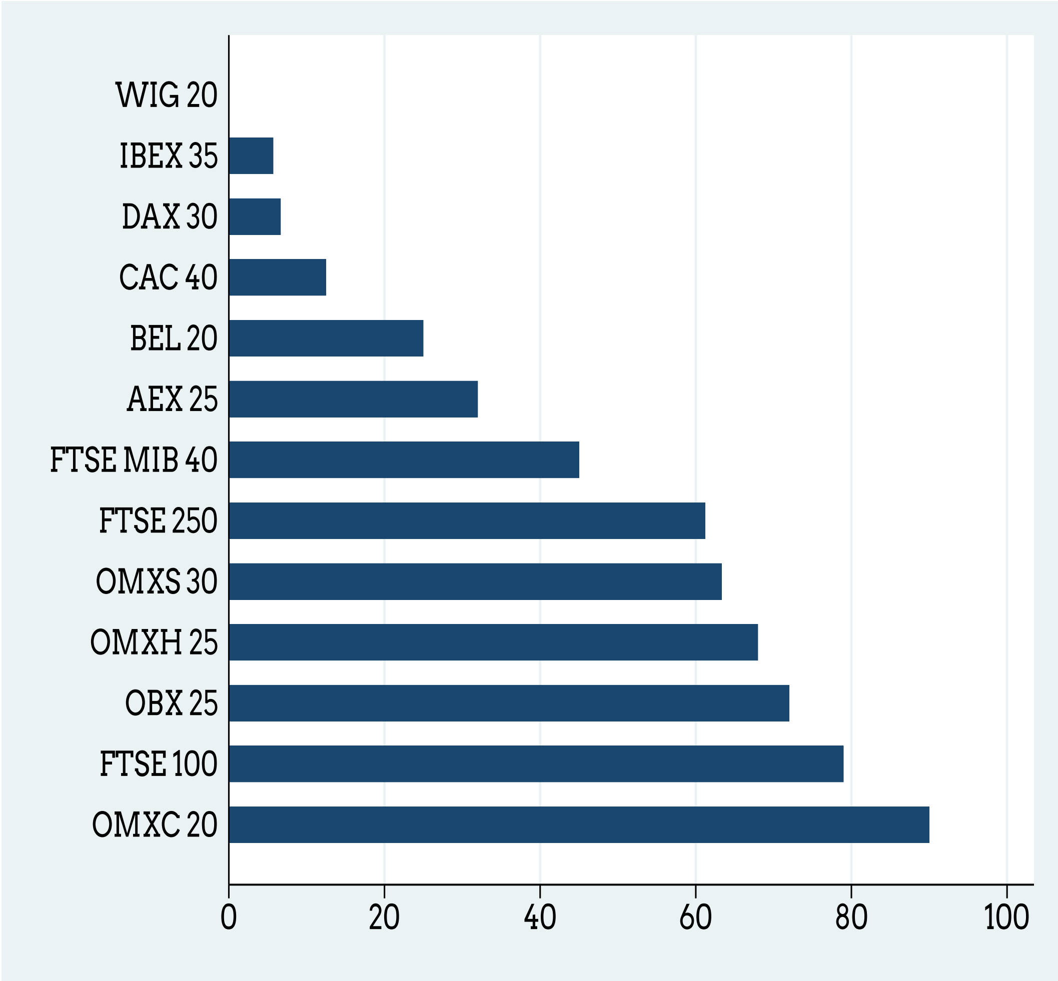

Figure 2. Dark Trading Bans by Stock Index
We plot the fraction of stocks banned from dark trading due to a breach in the 8% DVC for constituents of the main stock market index of each country included in our sample. The total number of stocks in each index is adjusted for stocks missing in the ESMA reports due to incomplete reporting by trading venues.

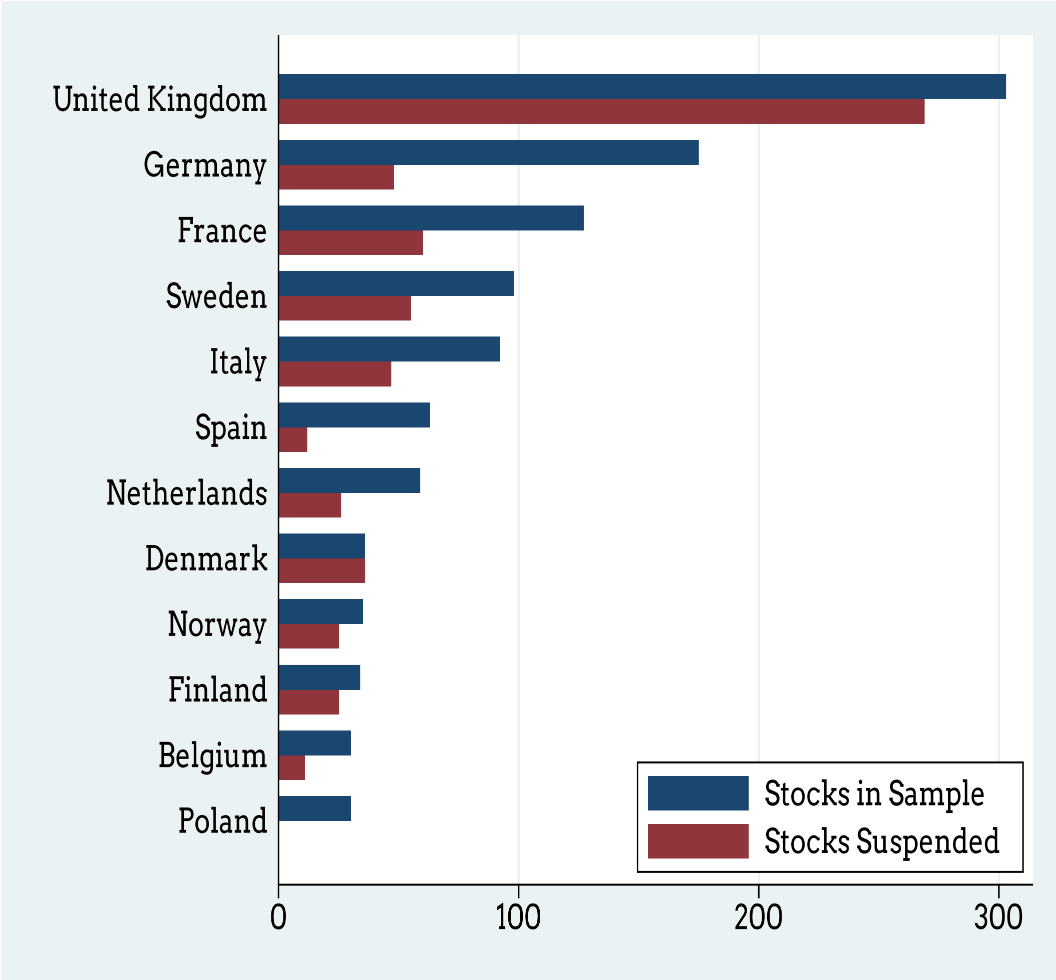

Figure 3. Dark Trading Bans by Country
We plot the the total/banned number of stocks in our main sample for each relevant competent authority.

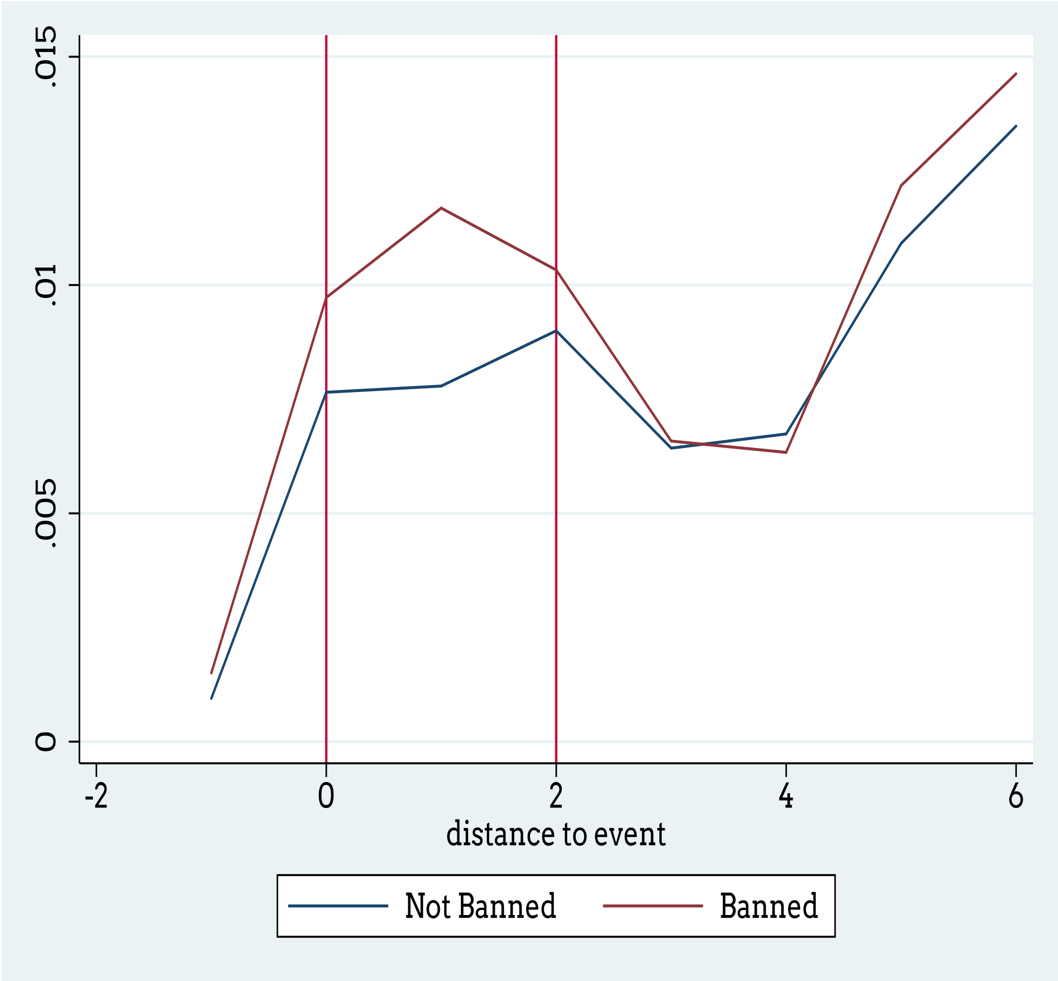

Figure 4. DVC Ban Announcement Returns
This figure shows the Cumulative Abnormal Returns one day prior and six days after 8 March 2018, for suspended and not suspended stocks. Day 2 marks the day of the actual implementation of the ban. Expected returns are calibrated using a Carhart (1997) 4-factor-model with country-specific factors from Andrea Frazzini's data library with an estimation period from 3 January to 2 March 2018.

Table 1. DVC Implementation Timeline

This table lists the important dates relevant for the implementation of the Double Volume Caps (DVCs) on dark trading imposed by MiFID II.

Date	Event Description
12 June 2014	MiFID II and MiFIR are adopted by the European Commission with the original implementation date of 3 January 2017
May 2016	The European Council and European Parliament agree to postpone the implementation by one year with a new implementation date of 3 January 2018
9 January 2018	ESMA decides to delay the publication of the data on DVCs for January-2018
7 March 2018	ESMA publishes the DVC data for the month of January-2018 and February-2018
12 March 2018	Dark trading bans kick-in for stocks identified in the January-2018 and February-2018 reports
10 April 2018	ESMA publishes the DVC data for the month of March-2018 and updates the January-2018 and February-2018 reports
13 April 2018	Dark trading ban kicks-in for stocks identified in the March-2018 report. Additionally, dark trading resumes for stocks incorrectly banned based on the January-2018 and February-2018 reports

Table 2. Stock Selection

This table lists the filters sequentially applied to the stock included in the ESMA DVC reports. *Main sample* includes stocks in the January-2018 and February-2018 reports published on 7 March 2018. *Placebo sample* includes stocks whose suspension status changed due to the (updated) ESMA January-2018, February-2018 and March-2018 reports published on 10 April 2018. Figures in parentheses indicate a change in status due to incomplete or erroneous reporting by trading venues in the January-2018 and/or February-2018 reports. † flags firms incorrectly not suspended in March-2018 that are the focus of our placebo analyses.

Filtering	Main Sample	Placebo Sample
Firms in the ESMA DVC reports	20,920	
Firms affected by the 8% cap only	20,888	
Equity type = Shares	18,437	
Liquidity status = Liquid	1,315	
2017 EU-wide volume > 5 million	1,311	
RCA reported in ESMA reports in BE, DE, DK, ES, FI, FR, GB, IT, NL, NO, PL, SE	1,246	
No delistings, stock splits or corporate actions	1,216	
Primary listing inside the EU or majority market share in an EU venue	1,183	
Exclude Swiss firms	1,168	
Exclude lower volume A/B stock	1,149	
Exclude/Keep firms that change status	1,082	67 (25)
Suspended	614	–
Not Suspended	468	–
Suspended to Not Suspended	–	1 (1)
Not Suspended to Suspended	–	21 (11†)
Not in Report to Suspended	–	18 (13†)
Not in Report to Not Suspended	–	17 (0)

Table 3. Variable Definitions

This table defines the variables used in our analyses. *Unit* is the unit of measurement, *Source* is the original data source, *Frequency* is the frequency of measurement (daily/weekly/static) in our final dataset, and *Definition* provides a short definition.

Variable	Unit	Source	Frequency	Definition
Panel A: Stock Characteristics and Trading Activity				
Market Value	mio	Eikon	Daily	$P \cdot MV$
Total Trading Volume	mio	Fragulator	Weekly	Total Euro volume across all trading mechanisms
Trading Volume (Most Relevant Market)	mio	TRTH	Daily	Total Turnover on the Most Relevant Market during continuous trading
Waiver Percentage	%	ESMA	Static	Fraction of trading under the negotiated trade and reference price waivers across EU
Percentage Negotiated Trade Waiver	%	ESMA	Static	Fraction of trading under the negotiated trade waivers across the EU
Percentage Reference Price Waiver	%	ESMA	Static	Fraction of trading under the reference price waiver across the EU
Suspension Dummy	%	ESMA	Static	1 if Waiver Percentage > 8%
Main Index Constituent	%	Eikon	Daily	1 if Stock is Constituent of index I on day t, I element of (AEX25 BEL20 CAC40 FTSE100 DAX30 IBEX35 FTSE MIB40 OMXC 20 OMXH 25 OMXS 30 OSEAX WIG 20)
Panel B: Liquidity				
Quoted Spread	bp	TRTH	Daily	Difference between best quotes divided by the mid-quote, time-weighted through the day
Effective Spread	bp	TRTH	Daily	Actual transaction costs paid by the trader submitting a market order
Depth	thousands	TRTH	Daily	Average of the euro depth at the best quotes, time-weighted through the day
Midpoint Volatilty (1 Minute)	bp	TRTH	Daily	Standard deviation of log returns measured at 1-minute interval
Price Impact	bp	TRTH	Daily	The information content of a transaction measured over 10, 30, and 60 seconds
Realized Spread	bp	TRTH	Daily	The compensation earned by the limit order trader after adjusting for any losses associated with adverse selection, measured over 10, 30, and 60 seconds
Relative Tick Size	bp	TRTH	Daily	Ratio of tick size over mid-quote, time-weighted through the day
Tick Constraint	%	TRTH	Daily	Indicator variable equal to 1 if difference between best quotes is 1 tick and 0 zero otherwise, time-weighted through the day
Panel C: Price Efficiency				
Midpoint Autocorrelation ($\lvert AutoCorr(r_{t'}^{\Delta})\rvert$)	%	TRTH	Daily	Absolute return first-order correlation based on Δ-second log returns where $\Delta \in \{10, 30, 60, 300\}$
Variance Ratio ($\lvert 1 - VR(n, m)\rvert$)	%	TRTH	Daily	Ratio of return variance calculated over intervals of length m, $r_{t'}^{m}$, to return variance over intervals of length n, $r_{t'}^{n}$, both scaled by the respective time periods. $(n, m) \in \{$(30, 10), (60, 10), (300, 10), (60, 30), (300, 30), (300, 60)$\}$
Panel D: Market Shares				
Market Share (j)	%	Fragulator	Weekly	Fraction of Total Trading Volume (defined above) in trading mechanism j. j is Auction, Periodic Auction, Dark LIS, Dark REF, Lit, OTC, or SI

Table 4. Descriptive Statistics

This table contains the descriptive statistics for the variables used in our analysis. For details and definitions of the different variables, see Subsection 4.2 and Table 3. Descriptives are shown for the main and placebo sample separately. We report the distribution of different variables over the full sample period (January 3, 2018 to May 11, 2018) and across stocks. *Unit* provides the unit of measurement, *Mean* the equal-weighted mean across all firm-days/weeks and *P(x)* the 5th/50th(median)/95th percentiles of the variables.

		Main Sample				Placebo Sample	
	Unit	Mean	P(5)	P(50)	P(95)	Mean	P(50)
Panel A: Stock Characteristics and Trading Activity							
Market Value	mio	7,957.06	381.73	2,581.83	36,615.18	9,785.62	2,638.35
Total Trading Volume	mio	61.39	0.94	14.07	290.61	69.41	13.75
Trading Volume (Most Relevant Market)	mio	14.55	0.26	3.93	64.41	13.88	4.33
Waiver Percentage	%	8.58	1.24	8.64	14.92	9.58	8.22
Percentage Negotiated Trade Waiver	%	2.54	0.32	1.96	6.05	4.28	4.09
Percentage Reference Price Waiver	%	6.02	0.73	5.95	10.84	6.44	6.62
Suspension Dummy	%	56.75	0.00	100.00	100.00	100.00	100.00
Main Index Constituent	%	32.62	0.00	0.00	100.00	58.33	100.00
Panel B: Liquidity							
Quoted Spread	bp	16.73	3.42	12.82	41.20	14.75	13.60
Depth	thousands	21.63	4.06	14.68	57.54	25.19	19.61
Effective Spread	bp	14.84	2.73	10.59	39.59	11.81	11.50
Realized Spread (10 sec.)	bp	1.41	-5.52	0.03	13.38	1.83	0.92
Price Impact (10 sec.)	bp	13.44	2.09	10.10	34.87	9.98	8.92
Midpoint Volatilty (1 Minute)	bp	7.02	3.22	5.99	13.98	6.48	5.55
Relative Tick Size	bp	7.05	2.07	5.57	17.96	7.91	6.95
Tick Constraint	%	27.49	0.42	21.41	76.40	36.51	32.72
Panel C: Price Efficiency							
Autocorrelation (10 sec.)	%	4.25	0.26	3.21	11.49	4.43	3.42
Variance Ratio (10/30 sec.)	%	7.59	0.46	5.67	20.73	7.90	5.99
Variance Ratio (30/300 sec.)	%	19.85	1.49	16.30	49.86	19.75	16.27

Table 5. Market Shares

This table contains the market shares of the different trading mechanisms. Market shares are shown for the main and placebo sample separately. For the main sample, they are calculated separately for *Suspended/Not Suspended* stocks and for the *Pre* (January 3 - March 9) and *Post* (March 12 - May 11) period. For the placebo sample, they are calculated separately for the *Pre* (January 3 - March 9) , *Mid* (March 12 - April 13) and *Post* (April 16 - May 11) period. We report the mean market shares of all stocks over the specified time periods for all trading mechanisms.

Panel A: Main Sample

	Suspended		Not Suspended	
	Pre	Post	Pre	Post
Dark Ref. Price	5.25	0.00	2.80	2.67
Dark LIS	2.37	2.86	0.87	0.87
Periodic Auctions	0.66	2.43	0.37	0.61
Continuous Lit	46.45	43.91	57.91	53.69
Call Auction	6.35	7.80	4.73	5.42
SI	18.85	21.30	14.48	16.77
OTC	20.06	21.65	18.84	19.97

Panel B: Placebo Sample

	Pre	Mid	Post
Dark Ref. Price	5.28	4.47	0.00
Dark LIS	2.93	3.06	2.26
Periodic Auctions	0.60	0.98	2.10
Continuous Lit	48.66	46.63	44.50
Call Auction	5.06	6.36	4.80
SI	18.74	19.63	24.23
OTC	18.73	18.87	22.09

Table 6. Pre-Ban Differences

This table contains the pre-ban (January 3 - March 9, 2018) mean for the variables used in our analysis, reported separately for *Suspended/Not Suspended* stocks of the main sample. For details and definitions of the different variables, see Table 3. *Unit* provides the unit of measurement.

	Unit	Suspended	Not Suspended
Panel A: Stock Characteristics and Trading Activity			
Market Value	mio	6,165.52	10,357.27
Total Trading Volume	mio	42.36	79.11
Trading Volume (Most Relevant Market)	mio	11.11	20.36
Waiver Percentage	%	11.33	4.98
Percentage Negotiated Trade Waiver	%	3.39	1.42
Percentage Reference Price Waiver	%	7.92	3.52
Main Index Constituent	%	31.60	33.97
Panel B: Liquidity			
Quoted Spread	bp	15.13	18.88
Effective Spread	bp	12.83	17.72
Depth	thousands	19.09	24.92
Midpoint Volatilty (1 Minute)	bp	7.08	7.18
Price Impact (10 sec.)	bp	12.31	14.83
Realized Spread (10 sec.)	bp	0.53	2.90
Relative Tick Size	bp	6.59	7.63
Tick Constraint	%	26.66	27.17
Panel C: Price Efficiency			
Autocorrelation (10 sec.)	%	4.17	4.27
Variance Ratio (10/30 sec.)	%	7.48	7.57
Variance Ratio (30/300 sec.)	%	20.01	19.81

Table 7. Semi-parametric DID: First Stage

This table presents the average marginal effects from the first stage logit estimation in the Abadie (2005) semi-parametric DID for all main sample stocks. The dependent variable is a dummy variable that equals to one if a stock is banned and zero otherwise. The independent variables are defined in Table 3. All independent variables are mean values from the pre-event period (January 3 - March 9). N is the number of observations. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Marginal Effect	T-Statistic
Rel. Tick Size	-2.062	(-0.87)
Tick Constraint	-1.640	(-0.85)
log(MV)	5.268**	(2.25)
% Ref. Price Waiver	19.468***	(13.26)
FTSE MIB40	0.259	(0.04)
AEX25	-17.396**	(-1.98)
BEL20	-9.361	(-0.91)
CAC40	-36.197***	(-4.65)
FTSE250	-6.599	(-0.79)
FTSE100	14.920	(1.27)
DAX30	-24.142**	(-2.17)
IBEX35	-39.667***	(-3.31)
OBX	-1.897	(-0.15)
OMXH25	-2.172	(-0.19)
OMXS30	9.552	(1.15)
Netherlands	-4.178	(-0.50)
Belgium	0.396	(0.04)
Germany	-23.832***	(-3.88)
Finland	3.211	(0.26)
United Kingdom	21.141**	(2.35)
Spain	-5.012	(-0.57)
Norway	12.264	(1.08)
France	-2.397	(-0.37)
Sweden	-7.276	(-1.05)
N	1016	

Table 8. Semi-parametric DID: Second Stage: Market Shares

This table shows the effects of the ban on the market shares of different trading mechanisms for the affected (main sample) stocks based on a semi-parametric difference-in-differences estimations (Abadie, 2005). The table presents the output from the second stage estimation (first stage results are reported in Table 7). Independent variable, as defined in Table 3, are centered around zero, such that *Unconditional* provides the unconditional effect of the ban. t-statistics are shown in parentheses. N is the number of observations. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Dark LIS	Periodic Auction	Continuous Lit	Call Auction	SI	OTC
Relative Tick Size	0.395	0.186	0.690	0.722	0.653	-3.814
	(0.33)	(0.98)	(0.31)	(1.23)	(0.42)	(-1.32)
Tick Constraint	-0.234	-0.250*	1.044	-0.995*	-1.059	1.445
	(-0.43)	(-1.86)	(0.77)	(-1.66)	(-1.01)	(0.93)
log(MV)	0.395	0.009	-0.493	0.737	1.579	-2.602
	(0.49)	(0.05)	(-0.21)	(1.16)	(0.99)	(-1.25)
% Reference Price Waiver	0.546	0.215	1.388	-0.081	-2.526	-1.622
	(0.32)	(1.10)	(0.69)	(-0.15)	(-0.71)	(-0.36)
Unconditional	0.649***	1.256***	1.175**	0.056	0.803**	-0.040
	(3.47)	(23.38)	(2.37)	(0.32)	(2.27)	(-0.09)
Observations	792	792	792	792	792	792

Table 9. RDD Results: Market Shares

This table shows the effects of the ban on the market shares of the different trading mechanisms for affected (main sample) stocks based on the RDD estimation. *Panel* denotes a panel RDD only controlling for the mean value of the dependent variable in the pre-event window. *Post–Pre* denotes a simple RDD where the dependent variable is the difference in market shares around the event date. A triangular kernel is employed while estimating the local linear regression. All estimates are computed using nearest neighbor heteroskedasticity-robust variance estimators. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively. *p-Values* are shown in parentheses. *Bandwidths* used are data-driven MSE-optimal and the resulting lower / upper cutoffs of the waiver percentage are shown in the table. N provides the number of observations to the left and right of the 8% threshold.

<table>
<tr><th></th><th></th><th>Dark LIS</th><th>Periodic Auction</th><th>Continuous LIT</th><th>Call Auction</th><th>SI</th><th>OTC</th></tr>
<tr><td>Panel</td><td>Coef.</td><td>1.3728***</td><td>.9393***</td><td>.9003</td><td>-.4126</td><td>.2162</td><td>.5085</td></tr>
<tr><td></td><td>p-Value</td><td>(.0039)</td><td>(0)</td><td>(.6066)</td><td>(.5702)</td><td>(.8739)</td><td>(.6837)</td></tr>
<tr><td></td><td>Bandwidth</td><td>4.41 | 11.59</td><td>2.49 | 13.51</td><td>4.48 | 11.52</td><td>4.48 | 11.52</td><td>5.03 | 10.97</td><td>4.34 | 11.66</td></tr>
<tr><td></td><td>N</td><td>2691 | 2988</td><td>3348 | 4392</td><td>2682 | 2925</td><td>2682 | 2925</td><td>2358 | 2484</td><td>2763 | 3060</td></tr>
<tr><td>Post - Pre</td><td>Coef.</td><td>1.0633**</td><td>.9432***</td><td>1.0819</td><td>-.1546</td><td>-.1462</td><td>.6745</td></tr>
<tr><td></td><td>p-Value</td><td>(.0147)</td><td>(0)</td><td>(.5224)</td><td>(.8404)</td><td>(.9186)</td><td>(.6142)</td></tr>
<tr><td></td><td>Bandwidth</td><td>3.66 | 12.34</td><td>2.09 | 13.91</td><td>4.44 | 11.56</td><td>4.67 | 11.33</td><td>5.14 | 10.86</td><td>4.28 | 11.72</td></tr>
<tr><td></td><td>N</td><td>329 | 408</td><td>380 | 518</td><td>298 | 327</td><td>290 | 311</td><td>252 | 263</td><td>309 | 347</td></tr>
</table>

Table 10. Semi-parametric DID: Second Stage: Liquidity

This table shows the effects of the ban on the primary market liquidity for the affected (main sample) stocks based on a semi-parametric difference-in-differences estimations (Abadie, 2005). The table presents the output from the second stage estimation (first stage results are reported in Table 7). Independent variable, as defined in Table 3, are centered around zero, such that *Unconditional* provides the unconditional effect of the ban. t-statistics are shown in parentheses. N is the number of observations. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Quoted Sp	Log Depth	Eff Sp	Real Sp 10s	P Imp 10s
Relative Tick Size	0.014	4.131	0.011	0.003	0.008
	(0.41)	(0.36)	(0.35)	(0.26)	(0.26)
Tick Constraint	-0.016	0.969	-0.014	-0.009	-0.005
	(-0.65)	(0.17)	(-0.65)	(-0.98)	(-0.28)
log(MV)	0.025	-6.895	0.025	0.012	0.014
	(0.59)	(-0.84)	(0.62)	(1.01)	(0.37)
% Reference Price Waiver	-0.027	21.977	-0.038	-0.003	-0.035
	(-0.72)	(0.86)	(-0.84)	(-0.17)	(-0.74)
Unconditional	-0.002	-0.174	-0.003	-0.003	-0.000
	(-0.69)	(-0.09)	(-0.98)	(-1.27)	(-0.09)
Observations	792	792	792	792	792

Table 11. RDD Results: Liquidity

This table shows the effects of the ban on the primary market liquidity for the affected (main sample) stocks based on the RDD estimation. *Panel* denotes a panel RDD only controlling for the mean value of the dependent variable in the pre-event window. *Post–Pre* denotes a simple RDD where the dependent variable is the difference in market shares around the event date. A triangular kernel is employed while estimating the local linear regression. All estimates are computed using nearest neighbor heteroskedasticity-robust variance estimators. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively. *p-Values* are shown in parentheses. *Bandwidths* used are data-driven MSE-optimal and the resulting lower / upper cutoffs of the waiver percentage are shown in the table. *N* provides the number of observations to the left and right of the 8% threshold.

		Quoted Sp	Log Depth	Eff Sp	Real Sp 10s	P Imp 10s
Panel	Coef.	.1442	.0284	-.2447	.1224	-.2829
	p-Value	(.8507)	(.5674)	(.6845)	(.7814)	(.6526)
	Bandwidth	4.92 | 11.08	2.87 | 13.13	4.86 | 11.14	4.58 | 11.42	4.1 | 11.9
	N	11461 | 11999	15106 | 19326	11710 | 12401	12255 | 13103	13135 | 15351
Post - Pre	Coef.	.2157	.0183	-.2709	.0434	-.2985
	p-Value	(.7864)	(.7218)	(.6798)	(.9309)	(.6595)
	Bandwidth	4.64 | 11.36	2.87 | 13.13	4.48 | 11.52	4.19 | 11.81	4.54 | 11.46
	N	292 | 312	361 | 464	298 | 325	312 | 358	294 | 319

Table 12. Semi-parametric DID: Second Stage: Price Efficiency

This table shows the effects of the ban on the short-term efficiency of primary market mid-quote for the affected (main sample) stocks based on a semi-parametric difference-in-differences estimation (Abadie, 2005). The table presents the output from the second stage estimation (first stage results are reported in Table 7). Independent variable, as defined in Table 3, are centered around zero, such that *Unconditional* provides the unconditional effect of the ban. t-statistics are shown in parentheses. N is the number of observations. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	AC 10s	AC 30s	AC 300s	VR 10/30s	VR 10/300s	VR30/300s
Relative Tick Size	0.213	-0.110	0.535	-0.275	-0.367	-0.947
	(0.33)	(-0.33)	(0.83)	(-0.27)	(-0.22)	(-0.57)
Tick Constraint	-0.554	-0.110	-0.176	-0.791	-0.461	-0.013
	(-1.11)	(-0.38)	(-0.49)	(-1.03)	(-0.57)	(-0.01)
log(MV)	0.475	-0.080	0.831	0.603	-0.437	-1.364
	(0.55)	(-0.21)	(1.36)	(0.49)	(-0.31)	(-0.72)
% Reference Price Waiver	-0.650	-0.062	-0.660	-2.116	2.595	2.614
	(-0.80)	(-0.15)	(-0.59)	(-0.92)	(0.90)	(0.84)
Unconditional	0.094	0.193*	-0.016	0.056	0.792**	0.586*
	(0.77)	(1.65)	(-0.11)	(0.25)	(2.02)	(1.74)
Observations	792	792	792	792	792	792

Table 13. RDD Results: Price Efficiency

This table shows the effects of the ban on the short-term efficiency of the primary market mid-quote for the affected (main sample) stocks based on the RDD estimation. *Panel* denotes a panel RDD only controlling for the mean value of the dependent variable in the pre-event window. *Post–Pre* denotes a simple RDD where the dependent variable is the difference in market shares around the event date. A triangular kernel is employed while estimating the local linear regression. All estimates are computed using nearest neighbor heteroskedasticity-robust variance estimators. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively. *p-Value*s are shown in parentheses. *Bandwidth*s used are data-driven MSE-optimal and the resulting lower / upper cutoffs of the waiver percentage are shown in the table. *N* provides the number of observations to the left and right of the 8% threshold.

<table>
<tr><th></th><th></th><th>AC 10s</th><th>AC 30s</th><th>AC 300s</th><th>VR 10/30s</th><th>VR 10/300s</th><th>VR 30/300s</th></tr>
<tr><td>Panel</td><td>Coef.</td><td>.3996</td><td>.1004</td><td>.3718</td><td>.7952*</td><td>1.9853</td><td>.5522</td></tr>
<tr><td></td><td>p-Value</td><td>(.1139)</td><td>(.6933)</td><td>(.2594)</td><td>(.0928)</td><td>(.1051)</td><td>(.5447)</td></tr>
<tr><td></td><td>Bandwidth</td><td>5.01 | 10.99</td><td>4.2 | 11.8</td><td>2.73 | 13.27</td><td>5.39 | 10.61</td><td>5.47 | 10.53</td><td>5.01 | 10.99</td></tr>
<tr><td></td><td>N</td><td>11082 | 11622</td><td>13053 | 14913</td><td>15396 | 19821</td><td>9491 | 9922</td><td>9365 | 9469</td><td>11082 | 11622</td></tr>
<tr><td>Post - Pre</td><td>Coef.</td><td>.1904</td><td>-.0452</td><td>.5948</td><td>.7791*</td><td>1.5029</td><td>.5406</td></tr>
<tr><td></td><td>p-Value</td><td>(.4566)</td><td>(.8566)</td><td>(.211)</td><td>(.0729)</td><td>(.2664)</td><td>(.6256)</td></tr>
<tr><td></td><td>Bandwidth</td><td>3.94 | 12.06</td><td>3.2 | 12.8</td><td>4.2 | 11.8</td><td>4.62 | 11.38</td><td>5.08 | 10.92</td><td>4.75 | 11.25</td></tr>
<tr><td></td><td>N</td><td>318 | 381</td><td>350 | 443</td><td>312 | 358</td><td>293 | 312</td><td>256 | 270</td><td>287 | 305</td></tr>
</table>

Table 14. Placebo Semi-parametric DID: Second Stage: March Report

This table shows the effects of the ban on the market shares (Panel A), primary market liquidity (Panel B) and price efficiency (Panel C) of different trading mechanisms for the incorrectly not banned stocks when these stocks erroneously did not get banned on 12 March 2018 based on a semi-parametric difference-in-differences estimations (Abadie, 2005). The table presents the output from the second stage estimation. t-statistics are shown in parentheses. N is the number of observations. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Market Shares

	Dark LIS	Periodic Auction	Continuous Lit	Call Auction	SI	OTC
Uncond.	0.165	-0.083	-2.115	0.270	0.979	1.041
	(0.18)	(-0.60)	(-1.58)	(0.37)	(0.87)	(0.84)
N	113	113	113	113	113	113

Panel B: Liquidity

	Quoted Sp	Log Depth	Eff Sp.	Real Sp 10s	P Imp 10s
Uncond.	-0.014	1.462	-0.010	0.000	-0.011*
	(-1.24)	(0.21)	(-1.34)	(0.05)	(-1.65)
N	113	113	113	113	113

Panel C: Price Efficiency

	AC 10s	AC 30s	AC 300s	VR 10/30s	VR 10/300s	VR 30/300s
Uncond.	-0.500	0.368	0.279	-0.755	0.732	0.898
	(-1.11)	(0.85)	(0.49)	(-1.05)	(0.43)	(0.48)
N	113	113	113	113	113	113

Table 15. Placebo Semi-parametric DID: Second Stage: April Report

This table shows the effects of the ban on the market shares (Panel A), primary market liquidity (Panel B) and price efficiency (Panel C) of different trading mechanisms for the previously incorrectly not banned stocks when these stocks get banned on 13 April 2018 based on a semi-parametric difference-in-differences estimations (Abadie, 2005). The table presents the output from the second stage estimation. t-statistics are shown in parentheses. N is the number of observations. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Market Shares

	Dark LIS	Periodic Auction	Continuous Lit	Call Auction	SI	OTC
Uncond.	-1.144	0.625**	-0.397	0.142	4.563***	0.110
	(-0.86)	(2.38)	(-0.19)	(0.29)	(2.87)	(0.05)
N	130	130	130	130	130	130

Panel B: Liquidity

	Quoted Sp	Log Depth	Eff Sp.	Real Sp 10s	P Imp 10s
Uncond.	-0.003	11.913	-0.002	0.000	-0.002
	(-0.74)	(1.62)	(-0.44)	(0.04)	(-0.41)
N	130	130	130	130	130

Panel C: Price Efficiency

	AC 10s	AC 30s	AC 300s	VR 10/30s	VR 10/300s	VR 30/300s
Uncond.	0.499*	-0.099	0.011	0.409	1.426	1.418
	(1.77)	(-0.28)	(0.02)	(0.88)	(1.11)	(1.04)
N	130	130	130	130	130	130

Table 16. Ban Returns

This table provides crosssectional OLS regression results of Cumulative Abnormal Returns on a suspension dummy as specified in Equation 3 around the publication of the March report on March 8, 2018 for the main sample. *Suspended* is a dummy that equals one for suspended and zero for non suspended stocks. *Abs. Distance* is the absolute difference between a stock's waiver percentage and 8%. Day 2 marks the day of the actual implementation of the ban. Expected returns are calibrated using a Carhart (1997) 4-factor-model with country-specific factors from Andrea Frazzini's data library with an estimation period from 3 January to 2 March 2018. t-statistics are shown in parentheses. N is the number of observations. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively. All regressions use heteroskedasticity robust standard errors.

	CAR(0;0)	CAR(-1;1)	CAR(-1;2)	CAR(-1;3)	CAR(-1;4)	CAR(-1;5)	CAR(-1;6)
Suspended	0.00214**	0.00541***	0.00417**	0.00646***	0.00532**	0.00453*	0.00270
	(2.03)	(3.31)	(2.22)	(3.10)	(2.33)	(1.82)	(0.99)
Constant	0.00662***	0.00745***	0.0107***	0.00500***	0.00734***	0.00958***	0.0119***
	(8.11)	(6.03)	(7.44)	(3.14)	(4.17)	(4.98)	(5.69)
N	1082	1082	1082	1082	1082	1082	1082
adj. R^2	0.00	0.01	0.00	0.01	0.00	0.00	0.00

	CAR(0;0)	CAR(-1;1)	CAR(-1;2)	CAR(-1;3)	CAR(-1;4)	CAR(-1;5)	CAR(-1;6)
Suspended	0.00282*	0.00702***	0.00663**	0.00961***	0.00845**	0.00737*	0.00657
	(1.72)	(2.86)	(2.35)	(3.13)	(2.48)	(1.94)	(1.56)
Abs. Distance	-0.000451	-0.000180	0.000142	0.000524	0.000712	0.000930	0.00110
	(-1.29)	(-0.31)	(0.23)	(0.79)	(0.98)	(1.13)	(1.29)
Suspended * Abs. Distance	-0.000103	-0.000393	-0.000673	-0.000924	-0.000949	-0.000909	-0.00121
	(-0.26)	(-0.61)	(-0.94)	(-1.21)	(-1.14)	(-0.93)	(-1.14)
Constant	0.00803***	0.00801***	0.0102***	0.00336	0.00511*	0.00666**	0.00846***
	(5.99)	(4.01)	(4.49)	(1.37)	(1.89)	(2.28)	(2.69)
N	1082	1082	1082	1082	1082	1082	1082
adj. R^2	0.01	0.01	0.00	0.01	0.00	0.00	0.00

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