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The Impact of Short Sale Bans on the Microstructure of Equity Markets: European Evidence

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Abstract

I investigate the effects of short sale bans on the market microstructure of European equity markets. In Spring 2020, Austria, Belgium, France, Greece, Italy, and Spain imposed short sale bans for a period of two months as a response to increasingly volatile financial markets driven by the Covid-19 crisis. I use daily stock data from 28 European countries to assess the impact of these bans on market liquidity, price efficiency, and stock prices. The analysis of stock liquidity measured by the percentage bid-ask spread, the Amihud illiquidity ratio, and the dollar volume points to a severe degradation in stock liquidity of large firms compared to the control group. I apply a matching procedure to verify the robustness of these results. Reverse effects take place once the ban is lifted again. Furthermore, my empirical evidence suggests that short sale bans reduce the price efficiency as proxied by a price delay measure. However, this effect is not stronger during market downturns. Moreover, it seems that the bans do not affect the speed of stock prices adjusting to firm-specific information and, contrary to the predictions made by Miller (1977), short sale bans appear to have no effect on stock prices. Overall, the results of my study using data from the Covid-19 crisis in 2020 mostly confirm the findings of previous studies that have examined the impact of short sale bans on the market microstructure.

Abstrakt

Ich untersuche die Effekte von Leerverkaufsverboten auf die Marktmikrostruktur europäischer Aktienmärkte. Als Reaktion auf die durch die Coronakrise bedingten hochvolatilen Finanzmärkte führten Österreich, Belgien, Frankreich, Griechenland, Italien und Spanien im Frühjahr 2020 Leerverkaufsverbote für einen Zeitraum von zwei Monaten ein. Ich nutze tägliche Aktiendaten aus 28 europäischen Ländern um den Einfluss von besagten Leerverkaufsverboten auf die Marktliquidität, die Preiseffizienz und die Aktienkurse zu untersuchen. Die Analyse der Liquidität mittels der prozentualen Geld-Brief-Spanne, der Amihud Illiquiditätskennzahl und des Dollar-Handelsvolumens deutet auf einen Rückgang der Liquidität großer Firmen im Vergleich zu der Kontrollgruppe hin. Ich nutze ein Matching-Algorithmus, um die Robustheit der Ergebnisse zu verifizieren. Nachdem das Verbot aufgehoben wird, treten entgegengesetzte Effekte auf. Des Weiteren deuten meine empirischen Ergebnisse darauf hin, dass Leerverkaufsrestriktionen die Preiseffizienz, gemessen anhand einer Preis-Verzögerungsvariable, reduzieren, wobei dieser Effekt in fallenden Märkten nicht stärker ist. Zudem scheinen die Leerverkaufsverbote keinen signifikanten Einfluss auf die Anpassung der Aktienpreise an firmenspezifische Informationen und, anders als von Miller (1977) prognostiziert, keinen Einfluss auf die Aktienkurse zu haben. Zusammenfassend kann ich mit meiner Studie anhand der Daten aus der Coronakrise im Frühjahr 2020 die Ergebnisse bisheriger Studien zur Wirkung von Leerverkaufsverboten auf die Marktmikrostruktur weitestgehend bestätigen.

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List of Abbreviations

ADR	American Depository Receipt
AMEX	American Stock Exchange
CDS	Credit Default Swap
ETF	Exchange-Traded Fund
EU	European Union
NASDAQ	National Association of Securities Dealers Automated Quotations
NYSE	New York Stock Exchange
OLS	Ordinary Least Squares
SE	Standard Errors
SEC	Securities and Exchange Commission
TARP	Troubled Asset Relief Program
UK	United Kingdom
US	United States of America

List of Symbols

AR	Abnormal return
BA	Percentage bid-ask spread
Ban	Ban dummy
β	Regression coefficient
$DolVol$	Dollar Trading Volume expressed in Euro
D	Distance metric
Δ	Delta
d_1	Price delay measure
ε	Error term
$ILLIQ$	Amihud illiquidity ratio
LIQ	Liquidity proxy
MV	Market value
P	Stock price at market close
P^A	Ask price at market close
P^B	Bid price at market close
r	Daily log-return
R	Weekly log-return
R^2	R-squared of a regression estimation
Vol	Number of shares traded on a given day
$Volatility$	20-days rolling standard deviation of returns

1 Introduction

On March 12, 2020, the EURO STOXX 50, the major European stock index declined by more than 12% - the largest loss ever reported on a single day since index inception in 1986.[1] Other major stock indices around the globe experienced a similar drop. The Covid-19 crisis was about to hit and there was great uncertainty among investors, reflected by very volatile stock markets. The corresponding volatility index VSTOXX had its peak on March 16, 2020, with an implied volatility of 86%.

To stabilize capital markets and restore the confidence of investors during volatile times, regulators can make use of temporary bans on short sales, i.e., restricting investors in their ability to profit from declining stock prices. This was the case in numerous countries during the financial crisis in 2007-09. National authorities prohibited short selling either only on financial stocks or all stocks. For example, the Securities and Exchange Commission (SEC), the national regulator for capital markets in the United States, banned short sales on 797 financial stocks in fall 2008. Another example is the short sale ban imposed by four European countries on August 11, 2011, as a reaction to the European debt crisis and the subsequent volatile capital markets.

As a response to extremely volatile markets in March 2020 caused by the Covid-19 pandemic, national authorities for financial markets in Austria, Belgium, France, Greece, Italy, and Spain prohibited covered short sales on all stocks in their country, respectively. The Italian regulator announced the ban to be active for three months while the other five national authorities initially set a ban period of approximately one month which was later extended until May 18, 2020. Following the other countries, Italy also announced to lift the ban on the same day. Thus, the short sale ban was in effect for two months in all six countries and ended on May 18, 2020.

In this paper, I empirically investigate the impact of short sale bans on the market microstructure of equity markets by focusing on three major stock market characteristics: (How) did the ban affect stock liquidity, (how) did the ban influence price efficiency, and did the regulators manage to stabilize stock prices by imposing short sale bans? The first two questions shall be analyzed to examine whether the bans had any undesired side effects such as a decrease in market quality or market efficiency. The latter one is of interest to the extent that price stabilization is one of the primary goals of regulators when restricting short sales. Herewith, I contribute to the existing literature by using the most recent data from the Covid-19 crisis in spring 2020 which caused six European countries to impose bans on covered short sales. I use

[1] See Statista (2020).

both daily and weekly data from 28 countries on firm-level for a time span of six months to conduct my analysis.

My empirical results suggest that overall market liquidity decreased drastically in March 2020, but it did so even stronger for large stocks that were subject to the short sale ban. The effect rebounded once the short sale ban was lifted again. These findings are mostly consistent with previous literature and robust against the identification strategy of the diff-in-diff structure as well as the type of control sample.[2] Furthermore, I find evidence suggesting that prices of banned stocks adjust more slowly to market-wide information than a set of non-banned control stocks, which is also in line with previous findings. My results, however, suggest that this effect is not stronger during market downturns. Moreover, stock prices appear to remain unaffected by short sale bans, suggesting that regulators could not artificially push stock prices upwards.

As of the start of my thesis, I was not aware of any research dealing with the same topic that had already been published. However, in the meantime, two working papers by Siciliano and Ventoruzzo (2020) and Della Corte et al. (2020) have been published on SSRN which also investigate the effects of short sale bans, but with a less comprehensive approach regarding the data and number of variables to proxy liquidity.

The remainder of this paper is structured as follows: In section 2, I introduce the idea of short selling, its historical evolution, and the corresponding regulatory measures against it in six European countries in Spring 2020. Section 3 presents a review of the three literature streams on stock liquidity, price efficiency, and stock prices. Section 4 elaborates on the liquidity analysis containing a description of the data, the constructed variables, and corresponding descriptive statistics. Furthermore, I explain the methodology, my empirical approach, and provide detailed results including a test for robustness. In section 5, I analyze the price efficiency during short sale bans and present my results. Section 6 provides the results of a stock return analysis in the context of short sale bans, followed by the overall conclusion in section 7.

[2] The diff-in-diff structure refers to the fact that I do not only want to test if stock liquidity changes due to a short sale ban but whether this change (or difference) is significantly different from the difference produced by the control sample.

2 Short Selling (Bans)

In this section, I describe the mechanics and general idea of short selling as well as the history of short sale bans before I provide a brief overview of the European short sale bans introduced in Spring 2020.

2.1 The Idea of Short Selling

If an investor enters a long position in any kind of financial instrument, she will gain if the price of the financial instrument increases. The most straightforward type of a long position is to buy stock in a publicly traded company. The opposite happens if an investor enters a short position in any kind of financial instrument. In that case, she will gain if the price of the financial instrument decreases. If an investor possesses the stock and wants to profit from potentially declining prices, she can simply sell the stock and buy it back after the stock price has fallen. The difference between the price at which the stock was sold and the price at which the stock is bought back equals the gross profit of the investor. However, if an investor does not possess the stock, but wants to profit from potentially falling prices, she can enter a short position.

As I use public equity data for this paper, I focus on short positions in stocks. There are multiple ways to enter a short position in a stock. An investor can borrow the stock from a lender (often a market dealer) at a certain fee. Usually, the borrower of the stock must pledge some collateral. The investor then sells the borrowed stock at the stock market, e.g., at a price of €100. After the stock price has fallen, she buys it back, e.g., at €90, and returns the stock to the lender. In this case, the gross profit for the short seller equals €10.[3] This is usually known as classic short selling.

Another way of entering a short position is to use derivates, i.e., financial instruments whose value depends on some underlying value (the stock price in this case). An investor can profit from a declining stock price by "selling" a futures contract on that stock. Thereby, she commits to deliver the stock on a certain date in the future receiving the forward price, e.g., €100. If the stock price has considerably fallen between the date of the contract's sale and the settlement date, and trades now at, e.g., €90, the investor will gain €10 because she buys the stock at €90 at the open market but receives €100 from the counterparty.[4]

Another potential derivative strategy is a put option. This gives the investor the right, though not the obligation, to sell a stock at a pre-determined price, the so-called strike price or exercise

[3] See D'Avolio (2002), p. 275-276.
[4] See Hull (2015), p. 50.

price, e.g., €100, to the option writer. If the stock trades at €90 shortly before the put option expires, the investor can buy the stock at the open market for €90 but sell it to the option writer for €100 at maturity, thereby earning a gross profit of €10. As the name already indicates, the buyer of a put option can but does not have to exercise the option whereas both counterparts of a futures contract are obliged to deliver when the contract expires.[5] Hence, the investor must pay a premium upfront for an option whereas a futures contract is simply a deal between the two parties.[6] The short sale bans which I investigate in this paper prohibited investors from entering new- or increasing existing short positions by any of the means described.

There are several risks associated with short selling. The most straightforward one is the risk of unlimited losses. If an investor enters a long position by buying a stock, the largest possible loss she faces equals the stock price as prices can (theoretically) not become negative. However, when entering a short position, the potential loss is (theoretically) unlimited as the stock price could increase infinitely. Furthermore, share lenders have usually the right to call in shares at any time. When this is the case, the borrower must return the borrowed shares. To do so, the borrower must either find another lender to roll-over the loan or close the position by buying the shares at an inconvenient price at the open market.[7]

There are five main reasons for market participants to enter short positions. First, investors such as hedge funds simply speculate on declining prices to make profits. They might have negative (private) information about a single firm that is not yet incorporated in the stock price but will be in the future. This is referred to as informed short selling. Boehmer et al. (2008) argue that institutional short sellers are usually very well informed, as heavy short positions taken by these informed institutions result in a strong risk-adjusted outperformance over the following month. This outperformance is strongest for very small stocks, which is not surprising given the public availability of data on small firms, to which institutional investors have certainly better access. Second, investors use short positions to hedge their portfolios and to steer the overall portfolio risk in accordance with the investment guidelines. Third, market makers enter short positions to provide liquidity to the overall market. If a market maker receives a buy order from an investor but does not have enough stock in the inventory, she simply borrows shares and sells them to the investor. High-frequency traders provide a substantial share of market liquidity and are therefore also referred to as informal market makers, which are not officially registered.[8]

[5] See Hull (2015), p. 214-215.
[6] See Hull (2015), p. 213.
[7] See Boehmer et al. (2008), p. 523.
[8] See Boehmer et al. (2013), p. 1396.

Market making accounts for the largest part of short selling activity. Officially registered market makers are generally exempt from short sale bans because otherwise, liquidity would deteriorate drastically.[9] Fourth, some investors use short selling to exploit market inefficiencies. For example, they sell the stock and buy a convertible bond of the same firm. This is referred to as arbitrage.[10] A fifth reason to enter a short position is a quantitative investment strategy. Over the years, many stock market anomalies have been discovered and published. They represent a large literature stream in asset pricing. One famous example is the study by Jegadeesh and Titman (2001) who document an outperformance of stocks that have exhibited high returns in the previous period (week or month), also referred to as momentum. Another anomaly was published by Sloan (1996) who shows that firms with high accruals underperform firms with low accruals. Quantitative investment strategies based on these anomalies work as follows: The investor buys the stocks in the (usually) highest decile of the anomaly variable distribution, i.e., in these cases the return from the previous period or the accruals, and shorts the stocks in the lowest decile of the distribution. This is referred to as a long-short portfolio, the intuition being that this approach requires an initial wealth of zero. If too many stocks in the short leg of the strategy are subject to a short sale ban, either the strategy no longer works properly, or the short leg includes only stocks that are exempt from the ban. Andrikopoulos et al. (2013) find that, on average, only about 30% of the firms in the short leg can actually be sold short, either due to non-availability or due to a short sale ban. Interestingly, however, they do not find any significant performance difference between a constrained- and unconstrained short leg.

Moreover, investors cannot only enter short positions on single stocks but also on a whole range of firms by simply shorting Exchange-Traded Funds (ETFs).[11] I provide a detailed explanation in section 2.3 whether and how ETFs and other index-based instruments were subject to the short sale bans in 2020.

The classic short selling, i.e., selling a share that the seller does not possess, can be split into naked- and covered short selling. Covered short selling implies that a short seller has actually borrowed the shares whereas naked short selling means that the share has not even been borrowed. The latter practice has been uniquely prohibited in the European Union (EU) since 2012 and is therefore not considered in my empirical analysis. The significance of short selling

[9] See McCaffrey (2010), p. 483.
[10] See Dechow et al. (2001), p. 81.
[11] ETFs are financial instruments that represent an entire basket of securities such as major stock indices. They are tradable like normal stocks on stock exchanges. See Mohamad et al. (2016), p. 1.

activity is stressed by the data. Wang et al. (2020) report a mean short volume ratio of 36.36% of stocks listed on the New York Stock Exchange (NYSE), the American Stock Exchange (AMEX), and the National Association of Securities Dealers Automated Quotations (NASDAQ) between 2010 and 2015. In other words, more than one-third of the trading volume in this period were short positions (excluding derivatives trading).

In times of market turbulences, the practice of short selling is often blamed for amplifying volatility and adding downwards pressure on security prices. However, many researchers argue that it enhances market efficiency and provides liquidity. I give a review of research findings regarding the effectiveness and side effects of short sale bans in section 3.

2.2 Evolution of Short Selling (Bans)

Short selling and resulting short selling bans have been common practice for centuries. One of the first reported short sales was conducted in the Netherlands in 1609 by a syndicate of traders led by a former director of the Royal East India Company. They sold forward contracts with a maturity of up to two years and tried to bring down the stock price of the Royal East India Company by spreading bad rumors about the business when maturity approached. Company directors reacted by submitting a petition to the States of Holland, basically requesting a short sale ban on company stocks. The States followed the request by setting restrictions against trading in forward contracts. In the end, the syndicate did not succeed anyway, as they did not manage to push down the stock price. In fact, the stock price rebounded (most likely due to a dividend distribution) after it had fallen, resulting in a significant loss for the short sellers.[12] Another rather historical example is the NYSE during World War I. It issued restrictions against short selling as they did not want war effort to be lowered by a demoralized stock market.[13] The short sale ban in 2008/09 during the financial crisis was the most extensive ban in this century due to the large number of countries worldwide that implemented temporary measures against short selling either on only financial- or all stocks. On September 19, 2008, Christopher Co, the SEC chairman back then, stated:

"The emergency order temporarily banning short selling of financial stocks will restore equilibrium to markets."[14]

Like many other national regulators, the SEC found it necessary to undertake this step to stabilize financial markets. Brunnermeier and Oehmke (2014) develop a model according to

[12] See Petram (2011), p. 24-28.
[13] See Macey et al. (1989), p. 801.
[14] See SEC (2008).

which short selling can indeed be harmful to financial institutions and justifying why it should be considered to protect these firms from short selling. They argue that speculative predatory short selling and the subsequently declining share price can lead uninsured depositors to withdraw their funds due to the fear of impending bankruptcy.[15] In the presence of leverage constraints, financial institutions are then forced to sell long-term assets at fire-sale discounts resulting in a loss. This theoretical model shows that a short sale ban may be reasonable in particular cases. However, Christopher Cox had a very critical opinion about the bans ex-post, as he stated in an interview with Reuters on December 31, 2008:

"Knowing what we know, I believe on balance the commission would not do it again. The costs appear to outweigh the benefits."[16]

This underlines the uncertainty about bans and their impacts on markets that regulators face. Yet, there are still authorities that impose bans in times of volatile financial markets. The ban adopted by four European countries as a reaction to the sovereign debt crisis in 2011 is a famous example from the near past. Bris et al. (2007) state that, although short sellers have been made responsible for market crashes for centuries and despite the ongoing debate between regulators and institutional investors, the effects of short sale restrictions are still ambiguous. Market crashes are indeed associated with short-selling activity as measured by the short interest, i.e., the number of shorted shares divided by the number of outstanding shares. Ofek and Richardson (2003) find that, during the dotcom bubble, the short interest of internet firms was significantly larger than the short interest of control firms, also referred to as old economy firms. However, they do not state that short sellers were the drivers of falling internet stock prices. Callen and Fang (2015) conduct an analysis with US equity data and state that short interest today is positively correlated with crash risk in one year. They argue that short sellers are capable of finding bad news that is not yet published by the management and trade on them.

Overall, this paper seeks to provide an overview of the specific literature streams and to extend previous findings using the latest data from the stock market crash in March 2020 as a consequence of the Covid-19 crisis.

2.3 European Short Sale Bans in 2020

The short sale ban in Spain started on March 17, while it was imposed one day later in Belgium, France, Greece, and Italy and two days later in Austria on March 19. All countries except Italy initially set a period of one month for the ban to be in effect with an additional remark to lift it

[15] The assumption is that depositors view sharply declining share prices as a signal of bad firm performance.
[16] See Younglai (December 31, 2008).

earlier or to extend it if found to be necessary. The latter was the case as all of these countries eventually extended the ban until May 18. The Italian Authority chose a ban period of three months straight away.

For each of these six countries, the ban applied to shares admitted to the respective national trading venue and for which the national regulator was the relevant competent authority. Two examples illustrate and clarify this setting: Apple is a US-American stock that is listed on an American stock exchange but also on the stock exchange in Milan, Italy. However, since the Italian regulator is not the relevant competent authority for Apple, its stock was not subject to the ban, even though it is traded in Italy. Furthermore, OMV is a stock that is traded on the Vienna Stock Exchange, for which the Austrian regulator is the relevant competent authority. Hence, OMV was subject to the ban. The stock is also traded on Xetra, a German trading venue, where the ban also applied to OMV. The ban implied that investors were no longer allowed to enter or increase net short positions. It also applied to transactions in related securities such as derivatives. Market making activities, specific hedging activities, and transactions in index-related instruments such as ETFs with a weight of banned stocks lower than a certain threshold were excluded from the ban.[17] Furthermore, the ban explicitly also applied to short positions that are closed within the same trading day. Despite the different announced ban periods at the start of the ban, the bans lasted until May 18, resulting in a ban period of two months in each of the six countries.

On the other hand, other large European countries did not ban short sales in Spring 2020. This emphasizes that the opinions on short sale bans vary widely. The Financial Conduct Authority, the British regulator, issued an opinion in which they state that there is no evidence for short sales to be the driver of falling markets. They argued that short selling is a crucial provider of liquidity. German authorities had a similar view and hence, did not issue any restrictions on short selling. The German fund association stated on March 20 that they decline a general ban on short selling. The fact that some European national regulators imposed short sale bans while others did not allows me to divide my data into a treatment group consisting of all firms from the six countries with a ban and a control group comprising all firms of the remaining European countries without a ban. I provide a detailed description of the data in section 4.

[17] While Austria, France, and Spain set a threshold of 50%, Belgium, Greece, and Italy set a threshold of 20%. From April 15 onwards, the threshold was set uniquely among all countries at 50%.

3 Literature Overview

To understand what kind of research has been done on short sale bans, this section provides an overview of the literature on the effects of short sale bans on stock liquidity, price efficiency, and stock prices. Since the inception of short sale bans is a rare event that usually only occurs during severe financial market downturns, a large share of the empirical papers focusses on the financial crisis in 2007-09. While some papers examine only a subset of the effects, others investigate the whole range of effects. In accordance with the empirical part of my paper, I divide the literature section into three parts: The effects of short-sale bans on stock liquidity, price efficiency, and on stock prices. During the financial crisis in 2007-09, short-sale bans varied from country to country with respect to the ban period, to whether only naked or also covered short sales were prohibited, and to whether the bans applied to all or only to financial stocks. As naked short sales have been banned in the EU since 2012 anyway and there were six countries in the EU that all imposed bans on covered sales on all stocks for an identical period of time in spring 2020, my investigation is somewhat less extensive since there is no variation among these characteristics in my data.

Liquidity Effects. Beber and Pagano (2013) use a global dataset comprising 30 countries from the 2007-09 crisis to examine the effects of short sale bans on stock liquidity. Their results suggest that short-sale bans adversely affect daily bid-ask spreads, which they use as a proxy for stock liquidity. These results remain significant no matter if estimated as a panel regression or one regression per country at a time. The decline in liquidity is strongest for small-cap stocks and stocks without listed options. The latter indicates that investors still use derivatives markets to enter short positions. Despite this they find that, once bans on covered short sales are lifted, liquidity tends to increase again. They verify the robustness of their findings by using the illiquidity measure introduced by Amihud (2002) as an additional proxy for liquidity and by running a Two-Stage Least Squares Regression to account for endogeneity. Boehmer et al. (2013) use a dataset from the United States (US) to investigate the effects of the US short-sale ban in 2008 on trading activity and bid-ask spreads. They report a significant decline in daily trading dollar volume for the largest 25% of the banned stocks. A similar picture arises for different measures of market quality, such as relative quoted and effective spreads or intraday price ranges, which tend to increase for the largest 50% of banned stocks after the ban inception. The results are consistent with Lobanova et al. (2010) who also investigate the effects of the 2008 short-sale ban in the US. They find a sharp increase in spreads and a decline in turnover ratio, trading volume, and dollar volume. Marsh and Payne (2012) investigate the effects of short sale bans by using a dataset from the United Kingdom (UK) from 2008/09. They report a reduction in trading volume and market liquidity once a short-sale ban is in effect. Alternative proxies of market liquidity such as the order book liquidity confirm the findings. Helmes et al. (2017) use an Australian dataset to investigate the short sale ban from 2008/09 in Australia. Their control group comprises matched comparable Canadian stocks with respect to industry and firm size. In line with the other studies, they find a reduction of trading activity and an increase of the bid-ask spread for stocks subject to the ban.

Contrary to all these papers, Appel and Fohlin (2010) find that short sale bans in 2008 had a positive or at least a neutral effect on stock liquidity. Similar to my paper, they consider only bans on covered short sales. Their sample comprises 35 stocks from eight (mostly) European countries. Their identification strategy differs from conventional ones in that they match each banned stock with its corresponding American Depository Receipt (ADR) that is traded on a US-American stock exchange and is therefore not subject to the ban.[18] They argue that non-banned ADRs differ from banned stocks only in the short sale regulation and are therefore ideally suited as a control group. Only some non-American firms have ADRs trading in the US which explains the low number of included firms in their sample. This reflects the trade-off between a large sample and an accurate control group.

Price Efficiency. Short selling is often associated with more efficient markets.[19] Boehmer et al. (2008) conclude that "short sellers possess important information, and that their trades are important contributors to more efficient stock prices"[20]. Diamond and Verrecchia (1987) show in their model that, once a short sale ban is active, stock prices adjust more slowly to private information. This effect is stronger for negative information, the intuition being that only those who own the stock can trade on negative news whereas others cannot. The result is that it takes longer for stock prices to incorporate information, especially negative news. This is in line with the intentions of regulators hoping that bad news is not immediately incorporated into stock prices as the impact of bad news might be overrated. As it is hard to capture private information, many papers also focus on market-wide information. This theory has been tested in numerous studies of which most support the theory. For example, Beber and Pagano (2013) entirely confirm the theoretical predictions. They try to capture private information by analyzing the autocorrelation of residuals. They report a higher autocorrelation during a ban, suggesting that it takes longer for firm-specific information to be impounded into stock prices during a ban. To

[18] ADRs are certificates that represent a legal claim on shares of a firm outside the US and whose shares are not listed on a US-American stock exchange. See Jayaraman et al. (1993), p. 91.
[19] See Beber and Pagano (2013), p. 344.
[20] See Boehmer et al. (2008), p. 525.

distinguish between negative and positive market-wide information, they compute the downside (upside) cross-autocorrelation, i.e., the autocorrelation between individual stock returns and negative (positive) market returns. They find that this measure significantly increases after the ban enactment, that downside cross-autocorrelations are larger than upside cross-autocorrelations and that this difference is larger for the ban period compared to non-ban periods. Intuitively spoken, this result indicates that it takes longer for negative market-wide information to be impounded into stock prices than it does for positive information, especially during ban periods. Bris et al. (2007) report a significantly higher market model's downside R^2 for banned stocks compared to non-banned stocks. Their findings also suggest that the price efficiency of banned stocks decreases due to short sale bans. Saffi and Sigurdsson (2011) use the lending supply of stocks and loan fees to proxy short sale restrictions.[21] Their findings are in line with the previously described papers: The higher the restrictions are, the lower the price efficiency is. Reed (2007) finds that when short selling is rather costly, the speed with which prices adopt after quarterly earnings announcements tends to be slow. This effect is stronger for bad news announcements. Chang et al. (2007) use data from the Hong Kong stock market and report that stock prices adjust slower when they are subject to short sale bans.

Stock Prices. Miller (1977) developed a theoretical model for short sale restrictions: His key assumption is that investors have heterogeneous beliefs about the development of security prices.[22] If a certain security is banned from short-selling, informed market participants who do not own the security but have a pessimistic opinion about it are not able to incorporate their valuation into the security's price. This leads to inefficiently overpriced securities above the fundamental value as prices reflect only the opinions of bullish investors and bearish market participants who possess the security. This, in turn, means that prices should decline once a short sale ban is lifted. Contrary to that, Diamond and Verrecchia (1987) argue that rational investors take into account the fact that some bearish investors cannot trade on negative information and adjust their valuation subsequently. This does not lead to upwards biased security prices. Beber and Pagano (2013) investigate whether short sale bans help stabilize stock prices. They argue that this is one of the regulator's main objectives when imposing short sale bans. They don't find any evidence for significant positive abnormal stock returns, except for the US. However, they suggest that other publicly available information such as the Troubled Asset Relief Program (TARP) announcements may have had positive effects on stock prices in

[21] Therefore, the variable is not binary (as in my paper) but continuous, i.e., a likelihood for short sale regulation.
[22] This is contrary to the famous Capital Asset Pricing Model by Sharpe (1964), where all investors have homogeneous beliefs about the firm's future cash flows.

the US. The authors conclude that the major advantage of the political measures that were taken to restrict short-selling activities is the large amount of data that was generated. Similar conclusions are drawn by Boehmer et al. (2013) who do not find any outperformance of banned stocks compared to a matched sample after controlling for the TARP announcement effects. In fact, they find a negative performance of firms that were added to the ban lister afterward, arguing that a lagged addition to the list is a signal of bad firm performance. Saffi and Sigurdsson (2011) do not find any effects on stock prices, either. Their findings suggest that relaxing short sale restrictions, i.e., higher stock lending supply, does not lead to an increase in the occurrence of extreme negative stock returns. Helmes et al. (2017) investigate the short sale ban in Australia in 2008/09. Consistent with the other studies, they don’t find any supporting effects on stock prices, either. Bailey and Zheng (2011) report that the stock market downturn during the financial crisis 2008/09 cannot be attributed to short selling and that, therefore, the regulatory measures against it were not justified.

Even though not quite related to short sale bans, Boehmer et al. (2008) provide valuable insight into the relationship between shorting activity and stock returns. They find that heavily shorted stocks underperform weakly shorted stocks. This underperformance is persistent over time and strongest for small stocks, indicating that informed short sellers are able to identify poorly performing stocks and to bet against them. They do not, however, push down prices artificially below the fundamental value which would justify a regulatory measure against short sales.

4 Stock Liquidity

This section comprises the liquidity analysis. I describe the variables and how I construct them and provide corresponding summary statistics before I introduce the methodology and give an overview of the empirical results including a modified identification strategy to assess the robustness of my results. Following the majority of studies described in the previous section, I hypothesize that the short sale bans in 2020 decreased stock liquidity.

4.1 Variables and Data

Variable Construction. I investigate the impact of short sale bans on stock liquidity. Plenty of variables have been used to proxy stock liquidity. In this paper, my main variable to measure liquidity is a version of the bid-ask spread. To verify my results, I use the Amihud Illiquidity ratio and the dollar trading volume as two additional measures. To compute the bid-ask spread, I follow Amihud and Mendelson (1986) who suggest the percentage bid-ask spread BA_t, which is, for a given stock, defined as

$$BA_t = \frac{P_t^A - P_t^B}{P_t}, \tag{1}$$

where P_t^A is the closing ask price on day t, P_t^B the closing bid price on day t and P_t the closing price on day t. The authors argue that this measure exhibits a negative correlation with other variables that are used to proxy liquidity such as trading volume or number of shareholders. The availability of daily bid-and ask prices on Datastream are another argument in favor of this measure.[23] A stock is considered more liquid, the lower the spread is. In this paper, I use the expressions “bid-ask spread”, “percentage bid-ask spread” and “spread” synonymously. I include the illiquidity measure introduced by Amihud (2002) and the dollar trading volume to complement the investigation on the effects on liquidity. For a given stock, the Amihud illiquidity ratio $ILLIQ_t$ is expressed as

$$ILLIQ_t = \frac{|r_t|}{P_t \cdot Vol_t}, \tag{2}$$

where $|r_t|$ is the absolute log-return on day t and Vol_t the number of shares traded on day t. The higher the Amihud illiquidity ratio, the more illiquid the stock. A stock is considered less liquid if ceteris paribus the daily dollar trading volume is low or the daily relative price change is high in absolute terms. There is no intuitive and straightforward meaning, though one can

[23] Note that despite the reasonably good data coverage, I still drop 1,466 out of 6,596 firms because they do not have enough data available or are excluded as part of the data cleaning.

interpret the Amihud illiquidity measure “as the daily price response associated with one dollar of trading volume, thus serving as a rough measure of price impact.”[24] To account for the very small numbers produced by this measure, I multiply it by 1,000,000. The third liquidity proxy is the dollar trading volume, which can be expressed as

$$DolVol_t = P_t \cdot Vol_t. \tag{3}$$

For a given stock on day t, it is defined as the number of traded stocks multiplied by the closing stock price, thus serving as a measure of turnover. Brennan et al. (1998) state that “this variable is associated with liquidity”[25] and Chordia et al. (2001) use the same measure to proxy liquidity in their paper. Note that, even though I use the term dollar volume, all my data is denoted in Euro currency. Furthermore, I compute a dummy variable to capture the effect of short sale bans. The dummy Ban equals one if a given stock is banned from short selling on day t and zero otherwise:

$$Ban_t = \begin{cases} 1 & if\ the\ stock\ is\ subject\ to\ a\ ban \\ 0 & zero\ otherweise. \end{cases} \tag{4}$$

Data. I use data from 28 European countries (27 EU countries + UK). This is somewhat different from other papers as the regulatory framework in all these countries is the same. Naked short sales have been forbidden in the EU since 2012 anyway and the threshold above which a short seller must disclose the short position is the same for all countries. Six out of 28 countries introduced short sale bans. Since the ban lasted two months, the time span of the data reaches from January 17, 2020, until May 18, 2020 (hereinafter referred to as the estimation period). I shift this period by two months in a later specification to test for effects of the ban lift. I retrieve daily close prices, as well as daily bid- and ask prices and daily trading volumes from Datastream. Additionally, the data set includes market value data for each firm as of January 1, 2020. For each country I apply the following filters: I select only the main national stock exchange, the domestic currency, major shares, and primary shares. The former makes sure that I include only the main equity instrument whereas the latter ensures that no share is included which is listed on a European stock exchange although the primary listing is outside Europe For example, Apple is also traded on the Milan Stock Exchange but is not subject to the investigation. Furthermore, I exclude ETFs and other securities that are traded as equity on a stock exchange but are not the major equity instrument of a publicly traded company. To compute Euro trading volumes and to rank firms according to their market value, all data is

[24] See Amihud (2002), p. 32.
[25] See Brennan et al. (1998), p. 351.

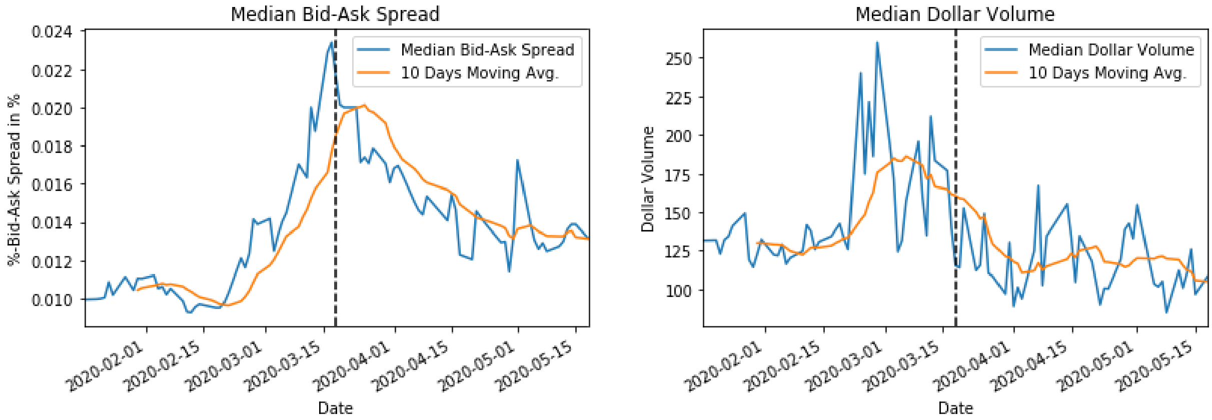
downloaded from Datastream in Euro. I exclude stocks for which the time series of the computed variables does not cover at least 80% of the days. Following related literature, I drop the top- and bottom 1% of the spread distribution from the (sub-) sample(s) to account for outliers and negative spreads, which, by definition, do not exist and do not make sense economically. After the raw dataset has been cleaned, I am left with a total of 5,130 stocks and 377,958 daily observations of the bid-ask spread. I apply the same data cleaning procedure to the other two variables for which I present summary statistics later in this section. For the bid-ask spreads, in 15.71% of the total daily observations, the ban dummy equals 1 and zero in the remaining 84.29%, indicating that roughly one-sixth of the stock-day observations were subject to the ban. Recall that only six out of 28 countries in the sample introduced a ban, which explains this relatively low number. Table 1 provides summary statistics of the three variables.

Table 1: Summary Statistics

This table provides descriptive statistics of the daily observations after the data adjustments described in the text of the three liquidity proxies (bid-ask spread, Amihud ratio, and dollar volume) during the period between January 17 and May 18. For practical reasons the Amihud ratio is multiplied by 1 million and the dollar volume is denoted in a thousand Euros. Although I use the term “Dollar Volume” as common in the literature, all values are denoted in Euro currency.

	Bid-Ask Spread	Amihud Ratio	Dollar Volume
No. of observations	377,958	267,781	316,103
Mean	0.0591	1,712	5,046
Std.	0.1392	5,946	15,751
Min	0.0000	0.0003	0.001
25%	0.0051	6,78	21.48
50%	0.0169	87.06	140.29
75%	0.0469	702.79	1,557
Max	1.3350	71,568	152,092
No. of stocks	5,130	4,007	4,032

The average bid-ask spread amounts to 5.91% while the median bid-ask spread is only 1.69%. This relation reflects the positive skewness of the sample and suggests that there are some extreme illiquid stocks in the sample which lead to a fat tail on the right-hand side of the distribution. Beber and Pagano (2013) report similar characteristics of their sample. The same applies to the Amihud illiquidity ratio where the mean is 1,712 whereas the median is only 87.06. The dollar volume exhibits similar characteristics, however, recall that here, an increase is associated with an increase in liquidity whereas an increase in the bid-ask spread or the Amihud ratio corresponds to a decrease in liquidity. The negative skewness indicates that there are some stocks that are very heavily traded and hence shift the mean to the right of the distribution. Amihud (2002) states that different measures capture different aspects of liquidity which may lead to the opposite skewness in the distribution of the dollar volume. The different numbers of daily observations are due to the data available on Datastream. For some stocks, closing price, bid-and ask prices as well as trading volume are available, for other stocks the dollar volume is missing and for some there are no data available at all.



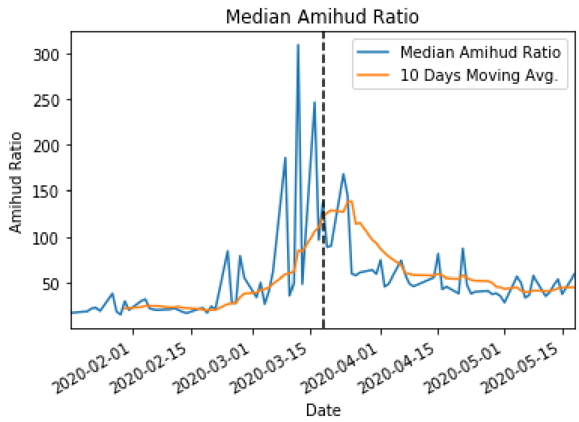


Figure 1: Overall Median Liquidity. Each of the three graph plots the median value of the corresponding liquidity proxy. The vertical dashed line indicates the inception date of the short sale ban. The orange line is the 10 days moving average of the cross-sectional median. Note that the Amihud Ratio is multiplied by 1 million to obtain decent numbers and the unit of the dollar volume is thousand Euros.

The number of firms included in the bid-ask spread analysis varies drastically among countries. While there are 1,143 stocks from the UK, no stocks from Malta are included in the analysis. The extreme case of Malta is attributed to the lack of available data. The distribution of stocks across countries is relatively similar for the two other variables. Figure 1 shows the median value and the corresponding 10-days moving average of each variable, respectively, of the entire sample from January 17 until May 18, 2020. It is observable that stock liquidity decreases around the peak of the stock market crash in Europe on March 12, 2020, no matter at which variable one looks. While bid-ask spreads and the Amihud ratio increase in the beginning of March but decline slowly in the following month again, the dollar volume tends to decrease at the beginning of March but stays at a constantly lower level afterward. The descriptive evidence suggests that stock liquidity declines during the hit of the Covid-19 crisis. Table 2 provides the median values of all liquidity proxies for the pre-and during-period as well as for banned- and non-banned stocks, respectively. No matter whether banned- or non-banned stocks, liquidity consistently deteriorates across all proxies significantly after the ban inception. The bid-ask spread of banned stocks increases by 38 basis points on average, whereas the mean difference for non-banned stocks is smaller, namely only 24 basis points. The same pattern applies for the Amihud ratio where the difference for banned stocks is 25.23 versus only 18.60 for non-banned stocks. The dollar volume of banned stocks declines by 48.01 while it declines by only 27.52 for non-banned stocks. The difference, however, is statistically significant at the 1% level for all groups. This inference is based on a Mann-Whitney-U test for the equality of medians. Therefore, the analysis requires a sharper approach that provides an estimate of the difference in the difference, also referred to as a “diff- in-diff” estimator. I conduct an in-depth analysis and implement an identification strategy using panel regression models.

Table 2: Median Liquidity

This table provides median values of all three liquidity proxies, respectively. The data is divided into the period “Pre” containing data of the two months prior to the ban and “During” containing data of the two months during the ban. “Banned” refers to stocks that are subject to the ban, i.e., stocks in the treatment group, whereas “Non-banned” refers to stocks not subject to the ban, i.e., stocks in the control group. The last row displays the difference between the pre-and during ban period. *, **, *** denote statistical significance at the 10%, 5%, and 1% level, respectively. Statistical inference is based on a Mann-Whitney-U test for the equality of medians.

	Bid-Ask Spread		Amihud Ratio		Dollar Volume	
	Banned	Non-banned	Banned	Non-banned	Banned	Non-banned
Pre	0.0119	0.0119	41.32	30.05	221.76	137.64
During	0.0157	0.0143	66.03	48.63	175.26	110.14
Difference	*0.0038****	*0.0024****	*24.71****	*18.58****	*-46.50****	*-27.50****

4.2 Methodology

The liquidity analysis aims to examine whether the liquidity of banned stocks, as measured by three different proxies, declined after the inception of short sale bans in six European countries during the Covid-19 crisis in spring 2020. This requires a diff-in-diff approach since I do not just want to test whether stock liquidity of banned stocks decreased but also whether this decrease was different from non-banned stocks. In the first approach, my treatment group consists of stocks from the six countries that imposed bans on short sales (banned stocks) while the control group comprises stocks from the remaining 22 countries where no ban was imposed (referred to as exempt stocks or non-banned stocks). I estimate a panel regression with fixed effects to control for individual stock characteristics as well as market-wide developments. To account for autocorrelation among residuals across time or firms, I use standard errors (SE) clustered by both firm and time introduced by Thompson (2011). In the baseline model, I regress the daily liquidity measure on the ban dummy using observations from all 28 countries.

If there is an increase (decrease) in bid-ask spreads of banned stocks as a consequence of the ban inception, one would expect the dummy coefficient to be positive (negative) and significantly different from zero. Beber and Pagano (2013) argue that dealers face higher inventory costs when holding more volatile stocks which they compensate by widening the spreads. Therefore, I follow their approach and add the rolling standard deviation of stock returns over the past 20 days as a control variable to account for the influence of risk on spreads.[26] The baseline model is the following:

$$LIQ_{i,t} = \beta_0 + \beta_1 \cdot Ban_{i,t} + \beta_2 \cdot Volatility_{i,t} + \varepsilon_{i,t}. \tag{5}$$

$LIQ_{i,t}$ is the liquidity proxy of stock i on day t, β_0 is the intercept (which is the same for all stocks), $Ban_{i,t}$ is the ban dummy that equals one if stock i is banned from short-selling on day t and zero otherwise, $Volatility_{i,t}$ is the 20-days rolling standard deviation of returns of stock i on day t and $\varepsilon_{i,t}$ is the error term of stock i on day t. β_1 and β_2 are the regression coefficients of the ban dummy and the volatility, respectively. Firm fixed effects are included in all specifications. I add time fixed effects to account for market-wide commonalities in the second specification. I do so to explicitly show that the estimates differ significantly when time effects are additionally controlled for.

4.3 Empirical Results

Table 3 provides the empirical results of the baseline regressions. In the first specification (1), I estimate the fixed effects model with firm fixed effects only, while I add day fixed effects in the second specification (2). I do so to demonstrate the impact that day fixed effects have on both estimated coefficients and estimated standard errors and subsequently on my statistical inference. In each regression, I control for autocorrelation and cross-sectional correlation among residuals by using standard errors that are clustered by both firm and time to obtain unbiased t-statistics. The variable of interest is the ban dummy coefficient which models the effect of a short sale ban. Looking at the first specification (1), the estimated coefficient of the ban dummy is significantly positive for the bid-ask spread and the Amihud ratio and significantly negative for the dollar volume at the 1% level. This confirms the hypotheses made at the beginning of section 4. However, the day fixed effects added in the second specification (2) lead to somewhat different estimates: The dummy coefficient of the bid-ask spread is no longer significant at the 1% level while the coefficient of the Amihud ratio is even negative, but not significant at the 1% level. This outlines the impact of time fixed effects on the

[26] See Beber and Pagano (2013), p. 360.

estimates. There is no major difference between the two specifications for the dollar volume. Each variable is affected differently by time fixed effects. The coefficient of *Volatility* is significant at the 1% level across all specifications and variables except for the bid-ask spread. The outputs indicate that this particular model might be miss-specified. Generally spoken, these results are not consistent across specifications and the three variables and are hence hard to interpret. They do not lead to a conclusion about the effects of short sale bans on stock liquidity.

Table 3: Liquidity Analysis Regression Output

This table provides the panel regression output of the baseline model. The dependent variable is the daily liquidity proxy (bid-ask spread, Amihud ratio, and dollar volume). For each variable, the difference between columns (1) and (2) is, additional to firm fixed effects, the inclusion of time fixed effects. The sample includes all stocks from all 28 countries. *Constant* is the intercept; *Ban* is the dummy that equals one if a stock is subject to a short sale ban and zero otherwise and *Volatility* is the 20-days rolling standard deviation of stock returns. Standard errors are clustered by firm and time. T-statistics are presented in parentheses below the parameter estimates. *, **, *** denote statistical significance at the 10%, 5% and 1% level, respectively.

	Bid-Ask Spread		Amihud Ratio		Dollar Volume	
	(1)	(2)	(1)	(2)	(1)	(2)
Constant	0.0499***	0.0582***	1,490.5***	2,183.9***	4,828.3***	5,175.3***
	(31.76)	*(33.33)*	*(26.04)*	*(33.50)*	*(25.43)*	*(94.50)*
Ban	0.0116***	0.0032*	272.85***	-136.35*	-2,194.3***	-1,644.1***
	(4.69)	*(1.69)*	*(2.77)*	*(-1.81)*	*(-6.28)*	*(-7.54)*
Volatility	0.1716***	-0.0048	4,414***	-10,920***	11,650***	2,216.6**
	(4.65)	*(-0.11)*	*(2.66)*	*(-7.00)*	*(5.01)*	*(2.27)*
Firm fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Day fixed effects	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
SE clustered by firm	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
SE clustered by time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
No. of observations	*377,263*	*377,263*	*267,781*	*267,781*	*315,019*	*315,019*
No. of firms	*5,130*	*5,130*	*4,007*	*4,007*	*4,032*	*4,032*

As a consequence, I apply a more sophisticated analysis following Boehmer et al. (2013). They argue that generally, a large share of shorting activity stems from high-frequency traders who are not officially registered as market makers and are therefore subject to the ban. These high-frequency traders focus on liquid stocks rather than on small, illiquid stocks, as they report no change in shorting activity after ban inception for small stocks. Therefore, I divide my sample into size quartiles and consider only large stocks defined as the largest 25% in terms of market value and small stocks defined as the smallest 25% in terms of market values. Market values are as of January 1, 2020 and drawn from Datastream in Euro for each stock. For each variable, I re-estimate the second specification for large stocks and small stocks, respectively. I leave out specification (1) for the sake of brevity. The results of the sub-sample analysis are shown in Table 4.[27]

Table 4: Liquidity Analysis Quartile Regression Output

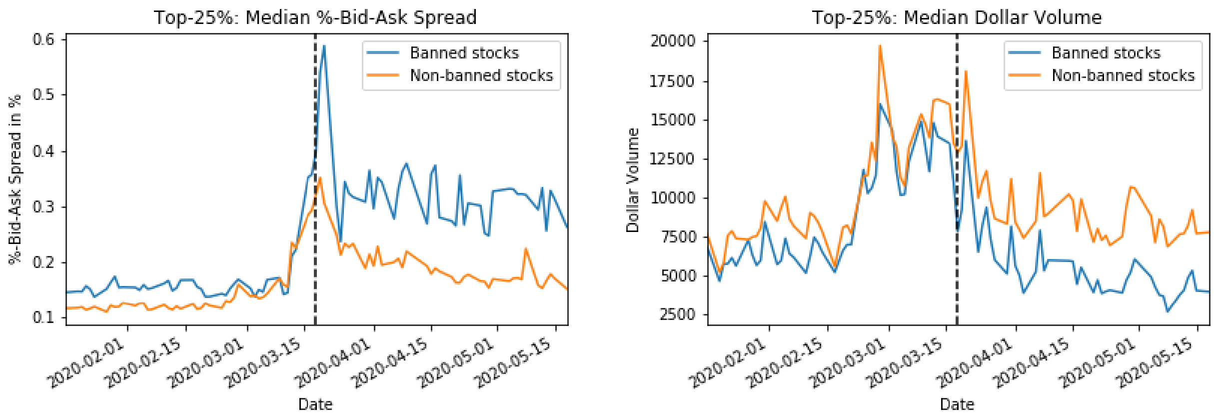
This table provides the panel regression output using only the extreme size quartiles of the sample. The dependent variable is the daily liquidity proxy (bid-ask spread, Amihud ratio, and dollar volume). For each variable, I estimate the model in equation (5) for the top 25% and bottom 25% in terms of size, respectively. Size is measured by the firm's market value as of January 1, 2020. I include time- and firm fixed effects. *Constant* is the intercept; *Ban* is the dummy that equals one if a stock is subject to a short sale ban and zero otherwise and *Volatility* is the 20-days rolling standard deviation of stock returns. Standard errors are clustered by firm and time. T-statistics are presented in parentheses below the parameter estimates. *, **, *** denote statistical significance at the 10%, 5% and 1% level, respectively.

	Bid-Ask Spread		Amihud Ratio		Dollar Volume	
Size Quartile	Top-25%	Bottom-25%	Top-25%	Bottom-25%	Top-25%	Bottom-25%
Constant	0.0038***	0.2675***	6.0448***	14,840***	24,360***	15.036***
	(32.71)	*(20.64)*	*(10.60)*	*(26.60)*	*(51.83)*	*(3.51)*
Ban	0.0011***	-0.0011	3.1739***	-661.36	-6,404.2***	16.726**
	(4.82)	*(-0.05)*	*(4.61)*	*(-0.82)*	*(-7.64)*	*(2.33)*
Volatility	0.0186***	-0.1518	89.912***	-39,360***	37,630***	599.35***
	(5.69)	*(-0.74)*	*(5.44)*	*(-4.34)*	*(2.81)*	*(9.64)*
Firm fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Day fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
SE clustered by firm	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
SE clustered by time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
No. of observations	*95,227*	*85,283*	*79,100*	*50,230*	*81,633*	*71,785*
No. of firms	*1,292*	*1,267*	*1,002*	*1,001*	*1,018*	*979*

The magnitude of the intercept among all regressions indicates that large stocks are indeed more liquid than small stocks across all liquidity dimensions. Note that this statement is based on the raw numbers of the estimated intercepts rather than on statistical inference regarding the differences between large- and small stocks. When considering only the largest size quartile, the estimated ban dummy coefficient is positive and significant at the 1% level for the bid-ask spread and the Amihud ratio and negative and significant at the 1% level for the dollar volume. On average, spreads increase by eleven basis points when a stock is banned from short selling. For the Amihud ratio, the estimated coefficient cannot be interpreted in a comparably straightforward way, however, it suggests that stock liquidity declines when a stock is banned from short selling. When looking at the dollar volume, the estimated coefficient of -6,404.2 suggests that, on average, a stock's trading activity declines when it is banned from short selling. All three estimates indicate a decline in liquidity of large stocks when short sale bans

[27] Corresponding results for the two remaining size quartiles are in Appendix Table 1.

are active. The large absolute t-statistics of 4.82, 4.61, and 7.64 underline the strong significance of these findings. When looking at the smallest 25% of firms in the sample, no effect can be observed. One exception is the dollar volume, where the ban dummy is even positive but only significant at the 5% level. No estimated dummy coefficient is significant at the 1% level which leads to the conclusion that small firms' stock liquidity is barely affected by a short sale ban. These findings are in line with Boehmer et al. (2013), who find effects for rather large stocks. Note that, for the sake of consistency, I include volatility as a control variable in the Amihud - and dollar volume regression analysis, although I provide economic intuition only for the impact on the bid-ask spread. However, volatility turns out to have some explanatory power for the other two variables as well, which is why I leave them included. The results, though, are qualitatively the same, no matter if volatility is included in the model or not. To further illustrate the findings, I plot the daily median values of all three variables during the estimation period for banned- and non-banned stocks, respectively. Figure 2 shows graphically the effect that the short sale ban has on the liquidity of large stocks. The dashed line in each of the graphs indicates the inception date of the ban.[28] It seems clear that, after ban inception, the median bid-ask spread of large banned stocks lies systematically above the median spread of non-banned stocks. The



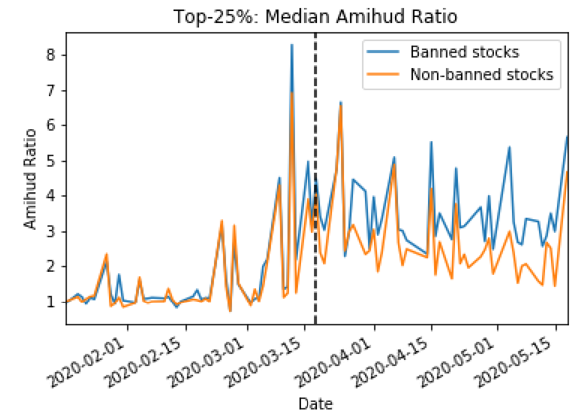


Figure 2: Liquidity Proxies over Time. For each variable, the median values of large banned stocks and large non-banned stocks, respectively, are plotted over time. The dashed line indicates the date of the ban inception on March 18, 2020. Note that I multiply the original Amihud ratios by 1m to obtain decent values. The dollar volume is expressed in a thousand Euros.

[28] I use March 18, 2020, as the overall inception date since this is the day when the majority of countries imposed the ban. The inception date was March 17 in Spain whereas it was March 19 in Austria.

same applies to the median Amihud ratio, even though the effect is not as clearly visible as it is for the bid-ask spread. The dollar median volume of banned stocks lies systematically below the median of non-banned stocks after ban inception, suggesting a large decline in trading activity when regulators impose short sale bans.[29] In the corresponding graphs that contain small firms, no comparable difference between banned- and non-banned stocks are observable. As explained in section 2, a large share of shorting volume is attributed to officially registered market makers and informal market makers such as high-frequency traders. As the latter are, along with all other short sellers, no longer allowed to enter short positions due to the ban, a significant share of the shorting activity disappears. The differences between the pre- and during median dollar volume for banned- and non-banned stocks are -46.5 and -27.5.[30] I interpret the difference of 19.0 as the median shorting activity that is normally attributed to informal market makers, hedge funds, and other short sellers that are subject to the ban.

4.4 Robustness Check

As explained above, so far, my treatment group has consisted of all stocks from the six countries that introduced a short sale ban whereas my control group comprised all stocks from the remaining 22 countries without a ban. To verify the robustness of the results in the previous section, I follow the methodology of Beber and Pagano (2013) by adapting the control group.[31] I use a matching algorithm that, for each banned firm in my sample, finds a non-banned firm with a similar stock price and market value. Therefore, I minimize the following distance metric D:

$$D = \left(\frac{P_B - P_M}{P_B}\right)^2 + \left(\frac{MV_B - MV_M}{MV_B}\right)^2, \tag{6}$$

where P_B is the stock price of the banned stock, P_M is the stock price on of the non-banned stock, MV_B is the market value of the banned stock and MV_M is the market value of the non-banned stock. Stock prices and market values are as of January 1, 2020. In other words, the algorithm minimizes the sum of squared relative deviation in stock price and squared relative deviation in market value and thus finds a matched non-banned firm for each banned firm that is similar in terms of stock price and market value. Like before, I consider only stocks that have at least 80% of the data available for the estimation period. For each banned observation, exactly one non-banned observation serves as a control data point. To obtain a diff-in-diff estimator, I

[29] Corresponding graphs for small stocks are in Appendix Figure 1.
[30] See Table 2 above.
[31] Beber and Pagano (2013) use an estimation period of 50 days before and 50 days after the inception date whereas my period after ban inception equals the whole ban period of two months and my (control-) period before inception subsequently equals two months as well, resulting in the same estimation period of four months (as above).

compute the difference of the liquidity proxy between banned- and non-banned stock for each day and regress it on the ban dummy to test whether this difference exhibits any change after ban inception. The fixed effects regression model can be written as

$$\Delta LIQ_{i,t} = \beta_0 + \beta_1 \cdot Ban_t + \varepsilon_{i,t}, \tag{7}$$

where $\Delta LIQ_{i,t}$ is the difference of the liquidity proxy between banned stock i and its matched stock on day t, β_0 the intercept, Ban_t the ban dummy on day t, β_1 its regression coefficient and $\varepsilon_{i,t}$ the error term of banned stock i on day t. I use the same robust standard errors as before and fixed effects for each stock pair. If banned stocks are more affected by the ban than non-banned stocks, I expect the ban dummy coefficient to be significantly positive for the bid-ask spread and the Amihud ratio and significantly negative for the dollar volume. Depending on the liquidity proxy, the number of included stock pairs ranges from 1,123 to 1,545. Note that the control group is much smaller than in my previous analysis and so is therefore the number of observations in the regression. Table 5 shows the estimation output of equation (7) for all three variables.

Table 5: Robustness Check Regression Output

This table provides the panel regression output of the robustness analysis. The dependent variable is the daily difference of the liquidity proxy between matched stock i and its corresponding matched non-banned stock. The sample comprises all banned stocks and their matched non-banned stocks. Matched stocks are selected by running an algorithm that minimizes the distance metric described in the text. *Constant* is the intercept; *Ban* is a dummy variable that equals one if day t lies in the ban period and zero otherwise. Standard errors are clustered by stock-pair and time. Fixed effects for each stock pair are included. T-statistics are presented in parentheses below the parameter estimates. *, **, *** denote statistical significance at the 10%, 5% and 1% level, respectively.

	Bid-Ask Spread	Amihud Ratio	Dollar Volume
Constant	-0.0388***	179.93	1,382.5***
	(-16.26)	*(0.38)*	*(6.13)*
Ban	0.0052	417.73	-2,562.7***
	(1.03)	*(0.48)*	*(-6.75)*
Pair fixed effects	*Yes*	*Yes*	*Yes*
Day fixed effects	*No*	*No*	*No*
SE clustered by firm	*Yes*	*Yes*	*Yes*
SE clustered by time	*Yes*	*Yes*	*Yes*
No. of observations	*128,197*	*93,209*	*93,209*
No. of stock pairs	*1,545*	*1,123*	*1,123*

The ban dummy coefficients of the bid-ask spread and the Amihud ratio are not statistically significant, however, the results suggest that the dollar volume sharply decreases due to a short sale ban as indicated by the large absolute t-statistic of 6.75. I conclude that, by looking at all banned stocks, my results suggest that stock liquidity is only affected by a short sale ban when using the dollar volume as a proxy. The output is somewhat similar to the one in the baseline regression from section 4.3 where no clear pattern could be seen across the three liquidity proxies. Deeper analysis in showed that the liquidity of only very large stocks is negatively affected by a short sale ban.

To test whether this finding is robust, I divide the sample of all banned stocks into four size quartiles to estimate the regression model separately for both extreme quartiles. The results of the regressions for the extreme size quartiles, respectively, are presented in Table 6.[32]

Table 6: Robustness Check Quartile Regression Output

This table provides the panel regression output of the robustness analysis. The sample is divided into large banned firms in the top quartile of the size distribution and small banned firms in the bottom quartile of the size distribution. Size is measured by the firm's market value as of January 1, 2020. The dependent variable is the daily difference of the liquidity proxy between matched stock i and its corresponding matched non-banned stock. Matched stocks are selected by running an algorithm that minimizes the distance metric described in the text. *Constant* is the intercept; *Ban* is a dummy variable that equals one if day t lies in the ban period and zero otherwise. Standard errors are clustered by stock-pair and time. Fixed effects for each stock pair are included. T-statistics are presented in parentheses below the parameter estimates. *, **, *** denote statistical significance at the 10%, 5% and 1% level, respectively.

	Bid-Ask Spread		Amihud Ratio		Dollar Volume	
Size Quartile	Top-25%	Bottom-25%	Top-25%	Bottom-25%	Top-25%	Bottom-25%
Constant	0.0024***	-0.1596***	-4.0010*	1,116.9	5,496.8***	148.81***
	(7.28)	*(-17.10)*	*(1.79)*	*(0.60)*	*(6.41)*	*(7.82)*
Ban	0.0020***	0.0123	4.2431	1,655.4	-9,893.0***	109.12***
	(2.76)	*(0.63)*	*(0.95)*	*(0.48)*	*(-6.98)*	*(2.62)*
Pair fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Day fixed effects	*No*	*No*	*No*	*No*	*No*	*No*
SE clustered by firm	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
SE clustered by time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
No. of observations	*32,038*	*32,025*	*23,323*	*23,323*	*23.323*	*23,323*
No. of stock pairs	*386*	*386*	*281*	*281*	*281*	*281*

First, I look at the sub-sample of only large firms, i.e., at the left column. For the bid-ask spread, the dummy coefficient is positive and significant at the 1% level. This indicates that the difference in liquidity between banned and matched stock increases after the ban inception. Precisely spoken, the difference increases by 20 basis points on average. This finding fully confirms the results from the previous section. The bid-ask spread of large firms declines after the inception of a short sale ban. The analysis of the Amihud ratio yields slightly different

[32] Corresponding results for the remaining two size quartiles are in Appendix Table 2.

results. The dummy coefficient is positive, but not significant, which is why I cannot confirm previous results, even though I cannot reject them, either. This result somehow reflects the visual evidence from Figure 2, where the ban effects were least clearly observable for the Amihud ratio. Noisy data might be a possible explanation for the insignificance. Contrary to this, the analysis of the dollar volume yields again very clear results. With a t-statistic of -6.98, the dummy coefficient is negative and highly significant. This result strongly confirms my findings above. Trading activity, as measured by the dollar volume, of large stocks declines after the inception of a short sale ban. Figure 3 provides graphs that plot the mean difference between banned- and matched large stocks for each variable, respectively, over time. Note that contrary to previous graphs, I use the mean instead of the median as the results are somewhat more visible. Similar graphs with median values are in Appendix Figure 2. For the bid-ask spread, one can observe a systematic increase in the difference after ban inception. For the Amihud ratio, there is no visible increase of the difference after ban inception. This is not surprising given the low t-statistic of 0.95 of the ban dummy coefficient in Table 6. This particular result does not support previous findings, however, the positive coefficient does not contradict previous findings, either. The visual results of the dollar volume are again very clear and leave little to no space for interpretations. One can observe a clear drop in the difference of

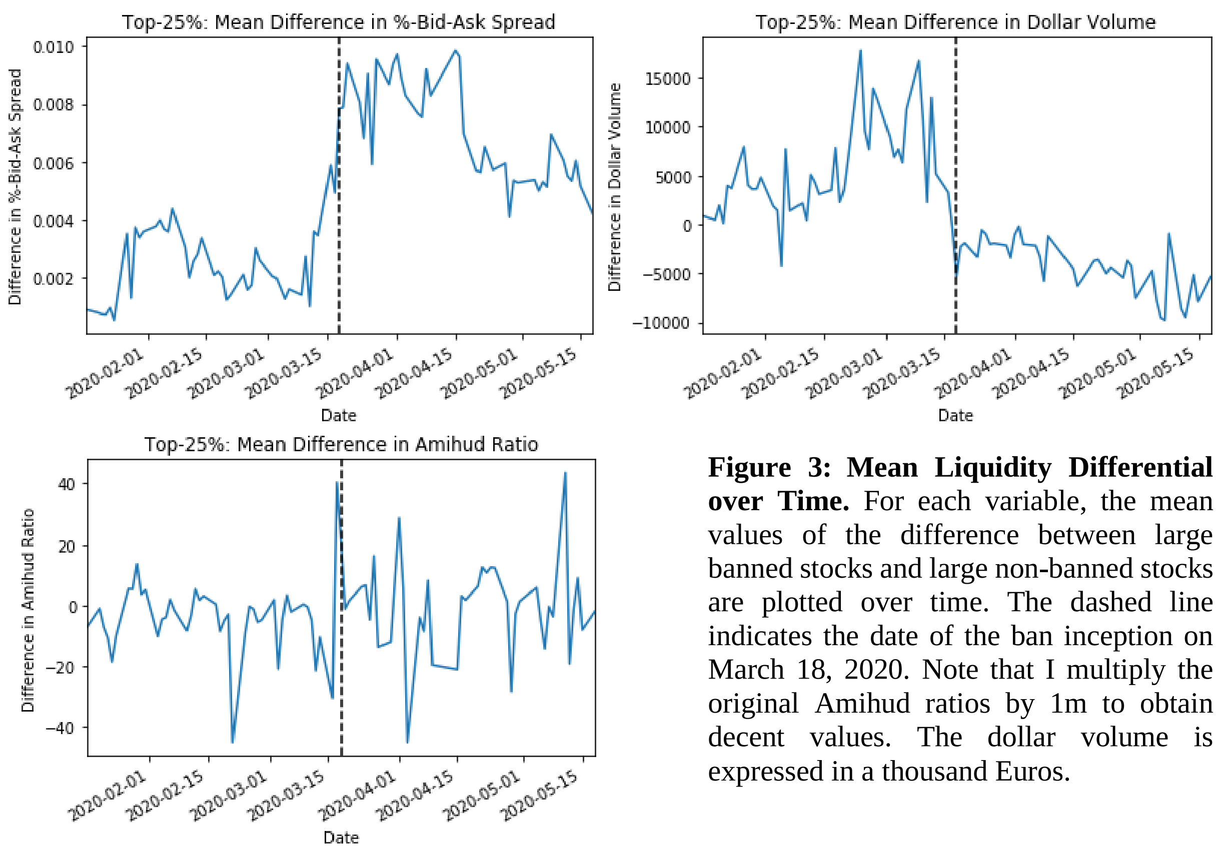


Figure 3: Mean Liquidity Differential over Time. For each variable, the mean values of the difference between large banned stocks and large non-banned stocks are plotted over time. The dashed line indicates the date of the ban inception on March 18, 2020. Note that I multiply the original Amihud ratios by 1m to obtain decent values. The dollar volume is expressed in a thousand Euros.

dollar volume between banned- and matched large stocks after ban inception. This change is reflected by the reasonably large t-statistic of -6.95 in Table 3. Overall, these results almost entirely confirm and certainly do not contradict previous findings and hence provide robust evidence for a drop in liquidity of large stocks due to short sale bans.

4.5 Effects of the Ban Lift

In line with previous literature, my findings suggest that liquidity of large stocks declines after the inception of a short sale ban. This raises the question of what happens when short sale bans are lifted again. If the drop in liquidity is indeed due to the ban, reverse effects should take place after the ban lift. As described above, the short sale bans in the six European countries were either lifted on May 18, 2020 or expired that day.[33] Therefore, I shift the estimation period by two months. The previous estimation period was January-May (pre- and during the ban) whereas the new adjusted estimation period is March-July (during- and post-ban). This allows me to identify precisely the liquidity differential between the ban period and the post-ban period. I use a dummy *Ban Lift* that equals one if a previously banned stock is no longer subject to a ban and zero otherwise. This dummy ought to capture all effects of the ban lift on stock liquidity. If the previously described liquidity effects are indeed due to the ban inception, I expect the lift dummy to be negative and significant for the bid-ask spread and the Amihud ratio. On the other hand, I expect it to be positive for the dollar volume as I expect that liquidity increases again once the ban is lifted. I regress the daily liquidity proxy on the lift dummy and on the volatility as a control variable using a panel regression with time- and firm fixed effects as well as standard errors clustered by both firm and time.

Table 7 presents the corresponding regression output. Starting with the bid-ask spread, the dummy coefficient is negative and significant at the 1% level. The output suggests that, on average, bid-ask spreads of large firms decreased by 12 basis points, thus, indicating an increase in liquidity again after the ban lift. The ban lift entirely reverses the liquidity effects of 11 basis points caused by the ban inception (see Table 3). A similar picture arises for the Amihud ratio. The estimated ban lift effect of -1.86 is statistically significant at the 1% level. The results suggest that the ban lift partly reverses the liquidity effects resulting from the ban inception where the estimated dummy coefficient was 3.17. As in my previous analysis, an even clearer picture evolves from the dollar volume estimation results. The estimated coefficient suggests that the dollar volume increases, on average, by 4,501,200€ after the ban lift, thereby partly

[33] For all countries but Italy, the short sale bans simply expired, whereas in Italy it was actively lifted earlier. Initially, the ban was supposed to be active for a period of three months in Italy.

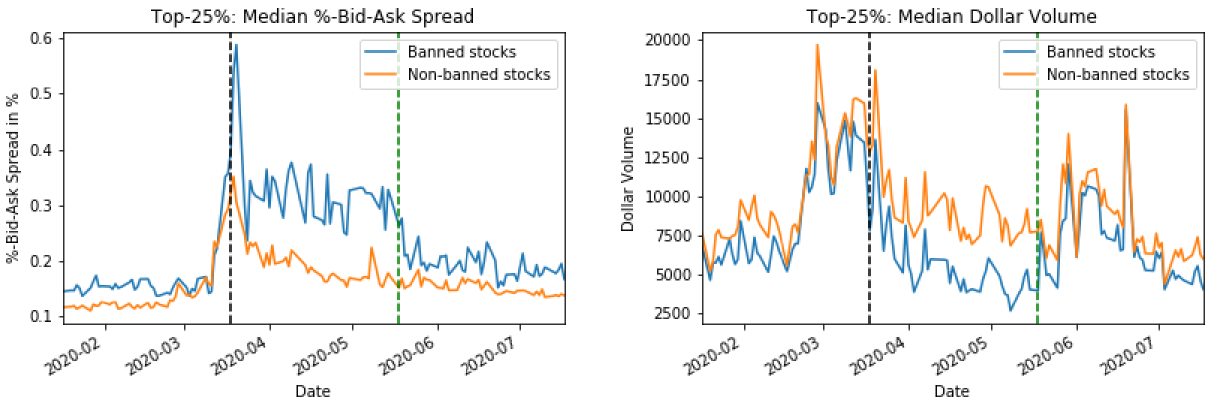
inverting the negative effects of the ban inception on the dollar volume. According to the regression estimates, spreads return to a pre-ban level once the ban is lifted again while the Amihud ratio does not decrease as strong as it increased after ban inception. The dollar volume stays below its pre-ban value indicating less trading activity post-ban than pre-ban. Overall, the results are entirely in line with my predictions made ex-ante and hence further confirm the hypothesis that short sale bans damage the liquidity of large stocks. To illustrate these results graphically, I plot the liquidity proxies over the entire period, i.e., from January until July in Figure 4.

Table 7: Effects of Ban Lifts

This table provides the panel regression output of the analysis of ban lifts for each liquidity proxy, respectively. The sample used for the estimation consists of all stocks in the top size quartile of the original sample. Size is measured by the firm's market value as of January 1, 2020. The dependent variable is the daily liquidity proxy. *Constant* is the intercept; *Ban Lift* is a dummy that equals one if a previously banned stock is no longer subject to a ban and zero otherwise. *Volatility* is the 20-days rolling standard deviation of stock returns. Standard errors are clustered by firm and time. I include time- and firm fixed effects. T-statistics are presented in parentheses below the parameter estimates. *, **, *** denote statistical significance at the 10%, 5% and 1% level, respectively.

Top-25%	Bid-Ask Spread	Amihud	Dollar Volume
Constant	0.0049***	8.9400***	21,900***
	(45.16)	*(13.74)*	*(9.60)*
Ban Lift	-0.0012***	-1.8592***	4,501.2***
	(-4.67)	*(-2.96)*	*(7.63)*
Volatility	0.0092***	58.12***	-30.050
	(3.52)	*(3.33)*	*(-0.48)*
Firm fixed effects	*Yes*	*Yes*	*Yes*
Day fixed effects	*Yes*	*Yes*	*Yes*
SE clustered by firm	*Yes*	*Yes*	*Yes*
SE clustered by time	*Yes*	*Yes*	*Yes*
No. of observations	*95,165*	*78,225*	*80,938*
No. of firms	*1,292*	*1,002*	*1,018*

The first (second) dashed line indicates the ban inception (lift). One can clearly observe that spreads of banned- and non-banned stocks diverge during the ban period and converge after ban lift. A similar image arises for the Amihud ratio. The Amihud ratio of banned- and non-banned stocks is more or less identical during the pre- and post-ban period, but the blue line lies slightly above the orange line during the ban period. As for the dollar volume, one can observe an identical pattern as for the other two variables. While trading activity before and after the ban period is nearly identical for banned- and non-banned stocks, the blue line lies systematically below the orange line during the ban period. Overall, these findings provide further evidence that stock liquidity of large stocks deteriorates due to the inception of short sale bans.



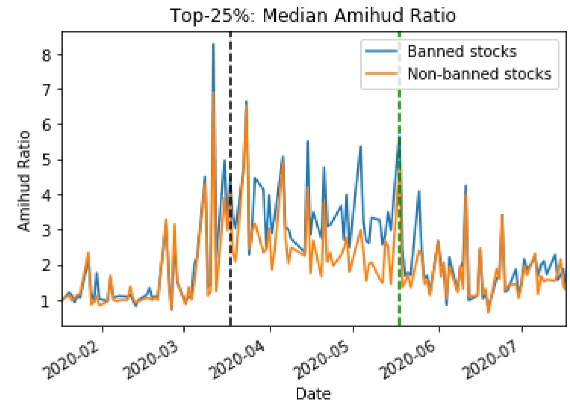


Figure 4: Liquidity Proxies over the whole Period. For each variable, the median values of large banned- and non-banned stocks, respectively, are plotted over the entire period. The black dashed line indicates the date of the ban inception on March 18, 2020 and the green dashed line indicates the date of the ban lift on May 18, 2020. Note that I multiply the original Amihud ratios by 1m to obtain decent values. The dollar volume is expressed in a thousand Euros.

4.6 Country-by-Country Analysis

In this section, I further divide my sample into sub-samples by estimating the dummy coefficients individually for each country with short sale bans.[34] The goal of this analysis is to examine whether the drop in liquidity caused by short sale bans is consistent across countries or if the effects stem from a few specific countries. To estimate the model for Austria only for example, I use all stocks of the 22 countries without ban in addition to Austrian stocks. I include only firms of the largest size quartile of this sub-sample in the regression. I do so for all six countries to obtain country-specific estimates. Note that this decreases the share of banned stocks in the sample. Table 8 reports the results of the country-by-country analysis. For each country and variable, I estimate one regression with a ban inception dummy and another one with a ban lift dummy and report the corresponding estimates of the dummy coefficients. The ban inception dummy is equal to the ban dummy in previous tables whereas the lift dummy is equal to the ban lift dummy from above. Starting with the bid-ask spread, three out of six

[34] I still include *Volatility* as a control variable but do not explicitly report it for the sake of brevity.

countries exhibit statistically significant estimates of the inception dummy at the 1% level. Austria's dummy is significant at the 5% level while Italy and Spain produce positive but insignificant estimates. As for the Amihud ratio, a similar picture evolves, i.e., only three out of six countries show significant results at the 1% level. The remaining three countries show positive but insignificant estimates. The inception dummy's coefficients of three countries for the dollar volume are negative and significant at the 1% level. Two out of the remaining three countries show the expected negative sign, even though none of the coefficients is significant. The most surprising exception is Austria with a positive coefficient. This would suggest that short sale bans lead to an increase in the trading volume of banned stocks which is not in line with all other findings so far. Note that the positive coefficient is not significant and should therefore not be overinterpreted. Overall, all countries but Austria show significant estimates at the 1% level for at least one liquidity proxy. As described above, liquidity is no straightforward construct and exhibits several dimensions. This might explain why different countries produce significant results for different variables.

Table 8: Country-by-Country Estimates

This table provides estimates of the inception dummy and lift dummy, respectively, for all countries and liquidity proxies. The sample used for the estimation consists of stocks in the top size quartile from all countries without ban and the one respective country with ban. Size is measured by the firm's market value as of January 1, 2020. The included period for the column "Inception" is pre- and during-ban whereas the included period for the column "Lift" is during- and post-ban. The daily liquidity proxy is regressed on the corresponding dummy (either *Inception* or *Lift*) and on volatility (as a control variable, though not reported, nor is the regression intercept). Standard errors are clustered by firm and time. I include time- and firm fixed effects. T-statistics are presented in parentheses below the parameter estimates. *, **, *** denote statistical significance at the 10%, 5% and 1% level, respectively.

	Bid-Ask Spread		Amihud Ratio		Dollar Volume	
	Inception	Lift	Inception	Lift	Inception	Lift
Austria	0.0011**	-0.0015***	3.4258	2.6625	596.84	2,338.4***
	(2.47)	*(-3.83)*	*(1.50)*	*(1.40)*	*(0.69)*	*(3.29)*
Belgium	0.0015***	-0.0010**	4.2284***	-2.9265**	-2,059.6*	3,769.1***
	(4.38)	*(-2.46)*	*(3.11)*	*(-2.43)*	*(-1.67)*	*(4.80)*
France	0.0015***	-0.0012***	3.5756***	-1.8651**	-9,076.4***	4,330.1***
	(4.97)	*(-4.61)*	*(2.94)*	*(-2.05)*	*(-5.51)*	*(4.38)*
Greece	0.0012***	-0.0010**	3.7880	-5.9556***	-277.02	2740.0***
	(3.12)	*(-2.51)*	*(1.51)*	*(-2.69)*	*(-0.25)*	*(3.21)*
Italy	0.0003	-0.0015**	1.6458	-2.4914**	-6,892.8***	5,245.5***
	(0.61)	*(-2.11)*	*(1.44)*	*(-2.37)*	*(-4.41)*	*(5.94)*
Spain	0.0004	-0.0005*	4.7267***	-3.5087***	-5,029.0***	4,525.2***
	(1.57)	*(-1.87)*	*(3.24)*	*(-2.79)*	*(-4.14)*	*(5.42)*

A slightly more robust picture evolves from the lift dummy coefficients. For the bid-ask spread, two countries show significant estimates at the 1% level, while the rest produces results that are significant at least at the 10% level or better. For the Amihud ratio, all countries but Austria produce results that are at least significant at the 5% level. The clearest and most unambiguous results are those for the dollar volume. All countries show positive estimates that are significant at the 1% level. This result is consistent with previous findings where the most robust effects are produced by the dollar volume. Overall, these findings suggest that liquidity of large, banned stocks increase in all countries once the ban is lifted, even though some estimates are insignificant. The most robust results are those produced by French stocks where all coefficients but one are significant at the 1% level. My country-specific estimates suggest that, although the effects appear to be quite significant in the panel analysis, the data is still somewhat noisy on a country level. The effects of the ban lift are more robust than the effects of the ban inception.

Financial markets were highly volatile around the ban inception in March when the Covid-19 pandemic was about to spread across entire Europe, while markets had considerably calmed down by the end of the ban. The volatility index VSTOXX had its peak on March 16 at 86, while it had fallen to roughly 30 by mid-May. An explanation for the noisy data may be irrational investors who were very active at the beginning of the crisis in March but no longer engaged in trading during May, thus contributing to less volatile markets and hence less noisy data.

5 Price Efficiency

In this section, I test the hypothesis made by Diamond and Verrecchia (1987) that stock prices adjust more slowly to information when short sales are prohibited by the regulator. According to Fama (1970) and his famous efficient market hypothesis, stock prices reflect all available information at any time if the market is efficient. The idea behind the theory in this section is, that bearish investors who do not own stock in a company cannot trade on their negative information as they can neither sell any stocks from their portfolio nor short-sell stocks. This implies that short sale bans can reduce market efficiency and stock prices are therefore expected to adjust more slowly to both idiosyncratic and market-wide information.

Data. While I use daily data in the liquidity analysis, I use weekly data in this section instead, following Hou and Moskowitz (2005) who argue that weekly stock returns exhibit a decent mix between noise and information. While daily stock data is exposed to too many confounding influences, monthly data would not be very helpful as stock prices react to information normally within one month. Weekly log-returns are measured between Wednesdays. This is motivated by findings from Keim and Stambaugh (1984) who report a pattern of large autocorrelation for Friday-to-Friday and Monday-to-Monday returns.[35] I winsorize the return data at the bottom- and top 1% to account for outliers, thereby eliminating all returns greater than 31.02% and smaller than -26.51%. Furthermore, I identify zero-returns and drop them to avoid nontrading bias. I also drop every firm for which the time series of the estimation period is not entirely complete as I could not compute autocorrelations otherwise. I am left with 31,936 weekly observations from a total of 1,996 companies. The original data retrieved from Datastream comprises a total of 6,767 firms. The drastically lower number of remaining firms in the final data set is due to the data cleaning described and the many error columns with no data at all or firms with missing prices which result in a missing return data point. The weekly mean-and median return equal -.175% and -.244%, respectively. Detailed descriptive statistics and a histogram of returns are in the appendix.[36]

Methodology. As described above, it is not trivial to capture private information. To investigate firm-specific information and the speed with which stock prices adjust to it, I follow Beber and Pagano (2013) and estimate a market model to analyze the residuals (which serve as a proxy for idiosyncratic information). The intuition is the following: The higher market model residuals are autocorrelated, the more slowly idiosyncratic information is incorporated into

[35] Beber and Pagano (2013) and Hou and Moskowitz (2005) compute their returns using Wednesdays as well.
[36] See Appendix Table 6 and Appendix Figure 4.

stock prices. I use the STOXX 600 as the overall market index[37]. For a given stock, the market model can be written as:

$$R_t = \alpha + \beta \cdot R_t^m + \varepsilon_t, \quad (8)$$

where R_t is the stock return in week t, R_t^m the market return in week t and ε_t the residual in week t. The regression coefficients are denoted by α (intercept) and β (slope). The estimation method is standard Ordinary Least Squares (OLS). Based on theoretical predictions, I expect the autocorrelation of residuals higher for stocks that are subject to a ban than for control stocks.

In the second part of the analysis, I investigate how quickly prices adjust to market-wide information. I carry out the analysis on a measurement that proxies price delay. Theoretically, one would expect prices to adjust more slowly to market-wide information in the presence of short sale bans, especially during market downturns. I follow Hou and Moskowitz (2005) to construct a normalized measure that implicitly takes into account the nature of the diff-in-diff estimation. The idea is that, if prices adjust very quickly, past market returns do not contain much explanatory information regarding individual stock returns. Additional lagged market returns would then not add explanatory power expressed by the R^2.[38] For each stock, I therefore estimate a) a restricted market model with only the contemporaneous market return as the explanatory factor and b) the same market model additionally including the prior week's market return as an explanatory factor. The models can be written as

$$R_t = \alpha + \beta \cdot R_t^m + \varepsilon_t, \quad \text{(9a)}$$

$$R_t = \alpha + \sum_{i=0}^{1} \beta_i \cdot R_{t-i}^m + \varepsilon_t. \quad \text{(9b)}$$

For each stock, I report the R^2 from model a) and model b). The estimation method is standard OLS. To normalize the R^2s of the unrestricted- and the restricted model, I compute a price delay measure d_1 for each stock that can be written as

$$d_1 = 1 - \frac{R^2_{restricted}}{R^2_{unrestricted}}, \quad (10)$$

[37] The STOXX 600 includes large-, mid-, and small caps as well as British firms unlike the well-known EURO STOXX 50. Choosing the STOXX 600 seems reasonable given the size structure and the large share of UK firms in the data set.
[38] Recall that the R^2 of an OLS estimation equals the percentage of variation in the dependent variable that can be explained by the variation in the independent variable.

where $R^2_{restricted}$ results from the restricted model with only the contemporaneous as explanatory factor and $R^2_{unrestricted}$ comes from the unrestricted market model with the lagged market return as an additional independent variable. I compute this measure for banned stocks and control stocks, respectively. The higher d_1, the more explanatory power lagged market returns have, and the more slowly stock prices adjust to market-wide information. Note that Hou and Moskowitz (2005) use the market return up to lag four as explanatory factors. As the ban period in my case lasted "only" close to nine weeks, I would lose four weeks of data if I included all four lags. Therefore, I choose the number of lags to be one. I give a detailed explanation of the computations in Appendix Table 5. Table 9 provides the empirical results of both analyses.[39]

Table 9: Price Efficiency

This table provides the results of the price efficiency analysis. The left panel called "Residual Autocorrelation" shows the median autocorrelation of market model residuals during ban- and non-ban periods, respectively, for the entire sample as well as for the top- and bottom size quartile. The right panel called "Price Delay Measure d_1" shows the median value of the price delay measure described in the text for banned- and non-banned observations, respectively, for the entire sample as well as for the top- and bottom size quartile. Size is measured by the firm's market value as of January 1, 2020. *Ban* is a dummy that equals 1 if the observations are in the ban period and zero otherwise. Two-sided p-values of the test for the equality of medians between banned- and non-banned observations are shown in parentheses. They are computed based on a Mann-Whitney-U test. The number of all firms included is presented in the bottom row.

	Residual Autocorrelation			Price Delay Measure d_1		
Size quartile	All	Top-25%	Bottom-25%	All	Top-25%	Bottom-25%
Ban = 0	-0.11	-0.15	-0.11	0.06	0.03	0.14
Ban = 1	-0.15	-0.14	-0.15	0.18	0.07	0.43
Difference	(0.026)	(0.75)	(0.48)	(0.000)	(0.000)	(0.000)
No. of firms	*1,996*	*500*	*500*	*1,996*	*500*	*500*

The autocorrelation of residuals is always negative across all (sub-) samples. For the entire dataset, the autocorrelation is more negative during ban periods (-0.15) compared to non-ban periods (-0.11), thereby supporting the hypothesis that it takes longer for idiosyncratic information to be impounded into prices. I test for the difference of the medians using a Mann-Whitney-U test. The medians of banned- and non-banned stocks are statistically different from each other only at the 5% level. I find practically no significant results for very large and very small stocks reflected by the high p-values of 0.75 and 0.48. These findings suggest that the speed with which prices incorporate firm-specific information does not change due to short sale

[39] Results for the remaining two size quartiles are in Appendix Table 3.

bans. This result is somewhat surprising and contrary to the findings of Beber and Pagano (2013). Another interesting difference are the signs of the autocorrelations. Those reported by Beber and Pagano (2013) are positive whereas mine are negative. Recall that my estimation period is only four months leaving only 16 weekly observation points per stock which might be an explanation for these somewhat odd results.

A much clearer picture evolves from the price delay measure d_1 which captures the speed with which stock prices adjust to market-wide information. When looking at the entire sample, the price adjustment is much quicker for non-banned observations (0.06) than for banned observations (0.18). As for the largest 25% of the firms, the price delay measures are much smaller (0.03 and 0.07) but still significantly different from each other at the 1% level. The intuition behind the small numbers is that large stocks are normally reasonably liquid and therefore incorporate information relatively quickly anyway, no matter if banned or not. Small stocks are less liquid and can therefore not adjust to new information as quickly. This is reflected by the large values of 0.14 for non-banned observations and 0.43 for banned observations. The medians differ significantly from each other at the 1% level. The results remain qualitatively the same when I use a standard t-test to test for the equality of means. Overall, the results suggest that stock prices do not adjust more slowly to idiosyncratic, i.e., firm-specific information, when they are subject to a short sale ban. They do, however, adjust more slowly to market-wide information when a short sale ban is in place. This is not only consistent with theoretical predictions made by Diamond and Verrecchia (1987), but also with empirical findings from Beber and Pagano (2013), even though their price delay measure is a different one.

Bull- and Bear Markets. As the regulator intends to limit the activity of bearish investors when imposing a ban on short selling, the price adjustment process should slow down particularly during declining markets. Therefore, I divide the sample into "bear markets" containing only negative market returns and into "bull markets" containing only positive market returns. This return refers to the lagged return. The intuition behind this approach is the following: If the market return of week t-1 is negative, bearish investors cannot short-sell single stocks. A portion of the negative market-wide information is not impounded into prices immediately, which delays the price adjustment process. The stock return of week t=0 should therefore contain still a significant share of previous week's market-wide information. In other words, the R^2 of market models where the lagged market return is negative should be greater than when the lagged market return is positive, given a short sale ban. Out of the 16 weeks, exactly nine lagged market returns are positive and seven are negative. I compute the price delay measure d_1 again for bear- and bull markets, respectively. If the price adjustment slows down indeed more intensively during bear markets, I expect the difference in d_1 between banned- and non-banned observations to be greater for bear markets than for bull markets. Table 10 presents the corresponding results.[40] For the bull markets, the level of all numbers seems to be overall slightly higher. Across all size groups, the price delay is larger for banned- than for non-banned observations. Consistent with the previous table where I have included all market returns, small firms exhibit overall a higher price delay than large firms, as small firms' stocks are less liquid and can therefore not adjust reasonably quickly to new information. The same applies to bear markets. Across all size groups, the differences between banned- and non-banned observations are, however, not larger as expected, but rather even slightly lower for bear markets. For the top size quartile for example, the difference for bull markets is 0.24 (0.30-0.06), whereas it is only 0.06 (0.12-0.06) for bear markets.

Table 10: Bull-and Bear Markets

This table provides the results of the price delay measures during bull- and bear markets, respectively. For the entire sample as well as for the top- and bottom size quartile, it contains the median values of the price delay Measure $d_1$for banned- and non-banned observations. In the left panel called "Bull Markets", I include only weeks with positive lagged market returns whereas in the right panel "Bear Markets", I include only weeks with negative lagged market returns. Size is measured by the firm's market value as of January 1, 2020. *Ban* is a dummy that equals 1 if the observations are in the ban period and zero otherwise. Two-sided p-values of the test for the equality of medians between banned- and non-banned observations are shown in parentheses. They are computed based on a Mann-Whitney-U test. The number of all firms included is presented in the bottom row.

	Bull Markets			Bear Markets		
Size quartile	All	Top-25%	Bottom-25%	All	Top-25%	Bottom-25%
Ban = 0	0.12	0.06	0.21	0.10	0.06	0.18
Ban = 1	0.44	0.30	0.51	0.22	0.12	0.35
	(0.000)	(0.000)	(0.00)	(0.000)	(0.000)	(0.004)
No. of firms	*1,996*	*500*	*500*	*1996*	*500*	*500*

These findings suggest that the price adjustment process does indeed slow down due to a short sale ban. This efficiency loss, however, is not stronger for bear markets than for bull markets. One possible explanation for this lacking evidence is the small number of observations per firm. The entire estimation period comprises 17 weeks. I exclude the last week before the ban period as the data would overlap otherwise. The subsample for bear markets contains only seven weeks. To compute the price delay of banned stocks for bear markets before the ban, the regression contains only three observations per stock, namely those three where the lagged

[40] Results for the remaining two size quartiles are in Appendix Table 4.

market return is negative. During the ban, only four observations are included in the regression. I provide a detailed explanation of the computation in the appendix. Although the number of included stocks is considerably large, the number of observations per stock still limits the meaningfulness of this sub-analysis.

6 Stock Prices

The last part of my empirical analysis focusses on the development of prices of banned stocks and how it differs from non-banned stocks. Miller (1977) argues that stocks that are banned from short selling should, on average, be overpriced. Investors who do not own stock but believe it is overpriced cannot incorporate their beliefs into stock prices leaving the stock overpriced. A price reversal should take place and the mispricing should be corrected once the short sale ban is lifted again. Diamond and Verrecchia (1987), however, argue that rational investors consider the fact that prices cannot be corrected by short sellers. Therefore, stock prices are not overpriced. Following the majority of studies explained in section 3, I hypothesize that stock prices of banned stock are indeed not upwards biased. As for the methodology, I follow Chang et al. (2007) to compute abnormal returns, however, I use weekly data instead of daily data. They conduct an event study with a 20-days event window whereas I apply the identification strategy of Beber and Pagano (2013) to estimate a similar model as for the liquidity analysis above with an estimation period of four months. This yields a reasonable number of observations for the regression analysis. I proceed as follows: I use the STOXX 600 as the overall market index and estimate a market model to obtain model parameters for each stock using 52 weeks before the estimation period. To compute weekly abnormal returns for the estimation period, I subtract the expected returns, i.e., fitted values from the market model, from the raw stock return. For a given stock, the abnormal return can be written as:

$$AR_t = R_t - \left(\hat{\alpha} + \hat{\beta} \cdot R_t^m\right), \tag{11}$$

where AR_t is the abnormal return of week t, R_t is the stock return and R_t^m the market return of week t. The estimated regression parameters are denoted by $\hat{\alpha}$ (intercept) and $\hat{\beta}$ (slope). Figure 5 plots the mean (cumulative) abnormal return during the estimation period for all banned- and non-banned stocks, respectively. No matter at which graph one looks, it is observable that the lines of banned- and non-banned stocks follow more or less the same pattern. Until mid-March, abnormal stock returns are constantly negative reflecting the downwards direction of the overall stock market. By the time of ban inception, banned stocks were down by roughly 17% compared to mid-January. The orange line in the left panel, i.e., cumulative returns, lies significantly above the blue line after ban inception. This visual evidence suggests that banned stocks are not overpriced when they are subject to a ban and does not support the argument made by Miller (1977). The blue and the orange line in the right panel are nearly identical during the entire estimation period, further suggesting that stock prices of banned- and non-banned stocks do not behave differently. Similar to the abnormal returns plotted here, the market return had its very minimum right at the time when the ban was imposed. A plot of the market return is in Appendix Figure 3. To confirm the visual evidence drawn from the graphs, I estimate another panel regression where I regress the weekly abnormal return on a ban dummy that equals one if a stock is subject to a ban and zero otherwise, thereby implicitly running a diff-in-diff estimation.

Table 11 shows the results of the panel regression. To assess the robustness of the insignificant results, I modify the methodology and re-compute each stock's abnormal return by simply subtracting the equally weighted cross-sectional mean return of all stocks from the raw stock return. The corresponding results are in the right panel in Table 11. Consistent with previous analyses, I report results for the entire sample as well as only for the extreme size quartiles, respectively. The reason why the number of stocks included is significantly higher than in section 4 is that I restrict firms to have only 20 data points to estimate the market model and five data points in the estimation period.[41] Recall that in the previous section, I use only firms without data missing in the estimation period to compute autocorrelations, whereas I can use any abnormal return data point in this analysis. Regarding the regression output, no ban dummy coefficient is even statistically significant at the 10% level and the corresponding t-statistics are all very low. Even though they are not reported, robust F-statistics suggest the models not to be appropriate for the data structure.[42] Overall these findings indicate that stock prices do not exhibit positive returns due to short sale bans.

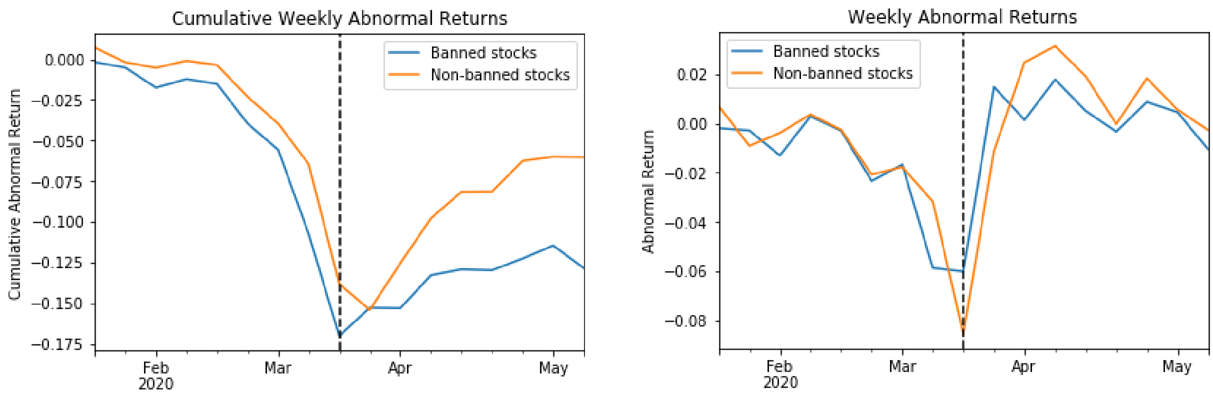


Figure 5: (Cumulative) Abnormal Returns. The left panel plots the average cumulative weekly abnormal return for banned- and non-banned stocks, respectively. The right panel plots the average weekly abnormal return for banned- and non-banned stocks, respectively. The dashed line in both graphs indicates the date of the ban inception on March 18, 2020.

[41] I chose these constraints arbitrarily since I must set restrictions somehow to exclude firms with too many missing data points. The results, however, remain qualitatively unchanged no matter whether I relax or tighten these constraints.

[42] F-statistics test for joint significance of the regression models. Robust F-statistics in my outputs are below one, basically suggesting that the model cannot explain the variation of the data at all.

As already mentioned in the literature section, additional interesting results report Boehmer et al. (2008). They analyze the performance of constructed portfolios that are heavily shorted by institutional investors and find a strong underperformance of these portfolios. This, in turn, means that these types of short sellers are very successful by identifying (especially small) firms that will underperform in the future and shorting them. This outperformance of the short positions, however, is persistent over time, indicating that short sellers do not push stock prices artificially below their fundamental value in the short term, but simply contribute to more efficient markets. The autocorrelations from the previous section of banned- versus non-banned stocks (0.11 versus 0.15) support this argument, however, recall that the difference was not significant at the 1% level. The findings of Boehmer et al. (2008) as well as my empirical findings in this section and previous studies strongly suggest that short sellers should not be blamed for falling stock prices. Therefore, regulatory measures of banning short sales to stabilize stock prices are useless, as there is simply no negative price effect that could be taken measures against.

Table 11: Abnormal Returns

This table provides the panel regression output of the stock price analysis for the entire sample as well as for the two extreme size quartiles. Size is measured by the firm's market value as of January 1, 2020. The dependent variable is the weekly abnormal return, computed by employing a market model and the cross-sectional average as a benchmark, respectively. *Constant* is the intercept; *Ban* is a dummy that equals one if the observation is subject to a short sale ban and zero otherwise. The estimation period is from January 2020 until May 2020. Standard errors are clustered by firm and time. I include time- and firm fixed effects. T-statistics are presented in parentheses below the parameter estimates. *, **, *** denote statistical significance at the 10%, 5% and 1% level, respectively.

	Market Model			Cross-Sectional Average		
Size Quartile	All	Top-25%	Bottom-25%	All	Top-25%	Bottom-25%
Constant	-0.0051***	-0.0028***	-0.0074***	-0.0014	0.0016**	-0.0074***
	(-5.70)	(-3.46)	(-7.69)	(-1.64)	(2.31)	(-8.92)
Ban	-0.0037	-0.0023	-0.013	0.0016	-0.0018	0.0069
	(-0.53)	(-0.32)	(1.33)	(0.27)	(-0.33)	(0.98)
Firm fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Time fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
SE clustered by firm	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
SE clustered by time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
No. of observations	*75,789*	*20,857*	*16,443*	*74,383*	*20,626*	*15,918*
No. of firms	*5,020*	*1,256*	*1,255*	*5,016*	*1,256*	*1,251*

7 Conclusion

In Spring 2020, Austria, Belgium, France, Greece, Italy, and Spain imposed a ban on covered short sales for a period of two months as a response to the volatile financial markets during the first wave of Covid-19. Researchers and policymakers all over the world have discussed short sale bans as a measure to stabilize volatile markets for decades. While some argue that short sellers artificially put negative pressure on stock prices and increase volatility, others say that they are important suppliers of liquidity and contribute to more efficient markets. Short positions account indeed for a large share in the daily trading volume that amounts up to one-third. However, short positions can often be attributed to market makers which are generally exempt from short sale bans anyway.

I use data from 28 European countries (EU plus UK) from 2020 to investigate the impact of short sale bans on the microstructure of European equity markets. Therefore, I analyze liquidity, price efficiency, and wealth effects of stocks that were subject to a short sale ban in comparison to control stocks. My treatment group consists of all stocks from the six ban countries and my control group comprises all stocks from the remaining 22 countries.

The results of the panel regression analysis suggest that the liquidity of stocks in the top size quartile deteriorates due to a short sale ban. This result is consistent across my three liquidity measures (percentage bid-ask spread, Amihud illiquidity ratio, and dollar volume). Furthermore, these findings are mostly robust against a computed control group consisting of a matched (non-banned) stock for each banned stock. I select the matched stocks by running an algorithm that minimizes a distance metric based on the stock price and the market value. Further analysis provides evidence that these liquidity effects are reversed once the ban is lifted again. This reverse effect is consistent across all three liquidity proxies.

Moreover, my results suggest that the speed with which stock prices incorporate firm-specific information does not change due to a short sale ban. Although it is not trivial to capture idiosyncratic information, I follow Beber and Pagano (2013) and use autocorrelations of residuals as a proxy. In contrast to firm- specific information, my results show that the speed with which stock prices incorporate market-wide information slows down due to a short sale ban. I use the STOXX 600 as the overall European market index. However, I do not find any evidence, that this effect is stronger during market downturns. The short time-series in the sub-samples may be an explanation for the lack of evidence.

My last analysis seeks to understand whether regulators can stabilize stock prices by imposing a short sale ban. Based on a market model and on a cross-sectional mean as benchmarks, I do not find any significant outperformance of banned stocks, suggesting that bans do not have any positive impact on stock prices.

Overall, this paper aims to investigate the negative side effects of short sale bans on the market microstructure and whether they are actually a measure to stabilize financial markets. My findings are mostly in line with other studies that examine earlier short sale bans such as those during the great financial crisis in 2007-09. Short sale bans reduce the liquidity of large stocks, reduce price efficiency with respect to market-wide information, and fail to support stock prices. Therefore, regulators should consider and justify very precisely why such measures are necessary in times of stock market turbulences.

Compared to previous studies, the scope of this paper is narrower. This can be attributed to the fact that my sample consists only of European stocks that are all subject to EU regulation and, thus, the measures are partially coordinated among EU member states. As a result, there is less variation in the data for my analysis than in previous studies, e.g., those about the financial crisis in 2007-09 because derivatives trading regulation, disclosure requirements of short positions and the ban period are the same across all six countries with a ban. Furthermore, there is less variation as the ban equally applied to all stocks in the respective country rather than only to stocks from specific industries. Lastly, my study does not distinguish between naked- and covered short selling since naked short selling has been prohibited in the EU since 2012.

The fact that only a few European countries imposed short sale bans, while the majority did not, raises the question for future research of why some regulators see the necessity of short sale regulation. In their robustness check accounting for endogeneity, Beber and Pagano (2013) use the country-wide level of Credit-Default-Swaps (CDS) spreads in the financial sector as an instrumental variable to predict the likelihood for a country to impose a ban.[43] They argue that higher financial distress of a country's financial sector leads to a higher likelihood of the national regulator to impose a ban. However, more research is needed that considers internal political structures of national authorities and external influences on them to help understand the underlying political mechanisms of short selling regulation.

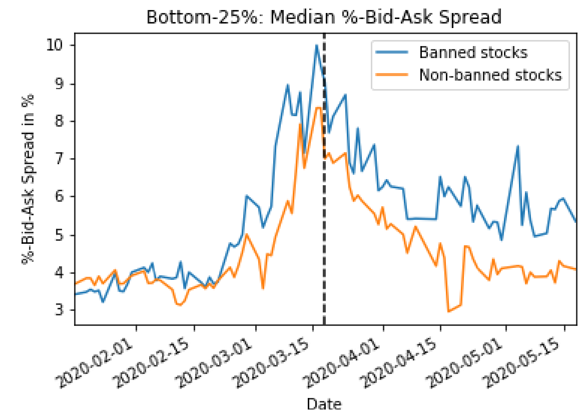
[43] See Beber and Pagano (2013), p. 365. They argue that the higher the CDS spread is, the higher is the financial distress.

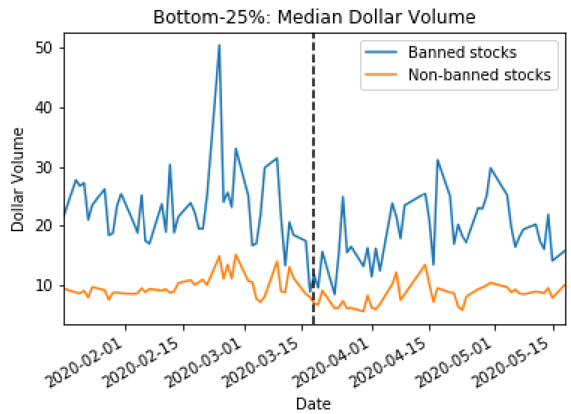
Appendix

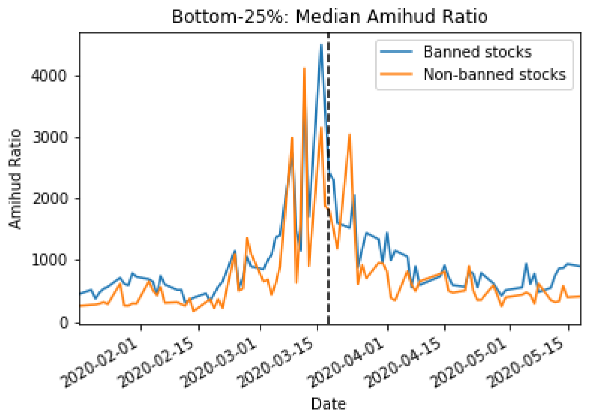
Appendix Table 1: Liquidity Analysis Quartile Regression Output

This table provides, analogously to Table 4, the panel regression output using only the two inner size quartiles of the sample. The dependent variable is the daily liquidity proxy (bid-ask spread, Amihud ratio and dollar volume). For each variable, I estimate the model for the firms in the second (25%-50%) - and third (50%-75%) size quartile. Size is measured by the firm's market value as of January 1, 2020. I include time- and firm fixed effects. *Constant* is the intercept; *Ban* is the dummy which equals one if a stock is subject to a short sale ban and zero otherwise and *Volatility* is the 20-days rolling standard deviation of stock returns. Standard errors are clustered by firm and time. T-statistics are presented in parentheses below the parameter estimates. *, **, *** denote statistical significance at the 10%, 5% and 1% level, respectively.

	Bid-Ask Spread		Amihud Ratio		Dollar Volume	
Size quartile	50%-75%	25%-50%	50%-75%	25%-50%	50%-75%	25%-50%
Constant	0.0195***	0.046***	212.71***	1,591.1***	599.56***	70.342***
	(39.15)	*(38.09)*	*(20.91)*	*(29.70)*	*(19.15)*	*(9.79)*
Ban	-0.0014**	-0.0038**	6.77	-155.34*	-114.91***	6.3859
	(-2.19)	*(-2.28)*	*(0.46)*	*(-1.86)*	*(-2.72)*	*(0.78)*
Volatility	0.0745***	0.0907***	924.97***	-1,484.7	4,542.9***	1,050.6***
	(7.48)	*(3.03)*	*(3.40)*	*(-1.17)*	*(5.51)*	*(6.48)*
Firm fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Day fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
SE clustered by firm	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
SE clustered by time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Number of Observations	*100,429*	*96,092*	*73,812*	*64,520*	*80,849*	*78,193*
Number of Firms	*1,291*	*1,281*	*1,001*	*1,002*	*1,016*	*1,015*





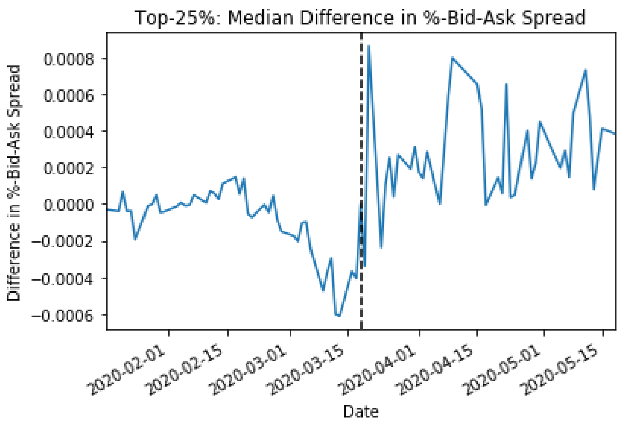


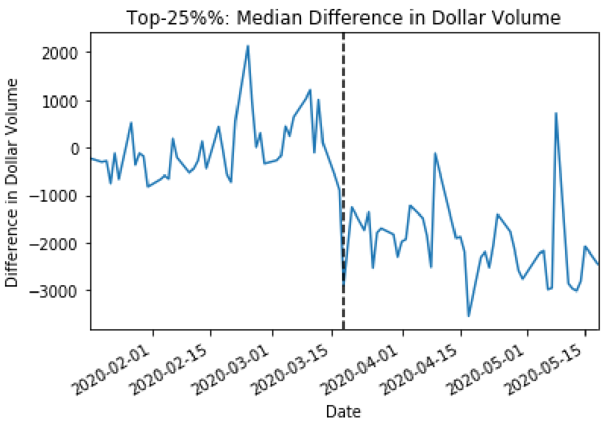
Appendix Figure 1: Mean Liquidity Proxies over Time. For each variable, the median values of small banned stocks and small non-banned stocks, respectively, are plotted over time. The dashed line indicates the date of the ban inception on March 18, 2020. Note that I multiply the original Amihud ratios by 1m to obtain decent values. The dollar volume is expressed in thousand Euros.

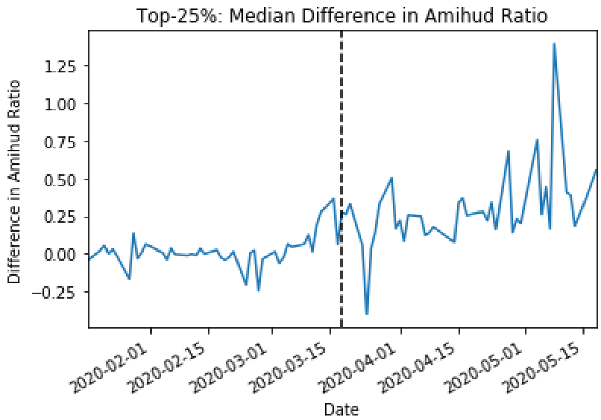
Appendix Table 2: Robustness Check Quartile Regression Output

This table provides, analogously to Table 6, the panel regression output of the robustness analysis. The sample comprises firms in the second- (25%-50%) and third (50%-75%) size quartile, respectively. Size is measured by the firm's market value as of January 1, 2020. The dependent variable is the daily difference of the liquidity proxy between matched stock i and its corresponding matched non-banned stock. Matched stocks are selected by running an algorithm that minimizes the distance metric described in the text. *Constant* is the intercept; *Ban* is a dummy variable that equals one if day t lies in the ban period and zero otherwise. Standard errors are clustered by stock-pair and time. Fixed effects for each stock pair are included. T-statistics are presented in parentheses below the parameter estimates. *, **, *** denote statistical significance at the 10%, 5% and 1% level, respectively.

	Bid-Ask Spread		Amihud Ratio		Dollar Volume	
Size quartile	50%-75%	25%-50%	50%-75%	25%-50%	50%-75%	25%-50%
Constant	-0.001	0.0028	-103.75***	-286.6*	76.31	-45.134**
	(-1.03)	*(1.45)*	*(-3.14)*	*(1.84)*	*(-1.29)*	*(-2.35)*
Ban	-0.0023	0.0091**	43.57	18.91	-468.53***	8.62
	(-1.25)	*(2.24)*	*(0.58)*	*(0.05)*	*(-3.97)*	*(0.22)*
Firm fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Day fixed effects	*No*	*No*	*No*	*No*	*No*	*No*
SE clustered by firm	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
SE clustered by time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Number of Observations	*31,955*	*31,930*	*23,240*	*23,240*	*23,240*	*23,240*
Number of Firms	*385*	*385*	*280*	*280*	*280*	*280*







Appendix Figure 2: Median Liquidity Differential over Time. For each variable, the median values of the difference between large banned stocks and large non-banned stocks are plotted over time. The dashed line indicates the date of the ban inception on March 18, 2020. Note that I multiply the original Amihud ratios by 1m to obtain decent values. The dollar volume is expressed in thousand Euros.

Appendix Table 3: Price Efficiency

This table provides, analogously to Table 9, the results of the price efficiency analysis. The left panel called "Residual Autocorrelation" shows the median autocorrelation of market model residuals during ban- and non-ban periods, respectively, for the second (25%-50%) and third (50%-75%) size quartile. The right panel called "Price Delay Measure d_1" shows the median value of the price delay measure described in the text for banned- and non-banned observations, respectively, for the second and third size quartile. Size is measured by the firm's market value as of January 1, 2020. *Ban* is a dummy that equals 1 if the observations are in the ban period and zero otherwise. Two-sided p-values of the test for the equality of medians between banned- and non-banned observations are shown in parentheses. They are computed based on a Mann-Whitney-U test. The number of all firms included is presented in the bottom row.

	Residual Autocorrelation		Price Delay Measure d1	
Size Quartile	50%-75%	25%-50%	50%-75%	25%-50%
Ban=0	-0.12	-0.08	0.05	0.07
Ban=1	-0.20	-0.13	0.22	0.19
Difference	(0.043)	(0.053)	(0.000)	(0.000)
No. of firms	*499*	*499*	*499*	*499*

Appendix Table 4: Bull- and Bear Markets

This table provides, analogously to Table 10, the results of the price delay measures during bull- and bear markets, respectively. For the second (25%-50%) and third (50%-75%) size quartile, it contains the median values of the price delay measure d_1 for banned- and non-banned observations. In the left panel called "Bull Markets" I include only weeks with positive lagged market returns whereas in the right panel "Bear Markets" I include only weeks with negative lagged market returns. Size is measured by the firm's market value as of January 1, 2020. *Ban* is a dummy that equals 1 if the observations are in the ban period and zero otherwise. Two-sided p-values of the test for the equality of medians between banned- and non-banned observations are shown in parentheses. They are computed based on a Mann-Whitney-U test. The number of all firms included is presented in the bottom row.

	Bull Markets		Bear Markets	
Size Quartile	50%-75%	25%-50%	50%-75%	25%-50%
Ban=0	0.09	0.18	0.09	0.11
Ban=1	0.45	0.60	0.22	0.27
Difference	(0.000)	(0.000)	(0.000)	(0.000)
No. of firms	*499*	*499*	*499*	*499*

Computation of Price Delay Measures for Bull- and Bear Markets

My observations, i.e., the price delay measure in section 5, exhibit three different dimensions: Whether the observation belongs to a stock from one of the six ban countries, whether the observations lies in the pre- or during ban period and whether the observations is made one week after a positive market return (bull market) or one week after a negative market return (bear market). This would leave me with eight sub-samples. Since I do not distinguish between observations from the remaining 22 countries that lie in the pre- or during ban period (as these observations are never subject to a ban), I reduce the number of sub-samples to a total of six: The following table presents these combinations and the corresponding ban dummy:

Appendix Table 5: Computation of Sub-Samples

This table displays the different sub-samples to compute the output in table 10 in section 5. *Country* can be either a ban country (Austria, Belgium, France, Greece, Italy or Spain), or a non-ban country (the remaining 22 countries). *Market* can be a bull (bear) market, where the previous week's market return is positive (negative). *Sub-period* can be either pre (8 weeks prior to the ban inception) or during (the ban period from March 18 until May 18). The *Ban Dummy* indicates whether the observations in the sub-sample are subject to the ban (Ban=1) or not (Ban=0).

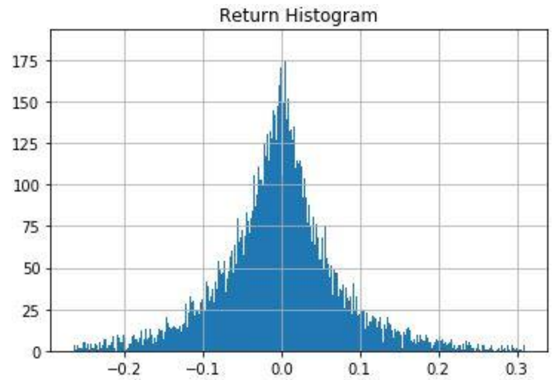
Sub-Sample	Country	Market	Sub-period	Ban Dummy
1	Ban country	Bear	Pre	Ban = 0
2	Ban country	Bull	Pre	Ban = 0
3	Ban country	Bear	During	Ban = 1
4	Ban country	Bull	During	Ban = 1
5	Non-ban country	Bear	Both	Ban = 0
6	Non-ban country	Bull	Both	Ban = 0

The observations of sub-sample number 1 are added to those of number 5, and those of number 2 are added to number 6. This leaves me with a 2x2 matrix with the dimensions "Ban" (banned- or non-banned observation) and "Market" (bull- or bear market). I compute the cross-sectional median of all four groups and conduct statistical inference based on a Mann-Whitney-U test that tests for the equality of the medians. Note that, consistent with previous analyses in this paper, I extend the analysis by applying this procedure also to only the top- and bottom size quartile, respectively. The computations for the right panel in table 9 in section 5 are similar, but without the distinction between bull- and bear markets.

Appendix Table 6: Weekly Stock Returns

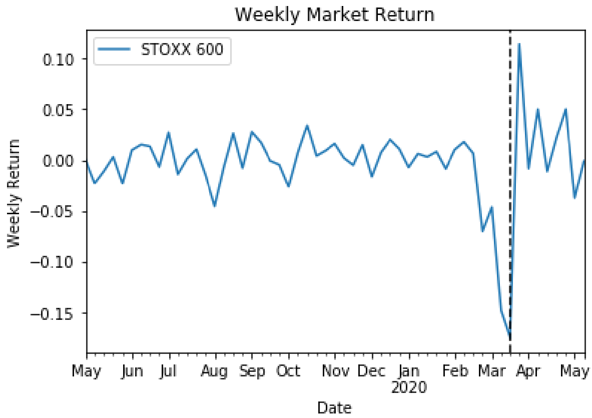
This table provides descriptive statistics of the weekly return data from section 5. Returns are computed as the difference between the natural logarithm of the stock price on day t and on day t-1. The data has been edited as described in the text. Returns are computed between Wednesdays.

	Weekly Stock Return
No. of observations	31,936
No. of stocks	1,996
Mean	-0.001753
Std.	0.077531
Min.	-0.264991
25%	-0.041673
50%	-0.002439
75%	0.035487
Max.	0.310155
Kurtosis	1.47
Skewness	0.16



Appendix Figure 3: Histogram. The graph plots the empirical distribution of the weekly return data from section 5. The vertical axis denotes the absolute frequency. The horizontal axis denotes the weekly stock return. The data is divided into 1,000 bins.

Appendix



Appendix Figure 4: Market Return. The graph plots the weekly log-return of the STOXX 600. Returns are computed between Wednesdays. The dashed line indicates the date of the ban inception.

Appendix Table 7: Abnormal Returns

This table provides, analogously to Table 10, the panel regression output of the stock price analysis for the second (25%-50%) and third (50%-75%) size quartile. Size is measured by market value (as of January 1, 2020). The dependent variable is the weekly abnormal return (computed by employing a market model and the cross-sectional average, respectively). *Constant* is the intercept; *Ban* is a dummy that equals one if the observation is subject to a short sale ban and zero otherwise. The estimation period is from January 2020 until May 2020. Standard errors are clustered by firm and time. I include time-and firm fixed effects. T-statistics are presented in parentheses below the parameter estimates. *, **, *** denote statistical significance at the 10%, 5% and 1% level, respectively.

	Market Model		Cross-Sectional Average	
Size Quartile	50%-75%	25%-50%	50%-75%	25%-50%
Constant	-0.0058	-0.047	0.0011	-0.0013
	(-5.50)	*(-5.89)*	*(0.95)*	*(1.51)*
Ban	-0.0138	-0.0100	0.0003	0.0016
	(-1.53)	*(-1.29)*	*(0.04)*	*(0.23)*
Firm fixed effects	*Yes*	*Yes*	*Yes*	*Yes*
Time fixed effects	*Yes*	*Yes*	*Yes*	*Yes*
SE clustered by firm	*Yes*	*Yes*	*Yes*	*Yes*
SE clustered by time	*Yes*	*Yes*	*Yes*	*Yes*
No. of observations	*19,763*	*18,696*	*19,525*	*18,344*
No. of firms	*1,254*	*1,254*	*1,254*	*1,253*

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